Multi-Dimensional Teacher Effects

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Abstract

I estimate the covariance structure of teacher effects on several outcomes: present and future test scores, present and future attendance, and high school graduation. Studying the covariance matrix of teacher effects reveals the magnitude of teacher effects on each outcome and the relationship between teacher effects on different outcomes while sidestepping the need to estimate individual teacher effects.

Teachers have substantial effects on high school graduation, and on test scores and attendance four years in the future. Students of a teacher who is one standard deviation above average at improving graduation rates are 5 percentage points more likely to graduate high school. Although teacher quality is an important determinant of graduation rates and of future test scores and attendance, long-term effects cannot be predicted well by short-term effects. For example, even if teacher effects on contemporaneous outcomes were perfectly measured, they would only explain about 3% of the variance in teacher effects on high school graduation. My results also suggest that teacher effects on attendance could be an important supplement to score-based measures of teacher value-added. Teachers who improve attendance tend to improve high school graduation rates. However, teacher effects on attendance are only weakly correlated with effects on test scores. But directly evaluating teachers based on their effects on long-term learning and school persistence would be much better, when feasible, because teacher effects on long-term outcomes are poorly captured by teacher effects on short-term outcomes. Factor analysis shows that the correlation matrix of teacher effects can be represented by three interpretable components.

Teacher quality is an important determinant of academic achievement (Chetty *et al.* (2014a),Kane and Staiger (2008), Hanushek and Rivkin (2006)). Many states require schools to use test score-based value-added measures in teacher evaluation. However, measures of teacher quality based on short-run test scores may be highly incomplete, because they neglect teacher effects on non-test score measures and on long-term outcomes. Education influences students' "non-cognitive skills" or "character skills"; these skills, important in life and the labor market, are not well-captured by test scores (Carneiro *et al.*, 2007). If teachers influence their students in ways that are not reflected in test scores, score-based value-added measures miss important components of teacher quality. Furthermore, if teacher effects on short-run outcomes are poor proxies for teacher effects on long-run outcomes, value-added measures based on short-term measurements will be highly incomplete. In this paper, I estimate the variance of teacher effects on students' test scores and attendance, both

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contemporaneously and up to four years in the future, and on high school graduation. I estimate the covariance structure of teacher effects on various outcomes and, using these covariances, investigate the relationships between teacher effects in different domains; how quickly the effects of teachers who improve a short-term outcome fade out; and how well short-term teacher effects predict long term teacher effects.

Using administrative data from the New York City public elementary and middle schools, I find that teachers' causal effects on attendance — their "attendance value-added" — are highly variable; a teacher whose attendance value-added is one standard deviation above average improves her students' attendance by about 0.07 standard deviations, or 1.5 days. The correlation between a teacher's effect on test scores and her effect on attendance is only about 0.12 to 0.15 [1], implying that teacher quality metrics that use only test scores miss much of a teacher's impact on her students. Much like teacher effects on test scores, these effects fade out quickly: students of a teacher who improves test scores or attendance have test scores or attendance four years later that are barely better than would be predicted by demographic characteristics.

However, teacher quality is an important determinant of high school graduation, future test scores, and future attendance. The presence of both substantial long-term effects and of effects that fade out quickly is not a paradox: the teachers who improve test scores in the long term are not particularly likely to be the teachers who improve test scores in the short term. The lack of similarity between teacher effects on short-term outcomes and on long-term outcomes highlights the presence of a multi-tasking problem: if improving easily-observed outcomes like same-year test scores and attendance is a very different from improving more welfare-relevant outcomes like graduation, incentivizing the more easily observed outcomes may have perverse effects. A teacher who is one standard deviation above average at improving her students' high school graduation rates increases graduation rates by 5 percentage points and graduation with an Advanced Regents designation by 6 percentage points, but only 3% of this variation can be predicted using immediately-available data (same-year test scores and attendance).

Methodologically, I build on the teacher value-added literature by extending conventional methods to treat value-added as a vector rather than a scalar. Teacher value-added methods typically estimate both the variance of teachers' causal effects on their students' outcomes and an individual causal effect, or "value-added", for each teacher; my method sidesteps the need to estimate individual value-added scores. My model is very similar to other value-added models in using variance decompositions and "moment-matching" to estimate what portion of variance in outcomes is due to teachers. These models must avoid giving teachers credit for receiving more able students rather than for their causal effects on test scores. They typically achieve this by controlling for a rich set of student covariates, such as demographic factors and previous test scores; thus, these models estimate the value a teacher *adds* to a student above what that student would achieve with an average teacher. This literature typically finds that teachers vary moderately to largely in their effects on students: teachers account for about 2% of the variance in test scores. In other words, a teacher who is one standard deviation above average in her effectiveness at increasing test scores ("score VA") increases her students' test scores by an average of 0.1 standard

[1] By contrast, the correlation between teacher effects on math test scores and on English Language Arts test scores is 0.66. See Table 11.

deviations. I extend this methodology by incorporating a variety of different outcomes, including leads of outcomes, and estimating not only the variance of teacher effects but the covariance of effects on different measures.

Value-added measures based on short-term test scores have become a common component of teacher evaluations, but there are reasons to suspect that these measures are incomplete, motivating a focus on other aspects of teacher effects. Chetty *et al.* (2014b) shows that students of teachers who improve test scores are more likely to go to college, have higher incomes, and live in better neighborhoods as adults, but these effects appear to be too large to be explicable by the increase in academic achievement implied by test score gains. And studies that do not specifically involve teachers find that quantity and quality of education improve skills that are not captured by test scores, but the mechanisms for this are unclear.

Rewarding teachers based on test scores is unpopular with many parents, partially due to concerns that test scores reflect only part of teachers' beneficial effects on their students.[2] If so, policymakers face a multitasking problem in the spirit of Holmstrom and Milgrom (1991): we want teachers to make their students motivated, persistent, creative, and informed, but we can't measure those characteristics. Teachers who are incentivized to increase test scores may behave in counterproductive ways. This concern has empirical merit: Teachers or administrators under low to moderate incentives to improve test scores cheat or manipulate scores (Jacob and Levitt (2003), Dee *et al.* (2016), Loughran and Comiskey (1999)), spend less time on non-tested subjects (Jacob, 2005), spend much more time on test preparation (Klein *et al.* (2000)), and move students into special education so that those students will not be counted in school progress indicators (Figlio and Getzler (2002), Jacob (2005)).

One area that could be given short shrift by an increasing focus on test scores is non-cognitive, or character, skills. Education is important in transmitting these skills, such as the drive and persistence to attend school and work hard, and while the mechanisms are poorly understood, teachers may be an important factor. In addition to test scores, I include two outcomes that plausibly reflect non-cognitive skills: attendance and high school graduation. Recent papers show that teachers influence such outcomes in the short term. Gershenson (2016) studies third through fifth graders in North Carolina and finds that teachers have "arguably causal, statistically significant effects on student absences that persist over time," and that "teachers who improve test scores do not necessarily improve student attendance." Similarly, Jackson (2016) studies ninth graders in North Carolina and finds that teachers have medium-term effects on student absences, suspensions, grades, and on-time grade progression.

My results replicate those of Jackson (2016) for teacher effects on test scores, attendance, and their relationship to graduation, where applicable. Jackson finds that teacher effects on test scores correlate with effects on a "behavioral factor" at 0.16, and I find that effects on test scores correlate with effects on attendance at 0.12 to 0.15. Jackson finds that increasing teacher value-added on test scores by one standard deviation increases high school graduation by 0.13 percentage points, and a one standard deviation increase in the behavioral factor increases graduation by 0.78 percentage points. I find that increasing

[2] 69% of New York State parents say that teacher pay "should not be based on how well their students perform on standardized tests," while 26% say it should be noa (2015).

value-added on test scores does not have a statistically significant effect on graduation, and increasing value-added on attendance by one standard deviation would increase graduation by 0.72 percentage points. We both find that using attendance and test scores instead of just test scores to predict teacher effects on graduation more than doubles predictive validity.

However, I emphasize that the ability to predict longer-term outcomes like graduation using short-term outcomes is quite low. Even if perfectly measured, teacher effects on attendance and test scores could only explain 3% of the variance in teacher effects on graduation (and 6% of the variation in graduating with an Advanced Regents designation). Raising teachers' "attendance value-added" by one standard deviation, and leaving the covariance of teacher effects on other outcomes intact, would boost graduation rates by 0.72 percentage points, while raising "graduation value-added" by one standard deviation would increase graduation rates by 5 percentage points. A similar result appears in Chamberlain (2013), who finds that teacher effects on test scores capture less than 20% of teacher effects on college graduation.

Recent policy changes hint at a shift in focus away from test scores and towards quantitative evaluations that incorporate other metrics. The federal Every Student Succeeds Act of 2015 (ESSA) mandates that each state measure school quality based on a metric of its own devising that includes test scores but also includes at least one substantially different outcome. Many states have chosen to measure and reward attendance.

In this paper, I demonstrate that teachers influence medium-term outcomes (high school graduation and test scores four years ahead), that immediate effects on test scores are a poor proxy for medium-term effects, and that teacher effects on attendance are slightly smaller and about as persistent as teacher effects on test scores.

My estimates are only credible insofar as identification restrictions are satisfied. Teachers must be sorted to students only on observables, meaning that conditional on covariates, no teacher should be systematically assigned students who have unobservable characteristics that cause high or low performance. The rich, longitudinal nature of my data makes it possible to control for a variety of student and classroom characteristics and lagged values of outcomes, making the sorting on observables requirement plausible. For example, it is possible that high-SES students or students who have been improving relative to their peers are, on average, assigned to better teachers. But since I observe and control for ethnicity, free lunch status, lagged values of test scores and attendance, and class-level means of these variables, this sorting would be predictable from the control variables and would not violate the sorting on observables restriction. I use a pre-trend test as an empirical check of this restriction. Although other authors have found that controlling for lagged scores is sufficient to find unbiased measures of parameter estimates (Chetty *et al.*, 2014a), I measure a small but statistically significant pre-trend in which teacher effects on contemporaneous outcomes "predict" past achievement. Results are robust to controlling for more lags, including higher-order interactions, and using a different estimator.

This paper also provides descriptive evidence on patterns of absenteeism. The patterns documented are consistent with poor and minority students often missing school voluntarily or for reasons other than illness. Students in New York City are absent extremely often, and chronic absenteeism — missing more than 10% of a school year — is common. Students are far more likely to be absent in later grades, and there are large ethnic gaps in school attendance.

This paper proceeds as follows. In Section 1, I recap the literature on teacher value-added and the influence of education on non-cognitive skills. In Section 2, I develop a model in which student outcomes like test scores or attendance are a function of teacher effects, covariates, and random shocks. In Section 3, I describe the data and provide descriptive evidence on the pervasiveness of poor attendance in the New York City public schools and correlates of poor attendance. Section 4 describes the estimation procedure and the conditions under which parameters of interest are identified. Section 5 contains results on the distribution of teacher effects on student attendance and test scores; the persistence of these effects; the usefulness of contemporaneous effects for predicting teacher effects on future student achievement; and factor analysis of the correlation matrix of teacher effects. Section 6 demonstrates that results are robust to using a different estimator – maximum likelihood rather than moment-matching – and to different choices of covariates. Section 7 concludes.

1 Literature

Skills that aren't well captured by traditional educational metrics, often termed "non-cognitive skills" or "character skills", are correlated with many outcomes, including earnings, educational attainment, health, and crime.

However, there is little direct evidence on the degree to which education — and teachers in particular — affects character skills, despite the growing literature on teacher effects on test scores, and the use of test score-based value-added measures in large school systems such as New York City, Los Angeles, Chicago, and Washington, DC. Jackson (2016) and Gershenson (2016) examine the effects of teachers on outcomes other than test scores. Gershenson studies third through fifth graders in North Carolina and finds that teachers have "arguably causal, statistically significant effects on student absences that persist over time," and that "teachers who improve test scores do not necessarily improve student attendance." Similarly, Jackson (2016) studies ninth graders in North Carolina and finds that teachers have medium-term effects on student absences, suspensions, grades, and on-time grade progression, and that teacher effects on test scores have modest correlations with teacher effects on behavioral variables. This paper is similar in spirit, but tracks teacher effects over a longer time period.

There are reasons to suspect that teachers may affect their students in the long term in ways that are not captured in test scores. First, increases in the quality or quantity of education are correlated with measures of socioeconomic success, even after conditioning on test scores. For example, Heckman and Rubinstein (2001) shows that conditional on AFQT scores, GED recipients earn less than high school graduates who do not attend college. (Chetty *et al.*, 2014b) show that teachers who improve test scores also cause their students to have higher incomes, attend college, and live in better neighborhoods. The mechanisms for this are unclear, since teacher effects on test scores fade out dramatically after several years (Chetty *et al.* (2014b), Chetty *et al.* (2011)); test scores effects alone do not seem sufficient to explain the magnitude of teacher effects on long-term outcomes. Evidence from Project STAR suggests that kindergarten "class quality has significant impacts on non-cognitive measures in fourth and eighth grade such as effort, initiative, and lack of disruptive behavior," and that high-quality kindergarten classes improve test scores in the short run, but it is not clear whether these effects are due to teacher quality, peer effects, or some other factor (Chetty *et al.*, 2011). In summary, teachers may impact their students' persistence and motivation, and these effects may be more meaningful or persistent than teacher effects on test scores.

Ultimately, we would like to know how important teachers are as a determinant of meaningful long-term outcomes like high school graduation. Even if we could perfectly measure teacher effects on test scores and other short-term outcomes, these teacher effects are only useful insofar as the proxy for more welfare-relevant effects. In this project, I demonstrate that teachers have significant effects on the medium-term outcomes of future test scores and high school graduation. Short-term teacher effects, however well-measured, are a poor proxy for long-term teacher effects, but teacher effects on attendance and on English test scores do help predict teacher effects on graduation.

2 Model

I follow Chamberlain (2013) in developing a model that defines teacher effects as best linear predictor coefficients. In order to preserve independence across teachers, I ensure that the same student does not appear in the data more than once by treating each grade separately and by dropping students who repeat a grade. [3] Thereafter, there is only one observation per student, so I index by student i and use $j(i)$ to refer to the teacher of student i and $c(i)$ to refer to teacher-year cohorts. That is, if $j(i) = j(i')$ but $c(i) \neq c(i')$, then students i and i' are taught by the same teacher but in different years. Subscripts index observations, and superscripts index outcomes. There are $H = 31$ outcomes: Math test scores, reading test scores, attendance z-scores, four years of leads and four years of lags for each of those variables, and indicators for four-year high school graduation, four-year high school graduation with a Regents diploma, and four-year high school graduation with Advanced Regents diploma. Outcomes are $y_i \in \mathbb{R}^H$ and $x_i \in \mathbb{R}^K$. Teacher j's effect on outcomes, $\mu_j \in \mathbb{R}^H$, is defined as a best linear predictor coefficient. For each outcome h,

$$y_i^h \equiv x_i^T \beta^h + \mu_{j(i)}^h + \nu_i^h$$

In order to interpret parameter estimates causally, we need sorting of students to teachers to be based on observables. In order to estimate β^h consistently, we need that errors be orthogonal to covariates and teacher quality: $\nu_i^h \perp x_i, \mu_{j(i)} \quad \forall h$. In addition, unobservable shocks to outcomes need to be independent of the teacher's identity, conditional on covariates. This restriction is necessary so that, when estimating variances, we don't mistake the tendency for some teachers to consistently receive better or worse students for the presence of teachers who consistently teach well or poorly. Imagine that all teachers are identical — $\mu_j = 0 \quad \forall j$ — but some teachers are consistently assigned students with high or low shocks. Some teachers will consistently have students who over- or under-perform what would be expected from their covariates, making it appear that teachers vary in quality when they do not.

[3] After estimating the covariance of teacher effects for each grade, I pool across grades with a frequency-weighted mean.

I further assume that errors across different classrooms are orthogonal: $\mathbb{E}\left[\nu_i \nu_{i'} \left| c(i) \neq c(i') \right.\right] = 0$. The parameter of interest is the covariance matrix of teacher effects,

$$\text{Var}\left(\mu_j\right) \equiv \Sigma_\mu \in \mathbb{R}^{H \times H} \tag{1}$$

3 Data, Setting, and Descriptive Statistics

The data includes almost all New York City public school students in grades preschool through 12 in the 2001-02 to 2015-16 school years. I observe rich individual-level data and can track students across years. For each student, I can observe several outcomes of interest: Test scores, attendance, and what type of diploma the student received.

While my data includes 9.4 million student-year observations that include demographic and attendance information, the effective size of the data is smaller. I can only link teachers to students starting in the 2005-06 school year, although I use observations from earlier years to construct lagged variables and for pre-trend tests. I avoid dropping observations due to missing data in independent variables. Instead, I impute the missing field as the average for that student's grade and year and also use an indicator variable for missingness. This happens most often when lagged variables are missing because the student recently moved into the district. However, I do require one lagged test score to be present.

I observe demographic information on each student. My data contains each student's ethnicity and date of birth, which are filled in by parents when the student enters the school system. Other information is recorded by the school administration: grade level, number of days absent, number of days present, and whether the student is in special education. I also observe the student's registrar data, which explains whether the student is still enrolled, whether the teacher has graduated and with what type of diploma, whether the student has a disability requiring an Individualized Education Program (IEP), and whether the student has dropped out.

New York State has a tiered system of high school diplomas. High school students must take standardized examinations known as Regents exams, and those who pass exams in global history, U.S. history, ELA, math, and science graduate with a "Regents diploma." Until the 2011-2012 school year, students who met their high school's graduation but did not meet the requirements for a Regents diploma earned a less prestigious "local diploma"; now, students cannot earn a local diploma unless they have a disability. There also exist diplomas that are harder to attain than a Regents diploma. [4] Students who pass additional exams earn a Regents Diploma with Advanced Designation, and students who satisfy the requirements of the Advanced Designation and attain high scores can attain a "Regents with Advanced Designation with Honors."

In addition to the high school Regents exams, students take standardized math and English Language Arts (ELA) tests every year in third through eighth grade. Starting in spring 2013, these tests have been based on Common Core standards.

Math and ELA scores are scored on a scale that varies every year. Therefore, I normalize test scores to have a mean of zero and variance of 1 within each grade and year. Normalization obscures the fact that New York City's test scores rose dramatically over this period,

[4]When the following results refer to a Regents Diploma, this refers to a Regents Diploma that is *not* with an Advanced Designation, so it is ambiguous whether increasing the number of Regents Diplomas is positive.

	Mean	St. Dev	Min	Max	Missing
Grade	5.65	3.94	-1	12	0.07%
Year	2009.39	3.11	2005	2015	0%
Disabled	0.15	0.36	0	1	0%
Female	0.49	0.50	0	1	0%
English Language Learner	0.13	0.34	0	1	0%
Free Lunch	0.77	0.42	0	1	0%
Days absent	16.19	22.16	0	187	6.39%
Days present	154.88	35.20	0	329	6.39%
Days Absent Lag (Z-Score)	0.03	0.97	-16.45	1.40	28.17%
Math Score (Z-Score)	0.01	1.00	-10.00	3.96	60.38%
Math score lag (Z-Score)	0.00	0.99	-10.00	3.96	66.3%
ELA Score (Z-Score)	0.00	1.00	-11.10	7.76	61.17%
ELA Score Lag (Z-Score)	-0.00	0.99	-11.10	7.76	67.14%
4-Year Graduation	0.59	0.49	0	1	70.32%
4-Year Graduation, Regents Diploma	0.35	0.48	0	1	68.24%
4-Year Graduation, Advanced Regents Diploma	0.15	0.36	0	1	67.49%
N = 10,000,453					

Table 1: *Student summary statistics, for whole sample. A grade level of -1 is pre-kindergarten.*

both in terms of the percentage of students scoring at the proficient level and in comparison to the rest of New York State. Four-year graduation rates rose from 45.5% for students starting ninth grade in 2001 to 70.5% for students starting ninth grade in 2011.

3.1 Descriptive Statistics

Table 1 contains summary statistics for the whole sample of 10 million students, and Table 2 contains summary statistics for the sample that is used for estimation, containing 2.5 million students in grades 4 through 8 who can be matched to a teacher. The district is relatively poor, with 77% of students qualifying for free or reduced-price lunch. A plurality of students are Hispanic. Although about 43% of students, as of 2013, speak a language other than English at home, only 13% of students are classified as English Language Learners (ELLs). English Language Learners are students who either take a class in English as a New Language or participate in bilingual education.

3.1.1 Attendance

Student absence is frequent in the New York City schools. Descriptive, non-causal evidence suggests that attendance matters for student achievement. Poor attendance is associated with lower test scores and a lower likelihood of graduating high school, and there are large socioeconomic gaps in attendance. Although this data cannot explain why students miss school so often, it is consistent with the hypothesis that students are absent far more often than necessitated by illness. The average New York City public school student is absent 16

	Mean	St. Dev	Min	Max	Missing
Grade	5.98	1.42	4	8	0%
Year	2009.21	2.08	2006	2013	0%
Disabled	0.16	0.37	0	1	0%
Female	0.49	0.50	0	1	0%
English Language Learner	0.12	0.32	0	1	0%
Free Lunch	0.83	0.37	0	1	0%
Days absent	11.43	11.95	0	182	0%
Days present	169.83	12.96	2	186	0%
Days Absent Lag (Z-Score)	0.06	0.89	-14.66	1.34	2.91%
Math Score (Z-Score)	0.07	0.97	-6.35	3.89	0%
Math score lag (Z-Score)	0.06	0.97	-10.00	3.96	5.34%
ELA Score (Z-Score)	0.05	0.98	-11.10	7.76	0%
ELA Score Lag (Z-Score)	0.05	0.97	-11.10	7.76	8.08%
4-Year Graduation	0.68	0.47	0	1	66.47%
4-Year Graduation, Regents Diploma	0.45	0.50	0	1	66.46%
4-Year Graduation, Advanced Regents Diploma	0.20	0.40	0	1	66.45%
N = 2,455,257					

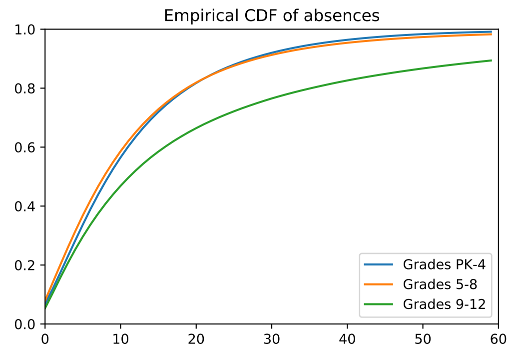
Table 2: *Student summary statistics, for students who can be matched to teachers.*

days in an approximately 180-day school year, or 9%. By comparison, the average student nationally is absent on about 7% of days.

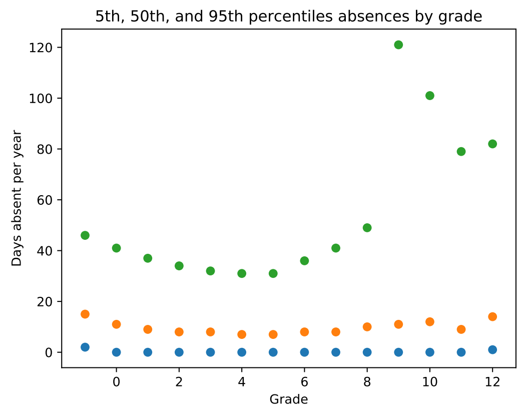
Attendance deteriorates dramatically across grades, as shown in Figure 1a. In each grade, about 8% of students are never absent. However, high school students are absent far more often than elementary or middle school students, especially in the right tail. Figure 1b shows this by plotting the fifth, fiftieth, and ninety-fifth percentiles of absences within each grade.

Figure 2 illustrates the large ethnic gaps in school attendance. In any grade, Hispanic, Black, and Native American students are absent almost twice as often as Asian students, with non-Hispanic White and multi-racial students in the middle.

Tables 3, 4, 5, and 6 show coefficients from regressing ELA scores, math scores, attendance, and high school graduation on student demographic characteristics, with and without lagged values of outcomes. All outcomes have been z-scored within grade-year, so coefficients are comparable in magnitude. A student with an attendance z-score of 1 is present one standard deviation more than other students in her grade and year. Although coefficients on regressions with ELA scores (Table 3) and with math scores (Table 4) as the dependent variable have similar coefficients, they are not similar to the coefficients from predicting attendance (Table 5). The only coefficients that are large in magnitude for predicting attendance are coefficients on lagged values of outcomes and having an Individualized Educational Plan (an IEP, for students with disabilities). Despite the large ethnic gaps in school attendance, attendance is hard to predict (within grade-years), with an R^2 of only 0.06 in a regression that includes ethnicity, indicators for common home languages, year dummies, and a student's status as disabled, ELL, receiving free lunch,



(a) *Empirical CDF of absences in elementary schools (grades PK through 4), middle schools (grades 5 through 8), and high schools (grades 9-12).*



(b) *Percentiles of attendance in each grade.*

Figure 1

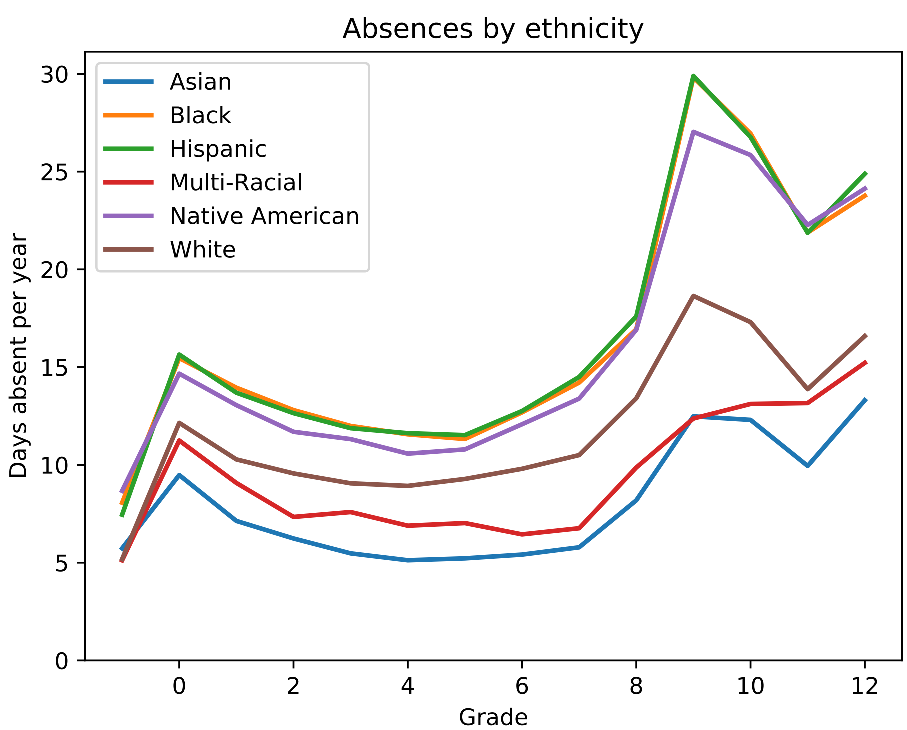


Figure 2: *Average number of days absent by ethnicity.*

	ELA Score (Normalized)		
	0	1	2
Constant	0.797***	0.513***	0.477***
	(0.003)	(0.002)	(0.002)
English Language Learner	-0.868***	-0.553***	-0.554***
	(0.002)	(0.001)	(0.001)
Female	0.147***	0.125***	0.123***
	(0.001)	(0.001)	(0.001)
Free Lunch	-0.306***	-0.194***	-0.179***
	(0.001)	(0.001)	(0.001)
IEP	-0.829***	-0.497***	-0.475***
	(0.001)	(0.001)	(0.001)
Attendance (Z Score)			0.13***
			(0.001)
Missing Attendance			-0.312***
			(0.006)
Lag ELA Score		0.379***	0.379***
		(0.001)	(0.001)
Lag Math Score		0.198***	0.196***
		(0.001)	(0.001)
Lagged Attendance		0.018***	-0.068***
		(0.001)	(0.001)
Lagged Values Missing		-0.106***	-0.105***
		(0.001)	(0.001)
Ethnicity	29869.5	13195.2	12461.8
	(0.0)	(0.0)	(0.0)
Home Language	1694.84	689.174	595.075
	(0.0)	(0.0)	(0.0)
Year	546.815	173.296	151.73
	(0.0)	(0.0)	(0.0)
N	3.9 M	3.9 M	3.9 M
R-squared	0.305656	0.446884	0.456547

Table 3: *Predictors of English Language Arts scores.*

Notes. For categorical variables – ethnicity, home language, and year – an F-statistic and its p-value are shown. Other variables have standard errors in parentheses. *** indicates significance at the 0.1% level.

	Math Score (Normalized)		
	0	1	2
Constant	0.87***	0.557***	0.505***
	(0.003)	(0.002)	(0.002)
English Language Learner	-0.655***	-0.37***	-0.382***
	(0.001)	(0.001)	(0.001)
Female	-0.02***	-0.014***	-0.018***
	(0.001)	(0.001)	(0.001)
Free Lunch	-0.266***	-0.151***	-0.131***
	(0.001)	(0.001)	(0.001)
IEP	-0.782***	-0.434***	-0.404***
	(0.001)	(0.001)	(0.001)
Attendance (Z Score)			0.191***
			(0.0)
Missing Attendance			-0.756***
			(0.005)
Lag ELA Score		0.11***	0.109***
		(0.001)	(0.001)
Lag Math Score		0.49***	0.486***
		(0.001)	(0.001)
Lagged Attendance		0.066***	-0.06***
		(0.001)	(0.001)
Lagged Values Missing		-0.113***	-0.102***
		(0.001)	(0.001)
Ethnicity	41380	20031.1	18964.4
	(0.0)	(0.0)	(0.0)
Home Language	6136.21	3397.84	2853.06
	(0.0)	(0.0)	(0.0)
Year	130.131	664.138	505.403
	(0.0)	(0.0)	(0.0)
N	4.0M	4.0M	4.0M
R-squared	0.297017	0.463534	0.486825

Table 4: *Predictors of math scores.*

Notes. For categorical variables – ethnicity, home language, and year – an F-statistic and its p-value are shown. Other variables have standard errors in parentheses. *** indicates significance at the 0.1% level.

	Attendance (Normalized)	
	0	1
Constant	0.462***	0.413***
	(0.002)	(0.002)
English Language Learner	-0.046***	0.011***
	(0.001)	(0.001)
Female	0.027***	0.021***
	(0.001)	(0.001)
Free Lunch	-0.125***	-0.093***
	(0.001)	(0.001)
IEP	-0.306***	-0.234***
	(0.001)	(0.001)
Lag ELA Score		-0.003***
		(0.001)
Lag Math Score		-0.01***
		(0.001)
Lagged Attendance		0.645***
		(0.001)
Lagged Values Missing		-0.095***
		(0.001)
Ethnicity	18691.9	13103.3
	(0.0)	(0.0)
Home Language	11216.6	6512.18
	(0.0)	(0.0)
Year	26.3365	261.221
	(0.0)	(0.0)
N	9.4M	9.4M
R-squared	0.064287	0.178718

Table 5: *Predictors of attendance.*

Notes. For categorical variables – ethnicity, home language, and year – an F-statistic and its p-value are shown. Other variables have standard errors in parentheses. *** indicates significance at the 0.1% level.

	Graduation	
	0	1
Constant	0.513*	0.355*
	(0.31)	(0.284)
English Language Learner	-0.156***	-0.032***
	(0.002)	(0.002)
Female	0.07***	0.058***
	(0.001)	(0.001)
Free Lunch	-0.097***	-0.034***
	(0.001)	(0.001)
IEP	-0.206***	-0.032***
	(0.001)	(0.001)
Attendance		0.143***
		(0.001)
Chronically Absent		0.003*
		(0.002)
ELA score		0.059***
		(0.001)
Math score		0.107***
		(0.001)
Ethnicity	2441.58	343.839
	(0.0)	(0.0)
Home Language	328.433	55.3449
	(0.0)	(0.0)
Year	213.836	198.975
	(0.0)	(0.0)
N	823370	823370
R-squared	0.112862	0.255901

Table 6: *Predictors of graduation.*

Notes. For categorical variables – ethnicity, home language, and year – an F-statistic and its p-value are shown. Other variables have standard errors in parentheses. *** indicates significance at the 0.1% level.

female, or in special education. R^2 rises to 0.179 in a regression that includes lagged values of attendance and test scores. Test scores, however, are much more predictable, generating R^2 values of 0.30 to 0.49 across specifications.

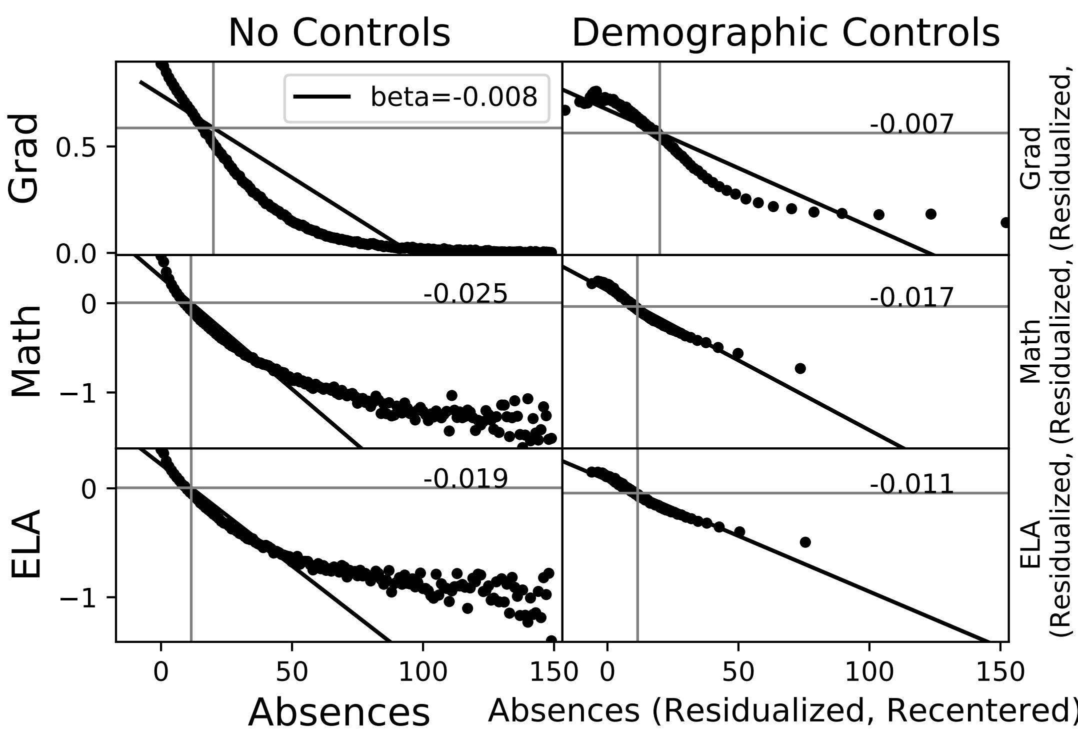


Figure 3: *Binned scatter plots: Relationship between graduation, attendance, and test scores.*

Notes: At left, the mean value of a four-year graduation indicator, math test scores, and ELA test scores, respectively, as a function of the number of days the student is absent in a year. At right, residual plots controlling for ELL status, gender, free lunch status, IEP status, year, grade, ethnicity, and home language.

Table 6 shows that attendance and test scores are helpful in predicting graduation. Attendance is a better predictor than test scores: a one standard deviation increase in attendance corresponds to a 14 percentage point increase in graduation rates, while one standard deviation increases in math or ELA test scores correspond to 11 percentage point and 6 percentage point increases, respectively.

Figure 3 displays the relationship between graduation, absences, math scores, and ELA scores nonparametrically. The top left panel is a binned scatter plot of graduation against absences, with one point for each integer number of absences. The middle and bottom left panels repeat this with math and English Language Arts test scores as the y variable. The right-side plots are recentered residual plots, controlling for English Language Learner status, gender, free lunch status, whether the student has an Individualized Education Plan, ethnicity, home language, year, and grade level. The figure plots residuals of the outcome plus the mean outcome against residuals attendance plus mean attendance. Technically, define $\widetilde{\text{graduation}}_i$ and $\widetilde{\text{absences}}_i$, where $\widetilde{\text{graduation}}_i = \text{graduation}_i - E^*[\text{graduation}_i|1, z_i]$, E^* is the best linear predictor operator, and z includes control variables. The regression line plots

$$E^* \left[\text{graduation}_i | 1, \text{absences}_i = \tilde{\text{absences}}_i + E^* \left[\text{absences}_i | z_i = \bar{z}\right]\right].$$

against $\tilde{\text{absences}}_i + E^* \left[\text{absences}_i | z_i = \bar{z}\right]$. The right panel answers the question, "what would the left panel look like if everyone had the same covariates?"

Figure 3 suggests a strong relationship between attendance and graduation, math, and ELA scores, at least for students absent less than fifty days per year. For students absent more than fifty days per year, the marginal cost of poor attendance appears to decline – perhaps because these students are already doing quite poorly, with a less than 10% chance of graduating high school and test scores nearly one standard deviation below the mean. On average, ten more days of attendance corresponds to an 8 percentage point greater chance of graduating high school (7 percentage points with controls), a gain of 0.25 standard deviations in math test scores (0.17 with controls), and 0.19 standard deviations in ELA test scores (0.11 with controls). These estimates are clearly not causal, but are perhaps an upper bound on the gains in graduation and test scores that might be expected to come from improved attendance.

4 Estimation

I estimate the covariance of teacher effects, Σ_μ, using a moment-matching procedure similar to that of Kane and Staiger (2008). In order to preserve independence across teachers, I prevent the same student from appearing in the data more than once by running estimation separately for each grade and by dropping students for repeating a grade. The first step in estimation is residualizing outcomes by estimating the β of Equation **??** by regressing within-teacher variation in outcomes on within-teacher variation in covariates:

$$\hat{\beta}^h = \arg\min_b \sum_i \left(y_i^h - \bar{y}_{j(i)}^h - \left(x_i - \bar{x}_{j(i)} \right)^T b \right)^2$$

Key to estimating Σ_μ is that the errors in Equation **??** are independent across different classrooms, so the average product of residuals in classrooms taught by the same teacher is the covariance of teacher effects. Let $C(j)$ be the set of all classrooms taught by teacher j, and let $\bar{y}_c$ and $\bar{x}_c$ be average outcomes and covariates in classroom c. When errors are independent across classrooms, then

$$\mathbb{E}\left[\left(\bar{y}_c - \bar{x}_c^T \beta \right) \left(\bar{y}_{c'} - \bar{x}_{c'}^T \beta \right)^T \middle| c, c' \in C(j), c \neq c' \right] = \Sigma_\mu.$$

Using a "moment-matching" procedure as in Kane and Staiger (2008) and Chamberlain (2013), $\hat{\Sigma}_\mu$ is the average variance of residualized outcomes in unique pairs of different classrooms taught by the same teacher:

$$\hat{\Sigma}_\mu = \frac{2}{\sum_j |C(j)| \left(|C(j)| - 1 \right)} \sum_j \sum_{c, c' \in C(j): c \neq c'} \left(\bar{y}_c - \bar{x}_c^T \hat{\beta} \right) \left(\bar{y}_{c'} - \bar{x}_{c'}^T \hat{\beta} \right)^T$$

4.1 Inference

I estimate the posterior distribution of Σ_μ using the Bayesian Bootstrap (Rubin, 1981). In the n^{th} Bayesian Bootstrap draw, reweight the data with teacher-level weights $\omega^n \in \mathcal{R}^{\text{N teachers}}$ drawn $\omega^n \sim \text{Dirichlet}(1, 1, \ldots, 1)$. First, estimate

$$\hat{\beta}^{h,n} = \arg\min_b \sum_i \omega^n_{j(i)} \left(y_i - \bar{y}_{j(i)} - \left(x_i - \bar{x}_{j(i)} \right)^T b \right)^2$$

Then the n^{th} draw of Σ_μ is

$$\hat{\Sigma}^n_\mu = \frac{2}{\sum_j \omega^n_j \left|C(j)\right| \left(\left|C(j)\right| - 1\right)} \sum_j \omega^n_j \sum_{c,c' \in C(j): c \neq c'} \left(\bar{y}_c - \bar{x}_c^T \hat{\beta} \right) \left(\bar{y}_{c'} - \bar{x}_{c'}^T \hat{\beta} \right)^T$$

4.2 Identification

In order to understand how teachers contribute to variation in student outcomes, we must ensure that teachers receive credit or blame only for changes in outcomes they *cause*, and not for changes that would have happened with an average teacher. A threat to our ability to identify the variance of teacher value-added is systematic sorting of students to teachers.

This "sorting on observables" restriction, discussed formally in Section 2, requires that random shocks be independent of a teacher's identity, conditional on covariates. I include a rich set of controls to make the sorting on observables restriction plausible. I estimate the model separately for each grade level, so coefficients can change from year to year to allow for, for instance, the persistence of test scores to vary with grade. I control for age, gender, limited English proficiency status, free or reduced-price lunch eligibility, twice-lagged absences, twice-lagged test scores, disability status, teacher-grade level averages of all of these variables, indicators for ethnicity, indicators for year, and indicators for the ten most common home languages. Rather than drop students with missing data, I set missing variables to zero and control for indicators for missingness. I do, however, drop students missing lagged attendance or test scores.

My controls are similar to those in Chetty *et al.* (2014a). There are three differences between their controls and mine: They control for suspensions, which I do not observe; they estimate their model on all grades and interact many variables with grade dummies; and they control for cubics in previous test scores and classroom means of previous test scores. To ensure that my results are robust to using their controls, I generate point estimates of the diagonal of Σ_μ while additionally controlling for cubics in previous test scores, previous attendance, and classroom means of previous test scores and previous attendance. These results are in Section 6; controlling for cubics does not substantially change results.

Although the sorting on observables restriction cannot be directly tested, I follow Chetty *et al.* (2014a) in using pre-trend tests to test whether students show unusual improvement or declines in outcomes in the years before being assigned to a higher value-added teacher. Pre-trend coefficients, discussed below, correspond to negative years in Figures 5 and 13, and in Tables 9 and 18. Visually, pre-trends appear small, but they are statistically significant, with zero not lying in the 95% credible set. These results suggest that controlling for more lags could (but might not) mitigate sorting on observables. In Section 6, I estimate the diagonal of Σ_μ while controlling for all available lags in addition to the baseline controls; additional lags to not appear to affect the results at all.

5 Results

I conducted analysis separately for math teachers and English teachers. Results for math and English teachers currently look very similar, because the sample includes many fourth and fifth grade teachers who teach both math and English. Figures for math teachers are in this section and figures for English teachers are in Appendix A.

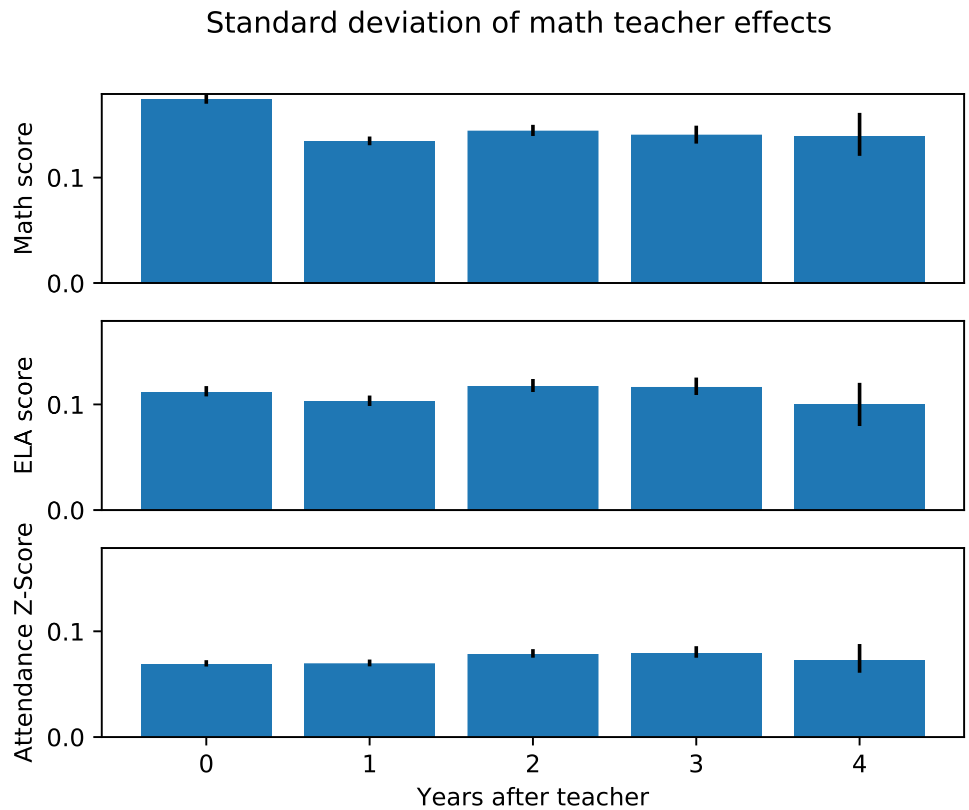


Figure 4: *Magnitude of math teacher effects on test scores and attendance.*

Notes: The top plot shows the standard deviation of math teachers' effects on math scores, in the same year that the student has this teacher and 1, 2, 3, and 4 years after. The second and third plots show the variances of math teachers' effects on English Language Arts scores and attendance. Error bars plot a 95% credible interval based on 1000 Bayesian Bootstrap iterations.

The square root of the diagonal of Σ_μ is the standard deviation of teacher effects on each outcome. For example, the square root of the diagonal element of Σ_μ corresponding to four-year high school graduation rates is 0.049, indicating that students of a teacher who is one standard deviation above average at improving graduation rates are 4.9 percentage points more likely to graduate high school. The variance of teacher effects on high school graduation is 0.0024 (0.049^2), accounting for 1% of the variance in high school graduation

	Year				
	0	1	2	3	4
ELA score	0.112	0.103	0.117	0.117	0.100
	(0.108, 0.117)	(0.099, 0.108)	(0.112, 0.124)	(0.109, 0.125)	(0.080, 0.121)
Math score	0.174	0.134	0.144	0.141	0.139
	(0.170, 0.179)	(0.131, 0.139)	(0.139, 0.150)	(0.132, 0.149)	(0.121, 0.161)
Attendance Z-Score	0.069	0.070	0.079	0.080	0.073
	(0.067, 0.073)	(0.067, 0.073)	(0.075, 0.083)	(0.075, 0.086)	(0.061, 0.088)

Table 7: *Magnitude of math teacher effects on test scores and attendance.*

Notes: The standard deviation of math teacher effects on test scores and attendance, one to four years out. This table displays the same information as Figure 4. 95% credible interval from 1000 Bayesian Bootstrap iterations in parentheses.

	Standard Deviation of Teacher Effect
Graduated, 4-year	0.049
	(0.047, 0.060)
Regents Diploma, 4-year	0.070
	(0.068, 0.079)
Advanced Regents Diploma, 4-year	0.061
	(0.058, 0.065)

Table 8: *Magnitude of math teacher effects on high school graduation.*

Notes: The standard deviation of math teacher effects on four-year high school graduation. 95% credible interval in parentheses.

rates. Figure 4, Table 7, and Table 8 show the effects of math teachers on math scores, ELA scores, and attendance. The standard deviation of math teacher effects on contemporaneous math scores is about 0.174, roughly in line with previous results, indicating that math teachers account for 3% of variance in same-year math test scores (0.174^2). The standard deviation of math teacher effects is slightly smaller in all succeeding years, around 0.14. By contrast, teacher effects on English Language Arts test scores are smaller – Appendix Table 16 shows the analog of 4 for ELA teachers – but are more consistent across years. One explanation for these different patterns in math and ELA scores is that the subject matter on math tests overlaps less across grades than the subject matter on ELA tests. For example, grades 6 through 8 share the same Common Core ELA standards, which cover broad topics like "Determine the central ideas or information of a primary or secondary source," while the math topics change every year and cover narrower topics like "rational and irrational numbers."

For attendance, the standard deviation of math teacher effects is about 0.07 in each year. Attendance, like test scores, has been Z-scored within each grade and year, so this implies that having a teacher who is one standard deviation above average at increasing attendance increases students' attendance by 0.07 of a standard deviation relative to their peers.

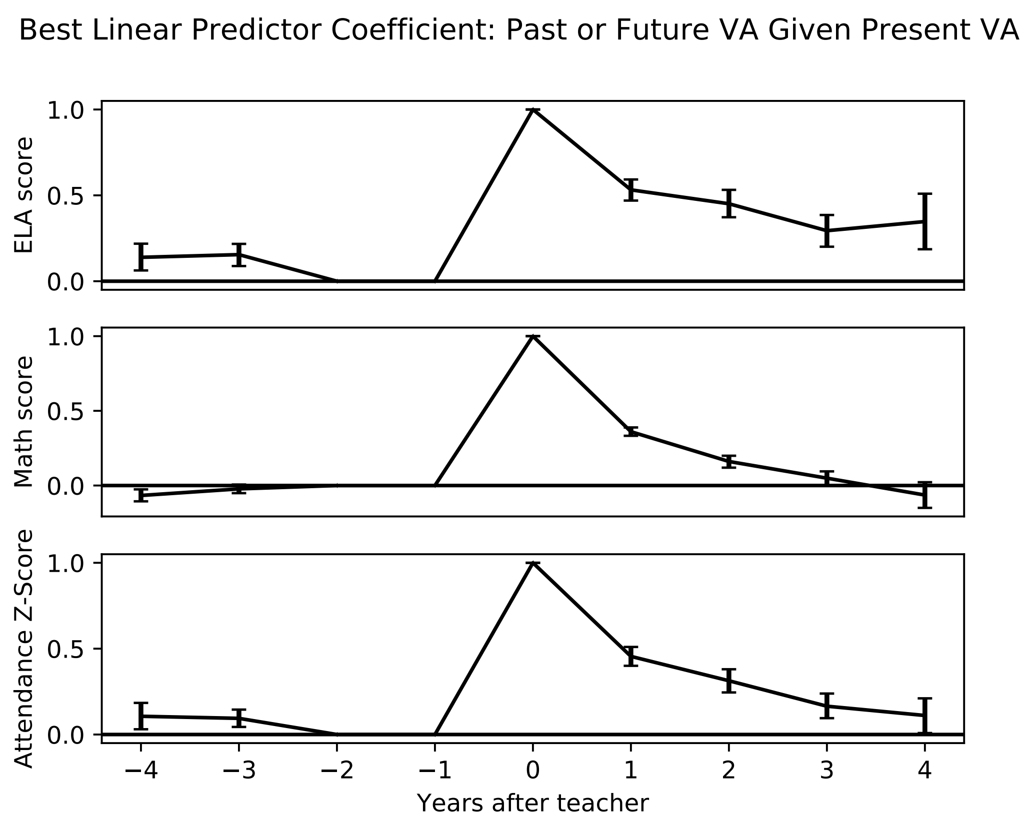


Figure 5: *Fade out of math teacher effects.*

Notes: The best linear predictor coefficient for predicting a teacher's effect on a future outcome given her effect on a present outcome and covariates. Error bars plot a 95% credible interval based on 1000 Bayesian Bootstrap iterations.

Table 9: *Fade out of math teacher effects.*

	Year				
	-4	-3	-2	-1	0
ELA score	0.139	0.155	0.000	0.000	1.000
	(0.063, 0.219)	(0.088, 0.217)	(0.000, 0.000)	(0.000, 0.000)	(1.000, 1.000)
Math score	-0.067	-0.023	0.000	0.000	1.000
	(-0.106, -0.026)	(-0.051, 0.006)	(0.000, 0.000)	(0.000, 0.000)	(1.000, 1.000)
Attendance Z-Score	0.106	0.094	0.000	0.000	1.000
	(0.031, 0.184)	(0.044, 0.145)	(0.000, 0.000)	(0.000, 0.000)	(1.000, 1.000)
	Year				
	1	2	3	4	Graduated (4-year)
ELA score	0.532	0.451	0.294	0.347	0.035
	(0.470, 0.593)	(0.372, 0.532)	(0.201, 0.386)	(0.186, 0.510)	(-0.060, 0.108)
Math score	0.360	0.161	0.049	-0.064	-0.003
	(0.333, 0.389)	(0.119, 0.199)	(-0.001, 0.095)	(-0.150, 0.022)	(-0.038, 0.024)
Attendance Z-Score	0.455	0.313	0.165	0.111	0.105
	(0.400, 0.510)	(0.245, 0.380)	(0.095, 0.239)	(0.009, 0.211)	(-0.001, 0.205)

Notes: Best linear predictor coefficients. 95% credible interval based on 1000 Bayesian Bootstrap iterations in parentheses. Coefficients on outcomes from years -2 and -1 are zero because lags are controlled for, and the coefficient on the year 0 outcome is 1 by construction.

We can also ask, given a teacher's effect on outcome h', what is her expected effect on outcome h? Other work has addressed this question by regressing outcomes on estimated teacher effects and controls, but it can also be answered using only Σ_μ, if we update Equation 1 to assume homoskedasticity:

$$\mathbb{E}\left[\mu_{j(i)}\mu_{j(i)}^T|x_i\right] = \Sigma_\mu. \tag{2}$$

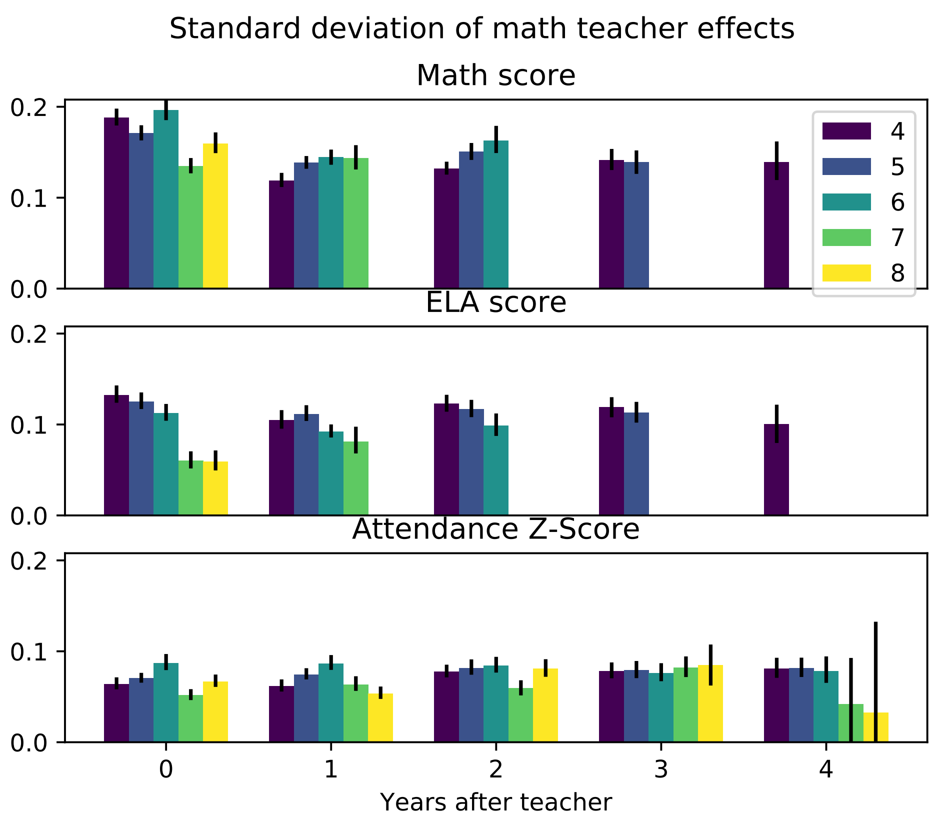
Combining Equations **??** and 2,

$$\begin{aligned} E^*\left[y_i^h|\mu_{j(i)}^{h'}, x_i\right] &= E^*\left[\mu_{j(i)}^H|\mu_{j(i)}^{h'}, x_i\right] + x_i^T\beta^h \\ &= \frac{Cov\left(\mu_j\right)_{h,h'}}{\text{Var}\left(\mu_j^{h'}\right)}\mu_j^{h'} + x_i^T\beta^h \\ &= \frac{\Sigma_\mu^{h,h'}}{\Sigma_\mu^{h',h'}}\mu_j^{h'} + x_i^T\beta^h \\ &\equiv \gamma^{h,h'}\mu_j^{h'} + x_i^T\beta^h \end{aligned}$$

Figure 5 plots $\gamma^{h,h'}$ where $h =$ contemporaneous English Language Arts scores, math scores, and attendance and h' represents leads and lags of those outcomes (again using only math teachers). Since controls include lagged test scores and attendance, the coefficients on previous-year value-added is zero. Figure 5 and Table 9 show that math teachers who improve math scores by x are expected to improve their students' scores in the next year by than half of x, and their scores in the year after by very little. This is a similar result to that found in Chetty *et al.* (2014a). Combined with Figure 4, we see that although teachers do vary in their effects on their students' test scores four years in the future, very little of that variation is captured by teacher effects on same-year test scores. That is, there are teachers who are much better or worse than average at boosting long-term test scores, but they not especially likely to be the teachers who raise short-term test scores. There appears to be more fade-out for math scores than for attendance and more for attendance than for ELA scores. The faster fade-out for math scores is perhaps not surprising, since the topics tested on Common Core ELA tests are more similar between grades for ELA than for math. Figure 13 and Table 18 repeat the same figure and table for English teachers.

5.0.1 Does Σ_μ vary by grade?

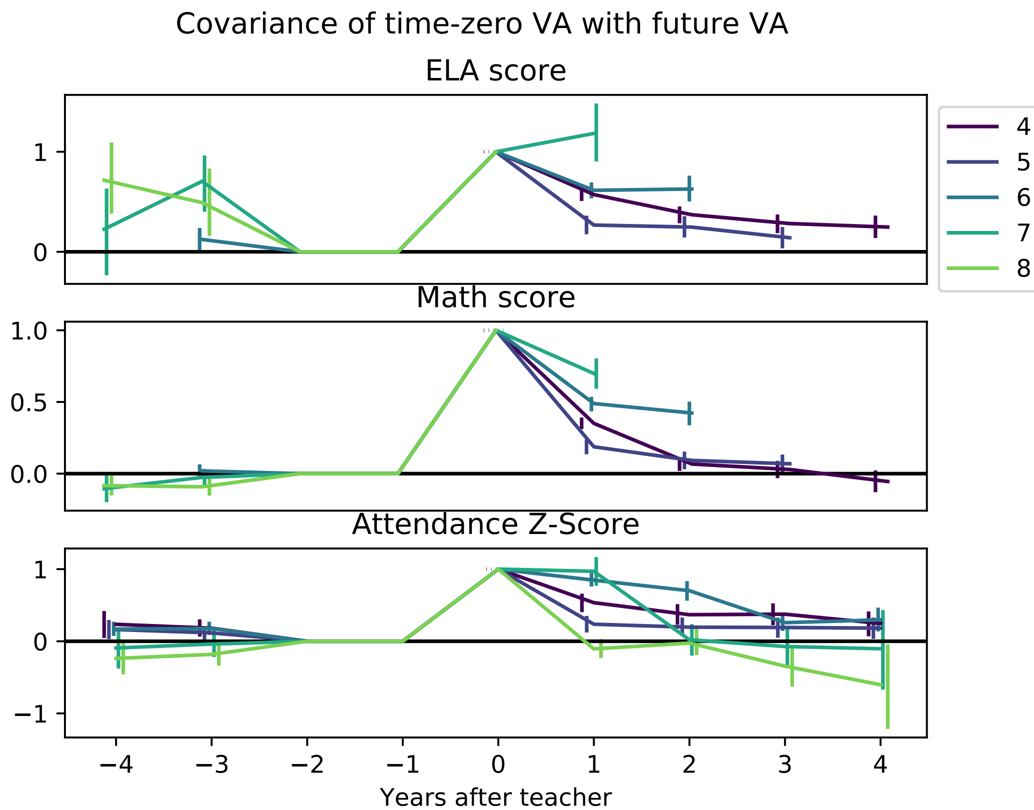
Figure 6: *Magnitude of math teacher effects by grade.*



Notes: The standard deviation of math teachers' effects on outcomes, or the square root of the diagonal of Σ_μ, within each year. Error bars plot a 95% credible interval based on 1000 Bayesian Bootstrap iterations.

Figure 6 and Appendix Figure 14 show the variance of teacher effects on each outcome, like Figure 4, but broken down by grade. It appears that teachers who teach later grades have smaller effects on ELA scores, but there is no consistent pattern for math scores or attendance. This is consistent with the hypothesis that ELA tests are more cumulative than math tests.

Figure 7: *Fade out of math teacher effects, by grade.*



Notes: The best linear predictor coefficient for predicting a teacher's effect on a future outcome given her effect on a present outcome and covariates, by grade. Error bars plot a 95% credible interval based on 1000 Bayesian Bootstrap iterations.

Figure 7 and Appendix Figure 15 show the best linear predictor coefficients for predicting a teacher's effect on a future or past outcome given her effect on current outcomes. Generally, teachers of later grades appear to have more persistent effects on test scores, and there is no obvious pattern for attendance. These results are in contrast with Heckman (2012)'s thesis that cognitive abilities are relatively more malleable than non-cognitive abilities at younger ages, which would have predicted smaller or less persistent effects on test scores in later grades, but the opposite for attendance.

5.1 Are current teacher effects a good proxy for future teacher effects?

How well can teacher effects on a long-term outcome be captured by teacher effects on short-term outcomes? We can answer this question by constructing an R^2-like statistic, "goodness of proxy," that reflects how much of the variation in some long-term teacher effect h is captured by short-term teacher effects on outcomes in set Q. If we assume that μ_j has a

Table 10: *Goodness of proxy.*

	(1) Scores	(2) Attendance	(3) Scores + Attend	(4) Ratio (3) / (1)
ELA score (4 years later)	0.244	0.072	0.294	1.205
	(0.107, 0.459)	(0.008, 0.191)	(0.140, 0.525)	(1.008, 1.867)
Math score (4 years later)	0.037	0.006	0.043	1.146
	(0.009, 0.104)	(0.000, 0.049)	(0.013, 0.125)	(1.001, 2.755)
Attendance Z-Score (4 years later)	0.028	0.011	0.037	1.311
	(0.004, 0.079)	(0.000, 0.041)	(0.010, 0.093)	(1.004, 4.827)
Graduated, 4-year	0.013	0.021	0.033	2.471
	(0.001, 0.072)	(0.000, 0.072)	(0.006, 0.118)	(1.036, 28.489)
Regents Diploma, 4-year	0.014	0.000	0.015	1.058
	(0.001, 0.076)	(0.000, 0.014)	(0.002, 0.077)	(1.000, 8.068)
Advanced Regents Diploma, 4-year	0.059	0.006	0.062	1.040
	(0.029, 0.096)	(0.000, 0.019)	(0.031, 0.101)	(1.000, 1.217)

Notes: Goodness of proxy for graduation and four-year-lead test scores and attendance, using same-year test scores and attendance. 95% credible set based on 1000 Bayesian Bootstrap iterations in parentheses.

multivariate normal distribution, then the expectation of one component of μ_j given other components is linear and can be expressed as a function of the covariances in Σ_μ. Say we are interested in predicting value-added at component h, μ_j^h, and know a vector-valued value-added $\mu_j^{\mathbf{Q}}$ for outcomes in set Q. Then the expectation of μ_j^h given $\mu_j^{\mathbf{Q}}$ is

$$\begin{aligned}\mathbb{E}\left[\mu_j^h|\mu_j^{\mathbf{Q}}\right] &= E^*\left[\mu_j^h|\mu_j^{\mathbf{Q}}\right] \\ &= \left(\mu_j^{\mathbf{Q}}\right)^T \text{Var}\left(\mu^{\mathbf{Q}}\right)^{-1} \text{Cov}\left(\mu^{\mathbf{Q}}, \mu^h\right)\end{aligned}$$

Equation 3 defines a "goodness of proxy" statistic that is zero when μ_j^Q does not help predict μ_j^h and equals one when it is perfectly predictable. This measure is also computable using only the components of Σ_μ.

$$\begin{aligned}\text{goodness of proxy}_{h,Q} &\equiv 1 - \frac{\mathbb{E}\left[\text{Var}\left(\mu^h|\mu^{\mathbf{Q}}\right)\right]}{\text{Var}\left(\mu^h\right)} \\ &= \frac{\text{Var}\left(E\left[\mu^h|\mu^{\mathbf{Q}}\right]\right)}{\text{Var}\left(\mu^h\right)} \\ &= \frac{\text{Cov}\left(\mu^{\mathbf{Q}}, \mu^h\right)^T \text{Var}\left(\mu^{\mathbf{Q}}\right)^{-1} \text{Cov}\left(\mu^{\mathbf{Q}}, \mu^h\right)}{\text{Var}\left(\mu^h\right)} \qquad (3)\end{aligned}$$

Table 10 and Appendix Table 19 show the goodness of proxy for graduation and four-year-lead test scores and attendance, using same-year test scores and attendance as a proxy. Teacher effects on four-year-ahead ELA scores are somewhat predictable, with a Goodness of

Table 11: *Correlations between teachers' value-added on different outcomes.*

	ELA score	Math score	Attendance	4-year Grad	4-yr Reg. Dip.
ELA score	1.00				
	(1.00, 1.00)				
Math score	0.66	1.00			
	(0.63, 0.68)	(1.00, 1.00)			
Attendance	0.15	0.12	1.00		
	(0.10, 0.20)	(0.08, 0.17)	(1.00, 1.00)		
4-year Grad	0.08	-0.01	0.15	1.00	
	(-0.12, 0.22)	(-0.12, 0.08)	(-0.00, 0.27)	(1.00, 1.00)	
4-yr Reg. Dip.	-0.11	-0.05	0.01	0.52	1.00
	(-0.27, 0.00)	(-0.15, 0.02)	(-0.09, 0.11)	(0.46, 0.63)	(1.00, 1.00)
4-yr Adv. Reg. Dip.	0.22	0.07	0.08	0.15	-0.74
	(0.15, 0.28)	(0.02, 0.12)	(0.01, 0.14)	(0.04, 0.21)	(-0.77, -0.66)

Proxy of 0.294 (95% CI [0.140, 0.525]), but teacher effects on all other longer-term outcomes are much less predictable, with a Goodness of Proxy of 0.015 to 0.062. This is consistent with Tables 7 and Appendix Table 16, which show that teachers vary substantially in their effects on test scores and attendance four years in the future, and with Tables 9 and 18, which show that teachers who improve math test scores or attendance do not have persistent effects, while teachers who improve ELA scores do.

5.2 Bivariate Correlations and Factor Analysis

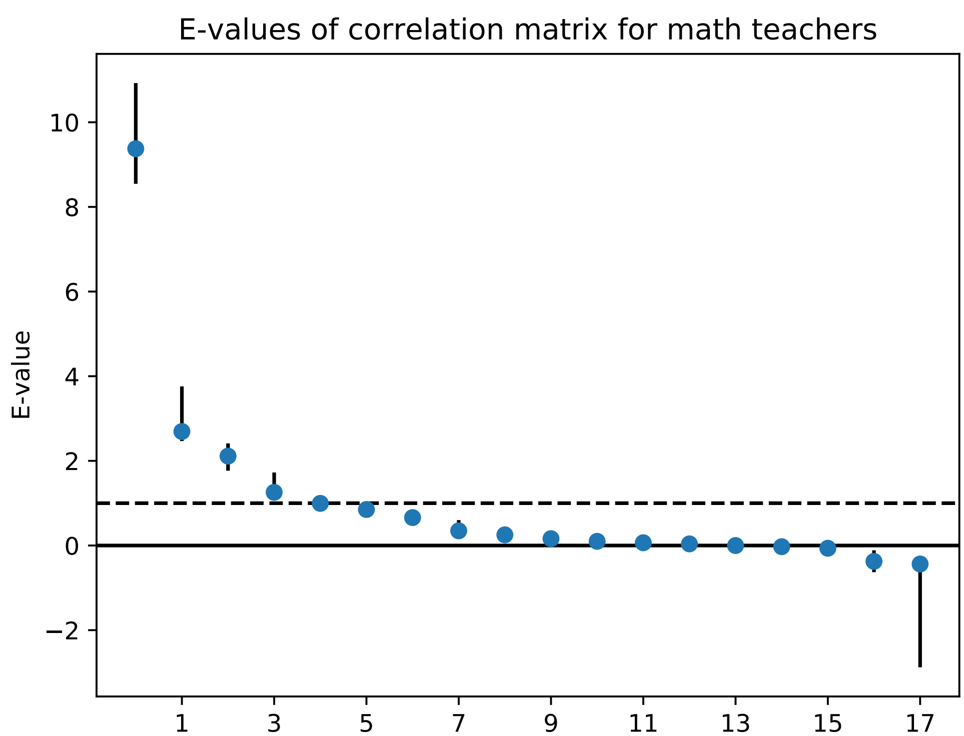
Can Σ_μ be represented well by several factors? What are they? I answer this question by transforming the covariance matrix Σ_μ matrix to a correlation matrix $\tilde{\Sigma}_\mu$, and estimating the factors of $\tilde{\Sigma}_\mu$ via maximum likelihood (Jöreskog, 1970). I use only the component of the matrix that corresponds to non-negative years, since these are the years in which we expect teachers to have real effects.

Before applying factor analysis, we can gain some intuition for what the factors might look like from Table 11, which shows the correlation structure of teacher effects on different present-year outcomes. Although teacher effects on math and reading test scores are highly correlated, with a correlation of 0.66, teacher effects on test scores and on attendance are much lower.

This low correlation indicates that my findings but do *not* resolve the "fade-out mystery" of teacher effects on test scores: if students of high score-VA teachers have only slightly improved test scores four years later, how is it that teachers have substantial effects on later student outcomes? Since teachers who improve test scores are not much better than average at improving attendance, the fade-out puzzle cannot be resolved by high score-VA teachers improving attendance.

In factor analysis there are several rules of thumb for choosing the number of components. One is to count the number of eigenvalues that are greater than one. This heuristic suggests four components for both math and English teachers. Another heuristic suggests plotting

Figure 8: *Eigenvalues of $\tilde{\Sigma}_{\mu}$ for math teachers.*



Notes: Error bars show a 95% credible interval based on 1000 Bayesian Bootstrap iterations.

the eigenvalues, as in Figures 8 and 16, and look for a point where the eigenvalues start to level off. This heuristic suggests three factors for math teachers and three for ELA teachers. For simplicity of exposition, I choose three factors.

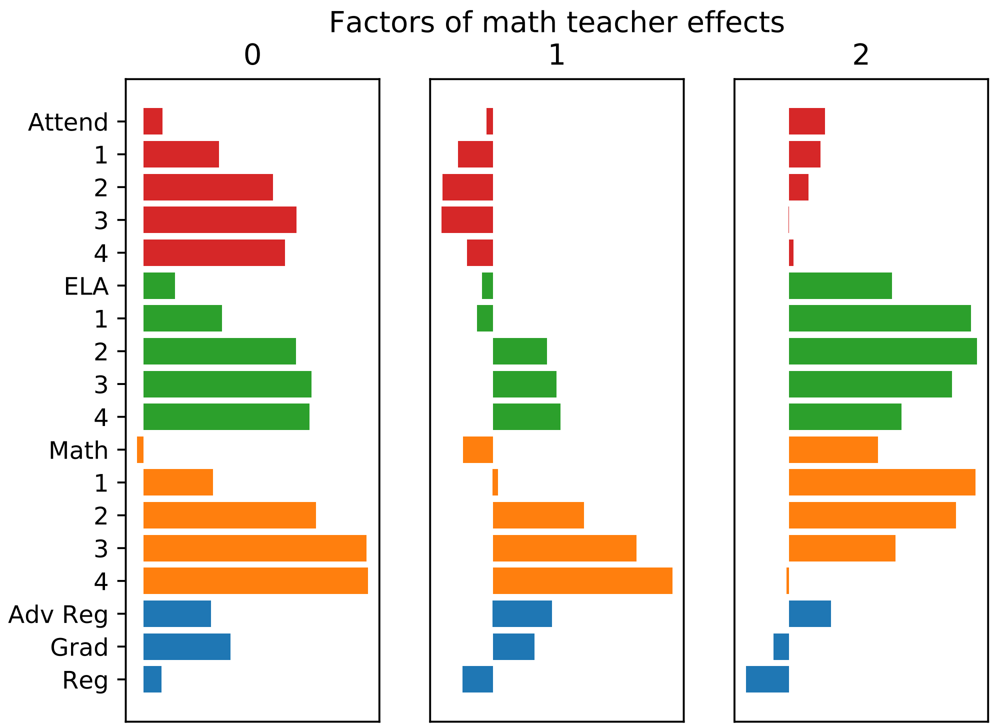


Figure 9: *Factors of $\tilde{\Sigma}_\mu$ for math teachers.*

Figure 9 shows the estimated factors for math teachers. (Appendix Figure 17 is the analog for ELA teachers; it is similar.) The first factor is associated with positive effects on all factors except for same-year math scores, and especially large effects on long-term test scores. A teacher whose effects resemble this component has positive long-term effects on her students – or negative, as the sign of the factor is not identified – but her effectiveness would not be noted by focusing on present-year outcomes. The second factor reflects positive effects on long-term test scores, especially in math, and on graduation, especially with an Advanced Regents designation, but negative effects on attendance and short-term test scores. The third factor has large, same-signed loadings on all test scores, but small loadings on attendance and graduation.

6 Robustness Checks

Various methods are available to estimate the diagonal of Σ_μ, which reveals the variance of teacher effects on each outcome, as surveyed in Chapter 2 of this dissertation. And of course, infinitely many sets of control variables are possible. This section estimates the square root of the diagonal of Σ_μ for two different estimators and three sets of controls for math teachers; results from English teachers are in the appendix.

The method used in this paper is a "moment-matching" estimator that residualizes test scores using within-teacher variation and compares mean residuals in different classrooms taught by the same teacher. This method can be expressed as an instance of the Generalized Method of Moments and is identical to the "modified-KS" estimator in Chapter 2. An alternative estimation method is maximum likelihood. See Figure 10 and Appendix Figure 18.

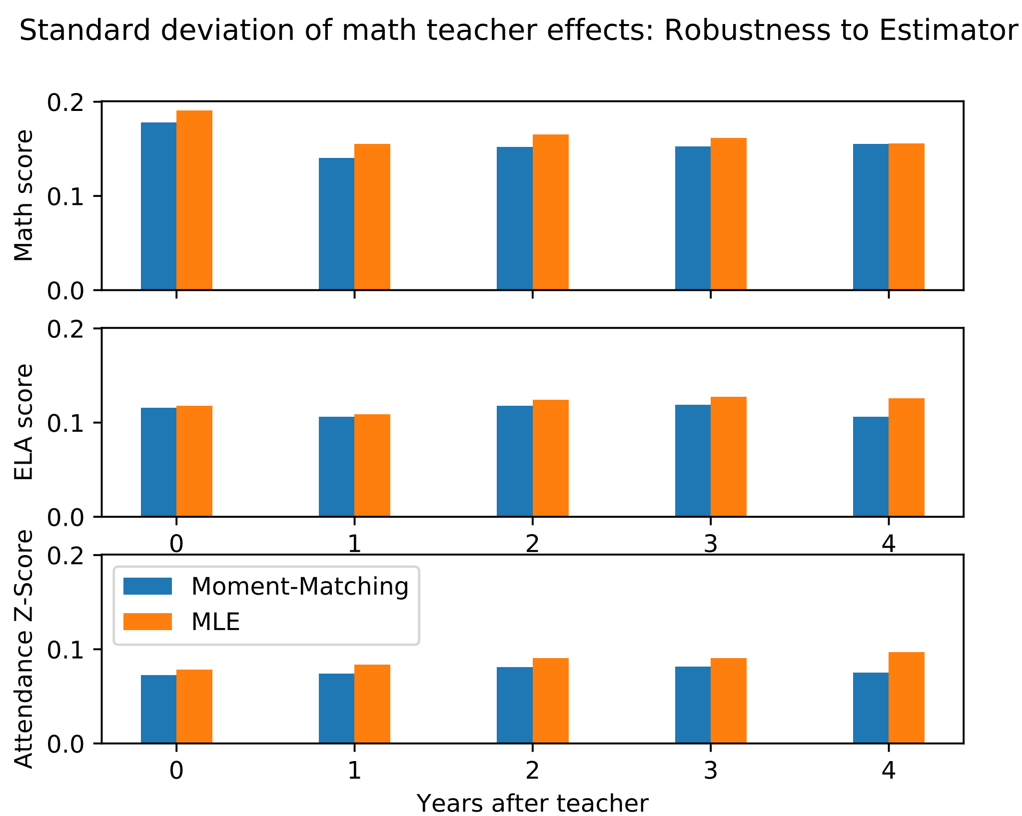


Figure 10: *Robustness to choice of estimator.*

Notes: Standard deviation of math teacher effects on present and future outcomes, for both the "moment-matching" estimator used above and maximum likelihood.

In the "baseline" controls used to generate the results in Section 5, I control for age, gender, limited English proficiency status, free or reduced-price lunch eligibility, twice-lagged absences, twice-lagged test scores, disability status, teacher-grade level averages of all of these variables, indicators for ethnicity, indicators for year, and indicators for the ten most common home languages. Rather than drop students with missing data, I set missing variables to zero and control for indicators for missingness. I do, however, drop students missing lagged attendance or test scores. Figure 11 and Appendix Figure 19 additionally show results from two other sets of control variables. The "Polynomial" controls control for all baseline variables, cubics in lagged test scores and attendance, and cubics in teacher-year level means of lagged test scores and attendance. The “Polynomial" controls are very similar to the controls used in Chetty *et al.* (2014a). The “All Lags" controls control for up to four years of lags, with missing lags imputed to zero and an indicator for missingness.

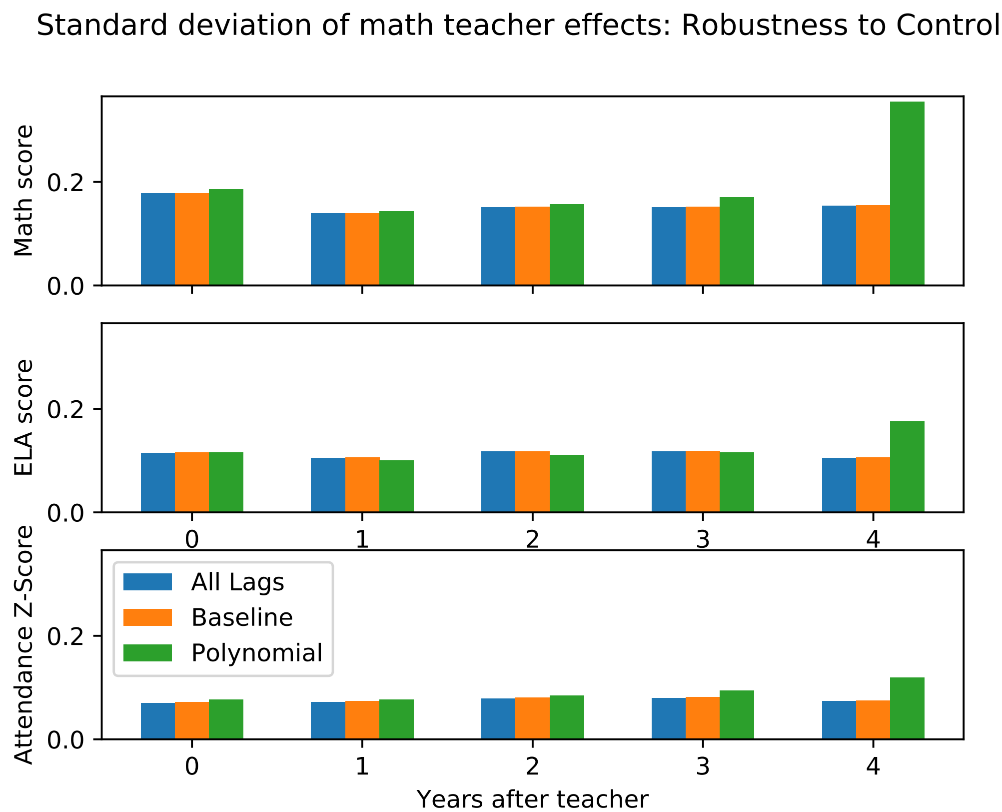


Figure 11: *Robustness to choice of controls.*

Notes: Standard deviation of math teacher effects on present and future outcomes, for the baseline controls used above, baseline controls plus all available lags, and for the controls from Chetty *et al.* (2014a), which include third-degree polynomials.

Tables 12, 13, 14, and 15 (and Appendix Tables 20, 21, 22, and 23) display all of the results found in the figures, as well as results from maximum likelihood estimation with the non-baseline sets of controls.

Figure 10 and Tables 12 through 15 show that using the likelihood-based estimator instead of the moment-matching estimator yields similar but uniformly larger results. Figure 11 and Tables 12 through 15 show that different sets of controls generally yield very similar results. However, the polynomial controls suggest larger teacher effects on test scores four years out, a result that holds for each estimator. Appendix Figure 19 shows that this is also true of ELA teachers (naturally, because only fourth grade teachers have students with four-year-later test scores, and fourth grade teachers generally teach both ELA and math). The reason for this discrepancy is unclear.

Controls	Estimator	Year 0	1	2	3	4
Baseline	Moment-Matching	0.116	0.106	0.118	0.119	0.106
Baseline	MLE	0.118	0.109	0.124	0.127	0.126
Polynomial	Moment-Matching	0.116	0.101	0.111	0.116	0.175
Polynomial	MLE	0.120	0.113	0.123	0.137	0.197
All Lags	Moment-Matching	0.115	0.105	0.117	0.118	0.105
All Lags	MLE	0.130	0.110	0.124	0.128	0.126

Table 12: *Standard deviation of math teacher effects on ELA scores: Robustness to choice of controls and estimator.*

Controls	Estimator	Year 0	1	2	3	4
Baseline	Moment-Matching	0.178	0.140	0.152	0.152	0.155
Baseline	MLE	0.191	0.155	0.165	0.162	0.156
Polynomial	Moment-Matching	0.186	0.144	0.157	0.171	0.355
Polynomial	MLE	0.200	0.163	0.177	0.180	0.390
All Lags	Moment-Matching	0.178	0.140	0.151	0.151	0.154
All Lags	MLE	0.194	0.155	0.165	0.162	0.159

Table 13: *Standard deviation of math teacher effects on math scores: Robustness to choice of controls and estimator.*

Controls	Estimator	Year 0	1	2	3	4
Baseline	Moment-Matching	0.072	0.074	0.081	0.081	0.075
Baseline	MLE	0.078	0.084	0.090	0.091	0.097
Polynomial	Moment-Matching	0.077	0.077	0.085	0.094	0.120
Polynomial	MLE	0.087	0.086	0.092	0.098	0.127
All Lags	Moment-Matching	0.070	0.072	0.079	0.080	0.074
All Lags	MLE	0.083	0.082	0.089	0.090	0.101

Table 14: *Standard deviation of math teacher effects on attendance: Robustness to choice of controls and estimator.*

Controls	Estimator	Graduated	Regents Diploma	Advanced Regents Diploma
Baseline	Moment-Matching	0.049	0.074	0.065
Baseline	MLE	0.051	0.076	0.065
Polynomial	Moment-Matching	0.059	0.072	0.076
Polynomial	MLE	0.059	0.072	0.062
All Lags	Moment-Matching	0.047	0.073	0.065
All Lags	MLE	0.052	0.078	0.065

Table 15: *Standard deviation of math teacher effects on graduation: Robustness to choice of controls and estimator.*

7 Conclusion

The results of this paper have two policy implications that are in tension. First, we might want to incorporate teacher effects on attendance into teacher value-added measures. Teachers have substantial effects on attendance, which is valuable in itself. And since teacher effects on attendance are approximately as large and persistent as teacher effects on test scores and more predictive of graduation, we might favor attendance as a useful proxy for teacher effects on student well-being. On the other hand, teacher effects on the most plausibly welfare-relevant outcomes, long-term test scores and high school graduation, are poorly captured by teacher effects on the immediately observable outcomes, test scores and attendance. If improving short-term and long-term outcomes are very different tasks, then causing teachers to improve short-term outcomes might distract them from more important tasks.

First, the case for measuring teacher effects on attendance. Absenteeism is pervasive in urban school districts, and while teachers explain only a small portion of the variance in student attendance, they can be effective in reducing absenteeism. A teacher one standard deviation above average at increasing attendance teaching a class of 25 students increases attendance by 78 student-days[5]. Holding teacher behavior constant, incorporating attendance into existing value-added measures would make these measures more fair and informative, since teachers who are effective in improving test scores are not especially likely to improve attendance. Since school districts routinely collect attendance data and since many districts have adopted quantitative value-added measures, it would be very feasible for these districts to incorporate attendance and other outcomes into value-added scoring.

However, there may be risks to compensating, evaluating, or firing teachers based on attendance, analogous to the risks in incentivizing high test scores. Although there is a wide literature on incentivizing *schools* for higher test scores or meeting proficiency standards, the effects of high-stakes incentives for individual *teachers* are far from clear. Several studies discuss group incentives: Fryer (2013) discusses a randomized trial in which New York City public schools were eligible for more funding if they reached test score targets, and

[5]This is true for the elementary and middle school students in the analysis sample, in which the variance of teacher effects on attendance is 0.069, and the standard deviation of attendance in the analysis sample is 11.95 days.

schools usually chose group incentives. Test scores did not improve in treatment schools. On the other hand, Lavy (2002) studied group incentives on several performance measures, including test scores, in Israel and found that they increased both contemporaneous test scores and a variety of outcome measures in the following year. Although incentives for higher test scores are intended to increase teacher effort, teachers often seem to respond in less desirable ways, such as increasing time spent on test prep (Glewwe *et al.*, 2010). And even in the presence of school-level performance incentives that only weakly affect individual teachers, teachers may "teach to the test" (Klein *et al.* (2000)) or cheat (Jacob and Levitt (2003), Jacob (2005), Loughran and Comiskey (1999)).

Although I show that high attendance value-added teachers have historically improved their students' achievement, this does not imply that teachers should be compensated, evaluated, or fired based on attendance-based value-added measures. Similarly, this paper has little to say about whether score-based value-added should be a component of teacher evaluation. Policymakers face a multitasking problem in the spirit of Holmstrom and Milgrom (1991): we want teachers to make their students motivated, persistent, and informed, but we can only design contracts on the basis of observable factors. The risk of perverse incentives that comes with test score-based teacher evaluation measures may make attendance-based value added more appealing as an alternative, but it should also caution us that incentivizing teachers for student behavior may lead to unintended outcomes. For example, teachers could encourage students to come to school even when sick, or make class more fun at the expense of being edifying by, for example, showing movies. In addition, in the long run, teachers may require significantly higher pay to compensate them for the stress and uncertainty that a merit-based pay and retention system could generate.

Many questions remain unanswered in this area. Using this data, it is possible to explore heterogeneity in teacher effects; for example, do teachers have larger effects on absences for students who are absent more often? It would also be helpful to track these students farther into the future to explore the effects of teachers who reduce absences on high school graduation rates, college attendance and completion rates, and income.

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A Appendix: Figures and Tables for ELA Teachers

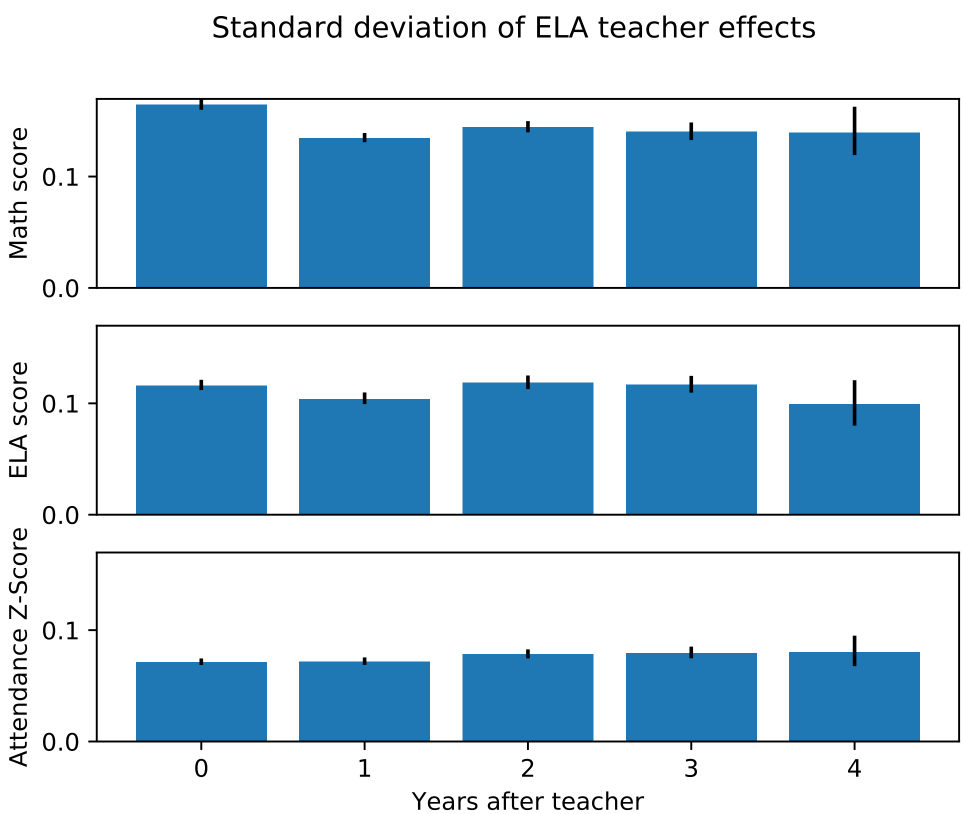

Figure 12: *Magnitude of ELA teacher effects.*

Notes: The top plot shows the variance of English teachers' effects on English Language Arts scores, in the same year that the student has this teacher and 1, 2, 3, and 4 years after. The second and third plots show the variances of English teachers' effects on math scores and attendance. Error bars plot a 95% credible interval based on 1000 Bayesian Bootstrap iterations.

	Year				
	0	1	2	3	4
ELA score	0.116	0.104	0.119	0.117	0.099
	(0.112, 0.121)	(0.099, 0.110)	(0.113, 0.125)	(0.110, 0.125)	(0.080, 0.121)
Math score	0.165	0.135	0.144	0.140	0.139
	(0.160, 0.170)	(0.131, 0.139)	(0.140, 0.150)	(0.133, 0.149)	(0.119, 0.163)
Attendance Z-Score	0.071	0.072	0.078	0.079	0.080
	(0.069, 0.075)	(0.069, 0.075)	(0.075, 0.083)	(0.075, 0.085)	(0.068, 0.095)

Table 16: *Magnitude of ELA teacher effects on test scores and attendance.*

Notes: The standard deviation of English teacher effects on test scores and attendance, zero to four years out. This table displays the same information as 12. 95% credible interval from 1000 Bayesian Bootstrap iterations in parentheses.

	Standard Deviation of Teacher Effect
Graduated, 4-year	0.048
	(0.046, 0.060)
Regents Diploma, 4-year	0.068
	(0.066, 0.077)
Advanced Regents Diploma, 4-year	0.061
	(0.059, 0.066)

Table 17: *Magnitude of ELA teacher effects on graduation.*

Notes: The standard deviation of English teacher effects on four-year high school graduation. This table displays the same information as 12. 95% credible interval in parentheses.

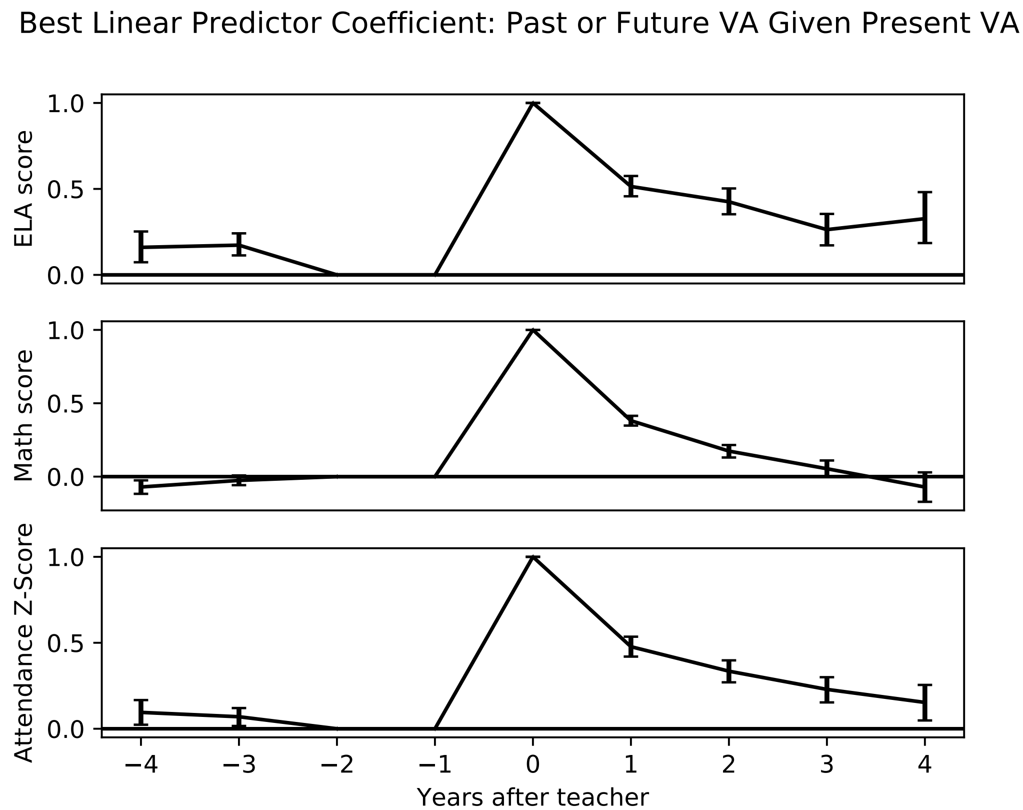

Figure 13: *Fade-out of ELA teacher effects.*

Notes: The best linear predictor coefficient of a teacher's effect on a future outcome given her effect on a present outcome. Error bars plot a 95% credible interval based on 1000 Bayesian Bootstrap iterations.

	Year				
	-4	-3	-2	-1	0
ELA score	0.160	0.173	0.000	0.000	1.000
	(0.073, 0.252)	(0.113, 0.242)	(0.000, 0.000)	(0.000, 0.000)	(1.000, 1.000)
Math score	-0.071	-0.026	0.000	0.000	1.000
	(-0.117, -0.026)	(-0.058, 0.008)	(0.000, 0.000)	(0.000, 0.000)	(1.000, 1.000)
Attendance Z-Score	0.095	0.070	0.000	0.000	1.000
	(0.023, 0.167)	(0.016, 0.120)	(0.000, 0.000)	(0.000, 0.000)	(1.000, 1.000)
	Year				
	1	2	3	4	Graduated (4-year)
ELA score	0.514	0.425	0.263	0.327	0.028
	(0.457, 0.575)	(0.353, 0.503)	(0.172, 0.355)	(0.185, 0.481)	(-0.057, 0.097)
Math score	0.381	0.174	0.054	-0.071	0.002
	(0.348, 0.414)	(0.130, 0.215)	(0.002, 0.110)	(-0.172, 0.029)	(-0.034, 0.032)
Attendance Z-Score	0.477	0.335	0.229	0.153	0.118
	(0.420, 0.535)	(0.270, 0.398)	(0.153, 0.300)	(0.048, 0.255)	(0.022, 0.214)

Table 18: *Fade-out of ELA teacher effects.*

Notes: Best linear predictor coefficients. 95% credible interval based on 1000 Bayesian Bootstrap iterations in parentheses.

	(1) Scores	(2) Attendance	(3) Scores + Attend	(4) Ratio (3) / (1)
ELA score (4 years later)	0.270	0.085	0.328	1.213
	(0.120, 0.520)	(0.014, 0.216)	(0.168, 0.593)	(1.010, 1.779)
Math score (4 years later)	0.043	0.011	0.053	1.218
	(0.009, 0.118)	(0.000, 0.057)	(0.018, 0.136)	(1.000, 3.699)
Attendance Z-Score (4 years later)	0.008	0.018	0.024	2.882
	(0.001, 0.036)	(0.002, 0.049)	(0.007, 0.061)	(1.071, 23.264)
Graduated, 4-year	0.008	0.030	0.036	4.498
	(0.000, 0.059)	(0.001, 0.085)	(0.007, 0.111)	(1.167, 78.278)
Regents Diploma, 4-year	0.025	0.000	0.025	1.016
	(0.002, 0.094)	(0.000, 0.014)	(0.005, 0.095)	(1.000, 3.951)
Advanced Regents Diploma, 4-year	0.065	0.011	0.070	1.071
	(0.036, 0.104)	(0.002, 0.027)	(0.039, 0.112)	(1.001, 1.320)

Table 19: *Goodness of proxy for ELA teachers.*

Notes: Goodness of proxy for graduation and four-year-lead test scores and attendance, using same-year test scores and attendance. 95% credible set based on 1000 Bayesian Bootstrap iterations in parentheses.

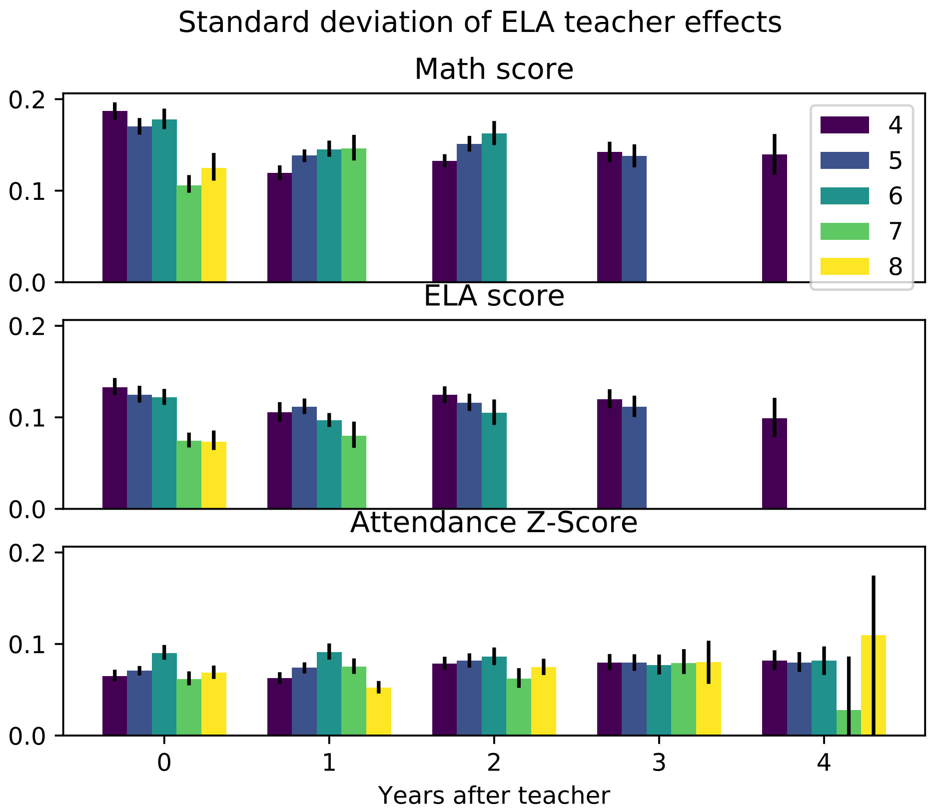

Figure 14: *Magnitude of ELA teacher effects by grade.*

Notes: The variance of ELA teachers' effects on outcomes, or the diagonal of Σ_μ, within each year. Error bars plot a 95% credible interval based on 1000 Bayesian Bootstrap iterations.

		Year				
Controls	Estimator	0	1	2	3	4
Baseline	Moment-Matching	0.117	0.106	0.118	0.119	0.106
Baseline	MLE	0.118	0.109	0.124	0.127	0.126
Polynomial	Moment-Matching	0.117	0.101	0.111	0.116	0.175
Polynomial	MLE	0.121	0.113	0.123	0.137	0.197
All Lags	Moment-Matching	0.116	0.105	0.117	0.118	0.105
All Lags	MLE	0.130	0.110	0.124	0.128	0.126

Table 20: *Standard deviation of ELA teacher effects on ELA scores: Robustness to choice of controls and estimator.*

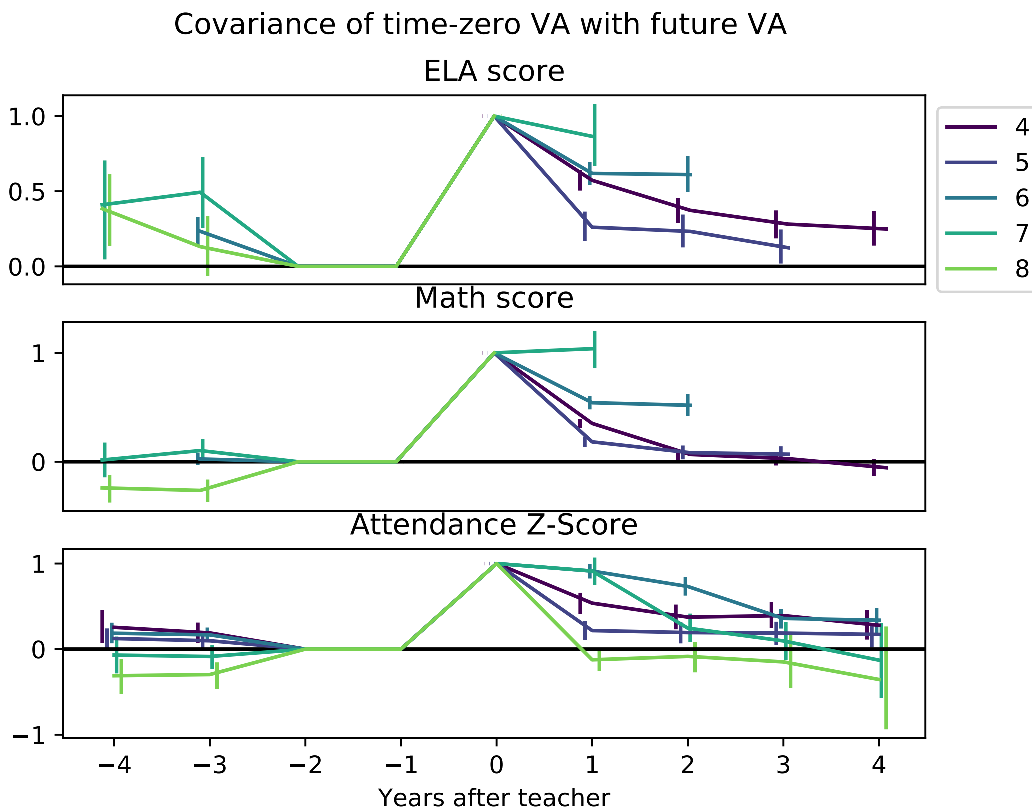

Figure 15: *Fade-out of ELA teacher effects by grade.*

Notes: The best linear predictor coefficient for predicting a teacher's effect on a future outcome given her effect on a present outcome and covariates, by grade. Error bars plot a 95% credible interval based on 1000 Bayesian Bootstrap iterations.

		Year				
Controls	Estimator	0	1	2	3	4
Baseline	Moment-Matching	0.179	0.140	0.152	0.152	0.155
Baseline	MLE	0.191	0.155	0.165	0.162	0.156
Polynomial	Moment-Matching	0.187	0.144	0.157	0.171	0.355
Polynomial	MLE	0.200	0.163	0.177	0.180	0.390
All Lags	Moment-Matching	0.179	0.140	0.151	0.151	0.154
All Lags	MLE	0.194	0.155	0.165	0.162	0.159

Table 21: *Standard deviation of English teacher effects on math scores: Robustness to choice of controls and estimator.*

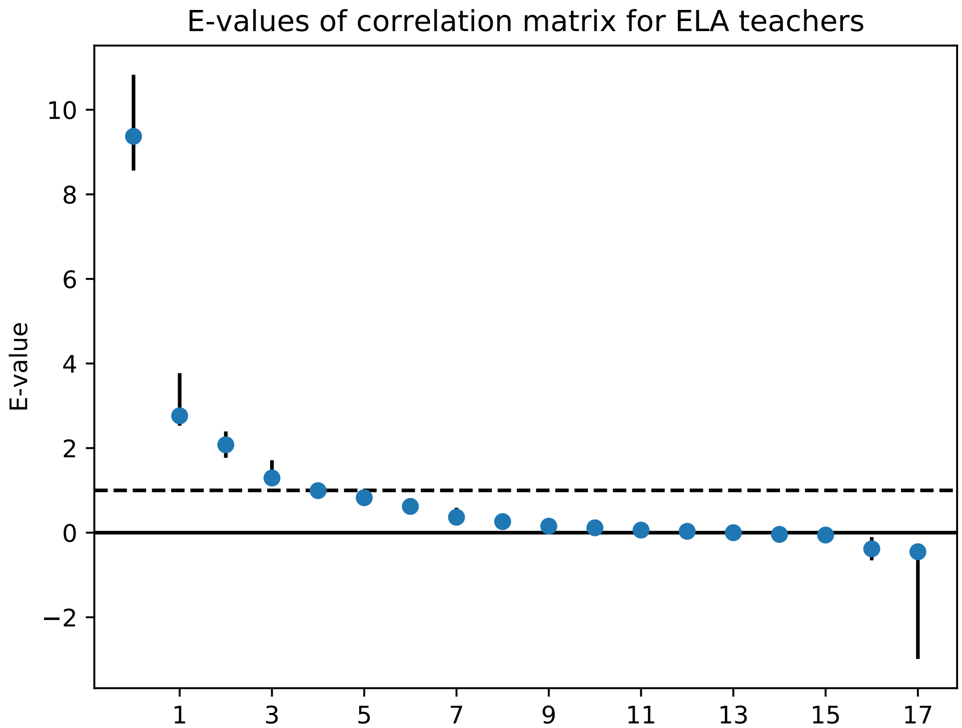

Figure 16: *Eigenvalues of $\tilde{\Sigma}_{\mu}$ for English teachers.*

Notes: Error bars show a 95% credible interval based on 1000 Bayesian Bootstrap iterations.

		Year				
Controls	Estimator	0	1	2	3	4
Baseline	Moment-Matching	0.073	0.074	0.081	0.081	0.075
Baseline	MLE	0.078	0.084	0.090	0.090	0.096
Polynomial	Moment-Matching	0.077	0.077	0.085	0.093	0.118
Polynomial	MLE	0.087	0.086	0.092	0.097	0.125
All Lags	Moment-Matching	0.070	0.072	0.079	0.079	0.075
All Lags	MLE	0.083	0.082	0.089	0.090	0.100

Table 22: *Standard deviation of English teacher effects on attendance: Robustness to choice of controls and estimator.*

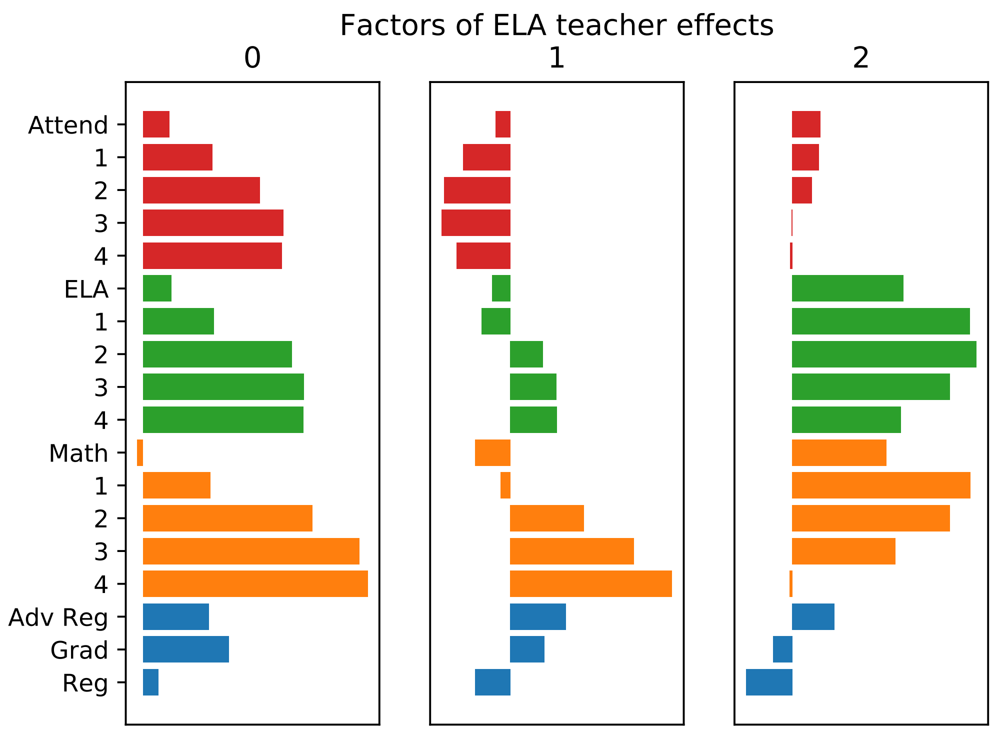

Figure 17: *Factors of $\tilde{\Sigma}_\mu$ for ELA teachers.*

Controls	Estimator	Graduated	Regents Diploma	Advanced Regents Diploma
Baseline	Moment-Matching	0.049	0.074	0.065
Baseline	MLE	0.051	0.076	0.065
Polynomial	Moment-Matching	0.059	0.073	0.077
Polynomial	MLE	0.059	0.072	0.062
All Lags	Moment-Matching	0.047	0.073	0.065
All Lags	MLE	0.052	0.078	0.065

Table 23: *Standard deviation of English teacher effects on graduation: Robustness to choice of controls and estimator.*

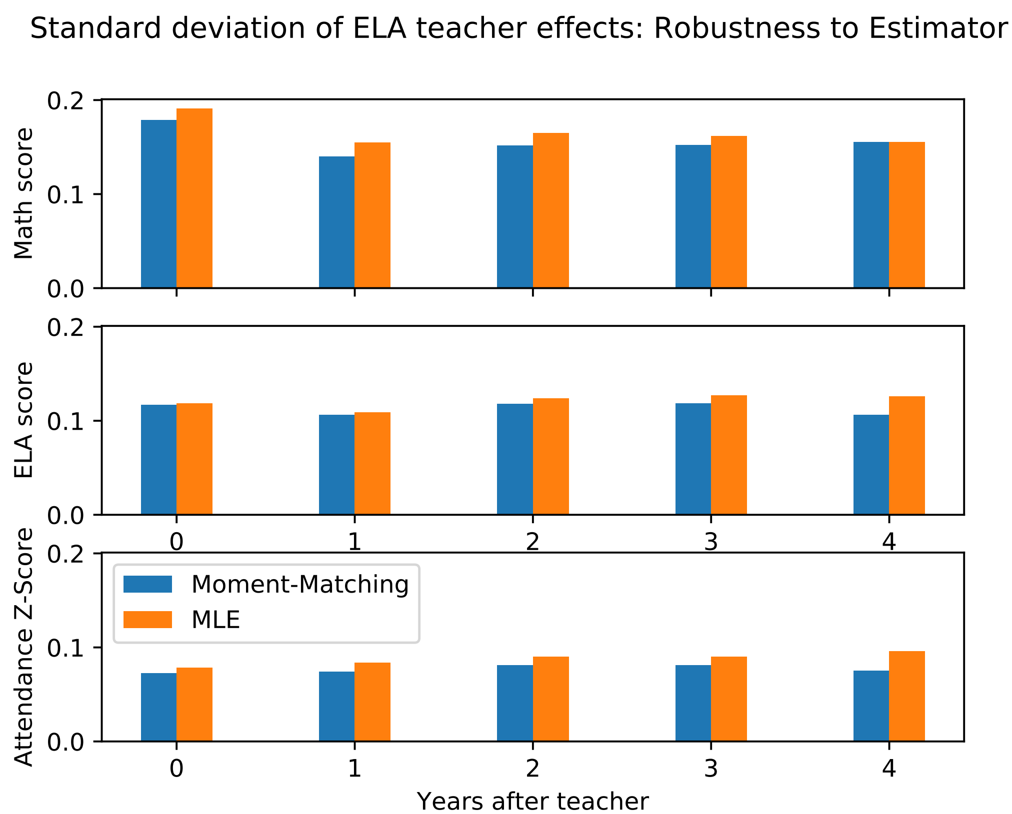

Figure 18: *Robustness of ELA teacher effects to estimator.*

Notes: Standard deviation of English teacher effects on present and future outcomes, for both the "moment-matching" estimator used above and maximum likelihood.

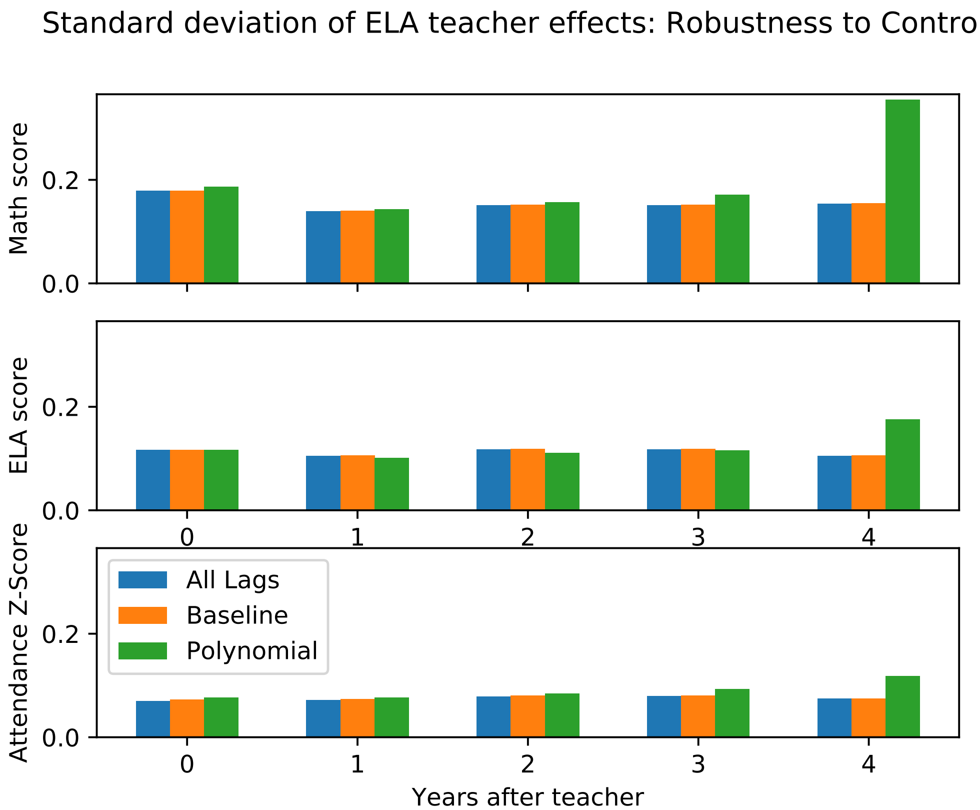

Figure 19: *Robustness of ELA teacher effects to choice of controls.*

Notes: Standard deviation of English teacher effects on present and future outcomes, for the baseline controls used above, baseline controls plus all available lags, and for the controls from Chetty *et al.* (2014a), which include third-degree polynomials.