

# **Teacher and Teaching Effects on Students' Attitudes and Behaviors**

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## **Abstract**

Research has focused predominantly on how teachers affect students' achievement on tests despite evidence that a broad range of attitudes and behaviors are equally important to their long-term success. We find that upper-elementary teachers have large effects on self-reported measures of students' self-efficacy in math, and happiness and behavior in class. Students' attitudes and behaviors are predicted by teaching practices most proximal to these measures, including teachers' emotional support and classroom organization. However, teachers who are effective at improving math test scores often are not equally effective at improving students' attitudes and behaviors. These findings lend evidence to well-established theory on the multidimensional nature of teaching and the need to identify strategies for improving the full range of teachers' skills.

*Keywords:* teacher effectiveness, instruction, non-cognitive outcomes, self-efficacy, happiness, behavior

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## **1. Introduction**

Empirical research on the education production function traditionally has examined how teachers and their background characteristics contribute to students' performance on standardized tests (Todd & Wolpin, 2003; Hanushek & Rivkin, 2010). However, a substantial body of evidence indicates that student learning is multidimensional, with many factors beyond their core academic knowledge as important contributors to both short- and long-term success.<sup>1</sup> For example, psychologists find that emotion and personality influence the quality of one's thinking (Baron, 1982) and how much a child learns in school (Duckworth, Quinn, & Tsukayama, 2012). Longitudinal studies document the strong predictive power of measures of childhood self-control, emotional stability, persistence, and motivation on health and labor market outcomes in adulthood (Borghans, Duckworth, Heckman, & Ter Weel, 2008; Chetty et al., 2011; Moffitt et. al., 2011). In fact, these sorts of attitudes and behaviors are stronger predictors of some long-term outcomes than test scores (Chetty et al., 2011).

Consistent with these findings, decades worth of theory also have characterized teaching as multidimensional. High-quality teachers are thought and expected not only to raise test scores but also to provide emotionally supportive environments that contribute to students' social and emotional development, to manage classroom behaviors, to deliver accurate content, and to support critical thinking (Cohen, 2011; Lampert, 2001; Pianta & Hamre, 2009). In recent years, two research traditions have emerged to test this theory using empirical evidence. The first tradition has focused on observations of classrooms as a means of identifying unique domains of teaching practice (Authors, 2015; Hamre et al., 2013). Several of these domains, including

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<sup>1</sup> Although student outcomes beyond test scores often are referred to as "non-cognitive" skills, our preference, like others (Duckworth & Yeager, 2015; Farrington et al., 2012), is to refer to each competency by name. For brevity, we refer to them as "attitudes and behaviors." We adopt these terms because they most closely characterize the measure we focus on in this paper.

teachers' interactions with students, classroom organization, and emphasis on critical thinking within specific content areas, aim to support students' development in areas beyond their core academic skill. The second research tradition has focused on estimating internally valid estimates of teachers' contribution to student outcomes, often referred to as "teacher effects" (Chetty, Friedman, & Rockoff, 2014; Hanushek & Rivkin, 2010). These studies have found that, as with test scores, teachers vary considerably in their ability to impact students' social and emotional development and a variety of observed school behaviors (Authors, 2016; Backes & Hansen, 2015; Gershenson, 2016; Jackson, 2012; Jennings & DiPrete, 2010; Koedel, 2008; Ladd & Sorensen, 2015; Ruzek et al., 2014). Further, weak to moderate correlations between teacher effects on different student outcomes suggest that test scores alone cannot identify teachers' skill in the classroom (Authors, 2016; Gershenson, 2016; Jackson, 2012; Jennings & DiPrete, 2010).

Our study is among the first to integrate these two research traditions, which largely have developed in isolation. Working at the intersection of these traditions, we aim both to maximize internal validity and to open up the "black box" of teacher effects by examining whether certain dimensions of teaching practice predict students' attitudes and behaviors. We refer to these relationships between teaching practice and student outcomes as "teaching effects." Specifically, we ask three research questions:

- (1) *To what extent do teachers impact students' attitudes and behaviors in class?*
- (2) *To what extent do specific teaching practices impact students' attitudes and behaviors in class?*
- (3) *Are teachers who are effective at raising test-score outcomes equally effective at developing positive attitudes and behaviors in class?*

To answer our research questions, we draw on a rich dataset from Blinded Project of upper-elementary teachers' math instruction that collected teacher-student links, observations of teaching practice on two established observation instruments, students' math performance on both high- and low-stakes tests, and a student survey that captured their attitudes and behaviors in class. We used this survey to construct our three primary outcomes: students' self-reported self-efficacy in math, happiness in class, and behavior in class. Although the specific attitudes and behaviors we examine are limited to those available in Blinded Project data, they are important outcomes of interest to researchers, policymakers, and parents (Borghans et al., 2008; Chetty et al., 2011; Farrington et al., 2012). They also align with theories linking teachers and teaching practice to outcomes beyond students' core academic skills (Bandura, Barbaranelli, Caprara, & Pastorelli, 1996; Pianta & Hamre, 2009), allowing us to test these theories explicitly.

We find that upper-elementary teachers in our sample have substantive impacts on students' self-reported attitudes and behaviors in addition to their math performance. We estimate that the variation in teacher effects on students' self-efficacy in math and behavior in class is of similar magnitude to the variation in teacher effects on math test scores. Teacher effects on students' happiness in class are even larger than those for test-based outcomes. Further, these outcomes are predicted by teaching practices most proximal to these measures, thus aligning with theory and providing important face and construct validity to these measures. Specifically, teachers' emotional support for students is related both to their self-efficacy in math and their happiness in class. Teachers' classroom organization predicts students' reports of their own behavior in class. Errors in teachers' presentation of mathematical content are negatively related to students' self-efficacy in math and happiness in class, as well as students' math performance. Finally, we find that teachers are not equally effective at improving all outcomes.

Compared to an unadjusted correlation between teacher effects on our two math achievement tests of 0.64, the strongest unadjusted correlation between teacher effects on students' math achievement and effects on their attitudes or behavior is 0.19.

Together, these findings add further evidence for the multidimensional nature of teaching and, thus, the need for researchers, policymakers, and practitioners to identify strategies for improving these skills. In our conclusion, we discuss several ways in which policymakers and practitioners may start to do so, including through the design and implementation of teacher evaluation systems, teacher recruitment policies, and strategic teacher assignments.

## **2. Review of Related Research**

Theories of teaching and learning have long emphasized the important role teachers play in supporting students' development in areas beyond their core academic skill. For example, in their conceptualization of high-quality teaching, Pianta and Hamre (2009) describe a set of emotional supports and organizational techniques that are equally important to learners as teachers' instructional methods. They posit that, by providing "emotional support and a predictable, consistent, and safe environment" (p. 113), teachers can help students become more self-reliant, motivated to learn, and willing to take risks. Further, by modeling strong organizational and management structures, teachers can help build students' own ability to self-regulate. Content-specific views of teaching also highlight the importance of teacher behaviors that develop students' attitudes and behaviors in ways that may not directly impact test scores. In mathematics, which is the focus of this paper, researchers and professional organizations have advocated for teaching practices that emphasize critical thinking and problem solving around authentic tasks (Lampert, 2001; National Council of Teachers of Mathematics [NCTM], 1989, 2014). Understanding the considerable stresses that this content can create for students, others

have pointed to teachers' equally important role of developing students' self-efficacy and decreasing their anxiety in math (Bandura et al., 1996; Usher & Pajares, 2008; Wigfield & Meece, 1988).

In recent years, development and use of observation instruments that capture the quality of teachers' instruction have provided a unique opportunity to examine these theories empirically. One instrument in particular, the Classroom Assessment Scoring System (CLASS), is organized around "meaningful patterns of [teacher] behavior...tied to underlying developmental processes [in students]" (Pianta & Hamre, 2009, p. 112). Factor analyses of data collected by this instrument have identified several unique aspects of teachers' instruction: teachers' social and emotional interactions with students, their ability to organize and manage the classroom environment, and their instructional supports in the delivery of content (Hafen et al., 2015; Hamre et al., 2013). A number of studies from developers of the CLASS instrument and their colleagues have described relationships between these dimensions and closely related student attitudes and behaviors. For example, teachers' interactions with students predicts students' social competence, engagement, and risk-taking; teachers' classroom organization predicts students' engagement and behavior in class (Burchinal et al., 2008; Downer, Rimm-Kaufman, & Pianta, 2007; Hamre, Hatfield, Pianta, & Jamil, 2014; Hamre & Pianta, 2001; Luckner & Pianta, 2011; Mashburn et al., 2008; Pianta, La Paro, Payne, Cox, & Bradley, 2002). With only a few exceptions (see Downer et al., 2007; Hamre & Pianta, 2001; Luckner & Pianta, 2011), these studies have focused on pre-kindergarten settings. Further, none of the studies can rule out the possibility of omitted variables bias – that is, that teachers who have strong interactions with students or behavior management techniques might also engage in additional practices that are responsible for higher student outcomes.

Additional content-specific observation instruments highlight several other teaching competencies with links to students' attitudes and behaviors. For example, in this study we draw on the Mathematical Quality of Instruction (MQI) to capture math-specific dimensions of teachers' classroom practice. Factor analyses of data captured both by this instrument and the CLASS identified two teaching skills in addition to those described above: the cognitive demand of math activities that teachers provide to students and the precision with which they deliver this content (Authors, 2015). Validity evidence for the MQI has focused on the relationship between these teaching practices and students' math test scores (Author, 2015; Kane & Staiger, 2012), which makes sense given the theoretical link between teachers' content knowledge, delivery of this content in the classroom, and students' own understanding of the content (Hill et al., 2008). However, as noted above, professional organizations and researchers also describe theoretical links between the sorts of teaching practices captured on the MQI and student outcomes beyond test scores (e.g., critical thinking, self-efficacy in math; Bandura et al., 1996; Lampert, 2001; NCTM, 1989, 2014; Usher & Pajares, 2008; Wigfield & Meece, 1988) that, to our knowledge, have not been tested.

In a separate line of research, several recent studies have borrowed from the literature on teachers' "value-added" to student test scores in order to document the magnitude of teacher effects on a range of other outcomes. Consistent with the teacher effectiveness literature more broadly, these studies attempt to isolate the unique effect of teachers on non-tested outcomes from factors outside of teachers' control (e.g., students' prior achievement, race, gender, socioeconomic status) and to limit any bias due to non-random sorting. In one of the first studies of this kind, Jennings and DiPrete (2010) used the Early Childhood Longitudinal Study – Kindergarten Cohort (ECLS-K) to estimate the role that teachers play in a composite measure of

kindergarten and first-grade students' social and behavioral outcomes. They found teacher effects on social and behavioral outcomes that were even larger (0.35 standard deviations [sd]) than effects on academic achievement. In a study of 35 middle school math teachers, Ruzek et al. (2014) found small but meaningful teacher effects on motivation between 0.03 sd and 0.08 sd among seventh graders. Authors (2016) found teacher effects on students' self-reported measures of grit, growth mindset and effort in class ranging between 0.14 and 0.17 sd. Additional studies identified teacher effects on observed school behaviors, including absences, suspensions, grades, grade progression, and graduation (Backes & Hansen, 2015; Gershenson, 2016; Jackson, 2012; Koedel, 2008; Ladd & Sorensen, 2015).

To date, evidence is mixed on the extent to which teachers who improve test scores also improve other outcomes. Four of the studies described above found weak relationships between teacher effects on students' academic performance and effects on other outcome measures. Compared to a correlation of 0.42 between teacher effects on math achievement versus effects on reading achievement, Jennings and DiPrete (2010) found correlations of 0.15 between teacher effects on students' social and behavioral outcomes and effects on either math or reading achievement. Authors (2016) found correlations between teacher effects on achievement outcomes and multiple social-emotional competencies were sometimes non-existent and never greater than 0.23. Similarly, Gershenson (2016) and Jackson (2012) found weak or null relationships between teacher effects on students' academic performance and effects on observed schools behaviors. However, correlations from two other studies were larger. Ruzek et al. (2014) estimated a correlation of 0.50 between teacher effects on achievement versus effects on students' motivation in math class. Drawing on data from the MET project, Mihaly, McCaffrey,

Staiger, and Lockwood (2013) found a correlation of 0.57 between middle school teacher effects on students' self-reported effort versus effects on math test scores.

Our analyses extend this body of research in several ways. First, we estimate teacher effects on additional attitudes and behaviors captured by students in upper-elementary grades. We also are able to leverage data that offer the unique combination of a moderately sized sample of teachers and students with lagged survey measures. Second, we utilize similar econometric approaches to test the relationship between teaching practice and these same attitudes and behaviors. These analyses allow us to examine the face and construct validity of our teacher effect estimates and the extent to which they align with theory. Finally, we examine teacher and teaching effects in the context of mathematics, which is essential for policy given a growing focus of education reform on STEM education (Duncan, 2010; U.S. Department of Education, 2010).

### **3. Data and Sample**

Beginning in the 2010-2011 school year, the Blinded Project engaged in a three-year data collection process. Data came from participating fourth- and fifth-grade teachers ( $N = 310$ ) in four anonymous, urban school districts on the East coast of the United States who agreed to have their classes videotaped, complete a teacher questionnaire, and help collect a set of student outcomes. Teachers were clustered within 52 schools, with an average of six teachers per school. Teacher-student links were verified for all study participants based on class rosters provided by teachers. While this study focused on teachers' math instruction, participants were generalists who taught all subject areas. This is important, as it allowed us to consider the contribution of individual teachers to students' attitudes and behaviors that was not confounded by the influence of multiple teachers in the same year. Despite having a non-random sample of teachers, evidence

from these same data indicated that teachers who participated in the study did not differ on their effectiveness at improving students' math test scores as those who did not participate (Authors, in press). We describe this sample in more depth below.

### *3.1. Students' Attitudes and Behaviors*

As part of the expansive data collection effort, researchers administered a student survey with items ( $N = 18$ ) that were adapted from other large-scale surveys including the TRIPOD survey project, the MET project, the National Assessment of Educational Progress (NAEP), and the Trends in International Mathematics and Science Study (TIMSS) (see Appendix Table 1 for a full list of items). Items were selected based on a review of the research literature and identification of constructs thought most likely to be influenced by upper-elementary teachers and math-specific teaching practices. Students rated all items on a five-point Likert scale where 1 = Totally Untrue and 5 = Totally True. We reverse coded items with negative valence in order to form composites with other items.

Researchers and policymakers have raised several concerns about the use of self-reported survey data to capture students' underlying attitudes and behaviors. Students – and elementary students in particular – may not be accurate reporters of their own attitudes and behaviors. Their responses can be prone to “social desirability bias,” in which students “provide answers that are desirable but not accurate” (Duckworth & Yeager, 2015, p. 239). Different frames of reference also can bias responses. For example, school-wide norms around behavior and effort may change the implicit standards of comparison that students use to judge their own behavior and effort (Authors, 2016). In response to these concerns, we describe validity evidence both from our own and other studies as we present each of our student outcomes below. We also attempted to minimize the potential threat posed by reference bias through our modeling strategy.

Specifically, we restricted comparisons to teachers and students in the same school, which helps limit potential differences in reference groups and social norms across schools that could confound our analyses.

We identified a parsimonious set of three outcome measures based on a combination of theory and exploratory factor analyses (see Appendix Table 1).<sup>2</sup> The first outcome, which we call *Self-Efficacy in Math* (10 items), is a variation on well-known constructs related to students' effort, initiative, and perception that they can complete tasks. In other datasets focused on elementary students, academic self-efficacy is correlated with math achievement around 0.21 (Multon, Brown, & Lent, 1991), which is quite close to the correlation we find between *Self-Efficacy in Math* and the two math test scores ( $r = 0.25$  and 0.22; see Table 1). These similarities provide important validity evidence for our construct. The second related outcome measure is *Happiness in Class* (5 items), which was collected in the second and third years of the study. Exploratory factor analyses suggested that these items clustered together with those from *Self-Efficacy in Math* to form a single construct. However, post-hoc review of these items against the psychology literature from which they were derived suggests that they can be divided into a separate domain. As above, this measure is a school-specific version of well-known scales that capture students' affect and enjoyment (Diener, 2000). Both *Self-Efficacy in Math* and *Happiness in Class* have relatively high internal consistency reliabilities (0.76 and 0.82, respectively) that are similar to those of self-reported attitudes and behaviors explored in other studies (Duckworth

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<sup>2</sup> We conducted factor analyses separately by year, given that there were fewer items in the first year. The Blinded Project project added additional items in subsequent years to help increase reliability. In the second and third years, each of the two factors has an eigenvalue above one, a conventionally used threshold for selecting factors (Kline, 1994). Even though the second factor consists of three items that also have loadings on the first factor between 0.35 and 0.48 – often taken as the minimum acceptable factor loading (Field, 2013; Kline, 1994) – this second factor explains roughly 20% more of the variation across teachers and, therefore, has strong support for a substantively separate construct (Field, 2013; Tabachnick & Fidell, 2001). In the first year of the study, the eigenvalue on this second factor is less strong (0.78), and the two items that load onto it also load onto the first factor.

et al., 2007; John & Srivastava, 1999; Tsukayama et al., 2013). Further, self-reported measures of similar constructs have been linked to long-term outcomes, including academic engagement and earnings in adulthood, even conditioning on cognitive ability (King, McInerney, Ganotice, & Villarosa, 2015; Lyubomirsky, King, & Diener, 2005).

The third and final construct consists of three items that were meant to hold together and which we call *Behavior in Class* (internal consistency reliability is 0.74). Higher scores reflect better, less disruptive behavior. Teacher reports of students' classroom behavior have been found to relate to antisocial behaviors in adolescence, criminal behavior in adulthood, and earnings (Chetty et al., 2011; Segal, 2013; Moffitt et al., 2011; Tremblay et al., 1992). Our analysis differs from these other studies in the self-reported nature of the behavior outcome. That said, other studies also drawing on elementary school students found correlations between self-reported and either parent- or teacher-reported measures of behavior that were similar in magnitude to correlations between parent and teacher reports of student behavior (Achenbach, McConaughy, & Howell, 1987; Goodman, 2001). Further, other studies have found correlations between teacher-reported behavior of elementary school students and either reading or math achievement ( $r = 0.22$  to  $0.28$ ; Miles & Stipek, 2006; Tremblay et al., 1992) similar to the correlation we find between students' self-reported *Behavior in Class* and our two math test scores ( $r = 0.24$  and  $0.26$ ; see Table 1). Together, this evidence provides both convergent and consequential validity evidence for this outcome measure.

For all three of these outcomes, we created final scales by averaging raw student responses across all available items and standardizing measures to have a mean of zero and a

standard deviation of one within each school year.<sup>3</sup> We standardized within years, given that, for some measures, the set of survey items varied across years.

### *3.2. Student Demographic and Test Score Information*

Student demographic and achievement data came from district administrative records.

Demographic data include gender, race/ethnicity, free- or reduced-price lunch (FRPL) eligibility, limited English proficiency (LEP) status, and special education (SPED) status. These records also included current- and prior-year test scores in math and English Language Arts (ELA) on state assessments, which we standardized within districts by grade, subject, and year using the entire sample of students in each district, grade, subject, and year.

The project also administered a low-stakes mathematics assessment to all students in the study. Validity evidence indicates internal consistency reliability of 0.82 or higher for each form across grade levels and school years (Hickman, Fu, & Hill, 2012). We used this assessment in addition to high-stakes tests given that teacher effects on two outcomes that aim to capture similar underlying constructs (i.e., math achievement) provide a unique point of comparison when examining the relationship between teacher effects on student outcomes that are less closely related (i.e., math achievement versus attitudes and behaviors). Indeed, students' high- and low-stake math test scores are correlated more strongly ( $r = 0.70$ ) than any other two outcomes (see Table 1). Coding of items from both the low- and high-stakes tests also identify a large degree of overlap in terms of content coverage and cognitive demand (Authors, 2015). All tests focused most on numbers and operations (40% to 60%), followed by geometry (roughly 15%), and algebra (15% to 20%). By asking students to provide explanations of their thinking to solve non-routine problems such as identifying patterns, the low-stakes test also was similar to

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<sup>3</sup> Depending on the outcome, between 4% and 8% of students were missing a subset of items from survey scales. In these instances, we created final scores by averaging across all available information.

the high-stakes tests in two districts; in the other two districts, items often asked students to execute basic procedures.

### 3.3. Mathematics Lessons

Teachers' mathematics lessons were captured over a three-year period, with an average of three lessons per teacher per year.<sup>4</sup> This number corresponds to recommendations by Authors (2012) to achieve sufficiently high levels of predictive reliability. Trained raters scored these lessons on two established observational instruments, the CLASS and the MQI. Analyses of these same data show that items cluster into four main factors (Authors, 2015). The two dimensions from the CLASS instrument capture general teaching practices: *Emotional Support* focuses on teachers' interactions with students and the emotional environment in the classroom, and is thought to increase students' social and emotional development; and *Classroom Organization* focuses on behavior management and productivity of the lesson, and is thought to improve students' self-regulatory behaviors (Pianta & Hamre, 2009).<sup>5</sup> The two dimensions from the MQI capture mathematics-specific practices: *Ambitious Mathematics Instruction* focuses on the complexity of the tasks that teachers provide to their students and their interactions around the content, thus corresponding to the set of professional standards described by NCTM (1989, 2014) and many elements contained within the *Common Core State Standards for Mathematics* (National Governors Association Center for Best Practices, 2010); *Mathematical Errors*

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<sup>4</sup> As described by Author (2015), capture occurred with a three-camera, digital recording device and lasted between 45 and 60 minutes. Teachers were allowed to choose the dates for capture in advance and directed to select typical lessons and exclude days on which students were taking a test. Although it is possible that these lessons were unique from a teachers' general instruction, teachers did not have any incentive to select lessons strategically as no rewards or sanctions were involved with data collection or analyses. In addition, analyses from the MET project indicate that teachers are ranked almost identically when they choose lessons themselves compared to when lessons are chosen for them (Ho & Kane, 2013).

<sup>5</sup> Developers of the CLASS instrument identify a third dimension, *Classroom Instructional Support*. Factor analyses of data used in this study showed that items from this dimension formed a single construct with items from *Emotional Support* (Authors, 2015). Given theoretical overlap between *Classroom Instructional Support* and dimensions from the MQI instrument, we excluded these items from our work and focused only on *Classroom Emotional Support*.

identifies any mathematical errors or imprecisions the teacher introduces into the lesson. Both dimensions from the MQI are linked to teachers' mathematical knowledge for teaching and, in turn, to students' math achievement (Author, 2015; Hill et al., 2008; Hill, Schilling, & Ball, 2004).

We estimate reliability for these metrics by calculating the amount of variance in teacher scores that is attributable to the teacher (the intraclass correlation [ICC]), adjusted for the modal number of lessons. These estimates are: 0.53, 0.63, 0.74, and 0.56 for *Emotional Support*, *Classroom Organization*, *Ambitious Mathematics Instruction*, and *Mathematical Errors*, respectively (see Table 2). Though some of these estimates are lower than conventionally acceptable levels (0.7), they are consistent with those generated from similar studies (Kane & Staiger, 2012). Correlations between dimensions range from roughly 0 (between *Emotional Support* and *Mathematical Errors*) to 0.46 (between *Emotional Support* and *Classroom Organization*). Given that teachers contributed different number of lessons to the project, which could lead to noise in these observational measures, we utilized empirical Bayes estimation to shrink scores back to the mean based on their precision (see below for more details). We standardized final scores within the full sample of teachers to have a mean of zero and a standard deviation of one.

### 3.4. Sample Restrictions

In choosing our analysis sample, we faced a tradeoff between precision and internal validity. Including all possible teachers would maximize the precision of our estimates. At the same time, we lacked critical data for some students and teachers that could have been used to guard against potential sources of bias. Thus, we chose to make two important restrictions to our original sample of teachers in order to strengthen the internal validity of our findings. First, for

all analyses predicting students' attitudes and behaviors, we only included fifth grade teachers who happened to have students who also had been part of the project in the fourth grade and, therefore, took the survey in the prior year. This group included between 51 and 111 teachers and between 548 and 1,529 students. For analyses predicting test score outcomes, we were able to maintain the full sample of 310 teachers, whose 10,575 students all had test scores in the previous year. Second, in analyses relating domains of teaching practice to student outcomes, we further restricted our sample to teachers who themselves were part of the study for more than one year, which allowed us to use out-of-year observation scores that were not confounded with the specific set of students in the classroom. This reduced our analysis samples to between 47 and 93 teachers and between 517 and 1,362 students when predicting students' attitudes and behaviors, and 196 teachers and 8,660 students when predicting math test scores. We describe the rationale for these restrictions in more detail below.

In Table 3, we present descriptive statistics on teachers and their students in the full sample (column 1), as well as those who were ever in any of our analyses predicting students' attitudes and behaviors (column 2).<sup>6</sup> We find that teachers look relatively similar across these two analytic samples, with no statistically significant differences on any observable characteristics.<sup>7</sup> Sixteen percent of teachers were male and 65% were white. Eight percent received their teaching certification through an alternative pathway. The average number of

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<sup>6</sup> Information on teachers' background and knowledge were captured on a questionnaire administered in the fall of each year. Survey items included gender, race/ethnicity, years teaching math, route to certification, and amount of undergraduate or graduate coursework in math and math courses for teaching (scored on a Likert scale from 1 to 4). For simplicity, we averaged these last two items to form one construct capturing teachers' mathematics coursework. Further, the survey included a test of teachers' mathematical content knowledge, with items from both the Mathematical Knowledge for Teaching assessment (Hill, Schilling, & Ball, 2004), which captures math-specific pedagogical knowledge, and the Massachusetts Test for Educator Licensure. Teacher scores were generated by IRTPro software and standardized in these models, with a reliability of 0.92. (For more information about these constructs, see Authors, 2015.)

<sup>7</sup> Descriptive statistics and formal comparisons of other samples show similar patterns and are available upon request.

years of teaching experience was roughly 10. Value-added scores on state math tests were right around the mean for each district (0.01 sd). Authors (in press) tested formally for differences in these value-added scores between project teachers and the full population of teachers in each district and found none, lending important external validity to our findings

We do observe some statistically significant differences between student characteristics in the full sample versus the subsample. For example, the percentage of students identified as limited English proficient was 20% in the full sample compared to 14% in the sample of students who ever were part of analyses drawing on our survey measures. Average prior achievement scores were 0.10 sd and 0.09 sd in math and ELA in the full sample, respectively, compared to 0.18 sd and 0.20 sd in the subsample. Although variation in samples could result in dissimilar estimates across models, the overall character of our findings is unlikely to be driven by these modest differences. Further, students in our samples look similar to those in many urban districts in the United States, where roughly 68% are eligible for free or reduced-price lunch, 14% are classified as in need of special education services, and 16% are identified as limited English proficient; roughly 31% are African American, 39% are Hispanic, and 28% are white (Council of the Great City Schools, 2013).

#### **4. Empirical Strategy**

##### *4.1. Estimating Teacher Effects on Students' Attitudes and Behaviors*

Like others who aim to examine the contribution of individual teachers to student outcomes, we began by specifying an education production function model of each outcome for student  $i$  in district  $d$ , school  $s$ , grade  $g$ , class  $c$  with teacher  $j$  at time  $t$ :

$$(1) \quad OUTCOME_{ids gjct} = \alpha f(A_{it-1}) + \pi X_{it} + \varphi \bar{X}_{it}^c + \tau_{dgt} + (\mu_j + \delta_{jc} + \varepsilon_{ids gjct})$$

$OUTCOME_{idsjct}$  is used interchangeably for both math test scores and students' attitudes and behaviors, which we modeled in separate equations as a cubic function of students' prior achievement,  $A_{it-1}$ , in both math and ELA on the high-stakes district tests<sup>8</sup>; demographic characteristics,  $X_{it}$ , including gender, race, FRPL eligibility, SPED status, and LEP status; these same test-score variables and demographic characteristics averaged to the class level,  $\bar{X}_{it}^c$ ; and district-by-grade-by-year fixed effects,  $\tau_{dgt}$ , that account for scaling of high-stakes test scores at this level. The error structure consists of both teacher- and class-level random effects,  $\mu_j$  and  $\delta_{jc}$ , respectively, and a student-specific error term,  $\varepsilon_{idsjct}$ . Given our focus on elementary teachers, over 97% of teachers in our sample worked with just one set of students in a given year. Thus, class effects are estimated by observing teachers in multiple years and are analogous to teacher-by-year effects.

The key identifying assumption of this model is that estimates are not biased by non-random sorting of students to teachers. Recent experimental (Kane, McCaffrey, Miller, & Staiger, 2013) and quasi-experimental (Chetty et al., 2014) analyses provide strong empirical support for this claim when student achievement is the outcome of interest. However, much less is known about bias and sorting mechanisms when other outcomes are used. For example, it is quite possible that students were sorted to teachers based on their classroom behavior in ways that were unrelated to their prior achievement. To address this possibility, we made two modifications to equation (2). First, we included school fixed effects,  $\sigma_s$ , to account for sorting of

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<sup>8</sup> We controlled for prior-year scores only on the high-stakes assessments and not on the low-stakes assessment for three reasons. First, including prior low-stakes test scores would reduce our full sample by more than 2,200 students. This is because the assessment was not given to students in District 4 in the first year of the study ( $N = 1,826$  students). Further, an additional 413 students were missing fall test scores given that they were not present in class on the day it was administered. Second, prior-year scores on the high- and low-stakes test are correlated at 0.71, suggesting that including both would not help to explain substantively more variation in our outcomes. Third, sorting of students to teachers is most likely to occur based on student performance on the high-stakes assessments since it was readily observable to schools; achievement on the low-stakes test was not.

students and teachers across schools. This means that estimates rely only on between-school variation, which has been common practice in the research literature when estimating teacher effects on student achievement. In their review of this literature, Hanushek and Rivkin (2010) propose ignoring the between-school component because it is “surprisingly small” and because including this component leads to “potential sorting, testing, and other interpretative problems” (p. 268). Other recent studies estimating teacher effects on student outcomes beyond test scores have used this same approach (Backes & Hansen, 2015; Gershenson, 2016; Jackson, 2012; Ladd & Sorensen, 2015). Another important benefit of within-school comparisons is that it minimizes the possibility of reference bias in our self-reported measures (Authors, 2016; Duckworth & Yeager, 2015). As a second modification for models that predict each of our three student survey measures, we included  $OUTCOME_{it-1}$  on the right-hand side of the equation in addition to prior achievement – that is, when predicting students’ *Behavior in Class*, we controlled for students’ self-reported *Behavior in Class* in the prior year.<sup>9</sup> This strategy helps account for within-school sorting on factors other than prior achievement.

Using equation (1), we estimated the variance of  $\mu_j$ , which is the stable component of teacher effects. We report the standard deviation of these estimates across outcomes. This parameter captures the magnitude of the variability of teacher effects. With the exception of teacher effects on students’ *Happiness in Class*, where survey items were not available in the first year of the study, we included  $\delta_{jc}$  in order to separate out the time-varying portion of

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<sup>9</sup> It is important to note that adding prior survey responses to the education production function is not entirely analogous to doing so with prior achievement scores. While achievement outcomes have roughly the same reference group across administrations, the surveys do not. This is because survey items often asked about students’ experiences “in this class.” All three *Behavior in Class* items and all five *Happiness in Class* items included this or similar language, as did five of the 10 items from *Self-Efficacy in Math*. That said, moderate year-to-year correlations of 0.39, 0.38, and 0.53 for *Self-Efficacy in Math*, *Happiness in Class*, and *Behavior in Class*, respectively, suggest that these items do serve as important controls. Comparatively, year-to-year correlations for the high- and low-stakes tests are 0.75 and 0.77.

teacher effects, combined with peer effects and any other class-level shocks. The fact that we are able to separate class effects from teacher effects is an important extension of prior studies examining teacher effects on outcomes beyond test scores, many of which only observed teachers at one point in time. Because  $\mu_j$  is measured imprecisely given typical class sizes, unadjusted estimates would overstate the true variation in teacher effects. Thus, we utilized empirical Bayes estimation to shrink each score for teacher  $j$  back toward the mean based on its precision (Raudenbush & Bryk, 2002), where precision is a function of the number of students attributed to each teacher or class. Like others interested in the variance of teacher effects (e.g., Chetty et al., 2011), we specified this parameter as a random effect, which provides unbiased model-based estimates of the true population variance of teacher effects.<sup>10</sup>

#### *4.2. Estimating Teaching Effects on Students' Attitudes and Behaviors*

We examined the contribution of teachers' classroom practices to our set of student outcomes by estimating a variation of equation (1):

$$(2) \quad OUTCOME_{idsjct} = \beta OBSERVATION_{lj,-t} + \alpha f(A_{it-1}) + \gamma OUTCOME_{it-1} + \pi X_{it} + \varphi \bar{X}_{it}^c + \sigma_s + \tau_{dgt} + (\mu_j + \delta_{jc} + \varepsilon_{idsjct})$$

This multi-level model includes the same set of control variables as above in order to account for the non-random sorting of students to teachers and for factors beyond teachers' control that might influence each of our outcomes. We further included a vector of their teacher  $j$ 's observation scores,  $OBSERVATION_{lj,-t}$ . The coefficients on these variables are our main

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<sup>10</sup> We estimated these variance components using restricted maximum likelihood estimation because full maximum likelihood estimates tend to be biased downward (Harville, 1977; Raudenbush & Bryk, 2002) and may be particularly problematic in our smaller subsample of students and teachers who had prior-year measures of their attitudes and behaviors.

parameters of interest and can be interpreted as the change in standard deviation units for each outcome associated with exposure to teaching practice one standard deviation above the mean.<sup>11</sup>

One concern when relating observation scores to student survey outcomes is that they may capture the same behaviors. For example, teachers may receive credit on the *Classroom Organization* domain when their students demonstrate orderly behavior. In this case, we would have the same observed behaviors on both the left and right side of our equation relating instructional quality to student outcomes, which would inflate our teaching effect estimates. A related concern is that the specific students in the classroom may influence teachers' instructional quality (Authors, 2015; Steinberg & Garrett, in press; Whitehurst, Chingos, & Lindquist, 2014).<sup>12</sup> While the direction of bias is not as clear here – as either lesser- or higher-quality teachers could be sorted to harder to educate classrooms – this possibility also could lead to incorrect estimates. To avoid these sources of bias, we only included lessons captured in years other than those in which student outcomes were measured, denoted by  $-t$  in the subscript of  $OBSERVATION_{lj,-t}$ . As noted above, these are predicted estimates that aim to reduce measurement error in our observation measures.<sup>13</sup> To the extent that instructional quality varies

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<sup>11</sup> Models were fit using full maximum likelihood, given our focus in this analysis on the fixed rather than the stochastic portion of the model; full maximum likelihood allows us to compare estimates from the fixed portion of the equation between nested models (Harville, 1977; Raudenbush & Bryk, 2002).

<sup>12</sup> In our dataset, observable classroom characteristics do not appear to influence teachers' observation ratings. Correlations between observation scores adjusted for classroom characteristics, including gender, race, free or reduced-price lunch eligibility, special education status, limited English proficiency, and prior achievement in both math and English language arts – and unadjusted scores range from 0.93 (for *Classroom Organization*) to 0.97 (for *Mathematical Errors*). Further, patterns of results in our teaching effect estimates are almost identical when we use adjusted versus unadjusted scores. Below, we present findings with unadjusted scores.

<sup>13</sup> To estimate these scores, we specified the following hierarchical linear model separately for each school year:

$$OBSERVATION_{lj,-t} = \gamma_j + \varepsilon_{ljt}$$

The outcome is the observation score for lesson  $l$  from teacher  $j$  in years other than  $t$ ;  $\gamma_j$  is a random effect for each teacher, and  $\varepsilon_{ljt}$  is the residual. For each domain of teaching practice and school year, we utilized standardized estimates of the teacher-level residual as each teacher's observation score in that year. Thus, scores vary across time. In the main text, we refer to these teacher-level residual as  $OBSERVATION_{lj,-t}$  rather than  $\hat{\gamma}_j$  for ease of interpretation for readers.

across years, using out-of-year observation scores creates a lower-bound estimate of the true relationship between instructional quality and student outcomes. We consider this an important tradeoff to minimize potential bias.

An additional concern for identification is the endogeneity of observed classroom quality. Our preferred analytic approach attempted to account for potential sources of bias by conditioning estimates of the relationship between one dimension of teaching practice and student outcomes on the three other dimensions.<sup>14</sup> An important caveat here is that we only observed teachers' instruction during math lessons and, thus, may not capture important pedagogical practices teachers used with these students when teaching other subjects. Including dimensions from the CLASS instrument, which are meant to capture instructional quality across subject areas (Pianta & Hamre, 2009), helps account for some of this concern. However, given that we were not able to isolate one dimension of teaching quality from all others, we consider this approach as providing suggestive rather than conclusive evidence on the underlying causal relationship between teaching practice and students' attitudes and behaviors.

#### *4.3. Estimating the Relationship Between Teacher Effects Across Multiple Student Outcomes*

In our third and final set of analyses, we examined whether teachers who are effective at raising math test scores are equally effective at developing students' attitudes and behaviors. To do so, we drew on equation (1) to estimate  $\hat{\mu}_j$  for each outcome and teacher  $j$ . These estimates capture the residual variation in each outcome attributable to each teacher, or their "value-added"

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<sup>14</sup> For our main analyses, we chose not to control for other observable characteristics of teachers (e.g., teaching experience, math content knowledge, certification pathway, education), as these factors may be tied directly to teachers' practices. From a policy perspective, we are less interested in *where* and *how* teachers picked up good practices, so long as they have them. That said, in separate analyses (available upon request), we re-ran models controlling for the four background characteristics listed above and found that patterns of results were unchanged. None of these teacher characteristics predicted student outcomes when also controlling for dimensions of teaching quality.

score. Then, we generated a correlation matrix of these teacher effect estimates. For consistency, we continued to specify this parameter as a random effect rather than fixed effects.

Despite attempts to increase the precision of these estimates through empirical Bayes estimation, estimates of individual teacher effects are measured with error that will attenuate these correlations (Spearman, 1904). Thus, if we were to find weak to moderate correlations between different measures of teacher effectiveness, this could identify multidimensionality or could result from measurement challenges, including the validity and reliability of individual constructs (Chin & Goldhaber, 2015). For example, prior research suggests that different tests of students' academic performance can lead to different teacher rankings, even when those tests measure similar underlying constructs (Lockwood et al., 2007; Papay, 2011). To address this concern, we focus our discussion on relative rankings in correlations between teacher effect estimates rather than their absolute magnitudes. Specifically, we examine how correlations between teacher effects on two closely related student outcomes (e.g., two math achievement tests) compare with correlations between teacher effects on outcomes that aim to capture different underlying constructs. In light of research highlighted above, we did not expect the correlation between teacher effects on high- and low-stakes math tests to be 1 (or, for that matter, close to 1). However, we hypothesized that these relationships should be stronger than the relationship between teacher effects on students' math performance and effects on their attitudes and behaviors. We also present disattenuated correlations in an online appendix to confirm that the conclusions we draw from these comparisons are not a product of differential measurement properties across outcomes.

## 5. Results

### 5.1. *Do Teachers Impact Students' Attitudes and Behaviors?*

We begin by presenting results of the magnitude of teacher effects in Table 4. Here, we observe sizable teacher effects on students' attitudes and behaviors that are similar to teacher effects on students' academic performance. Starting first with teacher effects on students' academic performance, we find that a one standard deviation difference in teacher effectiveness is equivalent to a 0.17 sd or 0.18 sd difference in students' math achievement. In other words, relative to an average teacher, teachers at the 84<sup>th</sup> percentile of the distribution of effectiveness move the medium student up to roughly the 57<sup>th</sup> percentile of math achievement. Notably, these findings are similar to those from other studies that also estimate within-school teacher effects in large administrative datasets (Hanushek & Rivkin, 2010). This suggests that our use of school fixed effects with a more limited number of teachers observed within a given school does not appear to overly restrict our identifying variation.<sup>15</sup> Estimated teacher effects on students' self-reported *Self-Efficacy in Math* and *Behavior in Class* are 0.14 sd and 0.15 sd, respectively. The largest teacher effects we observe are on students' *Happiness in Class*, of 0.31 sd. Given that we do not have multiple years of data to separate out class effects for this measure, we interpret this estimate as the upward bound of true teacher effects on *Happiness in Class*. Rescaling this estimate by the ratio of teacher effects with and without class effects for *Self-Efficacy in Math* ( $0.14/0.19 = 0.74$ ) produces an estimate of stable teacher effects on *Happiness in Class* of 0.23 sd, still larger than effects for other outcomes.<sup>16</sup>

## 5.2. *Do Specific Teaching Practices Impact Students' Attitudes and Behaviors?*

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<sup>15</sup> The variation in teacher effects on each outcome is similar in models that exclude school fixed effects. Specifically, these estimates are 0.19 sd for teacher effects on the high-stakes math test, 0.20 sd for teacher effects on the low-stakes math test, 0.15 sd for teacher effects on *Self-Efficacy in Math*, 0.33 sd for teacher effects on *Happiness in Class*, and 0.15 sd for teacher effects on *Behavior in Class*. Results are almost identical when we control for observable school characteristics.

<sup>16</sup> We find that teacher effects from models that exclude class effects are between 13% to 36% larger in magnitude than effects from models that include these class effects. This suggests that analyses that do not take into account classroom level shocks likely produce upwardly biased estimates of stable teacher effects.

Next, we examine whether certain characteristics of teachers' instructional practice help explain the sizable teacher effects described above (see Table 5). We present unconditional estimates in Panel A, where the relationship between one dimension of teaching practice and student outcomes is estimated without controlling for the other three dimensions. Thus, cells contain estimates from separate regression models. In Panel B, we present conditional estimates, where all four dimensions of teaching quality are included in the same regression model. Here, columns contain estimates from separate regression models. In all models, we control for student and class characteristics, and school fixed effects. We present all estimates as standardized effect sizes, which allows us to make comparisons across models and outcome measures. With one exception, which we highlight below, unconditional and conditional estimates are quite similar. Therefore, we focus on discussion on our preferred conditional estimates.

We find that students' attitudes and behaviors are predicted by both general and content-specific teaching practices in ways that generally align with theory. For example, teachers' *Emotional Support* is positively associated with the two closely related student constructs, *Self-Efficacy in Math* and *Happiness in Class*. Specifically, a one standard deviation increase in teachers' *Emotional Support* is associated with a 0.14 sd increase in students' *Self-Efficacy in Math* and a 0.37 sd increase in students' *Happiness in Class*. These finding makes sense given that *Emotional Support* captures teacher behaviors such as their sensitivity to students, regard for students' perspective, and the extent to which they create a positive climate in the classroom. We also find that *Classroom Organization*, which captures teachers' behavior management skills and productivity in delivering content, is positively related to students' reports of their own *Behavior in Class* (0.08 sd). This suggests that teachers who create an orderly classroom likely create a model for students' own ability to self-regulate. Despite this positive relationship, we find that

*Classroom Organization* is negatively associated with *Happiness in Class* (-0.23 sd), suggesting that classrooms that are overly focused on routines and management are negatively related to students' enjoyment in class. At the same time, this is the one instance where our estimate is sensitive to other teaching characteristics included in the model; when we estimate the relationship between teachers' *Classroom Organization* and students' *Happiness in Class* without controlling for the three other dimensions of teaching quality, this estimate is roughly 0 sd and is not statistically significant. Finally, we find that the degree to which teachers commit *Mathematical Errors* is negatively related to students' *Self-Efficacy in Math* (-0.09 sd) and *Happiness in Class* (-0.18 sd). These findings illuminate how a teacher's ability to present mathematics with clarity and without serious mistakes is related to their students' perceptions that they can complete math tasks and their enjoyment in class.<sup>17</sup>

Comparatively, when predicting scores on both math tests, we only find one marginally significant relationship – between *Mathematical Errors* and the high-stakes math test (-0.02 sd). For two other dimensions of teaching quality, *Emotional Support* and *Ambitious Mathematics Instruction*, estimates are signed in the way we would expect and with similar magnitudes, though they are not statistically significant. Given the consistency of estimates across the two math tests and our restricted sample size, it is possible that non-significant results are due to limited statistical power.<sup>18</sup> At the same time, even if true relationships exist between these

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<sup>17</sup> When we adjusted *p*-values for estimates presented in Table 5 to account for multiple hypothesis testing using both the Šidák and Bonferroni algorithms (Dunn, 1961; Šidák, 1967), relationships between *Emotional Support* and both *Self-Efficacy in Math* and *Happiness in Class*, as well as between *Mathematical Errors* and *Self-Efficacy in Math* remained statistically significant.

<sup>18</sup> In similar analyses in a subset of the Blinded Project data, Author (2015) did find a statistically significant relationship between *Ambitious Mathematics Instruction* and the low-stakes math test of 0.11 sd. The 95% confidence interval around that point estimate overlaps with the 95% confidence interval relating *Ambitious Mathematics Instruction* to the low-stakes math test in this analysis. Estimates of the relationship between the other three domains of teaching practice and low-stakes math test scores were of smaller magnitude and not statistically significant. Differences between the two studies likely emerge from the fact that we drew on a larger sample with an additional year of data, as well as slight modifications to our identification strategy.

teaching practices and students' math test scores, they are likely weaker than those between teaching practices and students' attitudes and behaviors. For example, we find that the 95% confidence intervals relating *Classroom Emotional Support* to *Self-Efficacy in Math* [0.068, 0.202] and *Happiness in Class* [0.162, 0.544] do not overlap with the 95% confidence intervals for any of the point estimates predicting math test scores. This suggests that, still, very little is known about how specific classroom teaching practices are related to student achievement in math.

### 5.3. Are Teachers Equally Effective at Raising Different Student Outcomes?

In Table 6, we present correlations between teacher effects on each of our student outcomes. The fact that teacher effects are measured with error makes it difficult to estimate the precise magnitude of these correlations. Instead, we describe relative differences in correlations, focusing on the extent to which teacher effects within outcome type – i.e., teacher effects on the two math achievement tests or effects on students' attitudes and behaviors – are similar or different from correlations between teacher effects across outcome type. We illustrate these differences in Figure 1, where Panel A presents scatter plots of these relationships between teacher effects within outcome type and Panel B does the same across outcome type. Recognizing that not all of our survey outcomes are meant to capture the same underlying construct, we also describe relative differences in correlations between teacher effects on these different measures. We also note that even an extremely conservative adjustment that scales correlations by the inverse of the square root reliabilities lead to a similar overall pattern of results (see Appendix Table 2 for reliabilities and Appendix Table 3 for disattenuated correlations).<sup>19</sup>

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<sup>19</sup> We estimated the reliability of our teacher effects estimate through the signal-to-noise ratio:

Examining the correlations of teacher effect estimates reveals that individual teachers vary considerably in their ability to impact different students outcomes. As hypothesized, we find the strongest correlations between teacher effects within outcome type. Similar to Corcoran, Jennings, and Beveridge (2012), we estimate a correlation of 0.64 between teacher effects on our high- and low-stakes math achievement tests. We also observe a strong correlation of 0.49 between teacher effects on two of the student survey measures, students' *Behavior in Class* and *Self-Efficacy in Math*. Comparatively, the correlations between teacher effects across outcome type are much weaker. Examining the scatter plots in Figure 1, we observe much more dispersion around the best-fit line in Panel B than in Panel A. The strongest relationship we observe across outcome types is between teacher effects on the low-stakes math test and effects on *Self-Efficacy in Math* ( $r = 0.19$ ). The lower bound of the 95% confidence interval around the correlation between teacher effects on the two achievement measures [0.56, 0.72] does not overlap with the 95% confidence interval of the correlation between teacher effects on the low-

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$$\frac{Var(\mu_j)}{Var(\mu_j) + \left( \frac{\sum_{j=1}^n se_j^2}{n} \right)}$$

The numerator is the observed variance in the teacher effect, or the squared value of the standard deviation of  $\mu_j$ , which is our main parameter of interest. The denominator is an estimate of the true teacher-level variance, which we approximate as the sum of the estimated variance in the teacher effect and the average squared standard error of individual teacher effect estimates. The number of teachers in the sample is denoted by  $n$ , and  $se_j$  is the standard error of the teacher effect for teacher  $j$ . See McCaffrey, Sass, Lockwood, & Mihaly (2009) for a similar approach.

In Appendix Table 2, we calculate two sets of estimates. The first calculates the precision of our main teacher effect estimates, which we use to calculate disattenuated correlations in Appendix Table 3. Given that these teacher effect estimates are derived from models with slightly different samples, which could impact reliability, we also calculated these estimates of precision in a balanced sample of teachers and students who had complete data on all measures (column 2;  $N = 51$  teachers and 548 students). Here, we found that precision was quite comparable across teacher effects, ranging from 0.50 (for teacher effects on *Self Efficacy in Math*) to 0.56 (for teacher effects on *Happiness in Class*).

In Appendix Table 3, relative differences in disattenuated correlations are similar to those presented above. We still observe much stronger relationships between teacher effects on the two math tests and between teacher effects on *Behavior in Class* and *Self-Efficacy in Math* than between other outcome measures. In some cases, these disattenuated correlations are close to 1, which we argue are unlikely to be the true relationships in the population. Overcorrections likely are driven by moderate reliabilities and moderate sample sizes (Zimmerman & Williams, 1997).

stakes math test and effects on *Self-Efficacy in Math* [-0.01, 0.39], indicating that these two correlations are substantively and statistically significantly different from each other. Using this same approach, we also can distinguish the correlation describing the relationship between teacher effects on the two math tests from all other correlations relating teacher effects on test scores to effects on students' attitudes and behaviors. We caution against placing too much emphasis on the negative correlations between teacher effects on test scores and effects on *Happiness in Class* ( $r = -0.09$  and  $-0.21$  for the high- and low-stakes tests, respectively). Given limited precision of this relationship, we cannot reject the null hypothesis of no relationship or rule out weak, positive or negative correlations among these measures.

Although it is useful to make comparisons between the strength of the relationships between teacher effects on different measures of students' attitudes and behaviors, measurement error limits our ability to do so precisely. At face value, we find correlations between teacher effects on *Happiness in Class* and effects on the two other survey measures ( $r = 0.26$  for *Self-Efficacy in Math* and  $0.21$  for *Behavior in Class*) that are weaker than the correlation between teacher effects on *Self-Efficacy in Math* and effects on *Behavior in Class* described above ( $r = 0.49$ ). One possible interpretation of these findings is that teachers who improve students' *Happiness in Class* are not equally effective at raising other attitudes and behaviors. For example, teachers might make students happy in class in unconstructive ways that do not also benefit their self-efficacy or behavior. At the same time, these correlations between teacher effects on *Happiness in Class* and the other two survey measures have large confidence intervals, likely due to imprecision in our estimate of teacher effects on *Happiness in Class*. Thus, we are not able to distinguish either correlation from the correlation between teacher effects on *Behavior in Class* and effects on *Self-Efficacy in Math*.

## **6. Discussion and Conclusion**

The teacher effectiveness literature has profoundly shaped education policy over the last decade and has served as the catalyst for sweeping reforms around teacher recruitment, evaluation, development, and retention. However, by and large, this literature has focused on teachers' contribution to students' test scores. Even research studies such as the MET project and new teacher evaluation systems that focus on "multiple measures" of teacher effectiveness (Center on Great Teachers and Leaders, 2013; Kane et al., 2013) generally attempt to validate other measures, such as observations of teaching practice, by examining their relationship to students' academic performance.

Our study extends an emerging body of research examining the effect of teachers on student outcomes beyond test scores. In many ways, our findings align with conclusions drawn from previous studies that also identify teacher effects on students' attitudes and behaviors (Authors, 2016; Jennings & DiPrete, 2010; Ruzek et al., 2014), as well as weak relationships between different measures of teacher effectiveness (Gershenson, 2016; Jackson, 2012; Jennings & DiPrete, 2010; Kane & Staiger, 2012). Although our study focuses on a small to moderate sample of teachers, our rich dataset builds on prior work in several ways. To our knowledge, this study is the first to identify teacher effects on measures of students' self-efficacy in math and happiness in class, as well as on a self-reported measure of student behavior. These findings suggest that teachers can and do help develop attitudes and behaviors among their students that are important for success in life. By interpreting teacher effects alongside teaching effects, we also provide strong face and construct validity for our teacher effect estimates. We find that improvements in upper-elementary students' attitudes and behaviors are predicted by general teaching practices in ways that align with hypotheses laid out by instrument developers (Pianta &

Hamre, 2009). Findings linking errors in teachers' presentation of math content to students' self-efficacy in math, in addition to their math performance, also are consistent with theory (Bandura et al., 1996). Finally, the broad data collection effort from Blinded Project allows us to examine relative differences in relationships between measures of teacher effectiveness, thus avoiding some concerns about how best to interpret correlations that differ substantively across studies (Chin & Goldhaber, 2015). We find that correlations between teacher effects on student outcomes that aim to capture different underlying constructs (e.g., math test scores and behavior in class) are weaker than correlations between teacher effects on two outcomes that are much more closely related (e.g., math achievement).

These findings can inform policy and practice in several key ways. Beginning first with policy, growing evidence of teacher effects on students' attitudes and behaviors may generate interest among some policymakers to incorporate these estimates into high-stakes decisions. This is particularly relevant after passage of the Every Student Succeeds Act (ESSA), which mandates that states select a nonacademic indicator with which to assess students' success in school (ESSA, 2015). Including measures of students' attitudes and behaviors in accountability or evaluation systems, even with very small associated weights, could serve as a strong signal that schools and educators should value and attend to developing these skills in the classroom. But, like other researchers (Duckworth & Yeager, 2015), we caution against a rush to incorporate these measures into high-stakes decisions. Hastily adding these measures into accountability systems without providing teachers with the training and support necessary to expand their skillsets could lead to superficial and, ultimately, counterproductive efforts to improve students' attitudes and behaviors. Furthermore, the science behind developing measures of students' attitudes and behaviors is relatively new compared to the long history of developing

valid and reliable assessment of cognitive aptitude and content knowledge. Most existing measures, including those used in this study, were developed for research purposes rather than large-scale testing with repeated administrations. In particular, open questions remain about whether reference bias substantially distorts comparisons across schools and about the susceptibility of these measures to “survey” coaching when high-stakes incentives are attached. Such incentives likely would render teacher assessments of their students’ attitudes and behaviors inappropriate. Thus, there is a clear need for additional research on the reliability and validity of self-report measures of students’ attitudes and behaviors, as well as the development of objective performance-based measures that can capture these outcomes.

In light of these concerns, we make three specific recommendations for the design and implementation of teacher performance evaluation systems. First, it is possible that measures of teachers’ effectiveness at improving students’ attitudes and behaviors may be suitable for low-stakes decision-making in schools. For example, these metrics could be used for early intervention efforts that diagnose areas of weakness and connect teachers to targeted professional development, which many argue should be the primary focus of teacher evaluation (Darling-Hammond, 2013; Hill & Grossman, 2013; Papay, 2012). Second, an alternative approach to incorporating teacher effects on students’ attitudes and behaviors into teacher evaluation may be through observations of teaching practice. Our findings suggest that specific domains captured on classroom observation instruments (i.e., *Emotional Support* and *Classroom Organization* from the CLASS and *Mathematical Errors* from the MQI) may serve as indirect proxy measures of improvements in students’ attitudes and behaviors. One benefit of this approach is that districts commonly collect related measures as part of teacher evaluation systems (Center on Great Teachers and Leaders, 2013), and such measures are not restricted to teachers who work in

tested grades and subjects. Third, performance evaluations – whether formative or summative – should avoid placing teachers into a single performance category whenever possible. Although many researchers and policymakers argue for creating a single weighted composite of different measures of teachers' effectiveness (Center on Great Teachers and Leaders, 2013; Kane et al., 2013), doing so likely oversimplifies the complex nature of teaching. For example, a teacher who excels at developing students' core math content knowledge but struggles to promote joy in learning or students' own self-efficacy in math is a very different teacher than one who is middling across all three measures. Looking at these two teachers' composite scores would suggest they are similarly effective. A single overall evaluation score lends itself to a systematized process for making binary decisions such as whether to grant teachers tenure, but such decisions would be better informed by recognizing the full complexity of classroom practice.

Next, we consider the implications of our findings for the teaching profession more broadly. While our findings lend empirical support to research on the multidimensional nature of teaching (Cohen, 2011; Lampert, 2001; Pianta & Hamre, 2009), we also identify tensions inherent in this sort of complexity and potential tradeoffs between some teaching practices. In our primary analyses, we find that high-quality instruction around classroom organization is positively related to students' self-reported behavior in class but negatively related to their happiness in class. Our results here are not conclusive, as the negative relationship between classroom organization and students' happiness in class is sensitive to model specification. However, if there indeed is a causal relationship, further research will be critical to gain a better understanding of how teachers can develop classroom environments that engender both constructive classroom behavior and students' happiness in class. Our findings also demonstrate

a need to integrate general and more content-specific perspectives on teaching, a historical challenge in both research and practice (Grossman & McDonald, 2008; Hamre et al., 2013). We find that both math-specific and general teaching practices predict a range of student outcomes. Yet, particularly at the elementary level, teachers' math training often is overlooked. Prospective elementary teachers often gain licensure without taking college-level math classes; in many states, they do not need to pass the math sub-section of their licensure exam in order to earn a passing grade overall (Epstein & Miller, 2011). Striking the right balance between general and content-specific teaching practices is not a trivial task, but it likely is a necessary one.

Finally, we see opportunities to maximize students' exposure to the range of teaching skills we examine through strategic teacher assignments. Creating a teacher workforce skilled in most or all areas of teaching practice is, in our view, the ultimate goal. However, this goal likely will require substantial changes to teacher preparation programs and curriculum materials, as well as new policies around teacher recruitment, evaluation, and development. In middle and high schools, content-area specialization or departmentalization often is used to ensure that students have access to teachers with skills in distinct content areas. Some, including the National Association of Elementary School Principals, also see this as a viable strategy at the elementary level (Chan & Jarman, 2004). Similar approaches may be taken to expose students to a collection of teachers who together can develop a range of academic skills, attitudes and behaviors. For example, when configuring grade-level teams, principals may pair a math teacher who excels in her ability to improve students' behavior with an ELA or reading teacher who excels in his ability to improve students' happiness and engagement. Viewing teachers as complements to each other may help maximize outcomes within existing resource constraints.

For decades, efforts to improve the quality of the teacher workforce have focused on teachers' abilities to raise students' academic achievement. Our work further illustrates the potential and importance of expanding this focus to include teachers' abilities to promote students' attitudes and behaviors that are equally important for students' long-term success.

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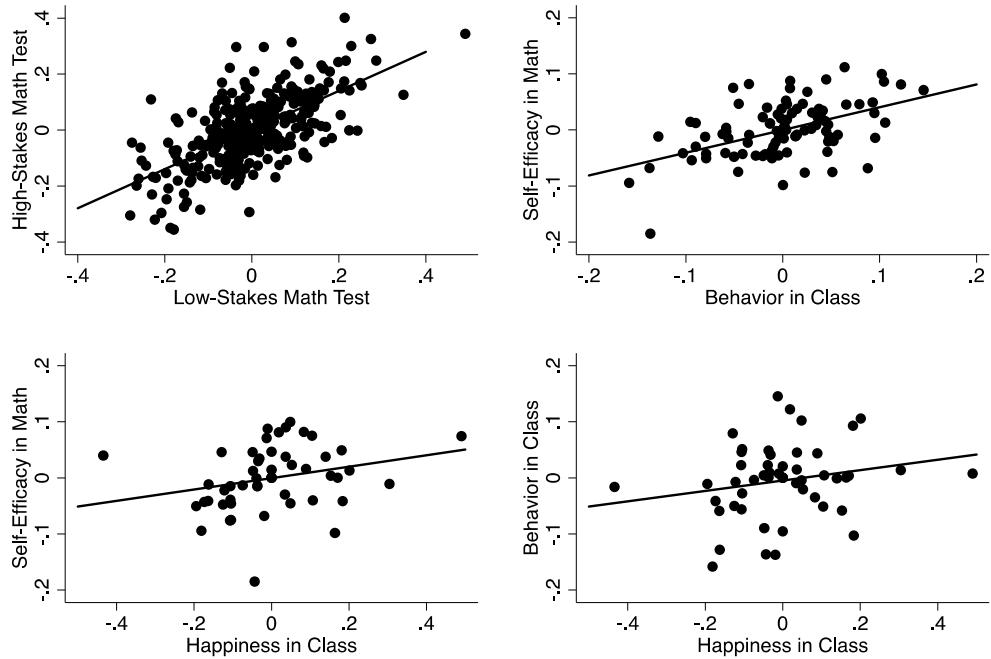
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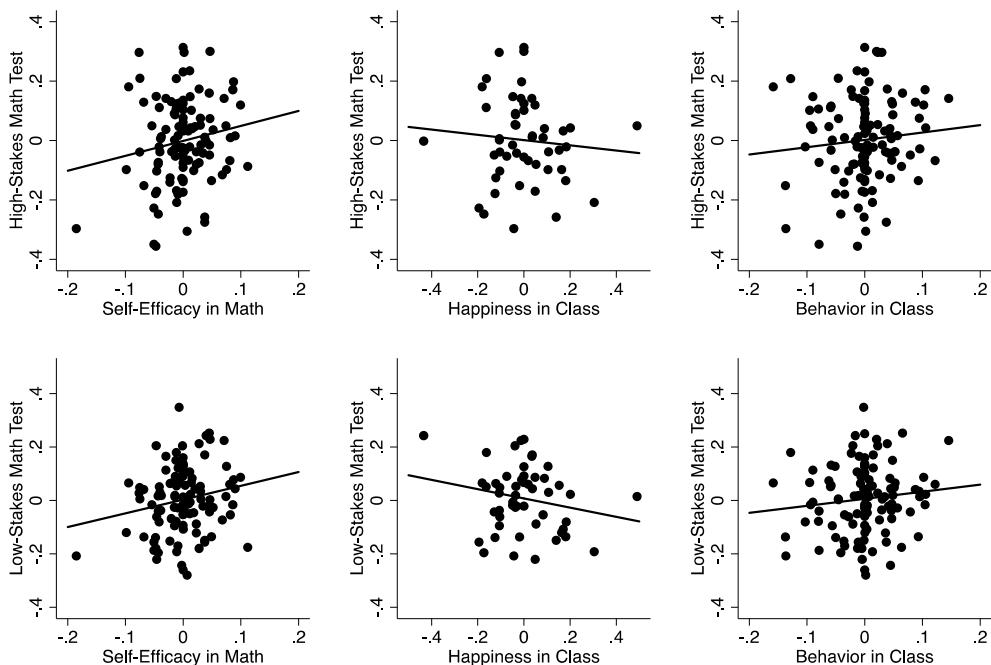
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## Figures

### Panel A: Within Outcome Type



### Panel B: Across Outcome Type



*Figure 1.* Scatter plots of teacher effects across outcomes. Solid lines represent the best-fit regression line.

## Tables

**Table 1**

Descriptive Statistics for Students' Attitudes, Behavior, and Academic Performance

	Univariate Statistics			Pairwise Correlations				
	Mean	SD	Internal Consistency Reliability	High-Stakes Math Test	Low-Stakes Math Test	Self-Efficacy in Math	Happiness in Class	Behavior in Class
High-Stakes Math Test	0.10	0.91	--	1.00				
Low-Stakes Math Test	0.61	1.1	0.82	0.70***	1.00			
Self-Efficacy in Math	4.17	0.58	0.76	0.25***	0.22***	1.00		
Happiness in Class	4.10	0.85	0.82	0.15***	0.10***	0.62***	1.00	
Behavior in Class	4.10	0.93	0.74	0.24***	0.26***	0.35***	0.27***	1.00

Notes: \*\*\*p<.001. For high-stakes math test, reliability varies by district; thus, we report the lower bound of these estimates. Behavior in Class, Self-Efficacy in Math, and Happiness in Class are measured on a 1 to 5 Likert Scale. Statistics were generated from all available data.

**Table 2**

Descriptive Statistics for CLASS and MQI Dimensions

	Univariate Statistics			Pairwise Correlations			
	Mean	SD	Adjusted Intraclass Correlation	Emotional Support	Classroom Organization	Ambitious Mathematics Instruction	Mathematical Errors
Emotional Support	4.28	0.48	0.53	1.00			
Classroom Organization	6.41	0.39	0.63	0.46***	1.00		
Ambitious Mathematics Instruction	1.27	0.11	0.74	0.22***	0.23***	1.00	
Mathematical Errors	1.12	0.09	0.56	0.01	0.09	-0.27***	1.00

Notes: \*\*\*p<.001. Intraclass correlations were adjusted for the modal number of lessons. CLASS items (from Emotional Support and Classroom Organization) were scored on a scale from 1 to 7. MQI items (from Ambitious Instruction and Errors) were scored on a scale from 1 to 3. Statistics were generated from all available data.

**Table 3**  
Participant Demographics

	Full Sample	Attitudes and Behaviors Sample	P-Value on Difference
<b>Teachers</b>			
Male	0.16	0.16	0.949
African-American	0.22	0.22	0.972
Asian	0.03	0.00	0.087
Hispanic	0.03	0.03	0.904
White	0.65	0.66	0.829
Mathematics Coursework (1 to 4 Likert scale)	2.58	2.55	0.697
Mathematical Content Knowledge (standardized scale)	0.01	0.03	0.859
Alternative Certification	0.08	0.08	0.884
Teaching Experience (years)	10.29	10.61	0.677
Value Added on High-Stakes Math Test (standardized scale)	0.01	0.00	0.505
Observations	310	111	
<b>Students</b>			
Male	0.50	0.49	0.371
African American	0.40	0.40	0.421
Asian	0.08	0.07	0.640
Hispanic	0.23	0.20	0.003
White	0.24	0.28	<0.001
FRPL	0.64	0.59	0.000
SPED	0.11	0.09	0.008
LEP	0.20	0.14	<0.001
Prior Score on High-Stakes Math Test (standardized scale)	0.10	0.18	<0.001
Prior Score on High-Stakes ELA Test (standardized scale)	0.09	0.20	<0.001
Observations	10,575	1,529	

**Table 4**  
 Teacher Effects on Students' Attitudes, Behavior, and Academic Performance

	Observations		SD of Teacher-Level Variance
	Teachers	Students	
High-Stakes Math Test	310	10,575	0.18
Low-Stakes Math Test	310	10,575	0.17
Self-Efficacy in Math	108	1,433	0.14
Happiness in Class	51	548	0.31
Behavior in Class	111	1,529	0.15

Notes: Cells contain estimates from separate multi-level regression models.  
 All non-zero effects are statistically significant at the 0.05 level.

**Table 5**  
Teaching Effects on Students' Attitudes, Behavior, and Academic Performance

	High-Stakes Math Test	Low-Stakes Math Test	Behavior in Class	Self-Efficacy in Math	Happiness in Class
<b><u>Panel A: Unconditional Estimates</u></b>					
Emotional Support	0.012 (0.013)	0.018 (0.014)	0.039 (0.027)	0.142*** (0.031)	0.279*** (0.082)
Classroom Organization	-0.017 (0.014)	-0.010 (0.014)	0.081* (0.033)	0.065~ (0.038)	0.001 (0.090)
Ambitious Mathematics Instruction	0.017 (0.015)	0.021 (0.015)	0.004 (0.032)	0.077* (0.036)	0.082 (0.068)
Mathematical Errors	-0.027* (0.013)	-0.009 (0.014)	-0.027 (0.027)	-0.107*** (0.030)	-0.164* (0.076)
<b><u>Panel B: Conditional Estimates</u></b>					
Emotional Support	0.015 (0.014)	0.020 (0.015)	0.030 (0.030)	0.135*** (0.034)	0.368*** (0.090)
Classroom Organization	-0.022 (0.014)	-0.018 (0.015)	0.077* (0.036)	-0.020 (0.042)	-0.227* (0.096)
Ambitious Mathematics Instruction	0.014 (0.015)	0.019 (0.016)	-0.034 (0.036)	-0.006 (0.040)	0.079 (0.068)
Mathematical Errors	-0.024~ (0.013)	-0.005 (0.014)	-0.009 (0.029)	-0.094** (0.033)	-0.181* (0.081)
Teacher Observations	196	196	93	90	47
Student Observations	8,660	8,660	1,362	1,275	517

Notes: ~ p<0.10, \* p<0.05, \*\*\*p<0.001. In Panel A, cells contain estimates from separate regression models. In Panel B, columns contain estimates from separate regression models, where estimates are conditioned on other teaching practices. All models control for student and class characteristics, and include school fixed effects and teacher random effects. Models predicting all outcomes except for Happiness in Class also include class random effects.

**Table 6**

Correlations Between Teacher Effects on Students' Attitudes, Behavior, and Academic Performance

	High-Stakes Math Test	Low-Stakes Math Test	Self- Efficacy in Math	Happiness in Class	Behavior in Class
High-Stakes Math Test	1.00				
	--				
Low-Stakes Math Test	0.64*** (0.04)	1.00 --			
Self-Efficacy in Math	0.16~ (0.10)	0.19* (0.10)	1.00 --		
Happiness in Class	-0.09 (0.14)	-0.21 (0.14)	0.26~ (0.14)	1.00 --	
Behavior in Class	0.10 (0.10)	0.12 (0.10)	0.49*** (0.08)	0.21~ (0.14)	1.00 --

Notes: ~ p<0.10, \* p<0.05, \*\*\*p<0.001. Standard errors in parentheses. See Table 4 for sample sizes used to calculate teacher effect estimates. The sample for each correlation is the minimum number of teachers between the two measures.

## Appendices

**Appendix Table 1**

Factor Loadings for Items from the Student Survey

	Year 1		Year 2		Year 3	
	Factor 1	Factor 2	Factor 1	Factor 2	Factor 1	Factor 2
Eigenvalue	2.13	0.78	4.84	1.33	5.44	1.26
Proportion of Variance Explained	0.92	0.34	0.79	0.22	0.82	0.19
<b><u>Self-Efficacy in Math</u></b>						
I have pushed myself hard to completely understand math in this class	0.32	0.18	0.43	0.00	0.44	-0.03
If I need help with math, I make sure that someone gives me the help I need.	0.34	0.25	0.42	0.09	0.49	0.01
If a math problem is hard to solve, I often give up before I solve it.	-0.46	0.01	-0.38	0.28	-0.42	0.25
Doing homework problems helps me get better at doing math.	0.30	0.31	0.54	0.24	0.52	0.18
In this class, math is too hard.	-0.39	-0.03	-0.38	0.22	-0.42	0.16
Even when math is hard, I know I can learn it.	0.47	0.35	0.56	0.05	0.64	0.02
I can do almost all the math in this class if I don't give up.	0.45	0.35	0.51	0.05	0.60	0.05
I'm certain I can master the math skills taught in this class.			0.53	0.01	0.56	0.03
When doing work for this math class, focus on learning not time work takes.			0.58	0.09	0.62	0.06
I have been able to figure out the most difficult work in this math class.			0.51	0.10	0.57	0.04
<b><u>Happiness in Class</u></b>						
This math class is a happy place for me to be.			0.67	0.18	0.68	0.20
Being in this math class makes me feel sad or angry.			-0.50	0.15	-0.54	0.16
The things we have done in math this year are interesting.			0.56	0.24	0.57	0.27
Because of this teacher, I am learning to love math.			0.67	0.26	0.67	0.28
I enjoy math class this year.			0.71	0.21	0.75	0.26
<b><u>Behavior in Class</u></b>						
My behavior in this class is good.	0.60	-0.18	0.47	-0.42	0.48	-0.37
My behavior in this class sometimes annoys the teacher.	-0.58	0.40	-0.35	0.59	-0.37	0.61
My behavior is a problem for the teacher in this class.	-0.59	0.39	-0.38	0.60	-0.36	0.57

Notes: Estimates drawn from all available data. Loadings of roughly 0.4 or higher are highlighted to identify patterns.

**Appendix Table 2**

Signal-to-Noise Ratio of Teacher Effect Estimates

	Original Sample	Common Sample
High-Stakes Math Test	0.67	0.54
Low-Stakes Math Test	0.64	0.50
Self-Efficacy	0.53	0.50
Happiness in Class	0.56	0.56
Behavior in Class	0.55	0.52

Notes: See Table 4 for sample sizes across outcomes in the original samples. The common sample includes 51 teachers and 548 students.

**Appendix Table 3**

Disattenuated Correlations Between Teacher Effects on Students' Attitudes, Behavior, and Academic Performance

	High- Stakes Math Test	Low- Stakes Math Test	Self- Efficacy in Math	Happiness in Class	Behavior in Class
High-Stakes Math Test	1.00				
Low-Stakes Math Test	0.98	1.00			
Self-Efficacy in Math	0.27	0.33	1.00		
Happiness in Class	-0.15	-0.35	0.48	1.00	
Behavior in Class	0.17	0.20	0.91	0.38	1.00