Principal Quality and Student Attendance

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Abstract

Student attendance is increasingly recognized as an important measure of educational success, which has spurred a body of research examining the extent to which schools can affect this outcome. However, prior work almost exclusively focuses on teachers, and no studies have explicitly examined the importance of school leaders. This study begins to fill this gap by estimating principal value-added to student absences. Drawing on statewide data from Tennessee over a decade, I find that principal effects on student absences are comparable in magnitude to effects on student achievement. Moving from the 25th to 75th percentile in principal value-added decreases student absences by 1.4 instructional days and lowers the probability of chronic absenteeism by 4 percentage points. Principals have larger effects in urban and high-poverty schools, which also have the highest baseline absenteeism rates. Finally, principals who excel at decreasing student absences may not be those who excel at increasing student test scores and high-stakes accountability measures, such as supervisor ratings, fail to identify principals who decrease student absenteeism.

Principal Quality and Student Attendance

Policymakers are increasingly focused on lowering rates of student absenteeism. Recent reports find that approximately fifteen percent of students are "chronically absent" each year, which is typically defined as missing ten percent or more of the instructional days in a school year (Jordan, Fothergill, & Rosende, 2018). Increased attention to absences is exemplified by recent reforms in the Every Student Succeeds Act (ESSA), which requires states to include chronic absenteeism rates as part of their report cards to the federal government. Additionally, as part of the ESSA reform that requires states to include in their accountability plan at least one indicator of school quality or student success that does not involve state test scores or graduation rates (commonly referred to as the "fifth indicator"[1]), 36 states have included an indicator related to student attendance (Jordan & Miller, 2017).

Undoubtedly, the federal requirement to track and report these outcomes explains the choice by many states to focus on attendance, whereas other potential indicators (e.g., school climate or students' social-emotional skills) require capacity and resources to design new measures and implement them across the state. Nevertheless, a large body of evidence highlights that reducing absenteeism is a worthy target for school improvement efforts. For instance, several prior studies establish a credible causal link between student absenteeism and lower academic achievement (Aucejo & Romano, 2016; Gershenson, Jacknowitz, & Brannegan, 2017; Goodman, 2014; Gottfried, 2009, 2010; Liu, Lee, & Gershenson, 2019). Further, Liu et al. (2019) show that, beyond lowering test scores, high school absences decrease the probability of on-time high school graduation and immediate college enrollment. Additional correlational evidence suggests that absences may lead to increased alcohol/drug use (Hallfors et al., 2002; Henry & Thornberry, 2010) and lower likelihood of future employment (Cattan, Kamhöfer, Karlsson, & Nilsson, 2017). Student attendance is

[1] The four other indicators are reading and math proficiency, high school graduation rate, English language proficiency, and student test score growth.

also a common proxy measure for character skills (e.g., conscientiousness), which are valued by employers in the labor market (Heckman & Kautz, 2013).

Increased attention to student absenteeism has spurred exploration of the extent to which schools affect this outcome, though existing work largely focuses on teachers. For instance, recent research demonstrates that teachers have substantial effects on students' non-test-score outcomes, including absences (Backes & Hansen, 2018; Gershenson, 2016; Jackson, 2018; Liu & Loeb, 2019).[2] These effects are comparable to, if not larger than, teachers' effects on test scores, and teachers who excel at raising test scores are not the same teachers who excel at decreasing absences, on average.

Drawing on 11 years of statewide data from Tennessee, this study contributes to the literature by examining whether principals affect student absences using a value-added (VA) framework. A large body of research links effective leadership to school performance, including higher student test scores (Branch, Hanushek, & Rivkin, 2012; Chiang, Lipscomb, & Gill, 2016; Coelli & Green, 2012; Dhuey & Smith, 2014, 2018; Grissom, Kalogrides, & Loeb, 2015), better school climate (Burkhauser, 2017; Kraft, Marinell, & Yee, 2016; Sebastian & Allensworth, 2012), and lower teacher turnover (Boyd et al., 2011; Grissom & Bartanen, 2019b; Ladd, 2011). To date, however, no studies have estimated principal effects on student absenteeism. To help fill this gap, I answer the following research questions:

1. What effect do principals have on student absences?
2. How does the magnitude of principal effects on absences vary by school context?
3. To what extent are estimates of principals' effects on absences correlated with other measures of principal effectiveness, including principal effects on achievement and rubric-based ratings from supervisors?

Isolating the effects of individual principals on student absences presents a formidable

[2] Beyond attendance, recent work links teacher quality to high school graduation (Jackson, 2018; Liu & Loeb, 2019), students' self-reported attitudes such as motivation and self-efficacy (Blazar & Kraft, 2017), and complex cognitive skills (e.g., problem-solving) and social-emotional competencies (Kraft, 2019).

empirical challenge. Consistent with approaches used in prior work to estimate principal effects on student achievement, I employ VA models with both principal and school fixed effects to isolate the impact of principal quality from other factors—such as the neighborhood or the quality of the school building—that might affect student absenteeism but that the principal cannot control. However, drawing from the teacher effects literature, I also implement a modified version of the drift-adjusted VA estimator proposed by Chetty, Friedman, and Rockoff (2014). This approach, which has not been applied in the principal effects literature, relaxes the assumption that principal quality is fixed over time.

The next section presents a framework connecting principal quality and student absenteeism. I then discuss the challenges of estimating principal effects on student outcomes, as well as the approaches pursued in prior studies. Next I describe the data and methods used to produce estimates of principals' effects on student absences, including the implementation of the drift-adjusted VA estimator. I then present the results. The concluding section discusses implications for policy and research, limitations, and avenues for future work.

The Role of Principals in Improving Student Attendance

While this study is the first to explicitly consider how principals affect attendance, a large literature explores how principals affect student achievement. A broadly accepted conclusion of this literature is that principal effects on student achievement are indirect (e.g., Grissom & Loeb, 2011; Hallinger & Heck, 1998; Sebastian & Allensworth, 2012; Witziers, Bosker, & Krüger, 2003), whereby principals influence school-level factors that in turn affect student learning. In particular, the quality of instruction students receive is the critical in-school factor for their learning. Principals affect this instruction directly through hiring and retention of effective teachers, providing feedback and coaching for existing teachers, and building a strong school climate (Sebastian & Allensworth, 2012). With respect to student attendance, I argue that while many of these indirect pathways are important, principals' leadership behaviors may also directly influence whether students come to school.

Clearly, an indirect channel remains teachers—prior evidence demonstrates that teachers have large effects on student absences (Gershenson, 2016; Jackson, 2018; Liu & Loeb, 2019). As primary human capital managers for schools, principals' influence over hiring and retention affects the quality of the school's teaching staff (Cohen-Vogel, 2011; Grissom & Bartanen, 2019b; Jacob, 2011; Rockoff, Staiger, Kane, & Taylor, 2012). Further, the recent studies of teacher effects on non-test-score outcomes underscore the multidimensional nature of teacher quality. In short, we know that teachers have differing strengths and that those who contribute most to improving attendance are systematically not those who produce the largest test score gains. Principals, then, can build and maintain a staff of teachers who excel at decreasing student absenteeism. They can also develop staff capacity to engage in effective attendance practices through professional development and coaching of teachers (Attendance Works, 2017).

Beyond influencing the quality of instruction students receive, principals may also directly affect student absences through a number of pathways.[3] One channel is communication with families. Principals are uniquely positioned to both personally contact parents and coordinate a school-wide policy that increases communication from school staff. Parents of highly truant students often believe that their child's attendance records are average compared to the child's peers (Rogers et al., 2017; Rogers & Feller, 2018). Relatedly, studies have found that informing parents about their child's attendance or the importance of attendance can help improve school attendance rates (e.g., Epstein & Sheldon, 2002; Robinson, Lee, Dearing, & Rogers, 2018; Roderick et al., 1997; Rogers et al., 2017; Smythe-Leistico & Page, 2018). For example, Robinson et al. (2018) demonstrated in a randomized field experiment that mail-based communication with parents that provided

[3] Here, I use "direct" to refer to the efforts of principals that are not mostly or completely mediated by teachers or other school staff.

personalized information about their child's absence record and reinforced the importance of regular attendance in grades K–5 lowered chronic absenteeism rates. Even communication with parents not explicitly focused on attendance may be beneficial. Kraft and Rogers (2015) found that in a high school credit recovery program, a randomly assigned intervention delivering weekly individualized text messages to parents about their child's schoolwork decreased the probability of class absence. Similarly, Bergman (2015) found experimental evidence that providing parents with biweekly information about their child's missed assignment and grades lowered absences during the semester.

Principals can also affect student absences through their control over school policies and programs. While empirical evidence in this area is lacking, one focus of states/districts and advocacy groups (e.g., Attendance Works) is to encourage principals to adopt an explicit strategy for reducing absenteeism. As an example, the Connecticut State Department of Education maintains a detailed website[4] that describes the state's vision for reducing chronic absenteeism and provides strategies that principals can implement in their schools. Attendance Works and the National Association of Elementary School Principals have published similar guides for school leaders. Beyond outreach to parents and families, common recommendations include implementing or leveraging existing data systems to target supports to students at risk of missing school, or establish procedures for mandatory interventions at a certain absence threshold. Principals can also integrate added supports for improving attendance into school policies that target struggling students, such as Response to Intervention or Positive Behavioral Interventions & Supports (Attendance Works, n.d.). Finally, principals' efforts may also include building relationships that engage local stakeholders, both to draw on outside expertise and to increase community awareness of chronic absenteeism as an important issue (Childs & Grooms, 2018).

To summarize, there are both direct and indirect channels through which principals may affect student absenteeism. While some of these indirect channels are likely the same

[4] https://portal.ct.gov/SDE/Publications/Reducing-Chronic-Absence-in-Connecticuts-Schools

channels through which principals affect test scores (e.g., human capital management, instructional leadership), principals' frequent interactions with students and families and their unique position to direct school policy are plausibly more direct ways of reducing student absences. While this study cannot identify the relative importance of these pathways, it serves to test the overall magnitude of principals' contributions to student attendance, which helps to lay the groundwork for future research.

Estimating Principal Effects

To examine the extent to which principal quality matters for student attendance, I draw on value-added (VA) modeling. This framework allows me to estimate an "effect" on a given outcome for each individual principal. The distribution of these VA estimates indicates the extent to which variation in principal quality leads to changes in student outcomes. This approach to identifying principal quality is conceptually distinct from a larger body of research that estimates the relationship between principal leadership behaviors and student outcomes (see Liebowitz and Porter (2019) for a review of this research). Here, "quality" is identified not by direct measurement of principals' behaviors or traits, but rather the systematic over- or under-performance of their students, adjusting for factors that the principal cannot control.

A handful of prior studies use VA modeling to estimate principal effects on student test scores.[5] Four studies use large state or district administrative datasets to estimate models with principal and school fixed effects (Branch et al., 2012; Chiang et al., 2016; Dhuey & Smith, 2018; Grissom et al., 2015). The magnitude of principals effects ranges between 0.05 and 0.20 SD—in other words, a 1 SD increase in principal quality increases student test scores by 0.05 to 0.20 SD. Two studies using data from British Columbia—Coelli and Green (2012) and Dhuey and Smith (2014)—find even larger effects, though they use estimation approaches that hinder direct comparison with other studies.

[5] For sake of simplicity, I also include in this group studies that directly estimate the variance of principal effects, but do not estimate effects for individual principals.

In contrast to the teacher VA literature, little work investigates the validity and reliability of principal VA estimates. The central validity issue is whether the model effectively isolates the "true" contribution of a principal. Neither students nor principals are randomly assigned to schools, which heightens concern that principals are rewarded or punished for factors beyond their control. In particular, prior work highlights the importance of separating principal effectiveness from school factors such as location or financial resources, and demonstrates empirically that school value-added is a poor measure of principal effectiveness (Chiang et al., 2016; Grissom et al., 2015).[6]

The challenge of accounting for school factors is that researchers typically do not observe them or have access to good proxy measures. The solution in the literature is to estimate models with principal and school fixed effects. Intuitively, these models make comparisons among principals who worked in the same school, such that persistent school factors cannot explain differences in student outcomes. This approach, for instance, avoids punishing principals who work in schools that struggle to attract high-quality teachers because of their location. However, including school fixed effects does not eliminate bias from time-varying school factors that the principal cannot control, such as planned facilities upgrades or an increase in community violence. These models also have a considerable practical limitation, which is that most principals can only be compared to small set of other principals.

A second validity concern is the timing of effects. Despite conceptual support for a more dynamic model of principal effects, prior work most often restricts principal quality to be fixed. However, principals' contributions to student outcomes may grow over time (e.g., Coelli & Green, 2012; Grissom et al., 2015). For instance, a new-to-school principal inherits many of the teachers hired under the old principal. To the extent that human

[6] If school factors outside the principal's control are unimportant for student learning, using school VA to measure principal performance should should produce the same ranking of principals as a model that separates principal and school effects. Both Grissom et al. (2015) and Chiang et al. (2016) show that these approaches produce very different results.

capital management is an important avenue through which principals affect student outcomes (Branch et al., 2012; Grissom & Bartanen, 2019b; Jacob, 2011), it may take several years for the effect of a high-quality principal to manifest itself through improved student outcomes. Additionally, principals likely improve as they gain experience (Bartanen, 2019; Clark, Martorell, & Rockoff, 2009; Grissom, Blissett, & Mitani, 2018), such that producing a single VA estimate for a principal's career masks substantial heterogeneity in their effectiveness over time.

Setting aside the validity issues outlined above, VA estimates contain both true differences in effectiveness among principals and measurement error. For teacher VA models, reliability is a non-trivial concern due in part to the small number of students per teacher. Principal VA benefits from larger sample sizes as all students in the school contribute to estimation and studies typically pool data across years to produce a single "career VA" estimate. Similar to the approach often implemented in the teacher VA literature, prior studies account for measurement error in principal VA using shrinkage estimators (e.g., empirical Bayes), though unsurprisingly these adjustments tend to be small in magnitude (Branch et al., 2012; Chiang et al., 2016; Grissom et al., 2015).

An overarching theme of the principal effects literature is that while principal VA has largely followed the methodological approach of teacher VA, there are unique challenges to successfully isolating the causal effect of principals on student outcomes. Prior work focuses, in particular, on addressing the threat of bias from school factors that the principal cannot control. These studies directly inform my empirical strategy, but I also aim to address issues that have yet to receive substantial attention, including the time-varying nature of principal effectiveness.

Data

This study analyzes administrative data from Tennessee covering the 2006–07 through 2016–17 school years, provided by the Tennessee Department of Education via the

Tennessee Education Research Alliance. The data contain information about students' enrollment and attendance, including enrollment dates at each school, dates of absences, and a flag for whether the absence was *excused* or *unexcused*–the criteria for these designations are determined at the district level. Additionally, I can access student demographics and the full test score history, which includes end-of-year achievement scores in math and reading for grades 3–8 and end-of-course (EOC) exams for high school students.[7] I also access staff files that allow me to identify school principals in each year. In total, I observe roughly 3,800 unique principals working in 1,700 schools from 2006–07 through 2016–2017.

Operationalizing Student Attendance

In Tennessee, a student who misses more than 50% of the school day is recorded as absent. Appendix Figure A1 shows the distribution of absence rates by grade level for the 2016–17 school year. Nearly 40% of students are absent more than five percent of instructional days, with 13% reaching the threshold for chronic absenteeism (10% absent rate or greater). However, chronic absenteeism is substantially higher in high schools. For example, 26% of 12th grade students are chronically absent, compared to only 13% of 8th grade students and 15% of 9th grade students. Kindergarten students also have high absenteeism rates relative to their older elementary school peers.

I examine four measures of absenteeism. First, I compute each student's total absence rate, which is the number of school days absent divided by the total number of enrolled school days. Additionally, I compute excused and unexcused absence rates by dividing excused/unexcused absences by days enrolled.[8] Differentiating between excused and unexcused absences is potentially informative, as prior work finds that unexcused

[7] EOC exam requirements vary by year. In 2016–17, students took exams for Algebra I, Geometry, Algebra II, English I, English II, and English III.

[8] Alternative ways of operationalizing absences (e.g., logarithmic transformations, using the raw count data) are very highly correlated with my preferred principal effect estimates.

absences are more detrimental to student learning than excused absences (Gershenson et al., 2017; Gottfried, 2009). Further, the efficacy of principals' efforts could vary between absence types. Finally, I also create a binary indicator for chronic absenteeism, using the standard threshold of 10% absence rate or above (Jordan et al., 2018). As shown in Appendix Table A3, the mean absence rate is 5.5% with a roughly equal split between excused and unexcused absences.

Methods

Approach 1: Principal and School Fixed Effects

I begin with the standard approach in the principal effects literature, which includes both principal and school effects. Specifically, I estimate via ordinary least squares the following model:

$$Y_{isjt} = \alpha Y_{i,t-1} + \beta X_{it} + \gamma S_{st} + \pi_{gt} + \delta_j + \theta_s + \epsilon_{isjt} \tag{1}$$

where i, s, j, and t index students, schools, principals, and years, respectively. Y, a student's absence or achievement outcome, is a function of their prior outcomes (though not necessarily the most recent prior year, as I discuss below); a vector of student characteristics, X; a vector of school characteristics, S; grade-by-year fixed effects, π; principal fixed effects, δ; school fixed effects, θ; and a random error term, ϵ. Achievement models use students' end-of-year exams for elementary and middle schools and end-of-course exams for high schools, such that I can estimate both achievement and absence VA across all three school levels. For absence models, the prior-year outcomes include non-parametric functions of a student's prior absence rates and suspensions.[9]

[9] Specifically, I construct categorical variables based on the student's prior-year absence rates. For total absences, the categories are as follows: 0%, 1–5%, 6–10%, 11–15%, 16–20%, 21–30%, 31–40%, 41–50%, 51–60%, 61–70%, 71–80%, 81–90%, 91–100%. For unexcused and excused absences, the categories are: 0%, 1%, 2%, 3%, 4%, 5%, 6–9%, 10–19%, 20–29%, 30–39%, 40–49%, 50–100%. For suspensions, I create separate variables for the number of in-school an out-of-school suspensions the student received: 0, 1, 2, 3+.

Given the changing distribution of student absenteeism across grades, I interact prior absence rates with a student's prior grade to allow for the possibility that the relationship between past and current attendance differs across grade levels. For achievement models, I control for cubic functions of prior test scores in math and reading, as well as prior-year attendance rates, each interacted with a student's prior grade.[10] Student characteristics include gender, race/ethnicity, free/reduced-price lunch eligibility, special education classification, gifted classification, whether the student is repeating the grade, and whether the student has any enrollment spells in another school in the current school year. School characteristics are school-level averages of the student characteristics.

The principal fixed effect, δ_j, is the parameter of interest; it captures the extent to which the actual absence rates of students of principal j are higher or lower than what would be predicted by students' prior absences and achievement, their individual characteristics, their grade, the school year, and their school. The model accounts for school quality in two ways: time-varying averages of student demographics and a school fixed effect, θ_s. Including school fixed effects accounts for any time-invariant unobserved school-level factors. Conceptually, this model identifies principal quality using within-school variation in student outcomes under different principals. The movement of principals across schools creates connected networks of principals, where a principal's estimated effect, δ_j, represents their effectiveness relative to other principals in the same network (Burkhauser, 2017; Chiang et al., 2016; Mansfield, 2015).

In practice, principal sorting patterns and high rates of attrition lead to a large number of disconnected networks that often contain only a few principals. The primary limitation of small networks is that principal VA estimates only reflect effectiveness relative to those in the same network, rather than across the entire state. Additionally, I cannot

[10] Not controlling for prior-year test scores allows me to use students from all grades, rather than the subset who were in grades 3–8 in the prior year or who took an EOC exam. Including these additional grades increases the network sizes in the two-way fixed effects model and also allows me to produce estimates for roughly 100 additional principals.

estimate VA for 17% of principals because they were the sole principal of a school across the data period.[11] Removing school fixed effects from the model allows for a global ranking of principals, but greatly heightens the possibility of bias from unobserved school heterogeneity. Appendix Table A1 shows the distribution of networks formed by Tennessee principals based on the analytic sample for estimating absence VA (Appendix Table A2 shows the networks for achievement VA). In sum, the analytic sample includes roughly 3,100 principals and 4.3 million student-by-year observations. Descriptive statistics for the analytic sample are shown in Appendix Tables A3 and A4.

It is important to note that because most students in year t had the same principal in year $t-1$, including prior-year outcomes is a violation of strict exogeneity and potentially biases the principal effect estimates. The degree of this bias likely depends on the nature of how principals affect student outcomes. Controlling for the prior-year outcome is effectively investigating whether a principal causes *continued* improvement in the outcome, as opposed to a one-time increase. For student achievement, examining continued improvement may be perfectly reasonable. In the case of attendance, however, the inclusion of prior-year outcomes may be more problematic. If, for instance, a new principal implements a program that reduces absenteeism, we might expect to observe a bump in attendance in that year. If absence rates are stable in the following year, we would like to still conclude that the principal was effective at reducing absenteeism. However, if we control for prior-year absences, we will conclude that the principal was not effective at reducing absenteeism in the second year.

One potential solution is to remove the prior-year outcome from the model entirely. However, this heightens the risk of bias from nonrandom student sorting between schools, particularly if students are responding to changes in school quality that are not captured

[11] More specifically, I impose a restriction that all principals in the analytic sample must have at least 50 student-by-year observations. This restriction results in dropping a handful of principals who were in networks with one other principal. Since dropping the principal with fewer than 50 observations changes the network to a single principal, the other principal in the network is effectively dropped from the analysis because they no longer have any comparison set.

by school fixed effects or demographic controls. I propose a different solution, which is to include in the model the student's most recent prior outcome from their previous school. For example, consider an 8th-grade student who has been in her current school since 6th grade but attended a different school in 5th grade. Instead of controlling for her 7th-grade absences, which are endogenous to both the current principal and school effect (assuming that the principal is not in her first year at the school), I control for her 5th-grade absences, which are not affected by the current school or principal. While this approach allows me to avoid the endogeneity concern, it excludes a non-trivial number of students. Specifically, I can only estimate this model for students that have prior-year outcomes in a different school, which systematically drops students in lower grades and students in the earlier years of the data (see Appendix Table A4). Fixed effects estimates for elementary school principals thus leverage fewer students (see Appendix Table A5) and contain more noise.

Approach 2: Drift-Adjusted Value-Added

A shortcoming of the simple two-way fixed effects approach is that it is inflexible with respect to changes in principal effectiveness over time. My preferred model allows for changes in performance by producing an effect estimate for each principal-by-year observation. Specifically, I use a modified version of the estimator developed by Chetty et al. (2014) for teacher VA. The estimator has three steps to produce VA for principal j in school s in year t: (1) residualize students' absences (or achievement) on a vector of observable characteristics (the same student- and school-level controls from equation 1); (2) estimate the best linear predictor of mean absence residuals for all students in school s with principal j in year t based on mean absence residuals for principal j in prior or future years; (3) use the coefficients of the best linear predictor to predict principal VA in year t. In essence, this approach begins by estimating the two-way fixed effects model from approach 1, but then leverages variation in principal-by-year average residuals to produce a time-varying measure of principal quality.[12] Appendix B contains details on the construction of principal VA using the drift-adjusted estimator. I also perform a validation check proposed by Chetty et al. (2014) that tests for forecast bias using students' twice-lagged outcomes (which are omitted from the VA model). Across both absence and achievement outcomes, I find minimal evidence of bias, supporting the claim that the VA estimates are valid measures of principal effectiveness.

There are three important differences between approaches 1 and 2. First, the VA estimates in approach 2 (drift-adjusted VA) are leave-year-out measures, meaning that principal-school estimates in year t do not incorporate student outcomes from year t. Second, whereas the principal estimates from the fixed effects models in approach 1 include school-level shocks and student errors (which is the motivation for the empirical Bayes approach), approach 2 inherently produces shrunken estimates. To the extent that student residuals in a given year are higher or lower due to transitory shocks and/or student-level measurement error (as opposed to the true principal effect), this variation is uncorrelated (in expectation) with residuals in past or future years. Using these past and future residuals to predict contemporaneous residuals, then, will produce an estimate that is shrunken towards the sample mean. Further, this shrinkage also accounts for the number of students that contribute to estimating each principal's effect, such that the adjustment to the estimates for principals in smaller schools will tend to be larger to reflect the fact these estimates are less precise. Finally, the drift-adjusted approach allows for the possibility that principal quality changes over time, rather than estimating an average

[12] One major difference between my approach and the approached used by Chetty et al. (2014) is that I include both school and principal fixed effects in the residualization step. Specifically, Chetty et al. (2014) residualize test scores on observable characteristics using within-teacher variation (i.e., with teacher fixed effects). When computing the residuals, they add back in the teacher fixed effects. I perform a similar process with principal effects, with the exception that the residualization includes both principal and school fixed effects. When computing the residuals, I add back in the principal fixed effects, but not the school fixed effects. As explained above, this approach accounts for the possibility that the vector of observable characteristics does not fully control for school-level heterogeneity that would otherwise be attributed to principal effectiveness. However, as with approach 1, employing both principal and school fixed effects means that the VA estimates are only comparable for principals within the same connected network.

effect across the principal's career.

Results

Do Principals Affect Student Absences?

Figure 1 plots the distribution of principal VA estimates from approaches 1 and 2—the fixed effects and drift-adjusted models—for absence and achievement outcomes, with the corresponding summary statistics shown in Table 1. The number of principals for whom I can estimate achievement VA is smaller, since some schools (e.g., grades K–2 only) do not have end-of-year or end-of-course exams. The standard deviations of the estimates represent how much principals vary in their effects on student outcomes; a larger standard deviation indicates that principal quality is more consequential for the particular outcome. The units for absence VA are rates (i.e., on a 0–100% scale), whereas chronic absenteeism VA is expressed in terms of probability. Finally, the units for math and reading achievement are student-level standard deviations.

Studies of principal and teacher effects typically interpret magnitude according the standard deviation of the VA estimates. This approach makes sense if the estimates are normally distributed. However, Figure 1, the distribution of principal VA estimates is non-normal. Specifically, the presence of a small number of outliers inflates the standard deviation. To provide a more meaningful interpretation of the magnitude of principal effects on student outcomes, I report the interquartile range (IQR): the difference between the 75th and 25th percentile in the distribution.

I focus my discussion on the estimates from the drift-adjusted approach. I find that principals have substantial effects on student attendance. The IQR of principal VA to absence rates is 0.8, meaning that replacing a principal at the 25th percentile with one at the 75th percentile decreases the absence rate of all students in the school by 0.8 percentage points, on average. This decrease corresponds to 1.4 fewer instructional days missed on a 180-day calendar and 13% of a standard deviation in absence rates among students in the analytic sample. I also find that principals' effects on attendance operate through both excused and unexcused absences, with only a slightly larger IQR for unexcused absence rates. Finally, I find that moving from the 25th to 75th percentile lowers the probability that a student will be chronically absent in the current year by 4 percentage points, or roughly 30% of the base rate among Tennessee students.[13]

Consistent with prior studies, principals are also an important input to student learning. The IQR of principal VA to math and reading achievement is 0.15 and 0.07 student-level standard deviations, respectively. How do principal effects on absences and achievement compare in terms of magnitude? To provide a direct comparison, I re-estimate absence VA using rates that are standardized within grade and year (the same process used for test scores). The distribution of these standardized VA estimates are shown in Appendix Table A6. The IQR of principal VA for total absences, unexcused absences, and excused absences, respectively, is 0.11 SD, 0.12 SD, and 0.14 SD. Thus, the impact of principal quality on student absences is similar in magnitude to the impact on achievement.

Does the Importance of Principal Quality for Student Absences Vary by School Context?

My second research question examines whether the magnitude of principal effects vary by three categories of school context: school level, school locale, and school poverty (as measured by the percentage of students who quality for free/reduced-price lunch).

[13] One concern is that there are ceiling effects with absences, such that principals in schools with many students with perfect or near-perfect attendance effectively cannot have high value-added, since there is no room to improve. While 8% of Tennessee students have zero absences and 23% have fewer than three absences, there are relatively few *schools* that have absence rates close to zero (see Appendix Figure A2), which suggests that ceiling effects do not substantially affect principals' VA estimates. Prior work investigating ceiling effects in teacher VA also finds that even under relatively severe test score ceiling, teachers' VA estimates are negligibly influenced (Koedel & Betts, 2009). As an additional check, I re-estimate the absence VA models under three sample restrictions: dropping students with zero absences, dropping students in the bottom 10% of the distribution of absence rates, and dropping students in the bottom 25% of the distribution of absence rates. Appendix Table A8 shows that the distributions of VA estimates using these restrictions are highly similar to the distribution using the full sample. Additionally, Appendix Table A9 shows that the rank correlations of principal absence VA are consistently high across these sample restrictions. In sum, these findings suggest that while ceiling effects are present, they likely do not substantially affect principals' VA estimates.

Table 2 shows the IQR of the drift-adjusted VA estimates within each of the subgroups of these categories.

The IQR of principal effects on attendance rates is similar across school levels. For instance, the IQR for absence rate VA is 0.9 percentage points among high school principals versus 0.8 for elementary and middle school principals. However, I do find that magnitude of principal effects for achievement outcomes is larger for elementary school principals.[14] The IQR for math (reading) achievement among elementary school principals is 0.18 SD (0.09 SD), compared to 0.14 SD (0.06 SD) for middle and high school principals.

For school locale and poverty level, a clear pattern emerges: principal effects in urban and high-poverty schools are larger across both absence and achievement outcomes. For instance, the IQR for absence rates among principals in urban schools is 1.1 percentage points, compared to 0.8 and 0.7 in suburban and rural schools, respectively. This difference is explained more by unexcused absences, where the magnitude in urban schools (1.1 percentage points) is roughly twice as large as suburban (0.6) and town/rural schools (0.6). I find similarly sized differences for student achievement in math and reading. The largest differences in the magnitudes of principal effects on absences are between high-poverty and low-poverty schools. The IQR for absence rates is twice as large in high-poverty schools (1.4 percentage points) than low-poverty schools (0.7 percentage points). Moreover, moving from the 25th to 75th percentile in principal quality lowers the probability of chronic absenteeism by 6 percentage points in high-poverty schools.

In sum, I do find heterogeneity across school contexts in the magnitude of principal effects for both absences and achievement. Principal effects are largest in urban and high-poverty schools—a noteworthy finding given that these schools tend to have students with lower baseline attendance and achievement. Further, prior analysis of the leadership labor market in Tennessee finds that high-poverty and low-achieving schools are the most

[14] Larger achievement impacts on elementary school students has been documented for teacher VA (e.g., Backes & Hansen, 2018; Chetty et al., 2014).

likely to be led by inexperienced and low-rated principals (Grissom, Bartanen, & Mitani, 2019). The findings here further underscore the importance of recruiting and retaining high-quality principals in disadvantaged schools. Finally, while the drivers of absenteeism may be quite different for younger versus older students, principals' contributions to reducing absences are similar in magnitude across school levels. This could suggest that there is not a single pathway through which principals reduce absenteeism, but rather that high-quality principals at different school levels tailor their efforts to address the factors that inhibit attendance for their specific student population.

Comparing Absence VA to Other Measures of Principal Quality

My final research questions investigates whether principals who excel at decreasing student absenteeism can be identified using other measures of principal effectiveness. Specifically, I compare principals' absence VA to their achievement VA, as well as high-stakes rubric-based observation scores from their supervisors and low-stakes survey-based ratings from teachers in their school. Table 3 compares principals' estimated VA to absence and achievement outcomes. A natural question is whether principals who increase student achievement also improve attendance. To answer this question, I compute Spearman rank correlations among the VA estimates. However, because the networks used to construct VA estimates for achievement and absences are different, I cannot directly compare these estimates. To provide an accurate comparison, I re-estimate absence VA using the networks created for achievement VA. It is also important to note that because VA estimates contain measurement error, correlations among these estimates are attenuated. However, applying disattenuation corrections similar to those used in prior studies of teacher VA (e.g., Kraft, 2019) yields very similar results, as the estimated reliability of principal VA estimates is high.[15]

[15] To explore the plausible magnitude of attenuation in correlations among principal VA estimates, I follow the approach proposed by Kraft (2019), which computes disattenuated Pearson correlations using the Spearman (1904) adjustment. Specifically, the adjustment multiplies the raw correlation by the inverse of the square root of the product of the reliability of the two measures. This disattenuation correction, for

The first column of Table 3 compares principals' drift-adjusted VA across absence and achievement outcomes.[16] Comparing principals' effects on absences to achievement scores, there are small positive correlations with math and reading VA for each model. For instance, the rank correlation between absence and math (reading) VA is 0.11 (0.12). In terms of excused versus unexcused absences, the link to achievement is slightly stronger for unexcused absence VA. Table 3 also shows a *negative* correlation between principal VA for excused and unexcused absences. This likely reflects heterogeneity across principals in terms of setting the criteria for classifying an absence as excused or unexcused. In substantive terms, these modest correlations between absence and achievement VA demonstrate that principals who improve student attendance are not necessarily the same principals whose students have the greatest achievement growth. To the extent that student attendance is an important educational outcome even beyond its relationship with achievement, the results in Table 3 suggest that focusing exclusively on student achievement to identify principal quality will fail to capture the contributions of principals to improving students' non-cognitive or character skills, such as attendance.

Table 4 examines correlations between drift-adjusted principal VA estimates and principals' rubric-based ratings from supervisors[17] and survey-based ratings from teachers.[18] Specifically, I regress standardized ratings on standardized principal VA and

instance, increases the Pearson correlation between absence VA and math (reading) VA from 0.13 (0.15) to 0.15 (0.17). Full results are available upon request.

[16] Appendix Table A7 shows the correlations for the fixed effects.

[17] These ratings are rubric-based scores that principals receive as part of Tennessee's statewide educator evaluation system (TEAM) implemented in 2011–12. Fifty percent of the TEAM evaluation for principals comes from ratings of principal performance on a rubric derived from the Tennessee Instructional Leadership Standards. These ratings are based on formal observations conducted by the principal's supervisor. Prior work shows that principals' ratings across indicators are highly inter-related and can be reduced to a single underlying performance score using factor analysis (Grissom et al., 2018). In this analysis, I use principals' average yearly observation scores—the exact measure used by the state to calculate summative evaluation ratings. Using the average observation score instead of the factor score described in Grissom et al. (2018) allows me to include principals in districts that used alternative observation rubrics (approximately one-quarter of principals in the state), as these districts do not report domain-specific scores for principals. However, for principals for whom I can calculate factor scores, the average observation score and the factor score are correlated at 0.95 or higher each year.

[18] As part of a yearly statewide survey of educators in Tennessee, teachers are asked to provide Likert-scale

include fixed effects for each connected network used to construct principal VA and fixed effects for district-by-year.

Columns 1–4 in Panel A show no correlation between absence VA and supervisor ratings. In other words, these rubric-based ratings do not contain information about principals' contributions to decreasing student absenteeism. Columns 5 and 6 show the results for achievement VA in math and reading. Here, the coefficients are positive and slightly larger in magnitude, though the relationship is only statistically significant at conventional levels for reading. Column 6, for instance, shows that a 1 SD increase in a principal's supervisor rating is associated with a 0.10 SD increase in their reading VA. Panel B shows the results for teacher ratings. Again, the estimated coefficients are all close to zero and none are statistically significant at the 95% level. Overall, the results in Table 4 demonstrate that subjective ratings of performance from supervisors or teachers contain little to no information about principals' contributions to improving student attendance or achievement.

Discussion and Conclusions

Student attendance is increasingly recognized as an important measure of school success, which has spurred research that examines the extent to which schools affect attendance outcomes. To date, studies have almost exclusively focused on teachers, and we have convincing evidence that teachers play an important role in decreasing student absenteeism. However, no studies have considered the effect of principals, despite strong conceptual reasons to believe that principals can influence absences and prior work demonstrating that principal quality matters for student achievement. I begin to fill this gap by estimating value-added models that isolate the impact of individual principals on

responses to items that evaluate principal performance. Examples of items include, "The principal at my school communicates a clear vision for this school" and "School leadership makes a sustained effort to address staff concerns." To construct a principal-by-year measure, I factor analyze the responses from teachers in each year, average the factor scores to the school-by-year level, and standardize the school averages within each year.

student absences. To my knowledge, this study is the first to extend the principal effects literature to a non-test-score outcome.

My central finding is that principals have substantive effects on student absences. Moving from the 25th to 75th percentile in principal quality lowers absence rates by 0.8 percentage points, which corresponds to 1.4 additional instructional days for each student in the school, on average. The magnitude of these impacts is roughly comparable to principal effects on test scores. Further, principals have even larger effects in high-poverty and urban schools, which also have the highest rates of chronic absenteeism. Similar to findings from studies that have estimated teacher effects on absences, principals who decrease student absences are not necessarily those who increase test scores. Modest correlations between principal VA to absence and achievement outcomes highlight the multidimensional nature of principal quality. In particular, this finding challenges the notion that effective school leadership is completely defined by a principal's ability to drive achievement gains. While there need not be a tradeoff between test-score and non-test-score outcomes, this study demonstrates that there is still much to be learned about what constitutes principal quality and how to identify effective school leaders.

From a policy perspective, insofar as attendance is an outcome worthy of attention, accountability systems designed around identifying principals who increase test scores will fail to identify principals who are improving attendance—and by extension, principals who improve other non-test-score outcomes. Indeed, I find that principals' rubric-based scores from supervisors, which comprise half of their summative evaluation rating under Tennessee's high-stakes evaluation system, are not predictive of their contributions to decreasing student absenteeism, and are only marginally predictive of principals' impacts on student achievement. Given prior research demonstrating a strong relationship between supervisor ratings and principal mobility decisions (Grissom & Bartanen, 2019a), the findings here suggest that districts may be making placement and retention decisions on the basis of a performance measure that has little connection to student outcomes.

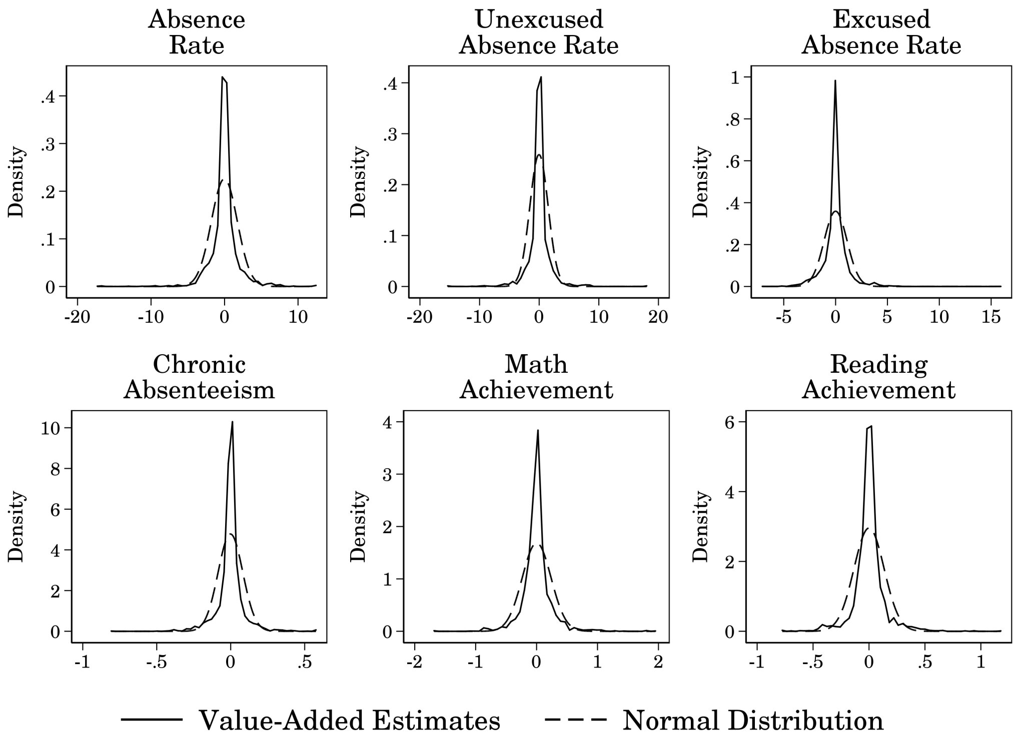
This study is particularly timely given the inclusion of an attendance-related outcome as a measure of school performance in the majority of state accountability plans under the Every Student Succeeds Act. From the perspective of policymakers and district leaders, my results suggest that intervening with principals could be an effective means to address high rates of chronic absenteeism. As states and districts invest additional resources into improving the quality of school leadership, they should consider the multi-dimensionality of principal effectiveness, as the skills and practices that promote test score growth may be different than those that promote regular attendance.

That said, while I document variation in principals' impacts on student absences, an important limitation of this study is that I am unable to identify the specific pathways through which principals influence attendance. Research connecting principals to student outcomes has largely focused on test scores, such that the results here highlight the need to expand our thinking about how principals influence other outcomes, especially given increased policy emphasis on understanding and measuring the extent to which schools affect students' non-cognitive or social-emotional skills. Future work should aim to identify *how* principals influence absences or other non-test-score outcomes. For instance, how do principals leverage attendance data to identify and support students who are likely to miss school? How do effective principals engage with parents to promote strong attendance habits? Better understanding these mechanisms could provide useful guidance about specific ways to target development opportunities for school leaders to help them lower absenteeism rates.

Additionally, while the methodological choice to use both principal and school fixed effects is important to credibly isolate the contributions of principals from school-level factors they cannot control, it limits the practical use (e.g., for accountability purposes) of these estimates. Even with the population of Tennessee principals across a decade, the majority of principals can only be compared within a small network of connected schools. This practical limitation is not faced by teacher VA models, which typically do not include these school effects and thus can produce a statewide ranking of teachers.[19] Given the continued emphasis on directly connecting educator evaluation to student outcomes, reconciling the empirical challenges of principal VA and the practical needs of accountability systems is another important avenue for future research. More generally, while I attempt to draw out and address some of the gaps in our understanding of the properties of principal VA estimates, this area remains relatively underdeveloped and worthy of additional attention.

[19] Further, even teacher VA models that include both teacher and school effects typically yield a singular connected network, as there are many teachers per school and relatively high mobility rates, which produces the variation to connect all of the schools in a state or district (e.g., Mansfield, 2015).

(a) Fixed Effects



(b) Drift-Adjusted

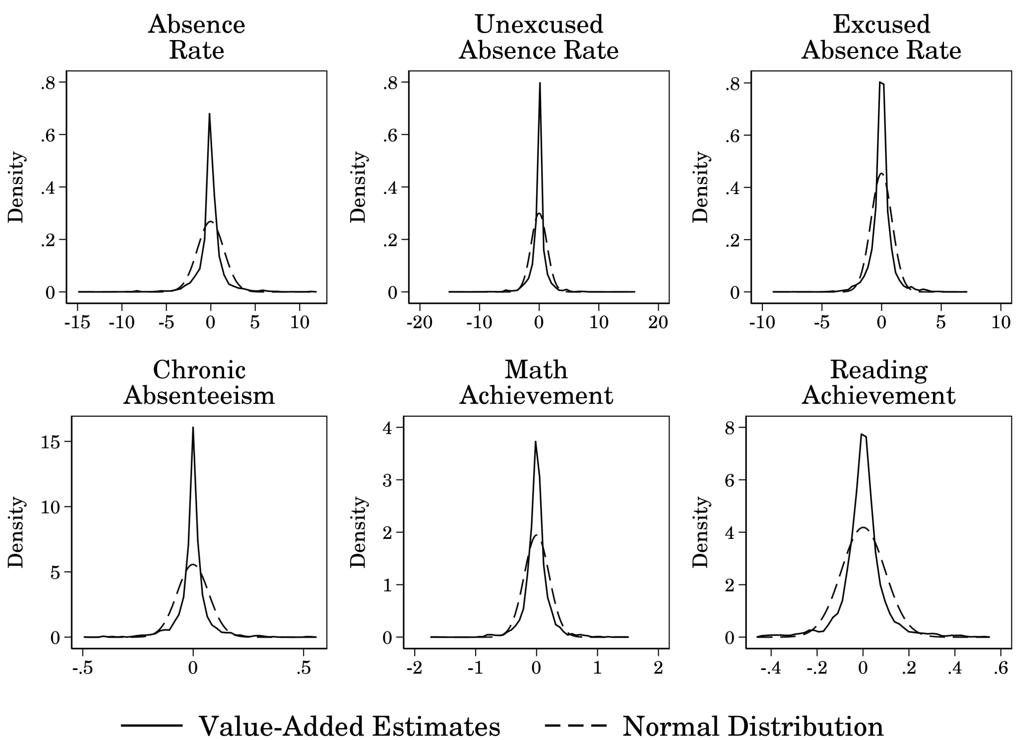


Figure 1. Distribution of Principal Value-Added Estimates

Table 1
Distribution of Principal Value-Added Estimates

				Percentile of Estimates				
	N	SD	IQR	10th	25th	50th	75th	90th
Fixed Effects								
Absence Rate	3141	1.9	1.0	-1.8	-0.5	-0.0	0.5	1.6
Unexcused Absence Rate	3141	1.7	0.9	-1.4	-0.5	-0.0	0.4	1.3
Excused Absence Rate	3141	1.2	0.8	-1.2	-0.4	0.0	0.4	1.2
Chronic Absenteeism	3141	0.09	0.05	-0.08	-0.03	0.00	0.02	0.08
Math Achievement	2045	0.24	0.19	-0.23	-0.10	-0.00	0.09	0.23
Reading Achievement	2058	0.13	0.10	-0.12	-0.05	-0.00	0.05	0.12
Drift-Adjusted								
Absence Rate	13173	1.5	0.8	-1.4	-0.4	0.0	0.4	1.2
Unexcused Absence Rate	13173	1.3	0.7	-1.1	-0.4	0.0	0.4	1.1
Excused Absence Rate	13173	0.9	0.6	-0.9	-0.3	0.0	0.3	0.8
Chronic Absenteeism	13173	0.07	0.04	-0.06	-0.02	-0.00	0.02	0.06
Math Achievement	8559	0.20	0.15	-0.18	-0.08	0.00	0.08	0.20
Reading Achievement	8408	0.10	0.07	-0.09	-0.04	0.00	0.04	0.09

Notes: Absence estimates are multiplied by -1 to facilitate comparison with achievement estimates. For absence rates, the scale is 0–100%. Chronic absenteeism is expressed as a probability on a 0 to 1 scale. Achievement outcomes are student-level standard deviation units. Sample sizes for fixed effects are at the principal level, whereas drift-adjusted are at the principal-by-year level. “IQR” refers to interquartile range, which is the difference between the 75th and 25th percentile of the distribution.

Table 2

Interquartile Range of Drift-Adjusted Value-Added Estimates by School Characteristics

	School Level			School Locale			School Poverty		
	Elem	Middle	High	Urban	Suburb	Rural	Low	Medium	High
Absence Rate	0.8	0.8	0.9	1.1	0.8	0.7	0.7	0.7	1.4
Unexcused Absence Rate	0.7	0.7	0.7	1.1	0.6	0.6	0.7	0.6	1.3
Excused Absence Rate	0.6	0.6	0.7	0.8	0.5	0.5	0.4	0.6	0.9
Chronic Absenteeism	0.04	0.04	0.04	0.05	0.04	0.04	0.03	0.04	0.06
Math Achievement	0.18	0.14	0.14	0.19	0.14	0.13	0.14	0.14	0.22
Reading Achievement	0.09	0.06	0.06	0.09	0.07	0.06	0.08	0.06	0.11

Notes: Absence estimates are multiplied by -1 to facilitate comparison with achievement estimates. For absence rates, the scale is 0–100%. Chronic absenteeism is expressed as a probability on a 0 to 1 scale. Achievement outcomes are student-level standard deviation units. School poverty is categorized by the percentage of students in the school who quality for free/reduced principal lunch (FRPL): 0–30% (low), 30–70% (medium), 70–100% (high). For school locale, rural includes schools classified as “town” by NCES.

Table 3

Spearman Correlations Among Drift-Adjusted Value-Added Estimates

	Abs	Abs (U)	Abs (E)	Chr Abs	Math	Read
Absence Rate	1.00					
Unexcused Absence Rate	0.63	1.00				
Excused Absence Rate	0.46	-0.22	1.00			
Chronic Absenteeism	0.91	0.58	0.42	1.00		
Math Achievement	0.11	0.06	0.02	0.09	1.00	
Reading Achievement	0.12	0.07	0.05	0.11	0.48	1.00

Notes: Absence estimates are multiplied by -1 to facilitate comparison with achievement estimates.

Table 4

Do Supervisor or Teacher Ratings Identify High Value-Added Principals?

	Abs (1)	Abs (U) (2)	Abs (E) (3)	Chr Abs (4)	Math (5)	Read (6)
Panel A						
Supervisor Rating	0.033	0.041	-0.013	0.036	0.062	0.099***
	(0.035)	(0.037)	(0.029)	(0.031)	(0.046)	(0.036)
N	7104	7104	7104	7104	4281	4113
Panel B						
Teacher Rating	0.021	0.019	0.004	0.040*	0.032	-0.014
	(0.032)	(0.034)	(0.022)	(0.024)	(0.022)	(0.023)
N	6656	6656	6656	6656	4028	3846

Notes: Each cell is a separate regression, where the independent variable is average standardized supervisor rating (panel A) or average standardized teacher rating (panel B) and the dependent variable is a principal's standardized VA score from the drift-adjusted approach for the outcome listed in the column header. All models include network and district-by-year fixed effects. Standard errors clustered by principal and network shown in parentheses. Absence VA estimates are multiplied by -1 to facilitate comparison with achievement estimates. Sample sizes refer to principal-by-year observations beginning in 2011–12, which is the first year that supervisor and teacher ratings are available.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Appendix A

Supplementary Figures and Tables

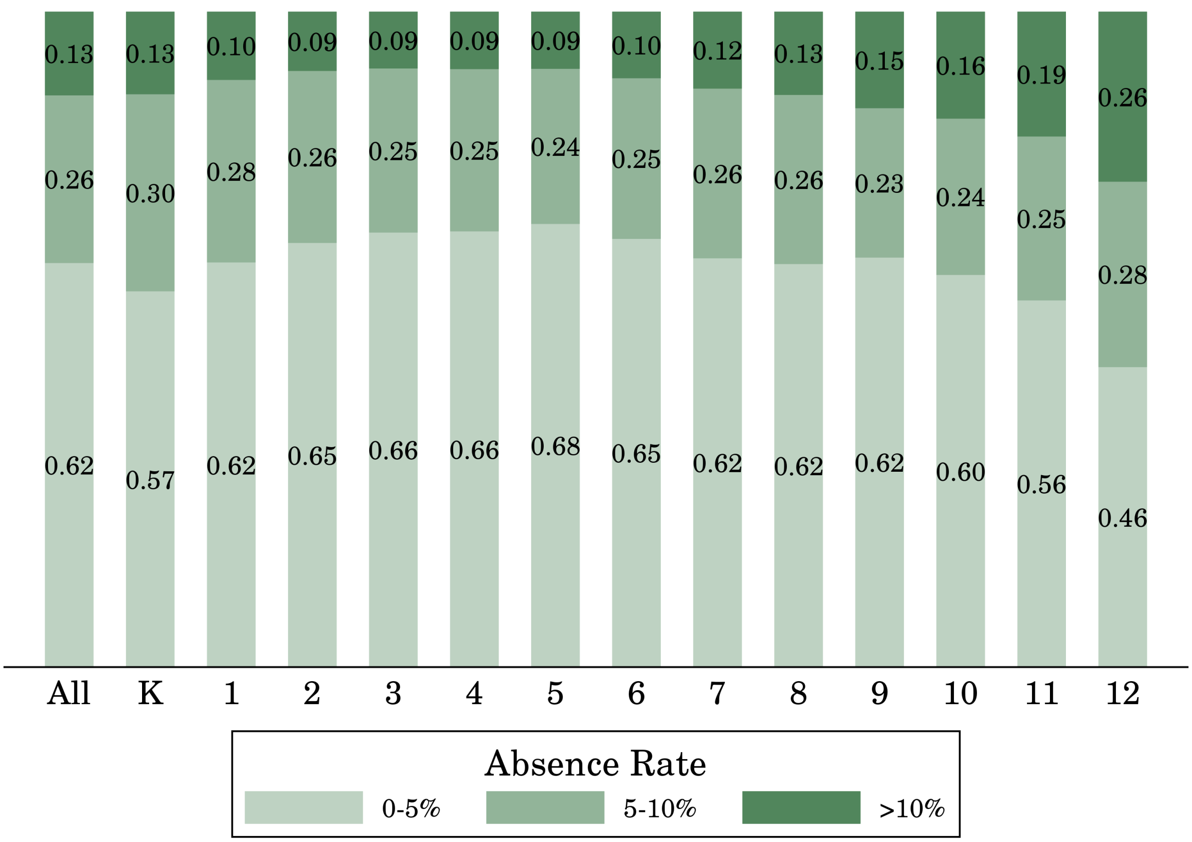

Figure A1. Student Absenteeism by Grade in 2016–17

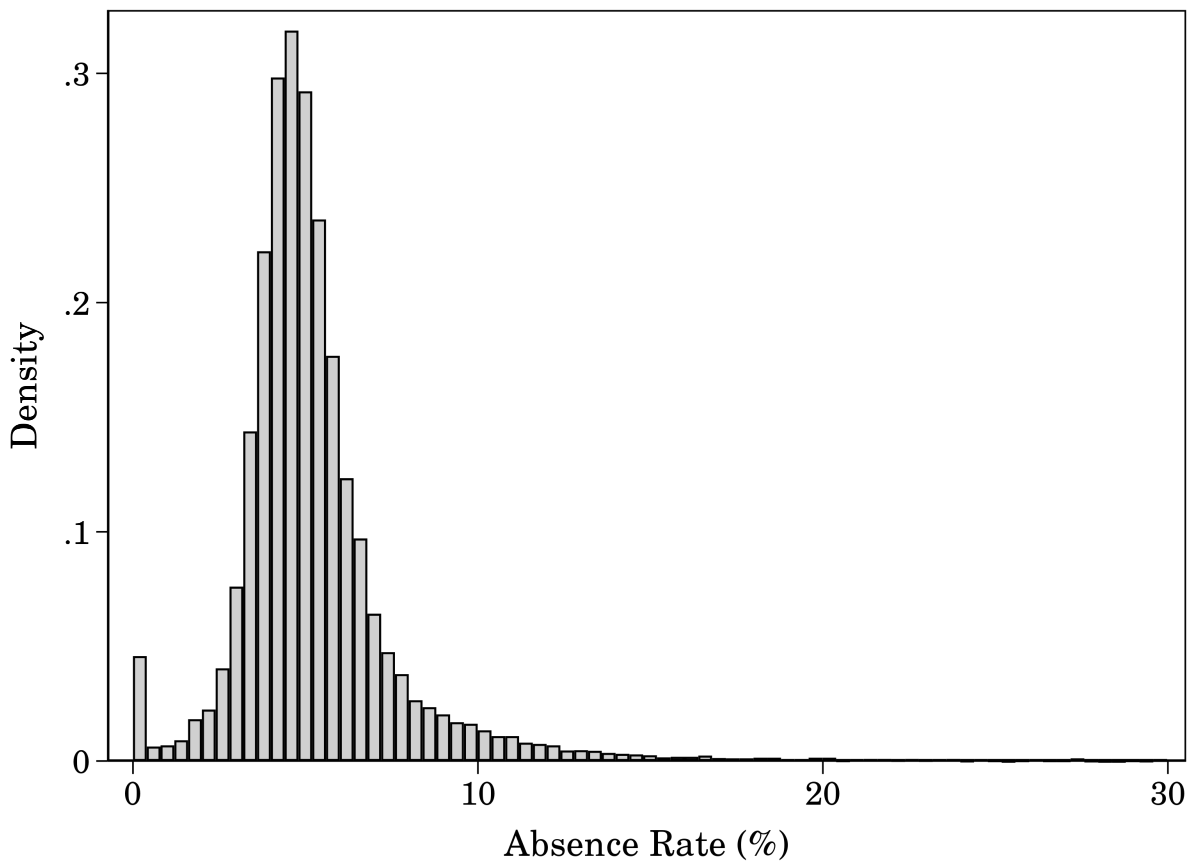

Figure A2. Distribution of School Absence Rates in Tennessee

Table A1
Distribution of Principal Networks for Absence Outcomes

Network Size (# of Schools)	# of Networks	Mean # of Principals	Total Principals	% of Principals
1	525	2.4	1234	39.3
2	134	3.7	500	15.9
3	52	5.2	272	8.7
4	23	7.8	179	5.7
5	10	9.0	90	2.9
6	4	11.5	46	1.5
7	7	14.1	99	3.2
8	4	17.8	71	2.3
9	4	18.3	73	2.3
10	1	19.0	19	0.6
11	1	23.0	23	0.7
12	1	14.0	14	0.4
15	1	27.0	27	0.9
41	1	74.0	74	2.4
46	1	85.0	85	2.7
74	1	149.0	149	4.7
95	1	186.0	186	5.9
All	771	4.1	3141	100.0

Notes: Networks refer to the mobility groups of principals and schools from a two-way fixed effects model for absence value-added.

Table A2

Distribution of Principal Networks for Achievement Outcomes

Network Size (# of Schools)	# of Networks	Mean # of Principals	Total Principals	% of Principals
1	301	2.3	706	34.6
2	129	3.3	425	20.8
3	52	4.4	228	11.2
4	20	6.1	122	6.0
5	8	7.9	63	3.1
6	7	9.4	66	3.2
7	6	12.5	75	3.7
8	2	14.0	28	1.4
9	3	12.3	37	1.8
10	1	15.0	15	0.7
14	1	25.0	25	1.2
15	1	15.0	15	0.7
27	1	42.0	42	2.1
28	1	50.0	50	2.4
42	1	68.0	68	3.3
43	1	77.0	77	3.8
All	535	3.8	2042	100.0

Notes: Networks refer to the mobility groups of principals and schools from a two-way fixed effects model for math achievement.

Table A3

Descriptive Statistics for Analytic Sample

	N	Mean	SD	Min	Max
Outcome Variables					
Absence Rate	4319247	5.5	6.4	0.0	100.0
Unexcused Absence Rate	4319247	2.5	4.3	0.0	100.0
Excused Absence Rate	4319247	2.9	4.0	0.0	100.0
Chronically Absent	4319247	0.14			
Math Score (standardized)	2363205	-0.01	0.98	-5.76	5.45
Reading Score (standardized)	2808894	-0.00	0.98	-8.22	9.57
Student Characteristics					
Female	4319247	0.49			
Free/Reduced-Price Lunch	4319247	0.56			
American Indian	4319247	0.00			
Asian	4319247	0.02			
Black	4319247	0.28			
Hispanic/Latino	4319247	0.07			
Pacific Islander	4319247	0.00			
White	4319247	0.63			
Special Education Classification	4319247	0.14			
Gifted Classification	4319247	0.02			
Same Grade Last Year	4319247	0.02			
School Characteristics					
Elementary	4239793	0.24			
Middle	4239793	0.31			
High	4239793	0.41			
Other	4239793	0.04			
Urban	4239793	0.32			
Suburban	4239793	0.18			
Town	4239793	0.18			
Rural	4239793	0.32			

Notes: Includes all student-by-year observations included in the analytic sample used to estimate principal value-added for absence outcomes. Analytic sample is defined by students with non-missing demographics, a prior-year absence outcome from a different school, and a principal in network with at least one other principal.

Table A4
Distribution of Student Grade and Years in Analytic Sample

Student Grade			School Year		
	N	%		*N*	%
KG	82393	1.9	2008	162431	3.8
1st	128580	3.0	2009	308788	7.1
2nd	162494	3.8	2010	413751	9.6
3rd	202535	4.7	2011	479644	11.1
4th	212775	4.9	2012	482811	11.2
5th	285481	6.6	2013	495345	11.5
6th	507422	11.8	2014	497998	11.5
7th	476485	11.1	2015	491647	11.4
8th	422702	9.8	2016	495589	11.5
9th	586349	13.6	2017	491243	11.4
10th	496502	11.5			
11th	407360	9.5			
12th	339564	7.9			

Notes: School year is defined as follows: 2008 = 2007–08. Analytic sample is defined by students with non-missing demographics, a prior-year absence outcome from a different school, and a principal in network with at least one other principal.

Table A5

Mean Number of Students per Principal Included in Calculation of Drift-Adjusted Principal VA by School Level

	Elementary	Middle	High
Absence VA	781	2465	3303
Achievement VA	315	2064	1652

Notes: These tabulations refer to the mean number of students in the analytic sample per principal used to estimate drift-adjusted value-added. For instance, the mean number of students who contribute to the estimation of an elementary principal's VA estimate for a given year is 315. By definition, this is the total number of students who contribute to the estimation of the principal fixed effect minus the number of students assigned to that principal in the given year (since current-year students do not contribute to estimation of current-year VA).

Table A6

Distribution of Principal Value-Added Estimates for Standardized Absence Outcomes

				Percentile of Estimates				
	N	SD	IQR	10th	25th	50th	75th	90th
Fixed Effects								
Absence Rate (SD)	3141	0.22	0.13	-0.22	-0.07	-0.00	0.06	0.20
Unexcused Absence Rate (SD)	3141	0.25	0.14	-0.23	-0.08	-0.00	0.07	0.21
Excused Absence Rate (SD)	3141	0.26	0.19	-0.27	-0.09	0.00	0.09	0.27
Drift-Adjusted								
Absence Rate (SD)	13173	0.18	0.11	-0.18	-0.06	-0.00	0.05	0.15
Unexcused Absence Rate (SD)	13173	0.21	0.12	-0.20	-0.06	-0.00	0.06	0.17
Excused Absence Rate (SD)	13173	0.19	0.14	-0.20	-0.07	0.00	0.07	0.18

Notes: Absence estimates are multiplied by -1 to facilitate comparison with attendance and achievement estimates. Attendance/absence rates are standardized within grade and year. Sample sizes for fixed effects are at the principal level, whereas drift-adjusted are at the principal-by-year level. “IQR” refers to interquartile range, which is the difference between the 75th and 25th percentile of the distribution.

Table A7

Spearman Correlations Among Value-Added Estimates (Fixed Effects)

	Abs	Abs (U)	Abs (E)	Chr Abs	Math	Read
Absence Rate	1.00					
Unexcused Absence Rate	0.62	1.00				
Excused Absence Rate	0.48	-0.23	1.00			
Chronic Absenteeism	0.93	0.58	0.46	1.00		
Math Achievement	0.15	0.11	0.04	0.12	1.00	
Reading Achievement	0.13	0.09	0.05	0.13	0.47	1.00

Notes: Absence estimates are multiplied by -1 to facilitate comparison with attendance and achievement estimates.

Table A8

Distribution of Principal Value-Added Estimates

			Percentile of Estimates				
	SD	IQR	10th	25th	50th	75th	90th
Absence Rate, All Students	1.5	0.8	-1.4	-0.4	0.0	0.4	1.2
Absence Rate, Drop Zero Absence Students	1.5	0.8	-1.3	-0.4	0.0	0.4	1.2
Absence Rate, Drop Bottom 10% Absence Students	1.5	0.8	-1.3	-0.4	0.0	0.4	1.2
Absence Rate, Drop Bottom 25% Absence Students	1.4	0.8	-1.2	-0.3	0.0	0.4	1.2

Notes: Absence estimates are multiplied by -1 to facilitate comparison with attendance and achievement estimates. For attendance/absence rates, the scale is 0–100%. Chronic absenteeism is expressed as a probability on a 0 to 1 scale. Achievement outcomes are student-level standard deviation units. Sample sizes for fixed effects are at the principal level, whereas drift-adjusted are at the principal-by-year level. “IQR” refers to interquartile range, which is the difference between the 75th and 25th percentile of the distribution.

Table A9

Spearman Correlations Among Drift-Adjusted Value-Added Estimates

Absence Rate, All Students	1.00				
Absence Rate, Drop Zero Absence Students	0.95	1.00			
Absence Rate, Drop Bottom 10% Absence Students	0.94	1.00	1.00		
Absence Rate, Drop Bottom 25% Absence Students	0.90	0.96	0.97	1.00	
Chronic Absenteeism	0.90	0.89	0.89	0.88	1.00

Notes: Absence estimates are multiplied by -1 to facilitate comparison with attendance and achievement estimates.

Appendix B

Details on Estimation of Principal Value-Added

As outlined in the methods section, the construction of the drift-adjusted VA follows three steps. First, I estimate the two-way fixed effects model (i.e., the same model that produces the fixed effects VA estimates), compute the student residuals (adding back the estimated principal fixed effect), and average these residuals at the principal-by-year level. Second, using these principal-by-year residuals, I estimate the best linear predictor of principal-by-year residuals in year t as a function of principal-by-year residuals in prior and future years. Finally, using the estimated coefficients from the best linear predictor, I predict a principal's VA in year t based on their average residuals from all available years other than year t.

Tables B1 and B2 show the autocorrelation vectors for principal VA across each attendance and achievement outcome (the corresponding autocovariances in Tables B4 and B5). The autocorrelations for achievement outcomes are substantially larger than those for teacher VA in Chetty et al. (2014). In other words, compared to teachers, principals' mean residuals from prior and future years are much more predictive of current-year residuals. For instance, math residuals from year $t+1$ or $t-1$ are correlated with math residuals from year t at 0.76, whereas Chetty et al. estimated correlations of 0.43 and 0.48 for teachers in elementary and middle school, respectively. This is not particularly surprising, given that the number of students used to produce these residuals is much larger for principals than for teachers (i.e., all students in the school versus a single classroom or handful of classrooms for teachers). An additional reason for the larger correlations is that, in contrast to teacher VA, principal VA draws on data from the same students in different years. In the case of teachers, VA in year t is predicted using test score residuals from different *cohorts* of students. As discussed above, however, since the majority of students remain in the same school in successive years, the residuals from prior and future years will contain many of the same students as the current year. This will increase the correlations among residuals in comparison to residuals from different cohorts of students.

Tables B1 and B2 also demonstrate the concept of "drift" in VA. In essence, drift simply refers to the phenomenon that residuals from proximal years are more highly correlated with current-year residuals than distal years. Thus, when predicting current-year VA, residuals from closer years receive greater weight. There are similar patterns of drift across each of the outcomes. Beyond the first lag,[20] the correlation with year t residuals decreases. However, the apparent lower bound on this drift is, again, much larger for principals than for teachers. Additionally, the drift pattern flattens out much more quickly—beyond the third lag, there is essentially no further drift except for reading achievement. An additional note about these correlations is that the principal attrition rate is quite high, which is evident from the decreases sample size for larger lags. Compared to teachers, a much smaller proportion of principals remain in the data (i.e., as a principal in any school) over time. This makes the estimated correlations at larger lags

[20] I refer to "lags" but note that both past and future residuals are used to estimate current-year VA, which is the same as Chetty et al..

Table B1

Autocorrelation Vectors for Principal Value-Added for Attendance Outcomes

Lag	N	Abs	Abs (U)	Abs (E)	Chr Abs
1	10255	0.64	0.70	0.58	0.71
2	7603	0.59	0.62	0.50	0.67
3	5488	0.54	0.54	0.44	0.64
4	3801	0.52	0.52	0.47	0.62
5	2535	0.54	0.50	0.46	0.65

Notes: N refers to the number of principal-year pairs that are x years apart, where x is the lag number shown in the left-most column.

Table B2

Autocorrelation Vectors for Principal Value-Added for Achievement Outcomes

Lag	N	Math	Reading
1	6326	0.76	0.68
2	4939	0.70	0.60
3	3542	0.67	0.56
4	2430	0.66	0.53
5	1587	0.66	0.48

Notes: N refers to the number of principal-year pairs that are x years apart, where x is the lag number shown in the left-most column.

substantially less precise. As such, when estimating principal VA I impose a drift limit of 3 (i.e., the covariances for lags larger than 3 are assumed to be the same as the covariance for 3). However, changing the drift limit does not substantially affect the VA estimates, since the magnitude of drift beyond the first lag is fairly low and few principals remain in the principalship for a long period of time.

To construct VA using prior and future residuals, Chetty et al. assume that VA follows a stationary process, meaning that the correlation between two sets of principal-year residuals only depends on the number of years between them. If the stationarity assumption holds, the estimator should produce VA estimates that do not systematically under- or over-predict actual student-level residuals in year t. Figure B1 shows the estimated relationship between principal drift-adjusted VA and current-year student residualized outcomes. Under the stationarity assumption, this regression should have a slope of 1. Indeed, across each outcome, the estimated slopes are close to 1 (for no outcome can I reject the null hypothesis at the 95% confidence level that the slope is equal to 1).

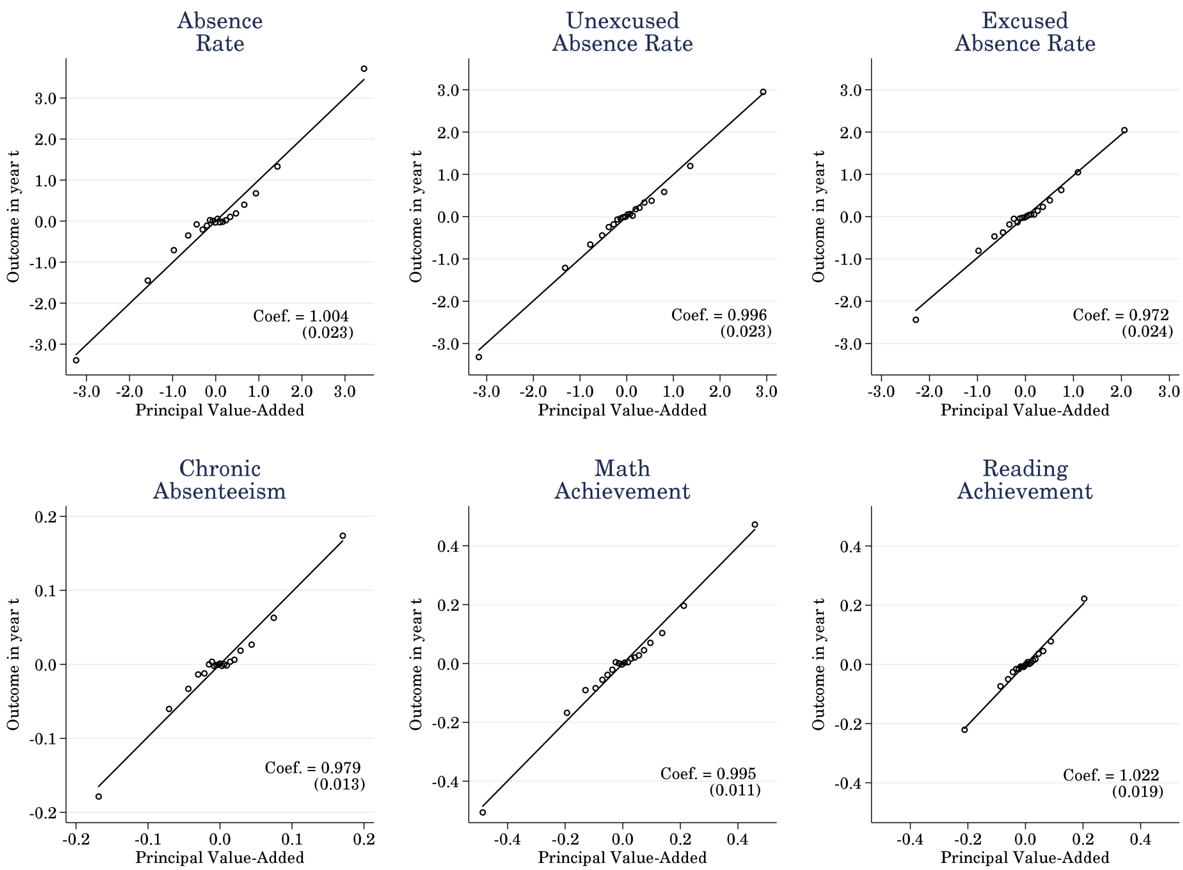

Figure B1. Effect of Principal Value-Added on Actual Student Residuals

Notes: Each plot shows a binned scatterplot of student residuals versus drift-adjusted principal value-added in the current year. The line and coefficient are from the corresponding model that regresses student residuals on principal VA. Standard errors clustered by school are shown in parentheses. For absence rates, the scale is 0–100%. Chronic absenteeism is expressed as a probability on a 0 to 1 scale. Achievement outcomes are student-level standard deviation units.

Given the relatively high correlations among principal-by-year average residuals for attendance and achievement and a very limited amount of drift, the drift-adjusted VA and fixed effects estimated should be very highly correlated, which I show in Table B3. Nonetheless, the drift-adjusted approach is appealing because it implicitly reduces measurement error in the VA estimates. Additionally, because it does not incorporate information about student performance in the current year, I can implement tests of forecast bias, which I outline below.

Table B3

Correlation Between Value-Added Estimates from Fixed Effects and Drift-Adjusted Models

	Spearman	Pearson
Absence Rate	0.91	0.96
Unexcused Absence Rate	0.90	0.96
Excused Absence Rate	0.90	0.94
Chronic Absenteeism	0.92	0.97
Math	0.93	0.97
Reading	0.93	0.95

One major shortcoming in the principal effects literature is that few studies have explicitly tested the extent to which the estimates produced by principal VA models are valid or reliable measures of principal performance. This stands in stark contrast to the teacher effects literature, where a large number of studies have attempted to validate teachers' VA estimates. Among these studies is Chetty et al., who propose sorting tests using students' twice-lagged test scores and information about family income from tax data, as well as a quasi-experiment that leverages plausibly exogenous variation in grade-level teacher turnover. Other studies have used random assignment of students and teachers to classrooms to test the validity of VAMs. Unfortunately, in the case of principals the options are much more limited. While I leave a more detailed exploration of the properties of principal VA to future work, I test for bias in drift-adjusted VA using a sorting test similar to that employed by Chetty et al..

Specifically, Chetty et al. show that one can estimate forecast bias in VA estimates by regressing predicted test scores (based on observable characteristics excluded from the VA model) on the VA estimates. The intuition of this approach is that, if VA estimates are forecast unbiased (i.e., the control vector fully accounts for nonrandom sorting), these omitted characteristics will not be correlated with their principal's VA in the current year. Among the characteristics used to test for bias is twice-lagged test score (Rothstein, 2010). I also draw on students' prior outcomes that are omitted from the VA estimation. Here, however, the procedure is slightly more complex given the endogeneity between principal fixed effects and prior-year outcomes. Specifically, my VA models control for a *prior-school* outcome, which is often not the most recent prior score. Thus, for the sorting test I draw on a student's attendance/achievement outcome from the year prior to the year of the score used to estimate principal VA. For instance, if an 8th grade student's most recent prior-school outcome is from 5th grade, I use their 4th grade outcome to estimate forecast bias.[21]

[21] Perhaps a more straightforward approach is to restrict the sample to students who made a structural move in the current year (i.e., 9th grade students who moved from a middle school to a high school). In this case, the prior-school outcome would simply be the prior-year outcome, and I could then use the

The steps to implementing the test are as follows. First, I residualize twice-lagged prior-school outcomes on the same set of controls used in the two-way fixed effects model. In other words, I follow the same first step for estimating drift-adjusted VA but use the lagged outcome instead of the current-year outcome. Second, I re-estimate drift-adjusted VA using the common set of students who have twice-lagged prior-school outcomes. This ensures that the forecast bias test results are not confounded by sample selection. Third, I estimate a "predicted" current-year score by regressing (with principal fixed effects) the actual current-year residualized score on the residualized twice-lagged prior-school score. Intuitively, the fitted values from this regression represent the prediction of a student's outcome in year t based on their performance in a prior year that has not been included in the model used to estimate VA. If the VA model inadequately accounts for nonrandom sorting, VA estimates will be significantly correlated with the predicted current-year outcomes.

The results of the test for forecast bias are shown for attendance and achievement outcomes in Figure B2. Each plot shows the coefficient from regressing the predicted score (based on twice-lagged prior-school outcomes) on the principal VA estimates. Across each outcome, the estimated slope is close to zero, suggesting minimal forecast bias. For instance, the math achievement results imply that a 1 SD increase in principal VA (roughly 0.20 on the VA scale) is associated with a 0.039 SD increase in predicted student math achievement. By comparison, the equivalent test by Chetty et al. for teacher VA in math and reading yielded coefficient of 0.022 SD.

twice-lagged outcome to test for bias. However, this greatly limits the sample and I find very similar results compared to when using all available students.

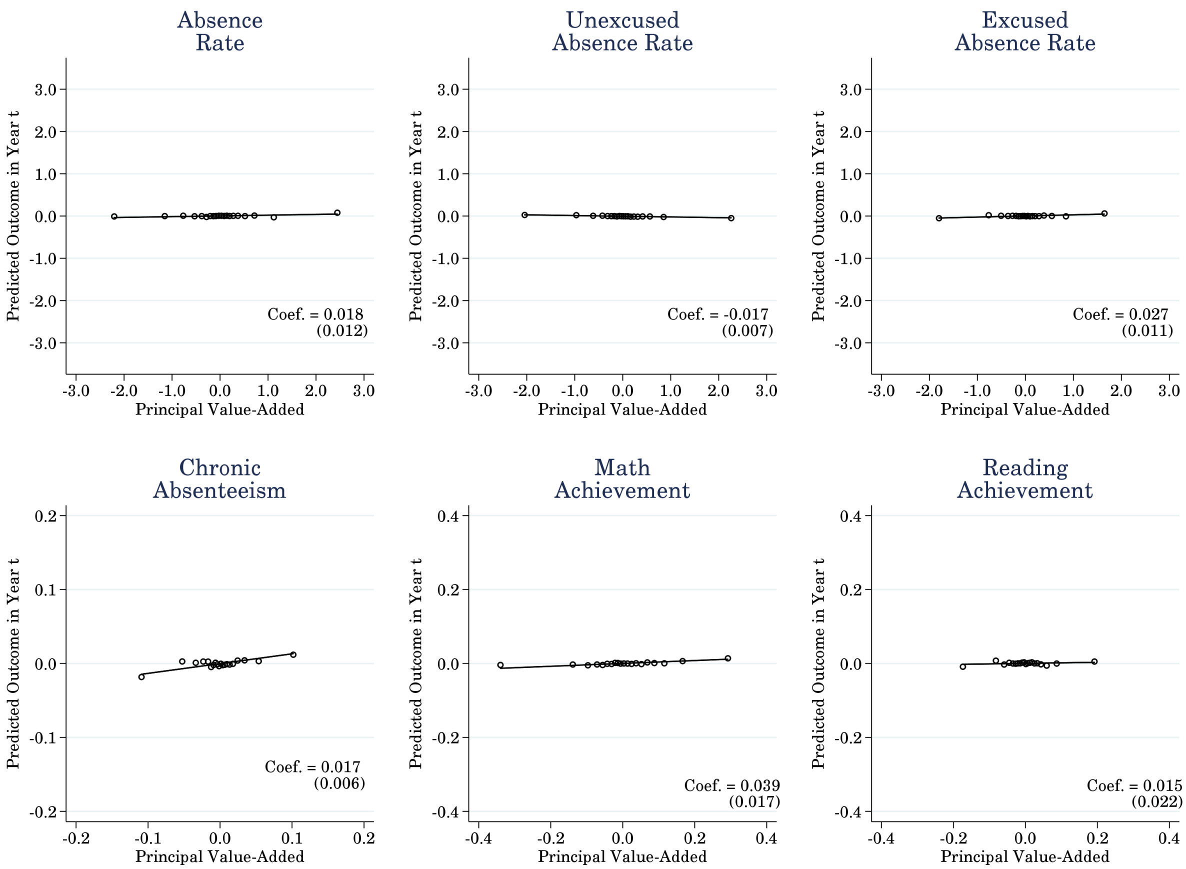

Figure B2. Effect of Principal Value-Added on Predicted Student Residuals using Twice-Lagged Prior-School Outcomes

Notes: Each plot shows a binned scatterplot of predicted student residuals versus drift-adjusted principal value-added in the current year. The line and coefficient are from the corresponding model that regresses student residuals on principal VA. Standard errors clustered by school are shown in parentheses. For absence rates, the scale is 0–100%. Chronic absenteeism is expressed as a probability on a 0 to 1 scale. Achievement outcomes are student-level standard deviation units.

Table B4
Autocovariance Vectors for Principal Value-Added for Absence Outcomes

Lag	N	Abs	Abs (U)	Abs (E)	Chr Abs
1	10255	2.25	1.74	0.94	0.0049
2	7603	2.14	1.58	0.83	0.0048
3	5488	2.07	1.46	0.75	0.0049
4	3801	2.10	1.41	0.78	0.0051
5	2535	2.49	1.48	0.78	0.0060

Notes: N refers to the number of principal-year pairs that are x years apart, where x is the lag number shown in the left-most column.

Table B5
Autocovariance Vectors for Principal Value-Added for Achievement Outcomes

Lag	N	Math	Reading
1	6326	0.041	0.009
2	4939	0.039	0.008
3	3542	0.039	0.008
4	2430	0.039	0.008
5	1587	0.041	0.008

Notes: N refers to the number of principal-year pairs that are x years apart, where x is the lag number shown in the left-most column.