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Measuring returns to experience using supervisor ratings of observed performance: The case of classroom teachers[†]

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Monitoring employee job performance is a fundamental task in personnel management. In particular, understanding how performance improves with experience—the "returns to experience"—is critical to decisions about hiring and turnover, investments in employee training, and others. Consider the choice between retaining a current employee or replacing that employee with a novice new hire; the optimal choice depends not simply on the current performance of the two individuals, but rather on each person's expected future performance over time. However, isolating the causal effects of experience is complicated by imperfect and incomplete performance measures, and selection on performance through hiring and turnover decisions.

Supervisor ratings of observed performance—a ubiquitous a job performance measure—present a particular challenge when measuring returns to experience. For example, the relative subjectivity of supervisor ratings creates scope for leniency bias (Prendergast 1999), and supervisors' leniency bias may itself depend on the employee's years of experience. We examine the case of classroom teachers, and the most common performance measure for public-school teachers: ratings by the school principal based on classroom observations.

Our focus is estimating the returns to experience in teaching using classroom observation ratings. We define "returns to experience" as the causal effect of one additional year of teaching experience on teacher performance, estimating returns separately for the first year of experience, second year, third year, etc. We define experience broadly to include whatever professional experiences occur over the course of a teacher's first year (or second year, etc.). Our primary objective is evaluating claims about returns to experience for (a) performance of the teaching practice inputs which the observation rubrics are designed to measure. But we also consider inferences about returns to experience on (b) broader output-based measures of teacher performance, like teachers' value-added contributions to student achievement scores. The extent to which experience affects (a) and (b) differently partly motivates our work, because input-based measures are much more common in schools than output-based measures.

We use a difference-in-differences framework to make explicit the causal inference features of the returns-to-experience estimates. Our preferred estimates come from applying a diff-in-diff strategy proposed by de Chaisemartin and D'Haultfœuille (2020, 2022a). Briefly, the first difference is the observed change in a teacher observation rating between year $(t-1)$ to t when her experience changes from $(e-1)$ to e. The second difference is between early-career (treated) teachers and veteran (comparison) teachers. These estimates are the solid lines in Figure 1 using data from Tennessee and the Washington, DC Public Schools (DCPS). The identifying assumptions require: First, that, on average, veteran (comparison) teachers no longer experience returns to an additional year of experience. Second, that the process, explicit or implicit, that maps true performance to ratings does not depend on a teacher's years of experience.

We evaluate several threats to the two identifying assumptions. Most threats are reasons why observation ratings might rise (fall) over time even if a teacher's true performance is unchanged. One simple example is when changes are made to the scoring rubric, as happened in DCPS in 2017. As we discuss in detail, changes to the rubric (or to rater training, or to rater-teacher matching rules) do not necessarily threaten causal inferences about returns to experience estimates. Veteran teachers—the diff-in-diff comparison group—provide an estimate of the effect of such changes under the first assumption above, and that estimate is a reasonable counterfactual for early-career teachers under the second assumption above. We use similar reasoning, combined with empirical evidence where available, to address other threats: rater leniency bias, raters using information from outside the observation, changes in incentives that distort teacher effort, manipulation behaviors by teachers which raise scores but not performance, the effect of job changes, and others. We find little evidence that these potential threats compromise a causal interpretation of our estimates.

Our preferred estimation method is new to the literature on teacher returns to experience. Thus, we compare our estimates to estimate using the conventional strategy. That strategy is also a difference-in-differences strategy using a two-way fixed effects estimator, and both strategies require the same two main identifying assumptions. However, the conventional strategy requires additional assumptions about the heterogeneity of treatment effects.

Understanding observation-based ratings is especially salient in the education sector. With differences in teacher effectiveness increasingly recognized, substantial attention has focused on the development, understanding, and application of measures of teacher performance (Goe, Bell, and Little 2008, Kane, Kerr, and Pianta 2014, Jackson, Rockoff, and Staiger 2014). Despite measure of performance, there is comparatively little evidence on whether or how teaching improves (Jackson, Rockoff, and Staiger 2014). One exception is that, on average, performance improves over the first few years of a teacher's career. This returns-to-experience finding has been widely replicated, but nearly all existing estimates measure performance with a teacher's "value added" to student achievement test scores (see, for example, Rockoff 2004, Papay and Kraft 2015). The growth in value-added measures hints at opportunities for training or other management interventions; however, the test-score value-added measures are outcomes, and offer little insight on the teaching tasks by which teachers could improve.

Classroom observations offer another measure of teaching performance that may provide insight on the specific skills teachers develop early in their careers. Standardized, rubric-scored classroom observations are now widely used, and most teachers receive at least one observation per year (Cohen and Goldhaber 2016, Steinberg and Kraft 2017). States and school districts use observations for a variety of purposes, including understanding changes in teaching performance over time.

For example, Figure 1 shows the average improvement over the first ten years of teaching for several cohorts of Tennessee and DCPS teachers as measured by their observation ratings. Figure 2 shows the same estimates but for value added to student test scores. The pattern of change over time is similar in these four graphs, which raises questions about whether either reflects true improvement and, if so, the relationship between skill development and teachers' ability to improve student achievement.

This is the first paper, to our knowledge, that studies the causal returns to experience reflected in supervisor ratings of observed performance. That contribution to the literature comes from combining explicit causal inference reasoning with observed performance ratings. Many prior papers have contributed causal estimates of the returns to experience using other measures of performance, for example, wages (e.g., Angrist 1990, Altonji and Williams 1992, Grogger 2009) or teacher value-added to student achievement scores (e.g., Rockoff 2004, Rivkin, Hanushek, and Kain 2005, Ost 2014). Our observation rating estimates of inputs complement the value-added estimates of outputs, in part by contributing to efforts to understand the mechanisms behind teachers' improvements in value added (Kraft and Papay 2014, Ost 2014, Atteberry, Loeb, and Wyckoff 2015).

Two other papers use classroom observation ratings to make claims about returns to experience: Kraft, Papay, and Chi (2020) using data from Charlotte, North Carolina, and work in-progress by Laski and Papay (2020) also using Tennessee's data. Both papers report estimates similar to Figure 1. However, these two papers focus largely on substantive explanations for the improvement in ratings over a teacher's career. Neither paper provides an explicit examination of causal inference considerations: identification strategy, identifying assumptions, threats to those assumptions, etc. As our paper makes clear, the causal inference considerations when studying observation ratings are more complex than when studying teacher value added.

A related contribution of our paper is a new estimation strategy for studying the returns to experience in teaching, suited to both observation ratings and value-added measures. That new strategy incorporates recent developments in difference-in-differences methods (for reviews see de Chaisemartin and D'Haultfœuille 2022b, Roth et al. 2022), and it requires weaker identifying assumptions than the currently-most-common two-way-FE strategy.

A final contribution comes in our analysis of threats to identification; that analysis incorporates several concerns about observation scores raised in prior papers, and in many cases provides new empirical evidence. These known concerns include rater leniency bias (Weisberg, Sexton, Mulhern, Keeling et al. 2009; Steinberg and Kraft 2017), the influence of the students in the classroom (Campbell and Ronfeldt 2018), unintended effects of teacher-rater pairings (Chi 2021), among other concerns (Cohen and Goldhaber 2016, Grissom and Bartanen 2019).

1. Data and setting

Both DCPS and Tennessee maintain panel data on teachers, including ratings from classroom observations over several years. In DCPS, the panel begins with the start of its current evaluation system, IMPACT, in 2009-10, and we use data through 2018-19. Tennessee's current evaluation system began in 2011-12, and our data run from that start date through 2018-19. In both cases the data include item-level ratings for several specific teaching tasks evaluated in a given observation visit. Teachers in tested grades and subjects can be linked to their students and achievement scores. Characteristics of the teachers and their students in our data are summarized in Table 1.

1.1 Features common to both settings

The DCPS and Tennessee settings share many features. In both locations, all teachers, regardless of experience level, are evaluated every school year by trained observers. The resulting observation ratings are a highly-weighted component, among a larger set of evaluation measures including value-added scores which measure teacher contributions to student achievement.[1] The larger evaluation systems are used to identify exemplary teachers, those in need of additional support or training, or individuals who will be dismissed. During most of the period we study, teachers in DCPS were observed five times per year. After a change in the rubric in 2017, teachers were observed up to three times per year depending on experience and performance. In Tennessee, the number of evaluations per year varies according to teachers' prior performance and licensure status, but teachers are typically evaluated multiple times per year. The median novice teacher in Tennessee receives 2.5 formal observations and the median novice teacher in DCPS receives five formal observations.

While the two systems use different observation rubrics, both rubrics assess similar tasks and teaching practices, and both rubrics have roots in Danielson's Framework for Teaching (1997). Tennessee uses the TEAM (Tennessee Educator Acceleration Model) evaluation rubric.[2] The TEAM rubric's 19 items are divided into three categories of skills: instruction, planning, and environment. Each category is comprised of multiple items for teaching tasks. Ratings for each item range from 1-5 (5 = significantly above expectations, 1 = significantly below expectations). During most of the period of our analysis, DCPS used an observation rubric called the Teaching and Learning Framework (TLF). The TLF rubric has a 1-4 rating scale (4 = highly effective, 1 = ineffective) for items measuring nine teaching tasks.[3] In 2017, DCPS transitioned to the Essential Practices (EP)

[1] In DCPS classroom observations account for 75 percent of overall IMPACT scores for the more than 80 percent of teachers without a value-added score. For teacher with value added as part of their evaluation, observations account for between 30 and 40 percent depending on the year. In Tennessee, classroom observations are 50 and 85 percent of the overall TEAM score for teachers with and without value-added scores, respectively.

[2] Not all Tennessee districts use the TEAM rubric, but our analysis in this paper uses only data from the TEAM rubric.

[3] The first seven tasks align generally with the domain of instruction, while the final two align with the domains of classroom management and environment.

observation rubric, which covers similar skills to the TLF, but with more concise definitions for each related task and explicit alignment to the Common Core State Standards.

One frequent, but misleading, criticism of such classroom observation systems is that the scores produced have little variation, with most teachers scoring in one or two top categories (Kraft and Gilmour 2017, Weisberg et al. 2009). This lack of variation arises in part because final scores are rounded off to integer values. In this paper we use observation scores that average across many item ratings (several items and several observations of a given item), and those scores vary meaningfully, with a relatively Gaussian density (as shown in Appendix Figure A1).

1.2 Differences between the two settings

While both evaluation systems share many features, there are a number of useful differences. First, both places use trained school administrators as raters (e.g., principals and assistant principals, or other instructional leaders). However, until a change in 2017, in DCPS teachers were also observed and rated by "master educators"—specialized observers external to the school with subject- and grade-specific expertise. Two of each teacher's annual observations were conducted by a master educator.

Second, the two systems have different incentives and consequences associated with teachers' performance scores. While both DCPS and Tennessee might be considered high-stakes evaluation systems, DCPS's has notably higher stakes. In DCPS, teachers with low performance (a final annual score below effective) are subject to involuntary dismissal. Prior work documents that these incentives influences teachers' behavior at work and their decision about remaining at DCPS (Dee and Wyckoff 2015, Dee, James, and Wyckoff 2021). There are also rewards in DCPS for high performance. Teachers who demonstrate exceptional performance (a final annual score of highly effective) are eligible for substantial bonuses and, if they continue to perform well, large base pay increases. In Tennessee, to earn tenure a teacher must receive a final composite score of “above expectations” or higher (roughly the top two-thirds of teachers) for two consecutive years, after working at least five years total. Tenure can be revoked based on evaluation scores but that is rare: a teacher must score “below expectations” or lower (roughly the bottom 5 percent of teachers) for two consecutive years, and this rule does not apply to teachers who were tenured before 2011-12.

Finally, in addition to the specifics of their evaluation systems, DCPS and Tennessee differ from each other in size and many other characteristics. TEAM is used by nearly the entire state of Tennessee, and therefore includes teachers and schools across a range of settings and demographics. Each year the Tennessee data include roughly 84,000 teachers, of whom 5,500 are in their first-year teaching, with 450,000 students at 1,350 schools. DCPS, on the other hand is an urban majority-minority and low-income district, with approximately 3,500 teachers (290 novice) at 125 schools serving 46,000 students each year.

1.3 Additional data

In addition to classroom observation ratings, we have access to other data for teachers and students. For DCPS and Tennessee teachers, we know when they entered teaching, their experience in teaching, and other demographic characteristics. We have information regarding the observation raters and timing of the observation visits. In both settings we have the usual information regarding each teacher’s students, for tested subjects and grades, including eligibility for free or reduced-price lunch, race and ethnicity, and standardized achievement scores.

Additionally, DCPS began using student surveys in 2016-17 as teacher performance measures. This measure is adapted from the Tripod survey (Ferguson and Danielson 2015), which ask students’ questions about their teachers’ practice. An example question: “When explaining new ideas or skills in class, my teacher tells us about common mistakes that students might make.”

2. Returns to experience estimates

2.1 Estimation methods

We estimate the “returns to experience”—the improvement in performance caused by additional experience—using a difference-in-differences strategy. Our measure of performance, $\bar{s}_{jt}$, is the classroom observation score for teacher j in school year t. At the start of year t, teacher j has $expr_{jt}$ years of prior teaching experience. Using these inputs, we apply the diff-in-diff estimator proposed by de Chaisemartin and D’Haultfœuille (2020, 2022a).

Let δ_e be the improvement in performance caused by gaining the eth year of teaching experience. Our estimate of δ_e is:

$$\hat{\delta}_e = \frac{\sum_t N_{et}\,\hat{\delta}_{et}}{\sum_t N_{et}}$$

$$\hat{\delta}_{et} = \left[\frac{1}{N_{et}} \sum_{\substack{j:expr_{j,t}=e,\\ expr_{j,t-1}=e-1}} \left(\bar{s}_{j,t} - \bar{s}_{j,t-1}\right)\right] - \left[\frac{1}{M_{et}} \sum_{j:expr_{j,t-1}\geq \bar{e}} \left(\bar{s}_{j,t} - \bar{s}_{j,t-1}\right)\right] \tag{1}$$

where δ_{et} is simply the δ_e effect for a specific school year t. The number of treated teachers is N_{et} and comparison teachers is M_{et}. Because a teacher, j, may contribute to several $\hat{\delta}_e$, our standard error estimates correct for clustering at the teacher level.[4]

The treatment is gaining an eth year of experience. Thus, teacher j is in the treated sample if she had e years of experience at the start of school year t, but only

[4] In practice, we estimate all the several $\hat{\delta}_e$ simultaneously in one system of weighted least squares regressions, stacking together one regression for each $\hat{\delta}_{et}$. The weight for each $\hat{\delta}_{et}$ is $\frac{N_{et}}{\sum_t N_{et}}$ as shown in Equation 1. The stacked regressions approach allows us to estimate standard errors which are cluster (teacher) corrected across regressions.

$(e-1)$ years of experience the prior school year. Inside the brackets on the left is the average first-difference in observation score, $\bar{s}_{jt}$, for the sample of treated teachers. That first-difference is the observed change in a teacher j's score between year $(t-1)$ to t when her experience changes from $(e-1)$ to e. Still, a given teacher's scores may change over time for reasons unrelated to her own experience, which motivates the second difference between treated and comparison teachers.

Our comparison sample is veteran teachers—teachers who have at least $\bar{e}$ years of experience. One identifying assumption, which we formalize below, is that past $\bar{e}$ years of teaching experience, there are no longer any returns to experience for the average teacher. Inside the brackets on the right is the average first-difference for the veteran comparison teachers. Any observed change from $(t-1)$ to t among veterans is, by assumption, unrelated to experience and differenced out. Our main estimates set $\bar{e} = 9$, but our estimates are robust to higher values of $\bar{e}$ as we show later.

Each $\hat{\delta}_{et}$ estimate uses data from just two school years: one treated year, t, and one pre year, $(t-1)$. A teacher's performance in year t is affected by her eth year of experience. A teacher's performance in year $(t+1)$ is also affected by her eth year of experience, but also affected by her $(e+1)$th year of experience. Thus, we observe the marginal effect of the eth year of experience for one school year, t, after which the eth year is confounded with further experience gains.

The outcome variable, $\bar{s}_{jt}$, is teacher j's classroom observation score for school year t. More precisely, $\bar{s}_{jt}$ is the average of several task-specific scores, $\bar{s}_{jt} = \frac{1}{K}\sum_{k=1}^{K} s_{kjt}$. The Tennessee rubric includes $K = 19$ items and DCPS $K = 9$. Our focus on the average observation score is motivated by an empirical constraint: While the tasks being scored are distinct—for example "teacher content knowledge" and "managing student behavior"—in practice the scores across tasks are highly correlated. In our Tennessee data, the mean correlation between items is

0.53 with a standard deviation of 0.05; in a factor analysis the first factor explains 95 percent of the variation in item scores. This correlation of items is common in classroom observation rubric scores (e.g., Kane et al. 2011). The $\bar{s}_{jt}$ scores are scaled in teacher standard deviation units, within jurisdiction (Tennessee or DC) by year cells.[5]

2.2 Main results

Teacher performance measured in classroom observations improves with experience. In Figure 1 the solid line plots our returns to experience estimates from the difference-in-differences strategy in Equation 1. Observation scores are scaled in standard deviation units, and, by construction, the zero line on the y-axis is the average score among veteran teachers, $expr_{jt} \geq \bar{e} = 10$. The vertical lines mark cluster-corrected 95 percent confidence intervals.

Just one year of teaching experience improves performance by one-quarter to one-third of a standard deviation. Over the first ten years of a teaching career, performance in observations improves one standard deviation. The patterns in DC and Tennessee are quite similar.

The pattern in Figure 1, using classroom observation data, is also similar to the pattern of returns to experience for teacher value added to student achievement scores. In Figure 2 the solid line plots estimates where the performance measure is a teacher's value-added contribution to student test scores. We first obtain value-added scores, $\hat{\mu}_{jt}$, then apply the estimator in Equation 1 substituting $\hat{\mu}_{jt}$ for $\bar{s}_{jt}$.[6] In Figure 2, the y-axis, $\hat{\mu}_{jt}$, is measured in student standard deviation units, and the

[5] We begin with the item-by-observation-visit data recorded by observers in the original rubric units (integer scores 1-4 in DCPS and 1-5 in Tennessee). Separately for DCPS and Tennessee: (i) We standardize the item-by-visit ratings so that, by school year, each item is mean 0, standard deviation 1. (ii) For each teacher j by item by school year, we calculate the school-year average of the standardized item-by-visit ratings. We then re-standardize the item-average scores. (iii) For each teacher j by school year, we average her item-average scores to create the overall average score, $\bar{s}_{jt}$. Finally, we again standardize the overall average scores by year.

[6] Appendix B provides details of our value-added estimation methods.

sample is limited to teachers of grades 4-8 in math and English language arts. The pattern for Tennessee in Figure 2 matches estimates from several other places (see Papay and Kraft 2015 for a review). The DC estimates are much nosier but consistent with the typical pattern. Additionally, Appendix Figure A2 reports results using student survey measures of teacher performance, and again the pattern of returns to experience is quite similar.

2.3 Causal inference

The difference-in-differences setup provides a familiar framework for evaluating causal claims about the estimates in Figure 1. Stated in general terms, the identifying assumption in this case is: Any change over time we observe in veteran (comparison) teachers' scores is the same change we would see in early-career (treated) teachers' scores if there were no returns to experience. We can clarify the identifying assumption further with the help of a simple conceptual framework.

2.3.1 Observation scores and true performance

A teacher's job involves many tasks—learning content, planning lessons, asking questions in class, responding to misbehavior, grading, communicating with parents, any many more. Each of those tasks produces some input to the production of student achievement or other goals of schooling. Let θ_k measure true performance of task k. Higher performance is synonymous with producing more or higher-quality task k inputs.

Classroom observation rubrics are designed to measure task performance, θ_k, at least for some subset of a teacher's tasks. Rubrics are not designed to measure outcomes like student achievement. For example, observers are asked to score the nature and frequency of questions teachers ask students, but observers are not asked to assess whether these questions generated student learning. Observation scores are also sometimes described as measures of a teacher's skills. But an observation score is a function of both skills and effort, thus we prefer describing those scores as measures of performance.

Still, classroom observations are an imperfect way to measure performance. An observation score, s_k, is inevitably some combination of true performance, θ_k, and other factors unrelated to performance, ν_k. For exposition we assume:

$$s_k = g(\theta_k, \nu_k) = \theta_k + \nu_k \tag{2}$$

Those other factors, ν_k, include much more than just classical measurement error. Even as the number of observations grows, features of the evaluation process will create some difference between $E[s_k]$ and $E[\theta_k]$. First, ν_k includes explicit features of the evaluation process, for example, the rubric itself, how evaluators are trained, how evaluators are assigned to teachers, incentives attached to scores. Such explicit features are (mostly) controllable by those designing and implementing the evaluation. But ν_k also includes less-explicit less-controllable features, for example, the behaviors teachers or evaluators choose in response to the explicit features.

2.3.2 Identifying assumptions

Interpreting Figure 1 as the returns to experience—the causal effect of teaching experience on true task performance—requires two identifying assumptions. *Assumption 1:* Factors which contribute to observation scores but are unrelated to performance, ν_k in Equation 2, do not depend on teaching experience. Specifically, $E\left[\nu_{kjt} | k, t, expr_{jt}\right] = E\left[\nu_{kjt} | k, t\right]$. This assumption requires that if an early-career and a veteran teacher both have the same true task performance, θ_k, they will have the same observation score, s_k. *Assumption 2:* True performance is not changing over time, on average, in the comparison group of teachers. Specifically, $E\left[\theta_{kjt} - \theta_{kj(t-1)} | expr_{jt} \geq \bar{e}\right] = 0$.

The importance of a comparison group is shown by stating the assumption that would replace Assumption 2 in the absence of a comparison group. *Assumption*

3: The ν_k factors do not change over time. Specifically, $E\left[\nu_{kjt}|k,t,expr_{jt}\right] = E\left[\nu_{kjt}|k\right]$. If we used only early-career teachers' data, we could not separate the returns to experience from changes in ν_k over time, because $expr$ and t are colinear within teacher. In Section 3 we discuss several different substantive threats to these identifying assumptions, but some of the quite-plausible threats are known changes in ν_k over time.

These are the assumptions required for claims about performance of the teaching tasks which classroom observations are designed to measure. We might also be interested in claims about other aspects of teacher performance, like teachers' value-added to student achievement scores. Imagine a production process for student achievement; some of the inputs will be the teaching tasks described in an observation rubric. However, to make any inference from observation scores to value-added would require a much better understanding of that production process than currently exists.[7] Later we provide some new empirical evidence relevant to that broader inference.

2.4 Alternative estimation methods

Our estimation methods, described in Section 2.1, are new to the literature on returns to experience in teaching. Here we compare our estimation strategy to the conventional estimation strategy—the strategy which, to date, has been most common in that literature.[8] The conventional strategy is also a difference-in-differences strategy using a two-way fixed effects estimator, though it is not often

[7] The literature does include many estimates of the correlation between observation scores and teacher value-added, which is typically much less than 0.50. In our data that correlation is 0.38 for Tennessee and 0.30 for DCPS. Appendix Table A1 reports on these estimates in detail. However, 0.38 and 0.30 are likely to underestimate the true correlation. First, there is the common attenuation because of measurement error. Second, the simple mean $\bar{s}_{jt}$ gives equal weight to each task k, but it seems unlikely the elasticity of value-added, μ, with respect to θ_k is equal for all k. If we knew the production function for student achievement, we would likely choose un-equal weights.

[8] Though the conventional strategy is common, in nearly all prior papers the performance measure is teachers' value-added contributions to student test scores.

described in those terms. Both strategies require the same core set of identifying assumptions, but the conventional strategy requires additional assumptions.

In the conventional approach, estimates of the returns to experience come from a least-squares regression. The basic specification is:

$$\bar{s}_{jt} = h\left(expr_{jt}\right) + \lambda_j + \pi_t + \varepsilon_{jt} \tag{3}$$

where the outcome is a measure of teacher performance, $\bar{s}_{jt}$ in our case.

Selective attrition is a fundamental threat to any returns-to-experience estimate; attrition from teaching is likely negatively correlated with performance. In response to that threat nearly all estimation strategies focus on variation within individual teachers over time. Our main strategy uses only within-teacher variation by first differences. The conventional approach uses teacher fixed effects (Rockoff 2004).

However, for a given teacher, years of experience, $expr_{jt}$, is colinear with school year, t, unless she takes a leave of absence. Specification 3 includes both teacher fixed effects, λ_j, and school year fixed effects, π_t, and thus requires some restriction on $h\left(expr_{jt}\right)$ to avoid the age-period-cohort problem. The typical restriction is to assume no returns to experience after some number of years, $\bar{e}$. Then $h\left(expr_{jt}\right)$ is a series of indicator variables for years of experience up to $\bar{e}$:

$$h\left(expr_{jt}\right) = \sum_{e=0}^{\bar{e}-1} \beta_e \times \mathbf{1}\left\{expr_{jt} = e\right\} \tag{4}$$

$$\text{and } \delta_e = \beta_e - \beta_{e-1}.$$

The omitted category is veterans, $\mathbf{1}\left\{expr_{jt} \geq \bar{e}\right\}$.[9] This restriction maps to identifying Assumption 2, as stated earlier. That required assumption is well known

[9] There are alternative specifications of $h\left(expr_{jt}\right)$ in the literature: (i) Specifying h as cubic, or other higher-order polynomial, in $expr_{jt}$, though often still with $expr_{jt}$ top-coded at some point (e.g., Rockoff 2004). (ii) Dividing $expr_{jt}$ into bins, e.g., 1–2, 3–4, 5–9, 10–14, 15–24, and 25+ (e.g., Harris and Sass 2011). (iii) Using the non-standard age-experience progressions, e.g., leaves of

in the literature on returns to experience in teaching; the assumption is stated explicitly (e.g., Rockoff 2004) and criticized (e.g., Papay and Kraft 2015).

This conventional estimation strategy uses a two-way fixed effects estimator. Notice in specification 3 the characteristic group and period fixed effects, λ_j and π_t, and a series of treatment indicators, $h\left(expr_{jt}\right)$. A recent, growing literature clarifies several properties of two-way FE estimators; in particular, how those estimators can produce biased estimates when treatment effects are heterogeneous (for reviews see de Chaisemartin and D'Haultfœuille 2022b, Roth et al. 2022).

In our setting, that potential bias arises from differences across cohorts of teachers in their returns-to-experience profiles—heterogeneity across cohorts in the vector of treatment effects $\{\delta_1, \delta_2, \dots \delta_{\bar{e}-1}\}$. A cohort in this case is defined by when a teacher began her career, given the link between time and experience. Heterogeneity in any one parameter, δ_e for a given e, can bias the estimate of that parameter itself, $\hat{\delta}_e$. This is a bias threat now widely recognized.[10] Additionally, the estimate for the eth year, $\hat{\delta}_e$, can also be biased by heterogeneity in the returns to experience for other years, δ_{-e}. For example, $\hat{\delta}_3$ can be biased by heterogeneity in δ_1 or δ_6 across cohorts. de Chaisemartin and D'Haultfœuille (2022a) detail this second bias threat which arises when there are several treatments.

The dashed line in Figure 1 shows our estimates from the common two-way FE strategy, alongside our preferred strategy. In Tennessee the two lines are nearly identical, suggesting little change from cohort to cohort in the returns to experience.

absence, to estimate specification 1 without restrictions on h (e.g., Wiswall 2013). Additionally, it is more common to make the first year of teaching the omitted category. We prefer to omit veterans in part for comparability with our main estimates.

[10] Bias is also a concern when there is heterogeneity of effects over time within groups, giving rise to the negative weights problem (for reviews see Chaisemartin and D'Haultfœuille 2022b, Roth et al. 2022). The specification of $h\left(expr_{jt}\right)$ is analogous to the common event study specification, and (largely) avoids the bias from heterogeneity of effects over time.

By contrast, there is some difference for DCPS, suggesting the two-way FE strategy underestimates the steepness of returns to experience. First, the differences are largely explained by changes in the distribution of teacher experience in DCPS over time. Appendix Figure A3 shows that the distribution of experience shifted away from early-career teachers over time but became more stable from 2014-15 on.[11] If we restrict out analysis to this more-stable more-recent period, the standard and alternative approaches are quite similar, as shown in Appendix Figure A4. Perhaps the observable changes in the distribution of experience in DCPS are correlated with changes in the returns to experience among DCPS teachers. Second, the difference in Figure 1 for DCPS is more a difference in intercepts, and less a difference in slopes over time. Up through the fifth or sixth year, the year-to-year estimated changes are nearly identical.

The two strategies also yield similar estimates when the performance measure is a teacher's value-added contribution to student test scores, as shown in Figure 2. The value-added returns-to-experience estimates are much noisier, given the much smaller samples, but we cannot reject the null hypothesis of no difference between the two strategies.[12]

3. Alternative explanations and threats to causal inference

Observation ratings may improve (decline) over time for reasons unrelated to a teacher's gains from experience. In this section we describe several alternative explanations for changing ratings, and whether an alternative explanation threatens a causal "returns to experience" interpretation of Figure 1. We focus specifically

[11] Our DCPS data begin in 2009-10 and thus the early years coincide with the slow labor recovery following the recession. We do not see the same pattern in Tennessee where the experience distribution has been stable over the years we study.

[12] Appendix B provides details of our value-added estimation methods.

on interpreting changes in observation ratings as the causal effect of experience on performance of the tasks which the rubric is designed to measure.

3.1 General evidence

Before taking up specific alternative explanations, we begin with some general evidence relevant to the plausibility of identifying Assumptions 1 and 2. First, Figure 3 reports a partial test of Assumption 1. For this test assume, first, that the ν_k component of scores (Equation 2) does not depend on experience (Assumption 1); and, second, that the production process which turns teaching input tasks, θ_k, into a teacher's value added contributions to student achievement also does not depend on experience. If both assumptions hold, we would expect that the relationship between observation ratings, s_k, and teacher value added should be unrelated to experience. We can test this latter relationship.

Figure 3 shows the relationship between observation ratings and test-score value added, and how that relationship changes with teacher experience. The x-axis is years of prior experience. The y-axis is the predicted increase in value added if we increase the teacher's observation score by one standard deviation.[13] The solid line uses only within-teacher over-time variation (by including teacher FE in the estimation), and the dashed line uses both within- and between-teacher variation (by omitting teacher FE). To get a sense of the *correlation* between observation scores and value added, multiply the y-axis by about five for Tennessee and three for DCPS.

The relationship between observation scores and value added is (largely) unrelated to experience in Figure 3. With perhaps one exception, there is no clear trend related to experience. And we cannot reject the null hypothesis that each point estimate is equal to the average of the series it belongs to, though the DCPS estimates are quite noisy. The exception is the earliest years in Tennessee using

[13] The estimation details for Figure 3 are summarized in its note and described in Appendix B.

only within-teacher variation (solid line series). Those estimates suggest the correlation declines from the first year to the fourth, but then remains stable afterward. Some of the specific threats described below could be a mechanism behind the declining correlation. That decline in correlation could be evidence that the ν_k component of scores does depend on experience, but it is not necessarily evidence against Assumption 1. Even if the general production process which turns teaching tasks, θ_k, into value added output does not depend on experience, the way in which a teacher chooses to optimize that production process may depend on experience. For example, perhaps as early-career teachers gain experience they shift more effort to tasks which are not measured by the observation rubric, or more subtly shift effort across tasks in a way not well captured by the simple average of ratings, $\bar{s}_{jt}$.

Additionally, Figure 3 is only a partial test. We have only one outcome: teachers' value-added contributions to student test scores. Teachers contribute to other important student outcomes, like social and behavioral skills (Jackson 2018), and classroom practices are likely important to those outcomes as well. Related, for DCPS teachers we have student surveys which may capture a different set of inputs to test and non-test student outcomes. Appendix Figure A5 repeats the test in Figure 3 with the surveys as outcomes, and we find steady, albeit noisy, correlations between classroom observation scores and the student survey scores.

We can also partially test identifying Assumption 2. That assumption requires that, on average, true performance, θ_k, is not changing over time among the comparison group of veteran teachers, i.e., $E\left[\theta_{kjt} - \theta_{kj(t-1)} | expr_{jt} \geq \bar{e}\right] = 0$. Our main estimates in Figure 1 set $\bar{e} = 9$ to define the veteran group. If Assumption 2 holds, then our estimates for returns at $e =$ 0-8 should be robust to setting $\bar{e}$ above 9.

Our returns to experience estimates are quite robust to changes in $\bar{e}$. The solid line in Figure 4 simply repeats the solid line in Figure 1 for convenient comparison, with $\bar{e} = 9$. The two dashed lines show estimates where $\bar{e} = 14$ and $\bar{e} = 19$. The three lines have different intercepts; the intercept in this case is the average performance among veteran teachers with $expr_{jt} \geq \bar{e}$. Still, the slopes of the lines are quite similar, over the range of 0-8 years of prior experience. Compare across estimates, for example, the change in performance between 0 and 1. Those changes in performance are the returns to experience we want to estimate, and those estimated changes are robust. However, the choice of $\bar{e}$ does matter for inferences about the level of performance for novices and early-career teachers.

Additionally, while we cannot observe $\Delta\theta = E\left[\theta_{kjt} - \theta_{kj(t-1)} | expr_{jt} \geq \bar{e}\right]$ directly, we can observe $\Delta\bar{s} = E\left[\bar{s}_{kjt} - \bar{s}_{kj(t-1)} | expr_{jt} \geq \bar{e}\right]$. Among veteran teachers, the mean first-difference in observation scores is 0.004 standard deviations (st.err. 0.002) in Tennessee and -0.073 standard deviations (st.err. 0.006) in DCPS.[14] Under what conditions would $\Delta\bar{s} \cong 0$ but $\Delta\theta \neq 0$? Only in the knife-edge case where any change in true performance, θ_k, is just offset by a change in the ν_k component of scores.

3.2 The evaluation system

Changes in observation ratings over time may be caused by changes to the evaluation system's tools and procedures. Key features of an evaluation system include the scoring rubric, the training provided to raters, and the rules for assigning

[14] In DCPS, compositional differences in the teaching force over time (Dee and Wyckoff 2015, Dee et al. 2021, James and Wyckoff 2020) could make it appear, with our preferred within-year standardization process, as if experienced teachers were declining over time as the average performance of incoming teachers improves. However, relying on alternative standardization approaches, including standardizing relative to veteran teachers within year and standardizing scores across years, do not change the slopes shown in Figure 1. Differences in point estimates across standardization approaches never exceed 0.037, with an average difference in point estimates across approaches and levels of experiences of 0.005. In rubric units, the average first difference for veteran teachers is also quite small, at -0.014 (st.err. 0.003).

teachers to raters.[15] Even if a teacher's performance, θ_k, remains constant, the rating assigned to that performance, s_k, may go up or down if the system's processes change. In other words, the evaluation system's tools and procedures are key features of ν_k in Equation 2 where $s_k = \theta_k + \nu_k$. (The incentives or consequences attached to performance measures are also a key feature of an evaluation system, and we discuss those incentives below.)

The most straightforward example of a change in ν_k is a change in the scoring rubric. In 2017 DCPS switched from the Teaching and Learning Framework (TLF) rubric to an entirely new Essential Practices (EP) rubric. The new EP rubric did not measure exactly the same set of tasks, k, as the old TLF rubric. Changes in other settings might be smaller, like word choices, even if the tasks scored remain the same. Still, large or small rubric changes would not necessarily threaten our identifying assumptions, as long as the rubric changes affect early-career (treatment) and veteran (comparison) teachers equally.

The DCPS changes allow us to compare estimates from different rubrics. In Figure 5 the short dash line shows estimates of returns to experience using only ratings generated by the TLF, while the long dash blue line uses only EP ratings. Both dashed lines are limited to scores from school administrators. For both rubrics the average first-year teacher's rating is much lower than the average veteran's rating, but that starting gap is smaller with the EP rubric. In both cases teachers make larger improvements over the first five years compared to the next five, but the improvements are somewhat steeper as judged by the TLF rubric. The differences suggest a potential threat to Assumption 1—that ν_k does not depend on experience—at the time of the change in rubrics in DCPS. However, the difference between the dashed (TLF) and long-dashed (EP) estimates could be a compositional

[15] Our language and examples in this discussion mainly imply the evaluation systems designed or used by schools, districts, or states. The features and reasoning also apply to scores collected by researchers or for other purposes.

change. Starting in 2011, and thus concurrent with our data, DCPS became more selective in both hiring and retention decisions, with selection strategies based explicitly on performance measure (Dee and Wyckoff 2015, Jacob et al. 2018). There were noticeably fewer early-career teachers by 2017 (Appendix Figure A3). Thus, in Figure 5, the higher scores with the EP rubric may reflect true higher performance because of selection.

Choosing raters is also a key evaluation design decision, and a decision which itself may change over time. Figure 5 also compares estimates by rater type for DCPS. The solid red line uses only ratings from the master educator raters, who specialize in rating and are external to the school, while the dashed red line uses only ratings from school administrators. Both lines are limited to scores generated by the TLF rubric. The slopes of the two TLF lines are quite similar, especially over the first five years of a teacher's career. Additionally, in this comparison there is no composition change concern since each teacher was rated by both a school administrator and master educator each school year. Figure 5 does obscure one important difference between master educator scores and school administrator scores. School administrators give higher average scores on the 1-4 scale; in other words, the ν_k component in Equation 2 does depend on rater type. However, the difference in scores between the rater types is the same for all teachers regardless of experience; thus, the rater type difference in ν_k does not violate Assumption 1.

In general, changes to the evaluation system are changes to the ν_k component in Equation 2. Interpreting Figure 1 as the causal returns to experience does not require that ν_k remain unchanged over time. The only restriction on ν_k is that ν_k not depend on experience. This applies to obvious changes in ν_k, like the rubric or types of evaluators, and to changes which are more difficult (for the researcher) to observe. One potentially difficult to observe change is changes to the training of raters. Imagine that system administrators determine, at a given point in time, that raters need to be re-trained on some aspect of scoring. That re-training might be in fact be motivated by administrators' belief that scores, s_k, are not reflecting performance, θ_k, as they should. A second example is a change to the rules for assigning teachers to raters. Chi (2020), among others, has documented teacher-rater match effects on observation scores; for example, when a teacher and rater share a gender or race, the teacher's scores are higher. Imagine the evaluation system administrators decide, at some point, to make gender or race an explicit factor in the rules for making assignments.

3.3 Behavior of the raters

Changes in ratings over time may reflect changes in the behavior of the raters. Raters have some discretion within any evaluation system's designed procedures. Rubric-based classroom observation ratings fall somewhere in between the theoretical poles of truly objective evaluation and purely subjective evaluation. Moreover, raters may also take actions which violate the designed procedures they were trained to follow. The behavior of raters, whether intended or unintended in the system design, is part of the ν_k component in Equation 2.

One behavior that is frequently cited, given rater discretion, is leniency bias—the tendency for raters to give scores which are higher than warranted. Histograms of observation ratings (Appendix Figure A1) are consistent with systematic leniency bias in both Tennessee and DCPS, although such bias is less evident for ratings assigned by the master educators in DCPS. The skew in the ratings distribution could also accurately reflect teacher performance using a rubric with ceiling effects. Leniency bias is often cited as a concern in classroom observation scores by both researchers and in public debate (Kraft and Gilmour 2017, New York Times 2013), but leniency bias is common in many occupations beyond teaching (Prendergast 1999).

However, leniency bias does not necessarily threaten our interpretation of Figure 1 as the causal returns to experience. To violate Assumption 1—ν_k does not depend on experience—rater leniency would need to be correlated with teacher experience. For example, imagine that raters are less lenient with a first-year teacher compared to their rating of the same teacher in her second year; then Figure 1 would over-state the returns to the first year of teaching. Such a change in leniency might be a mechanism behind the early-years decline in Tennessee in Figure 3. However, if it is not correlated with experience, leniency bias will be differenced out in the same way as rubric changes or other evaluation system features.

Another potential mechanism is that raters may use information learned outside an official observation visit. Consider the case of a teacher rated by her school principal. A few brief classroom observations are a small fraction of the interactions a teacher and principal will have in a school year; the principal likely learns much about the teacher's performance outside of official observations. Ho and Kane (2013) show evidence that a teacher's own principal scores a video of her classroom differently than a principal from another school in the district scores the same video, perhaps because the teacher's own principal begins the scoring with a prior on the teacher's performance. Additionally, because the rubric covers only some teaching tasks, k, a principal may raise (lower) observation scores to reflect the principal's beliefs about the teacher's performance of tasks not covered by the rubric. A principal using outside information is a potentially rational behavior if the observation ratings are used for personnel decisions and the principal cares much less about observation scores than she cares about student outcomes and teacher value-added to those outcomes.

This outside information explanation may threaten Assumption 1—ν_k does not depend on experience—but only if raters both have and use different outside information depending on a teacher's years of experience. The number of years a teacher-principal pair has worked together likely will be correlated with the teacher's years of experience, but it does not need to be strongly correlated if school principals switch schools frequently. A high correlation would suggest principal raters might have different outside information on early-career and veteran teachers. Empirically the correlation between years-worked-together and experience is 0.17 in the DCPS data and 0.15 in the Tennessee data.

One test relevant to this outside-information question is the event study of ratings in Figure 6. Event time is relative to a change in the school principal, with year zero the new principal's first year, and we allow the time series to differ for early-career and veteran teachers as shown by the two plotted lines. If principals learn about a teacher's performance outside of formal classroom observations, we might expect observation scores to rise or fall. However, scores do not change on average as a principal and teacher work together longer. This pattern holds for both early-career and veteran teachers. In Tennessee there is some evidence that principals give slightly lower scores in their first year in a new school.[16]

3.4 Incentives and distortion of effort

Changes in ratings may reflect changes in the incentives attached to those ratings. Those incentives might be explicitly linked to observation ratings, like monetary bonuses or the threat of dismissal, or less-explicit career concerns incentives. Still, a change in incentives alone does not threaten inferences about true performance, θ_k, for tasks covered by the rubric. A new or stronger incentive attached to task k's score, s_k, can induce a teacher to raise her performance of that task, θ_k, through more effort for task k or investing in skills for task k. Thus, inferences about true performance, θ_k, of tasks covered by the rubric are not necessarily threatened by a change in incentives attached to ratings, s_k.

[16] On additional note on rater behavior. As described in Section 2.1, the item level observation scores for specific tasks s_k are strongly correlated, in these data and most teacher observation data. This fact is sometimes interpreted as evidence that raters do not actually differentiate between tasks, k, but instead score teachers on some single general dimension of teaching performance. This seems unlikely given that the item level correlations are not equal to one. A more plausible explanation is that the rubrics define tasks where true performance is in fact strongly correlated. Whatever the explanation, this issue is not central to our analysis in this paper which focuses on the average score. This issue does limit our ability to make conclusions about how experience may affect tasks differentially.

However, an increase in effort for tasks covered by the rubric, k, may come at the expense of teacher performance in other tasks not covered, $-k$. This asymmetry between scored tasks and un-unscored tasks suggests scope for the well-known multitask distortion problem (Holmstrom and Milgrom 1991). Given that potential distortion, a change in incentives attached to rubric ratings can threaten inferences about teacher performance beyond the scope of what is covered by the rubric. Recall that the rubric tasks are inputs to the broader education production responsibilities of teachers, including improving student math achievement, social skills, earnings as an adult, etc.

Still, using ratings and incentives to shift teacher effort away from some tasks and toward other tasks will not necessarily lead to distortion. There is (quasi-)experimental evidence that rubric-based classroom observations can improve teachers' contributions to student test scores, even when teachers are not evaluated based on those test scores (Taylor and Tyler 2012, Burgess, Rawal, and Taylor 2021, Briole and Maurin in-press). In DCPS specifically, teacher performance improves more when the teacher spends more of the year anticipating an unannounced rater visit (Phipps 2018, Phipps and Wiseman 2021).

While incentives do not necessarily threaten our causal interpretation of Figure 1 as the returns to experience, changes in incentives may be a mechanism behind the improvements seen in Figure 1. The simplest example is tenure rules. In Tennessee, teachers can earn tenure after five years, but tenure requires sufficiently high observation ratings in years four and five.[17] Thus, teachers have somewhat more incentive to focus effort on the rubric-measured tasks in years four and five compared to years one, two, and three, which might contribute to the pattern in Figure 1. Still, it seems unlikely a teacher concerned about tenure would wait until

[17] More precisely, tenure requires being rated "4. Effective" or "5. Highly Effective" on the 1-5 integer scale. While only one input to that overall final rating, classroom observation scores get a weight of 50-85 percent for the teachers.

year four to pay attention to the rubric, and the slope from years three to four in Figure 1 is not obviously a departure from the trend suggested by the other year-to-year slopes.

Unlike Tennessee, the evaluation incentives in DCPS were not explicitly a function of years of experience but could have been correlated with experience. DCPS teachers are dismissed if rated "Minimally Effective" (the second-lowest rating) in two consecutive years or if they fail to exceed a "Developing" rating (the third-lowest rating) within three consecutive years. Before fall 2012, teachers could receive permanent salary increases after two consecutive years of being rated "Highly Effective" (the top rating). Figure 7 shows the proportion of teachers in each rating category by years of experience, suggesting the incentives are not strongly correlated with experience.[18]

3.5 Manipulation of ratings

Observation ratings may reflect changes in teachers' actions unrelated to their job performance. Teachers, like professionals in any other occupation, may adopt behaviors or actions which do raise their ratings, s_k, but do not raise their true job performance, θ_k. In the literature on job performance evaluation these actions are known as manipulation.[19] This manipulation of observation ratings might occur, for example, because classroom observations are infrequent and brief; thus, a teacher could prepare a special lesson or even rehearse the lesson with his students in advance of the rater's visit. By contrast, if the evaluation process or incentives prompted a teacher to improve her lessons on all (many of) the days the

[18] Also studying DCPS, Adnot (2016) reports evidence that teachers facing the two-consecutive-years-minimally-effective dismissal threat shift effort across tasks within the rubric toward tasks which are more likely raise their scores. This is a sort of distortion within measured tasks but suggests that teachers are aware of this margin.

[19] Empirical examples of manipulation by teachers include cheating on student tests (Jacob and Levitt 2003) and intentionally excluding low-scoring students from high-stakes tests (Jacob 2005, Cullen and Reback 2006, Figlio 2006, Figlio and Getzler 2006).

rater would not be present, that would be an improvement in performance and not manipulation.

Manipulation plausibly threatens our casual returns-to-experience interpretation of Figure 1. In our framework, teacher manipulation results from the evaluation system's procedures and incentives, and is part of the ν_k component in Equation 2. A teacher's awareness of how to manipulate likely grows as he gains experience with the evaluation system. That suggests a plausible correlation between potential for manipulation and general teaching experience, which threatens Assumption 1 that ν_k is invariant to experience. However, that correlation might be weakened if more-experienced teachers share their manipulation strategies with newly-hired teachers. If the manipulation component of observation scores is unrelated to general experience, then manipulation will be differenced out in Figure 1.

The decline in correlation over years 1-4 in Tennessee in Figure 3 may be explained by increasing manipulation over the first few years of a teacher's career. However, we cannot rule out other mechanisms, such as, for example, raters becoming more lenient as a teacher moves from first to second to third year. And there are other limitations to the test in Figure 3, as discussed above. On the other hand, while underpowered, the evidence from DCPS in Figure 3 does not indicate a decline in the relationship between classroom observation scores and student achievement over experience. In addition, the relatively stable correlation between classroom observation ratings and student survey scores across levels of teaching experience in DCPS (Appendix Figure A5) provide evidence against the presence of manipulation, unless teachers were similarly able to manipulate scores on both measures across levels of experience.

Dee and Wyckoff (2015) examine whether DCPS school administers manipulate observation scores, s_k, in the face of increased incentives. Consider the teachers who received their first Minimally Effective rating in 2010-11, and thus were under a significant threat of dismissal during 2011-12. Observation ratings did improve in 2011-12 for these teachers, on average. However, master educators also scored these teachers as having improved, and the increase in observation scores was similar across both types of raters. Additionally, these teachers under dismissal threat also improved on their test-score value added. Taken together, these results suggest that the dismissal threat did not improve observation ratings through manipulation alone.

3.6 Changes in job assignments

Changes in a teacher's ratings may reflect changes in her job assignment. A teacher's observation ratings, s_k, might decline (improve) after a job change for either of two reasons: First, the teacher's actual performance, θ_k, could decline (improve) because of the job change. Using teacher value-added to student test score, Ost (2014) provides evidence that teaching skills and experience are not fully transferable across grade levels. Switching from 3rd to 5th grade, for example, likely requires some adjusting of questioning techniques, or shifting effort to new lesson plans at the expense of in-class performance.

Let a and a' be two different job assignments; θ_{kjta} is the actual performance of teacher j in task k during school year t with job assignment a. We can write:

$$E\left[\theta_{kjt} - \theta_{kj(t-1)}\right] = \underbrace{E\left[\theta_{kjta} - \theta_{kj(t-1)a}\right]}_{\Delta^t} + p\,\underbrace{E\left[\theta_{kj(t-1)a} - \theta_{kj(t-1)a'}\right]}_{\Delta^a} \qquad (5)$$

where p is the probability of switching from job a' to a.

The intuitive notion of "returns to experience" implies that the job is constant and experience increases, which matches Δ^t in Expression 5. If identifying Assumption 2 holds—no returns to additional experience for veterans—then Figure 1 reports estimates of $(\Delta^t + p\Delta^a)$. Assuming further that job changes reduce performance, $\Delta^a < 0$, then Figure 1 underestimates the intuitive Δ^t. Alternatively, some researchers or policymakers may be interested $(\Delta^t + p\Delta^a)$, which we could describe as the "returns to experience including job changes typical of early-career teachers."

Job changes do threaten identifying Assumption 2, which requires that $E\left[\theta_{kjt} - \theta_{kj(t-1)} | expr \geq \bar{e}\right] = 0$ in our comparison group of veteran teachers. A veteran's performance might change because of a job change, $\Delta^a \neq 0$, even if her performance would not have otherwise changed, $\Delta^t = 0$. If job changes do reduce veteran (comparison) teacher performance, $\Delta^a < 0$, then the estimates in Figure 1 overstate the intuitive Δ^t for novices. This bias is positive, and the bias described in the prior paragraph is negative, but the two would only cancel each other out under the assumption that p and Δ^a do not depend on experience.[20]

The second reason scores might change is that the ν_k component in Equation 2 might differ across jobs. For example, typically the same rubric is used for all teachers, leaving any adaptation to grade-level or subject circumstances up to the rater or training process. More subtly, ν_k might depend on the students in the classroom (Campbell and Ronfeldt 2018). Students are themselves an important feature of a teacher's job assignment, and a feature which can change even if grade level or subject do not. The threat to identification parallels other features of ν_k discussed above. As long as job-specific differences in ν_k are unrelated to experience, this second reason is not a serious threat to identification. A job-specific difference might be, for example, if raters are more lenient with novices after a job change than they are with veterans.

In Figure 8 we test the robustness of Figure 1 to changes in the students a teacher is assigned. Using data from Tennessee and DCPS, we plot returns-to-experience estimates with and without controls for students prior-year test scores.[21]

[20] This assumption is sufficient but not strictly necessary. We only require that the product $p\Delta^a$ not depend on experience, which should be a weaker assumption.

[21] The estimation for Figure 8 is identical to our preferred strategy used in Figure 1 with two exceptions. First, we limit the sample to teacher-by-year, jt, observations where we have prior-year test scores for students assigned to the teacher, grades 4-8 math and language classes. Second, for

Accounting for changes in students assigned does not affect our estimates. The similarity of all the estimates in Figures 1 and 8 is partly because they all use only within-teacher variation. The ν_k component might well depend on the students in the classroom (Campbell and Ronfeldt 2018), but most of the variation in students assigned is between teachers or schools, not within teachers over time.

3.7 Performance improvements among veteran teachers

The true performance of veteran (comparison group) teachers may change over time—violating Assumption 2—even if there are no returns to experience for veterans. For example, veterans may increase their effort in response to incentives. How would interpretation change if Assumption 2 was violated in this way, but Assumption 1 held? If the veteran gains were only among veterans, then the estimates in Figure 1 would likely understate the true returns to experience for early-career teachers. The veterans' improvements would be subtracted off any improvements for early-career teachers.[22]

3.8 Turnover

One final consideration in interpreting Figure 1 is turnover or attrition from our estimation sample. The estimates in Figure 1 use only within-teacher variation in observation scores. This feature addresses a first-order potential bias: average observation ratings might rise with experience, even if each individual teacher's scores remain constant, if lower-rated teachers are more likely to leave teaching (or at least leave the district or state).

Still, even using only within-teacher variation, Figure 1 is still partly determined by turnover. In Figure 1 the slope between year one and year two is an

the dashed line, the outcome variable is the residual from a regression of observation score, $\bar{s}_{jt}$, on the average prior-year test score for students assigned to the teacher.

[22] This subtraction might be desirable in specific cases. Imagine, for example, that veterans improved because of some new training, and that training was given to all teachers, early-career and veteran. If, roughly, the effect of the training was similar for all teachers, then the subtraction makes the Figure 1 estimates returns to experience controlling for any general training effects.

average of $N_{1,2}$ different individual teacher slopes, where $N_{1,2}$ is the sample of individuals who are observed in year one and year two (and perhaps future years). Similarly, the slope between year four and year five uses only the $N_{4,5}$ sample. However, these are not the same samples: $N_{4,5} \neq N_{1,2}$. First, for any given cohort of novice hires, attrition from the profession over time will make $N_{4,5} \subset N_{1,2}$. Second, experienced teachers who transfer into the system from elsewhere may contribute to $N_{4,5}$ even if they do not contribute to $N_{1,2}$. The slope from year one to year two in Figure 1 might be different if we could estimate it with the $N_{4,5}$ sample.

Empirically, however, our Figure 1 estimates are not strongly influenced by this second-order composition concern. Figure 9 shows our returns-to-experience estimates using subsamples defined by when the teacher leaves teaching in Tennessee or DCPS. The changes from year one to two, two to three, etc. are quite similar across samples. The exception is that the trajectory appears to change in a teacher's final year before leaving teaching in Tennessee or DCPS.

4. Conclusion

We conclude that the typical estimates of returns to experience, applied to observation ratings, can reasonably be interpreted as the causal effect of additional experience on teachers' job performance—specifically, performance of the input tasks covered by the rubric. The estimates are difference-in-differences estimates, where veteran teachers are the comparison group. Veterans provide a plausible counterfactual estimate for several often-stated threats, including for example, leniency bias from raters, manipulation by teachers, changes in the evaluation system, and changes in teachers' job assignments. Our estimates are robust to changes in the rubric, different rater types, and controlling for student baseline achievement, among other things. Still, there are reasons to remain cautious about a causal interpretation. We find, in one setting, a weakening correlation between teacher observation scores and student test scores as teacher experience grows. That weakening is consistent with some threats to the identifying assumptions, but it would also be consistent with changes in optimal teaching strategies as experience increases.

Our analyses should be interpreted carefully. We focus on the performance of the input tasks covered by classroom observation rubrics. Stronger assumptions are required for using observation ratings to make inferences about teacher performance measured by contributions to student outcomes. Taking differences in scores over time addresses several concerns which are left unaddressed in results based on score levels at a single point in time. Additionally, estimates in Tennessee and DCPS may differ from other settings employing teacher observations. While the identification strategy, using differences in scores over time and between early career and veteran teachers, employed in this paper applies generally, the implementation of observations in other settings may open those systems to violations of assumptions explained and explored here.

Finally, the estimates in this paper—the improvements in performance caused by teaching experience—provide a foundation for examining the mechanisms of early-career teacher development. Understanding those mechanisms may suggest interventions to expedite that development. For example, does early-career development depend on formal training, either in teacher certification programs or professional development for new teachers? Do teachers improve differentially across the various tasks of teaching, like managing student behavior, planning, or instruction? Answering these questions may provide useful insights to school managers and policymakers.

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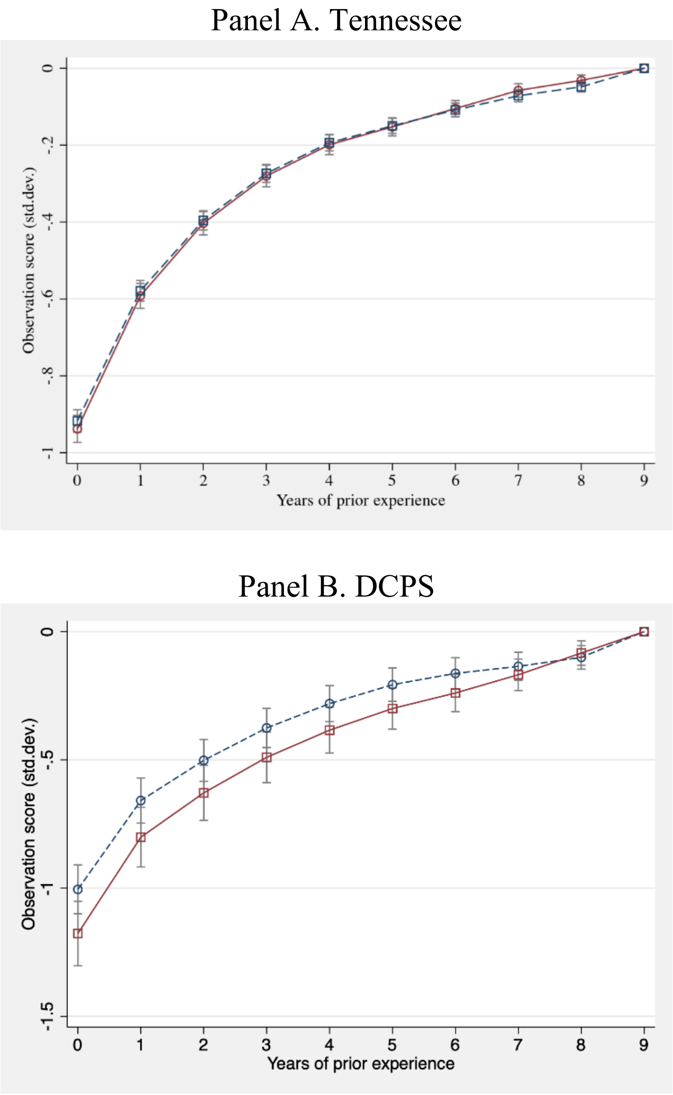


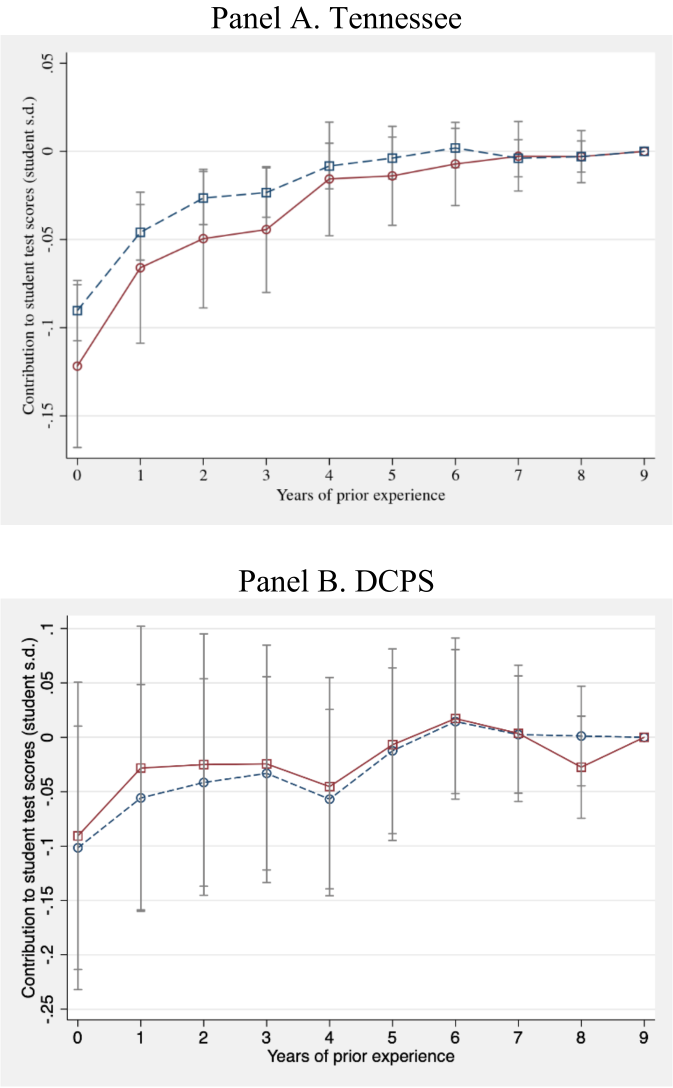
Panel A. Tennessee

Panel B. DCPS

Figure 1—Returns to experience measured in classroom observation ratings

Note: The solid line reports estimates using our preferred diff-in-diff strategy described in Section 2.1. The dashed line reports estimates using the conventional two-way fixed effects approach described in Section 2.4. The vertical lines mark the 95 percent confidence intervals which are corrected for clustering (teacher). In both cases the outcome variable is teacher j's classroom observation score, $\bar{s}_{jt}$, which is an average of several item-level ratings recorded during a given school year t. Observation scores are standardized (mean 0, st.dev. 1) by school year using the distribution of all teachers in the jurisdiction, Tennessee or DCPS respectively. The solid line estimates are the difference between two means: (a) The average first-difference, $(\bar{s}_{jt} - \bar{s}_{j,t-1})$, among "treated" teachers—those with e years of prior experience (x-axis) in school year t, and $e - 1$ years in school year $t - 1$. (b) The average first-difference, $(\bar{s}_{jt} - \bar{s}_{j,t-1})$, among "comparison" teachers—those with ≥ 9 years of prior experience in both year t and $t - 1$. The (a) minus (b) second-difference is calculated separately for each unique combination of e and t in the data. Then the plotted points are the weighted average across t for a given e, where the weights are the number of "treated" teachers. For the dashed line estimates we fit a single two-way fixed effects regression, with teacher j and school year t fixed effects. The specification includes indicators for years of prior experience 0 through 8 individually, with ≥ 9 years the omitted category, but no other controls. The plotted points are the coefficients on the experience indicators. The sample size for the dashed line in Tennessee is 375,072 teacher-by-year observations for 81,847 unique teachers;

and similarly 349,920 and 66,156 for solid line Tennessee, 33,484 and 7,268 for dashed line DCPS, and 33,040 and 7,201 for solid line DCPS.

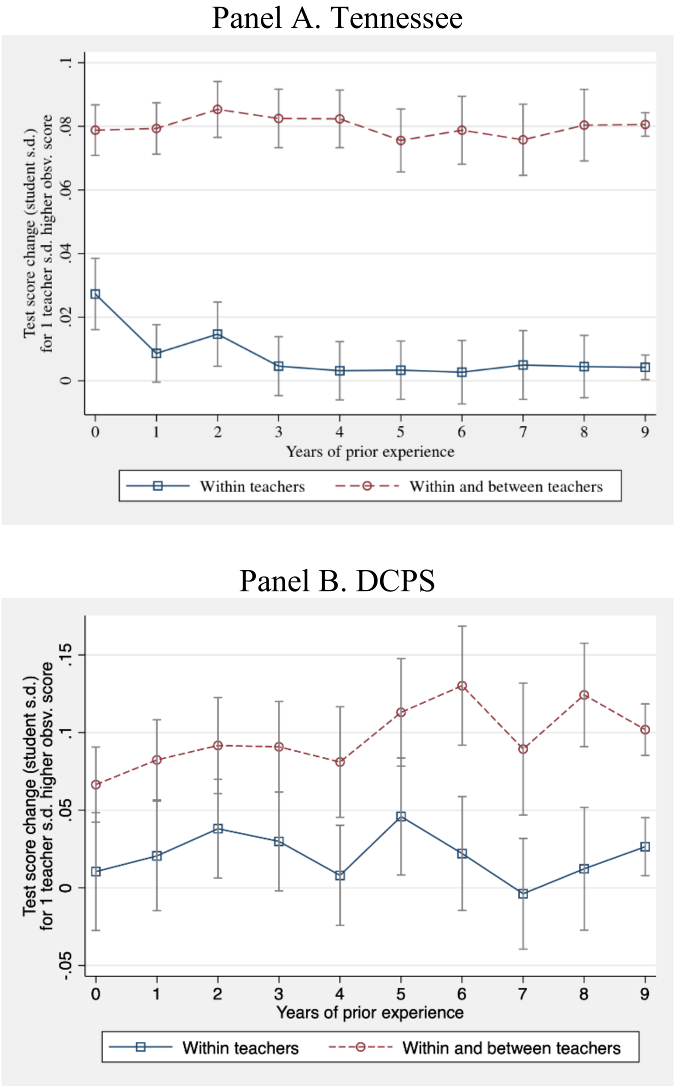


Panel A. Tennessee

Panel B. DCPS

Figure 2—Returns to experience measured in value-added contributions to student achievement

Note: The solid line reports estimates using our preferred diff-in-diff strategy described in Section 2.1. The solid line reports estimates using the conventional two-way fixed effects approach described in Section 2.4. The vertical lines mark the 95 percent confidence intervals which are corrected for clustering (teacher). In both cases the outcome variable is student i's test score, A_{ijst}, in subject s and school year t. Test scores are standardized (mean 0, s.d. 1) within each grade-by-subject-by-year cell using the distribution for all students in the jurisdiction, Tennessee or DCPS respectively. For the dashed line estimates we fit a single two-way fixed effects regression, with teacher j and school year t fixed effects. The specification includes indicators for years of prior experience 0 through 8 individually, with ≥ 9 years the omitted category. Additional controls are a quadratic in prior-year test score, where the parameters are allowed to differ across grade-by-subject-by-year cells, $b(A_{is(t-1)})$. The plotted points are the coefficients on the experience indicators. For the solid line estimates, we begin by estimating teacher contributions to student test scores, $\hat{\mu}_{jt}$. We fit a regression of student scores A_{ijst} on the same prior score controls, $b(A_{is(t-1)})$, and teacher fixed effects; and then obtain the residuals $A_{ijst} - \hat{b}(A_{is(t-1)})$. Our estimate $\hat{\mu}_{jt}$ is the average residual for teacher j in year t. The dashed line estimates are the difference between two means: (a) The average first-difference, $(\hat{\mu}_{jt} - \hat{\mu}_{j,t-1})$, among "treated" teachers—those with e years of prior experience (x-axis) in school year t, and $e - 1$ years in school year $t - 1$. (b) The average first-difference, $(\hat{\mu}_{jt} - \hat{\mu}_{j,t-1})$, among "comparison" teachers—those with ≥ 9 years of prior experience in both year t and $t - 1$. The (a) minus (b) second-difference is calculated separately for each unique combination of e and t in the data. Then the plotted points are the weighted average across t for a given e, where the weights are the number of "treated" teachers. The sample size for the dashed line in Tennessee is 4,222,939 student-by-subject-by-year observations and 92,403 teacher-by-year observations for 34,395 unique teachers; and similarly 71,474 and 20,954 for solid line Tennessee, 247,005, 5,413 and 2,268 for dashed line DCPS, and 4,249 and 1,280 for solid line DCPS.

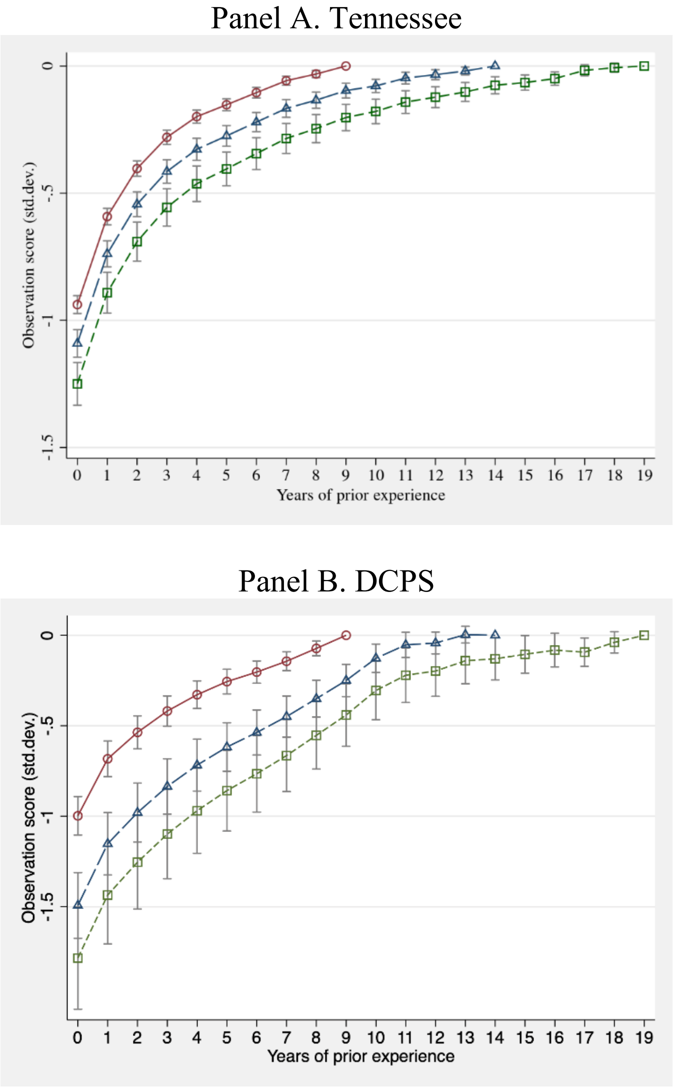


Panel A. Tennessee

Panel B. DCPS

Figure 3—Predicting student test scores with teacher observation scores by years of teacher experience

Note: The solid and dashed lines each report estimates from a separate linear regression. The vertical lines mark the 95 percent confidence intervals which are corrected for clustering (teacher). In both cases the outcome variable is student i's test score, A_{ijst}, in subject s (maths or English language arts pooled) and school year t. Test scores are standardized (mean 0, s.d. 1) within each grade-by-subject-by-year cell using the distribution for all students in the jurisdiction, Tennessee or DCPS respectively. In both cases the specification includes (a) indicators for years of prior experience 0 through 8 individually, with ≥ 9 years the omitted category; (b) classroom observation score, $\bar{s}_{jt}$; and (c) the interactions of (a) and (b). Each plotted point is sum of the coefficient on the (a)*(b) interaction for e years of prior experience (x-axis) plus the main-effect coefficient on (b). Additional controls are a quadratic in prior-year test score, where the parameters are allowed to differ across grade-by-subject-by-year cells, $b\left(A_{is(t-1)}\right)$. The solid line specification includes year and teacher fixed effects. The dashed line includes only year fixed effects, omitting the teacher fixed effects. The sample size the same for the two lines; in Tennessee 4,222,939 student-by-subject-by-year observations and 92,403 teacher-by-year observations for 34,395 unique teachers, and similarly in DCPS 252,400, 5,429, and 2,274.


Panel A. Tennessee

Panel B. DCPS

Figure 4—Estimates by definition of comparison group

Note: Each of the three lines reports estimates using our preferred diff-in-diff strategy described in Section 2.1. The vertical lines mark the 95 percent confidence intervals which are corrected for clustering (teacher). The solid line is identical to the solid line in Figure 1. For the two dashed lines, the details of estimation are identical to the solid with one exception. For the solid line, the comparison group is teachers with ≥ 9 years of experience, $\bar{e} = 9$. The two dashed lines show $\bar{e} = 14$ and $\bar{e} = 19$ respectively. The sample size the same for all three lines; in Tennessee 375,072 teacher-by-year observations for 81,847 unique teachers, and similarly in DCPS 33,484 and 7,267.

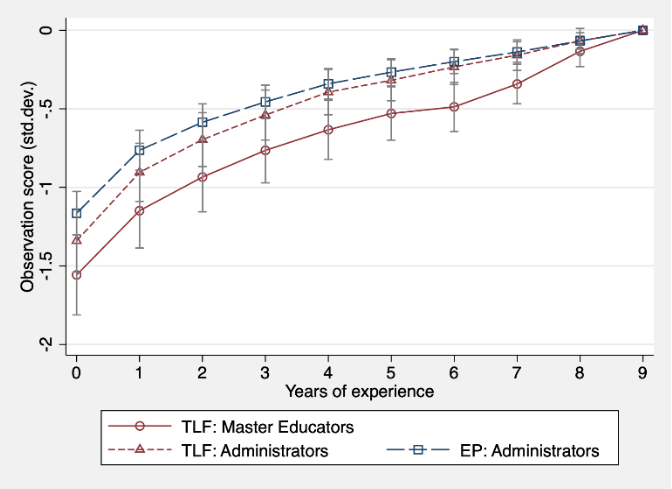
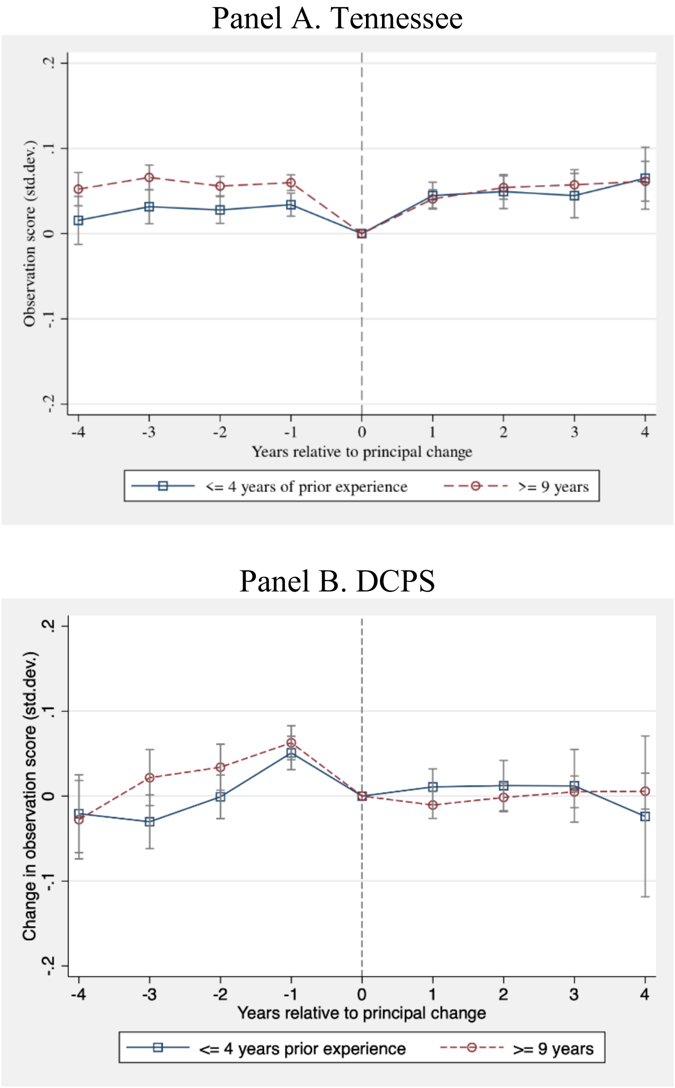


Figure 5—Estimates using different rubrics and rater types (DCPS)

Note: Each of the three lines reports estimates using our preferred diff-in-diff strategy described in Section 2.1. The vertical lines mark the 95 percent confidence intervals which are corrected for clustering (teacher). The details of estimation are identical to the solid line in Figure 1 with the following exceptions. First, the estimation sample is limited by the type of rater: external "Master Educators" for the solid line, and school administrators for the dashed and long dashed lines. Second, the estimation sample is limited by the rubric used: TLF from 2010-2016 and EP from 2017-2019. The sample size for the solid line is 18,715 teacher-by-year observations for 5,118 unique teachers; and similarly 21,080 and 5,380 for dashed line, and 10,190 and 3,726 for the long dash line.



Panel A. Tennessee

Panel B. DCPS

Figure 6—Event study of a change in school principal

Note: All estimates are from a single linear regression. The vertical lines mark the 95 percent confidence intervals which are corrected for clustering (teacher). The dependent variable is teacher j's classroom observation score, $\bar{s}_{jt}$, which is an average of several item-level scores recorded during a given school year t. Observation scores are standardized (mean 0, st.dev. 1) by school year using the distribution of all teachers in the jurisdiction, Tennessee or DCPS respectively. The specification includes (a) indicators for year relative to a change in school principal; (b) an indicator $= 1$ if teacher j has ≤ 4 years of prior experience, and $= 0$ if teacher j has ≥ 9 years; and the interaction of (a) and (b). The new principal's first year, x-axis $= 0$, is omitted for both groups defined by (b). The specification also includes indicators for years of prior experience, with ≥ 9 years omitted, plus teacher and year fixed effects. If a teacher experiences two (or more) principal changes, we stack the data to include each teacher-by-event-study case in the data. DCPS observation scores in Panel B represent administrator-assigned scores only, but can include multiple administrators (i.e., principals and assistant principals) within a given teacher-year. The sample size for the solid line in Tennessee is 72,850 teacher-by-year observations for 29,193 unique teachers; and similarly 136,443 and 32,244 for dashed line Tennessee, 6,927 and 2,511 for solid line DCPS, and 9,597 and 2,406 for dashed line DCPS.

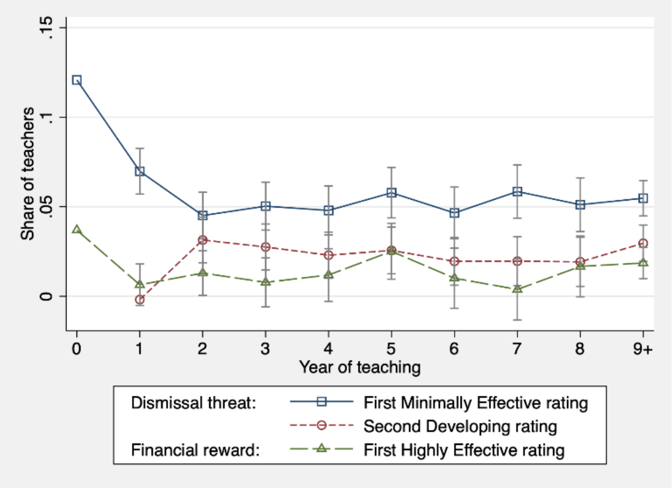


Figure 7—Incidence of consequential performance ratings (DCPS)

Note: Each plotted series reports the percentage of teachers scoring at the relevant consequential rating level. In DCPS, teachers who receive their first Minimally Effective rating must improve the following year or risk dismissal. Beginning in 2012-13, teachers who have earned a second consecutive Developing rating are likewise subject to dismissal if they fail to improve. Through spring 2012, Highly Effective teachers were conversely eligible for large financial rewards. The share of teachers facing each performance incentive are estimated only within the respective years in which the incentive was in place. The sample for the solid line includes 35,672 teachers-by-year and 9,455 unique teachers; and similarly for the dashed line 22,344 and 6,936, and for the long dashed line 10,004 and 4,755.
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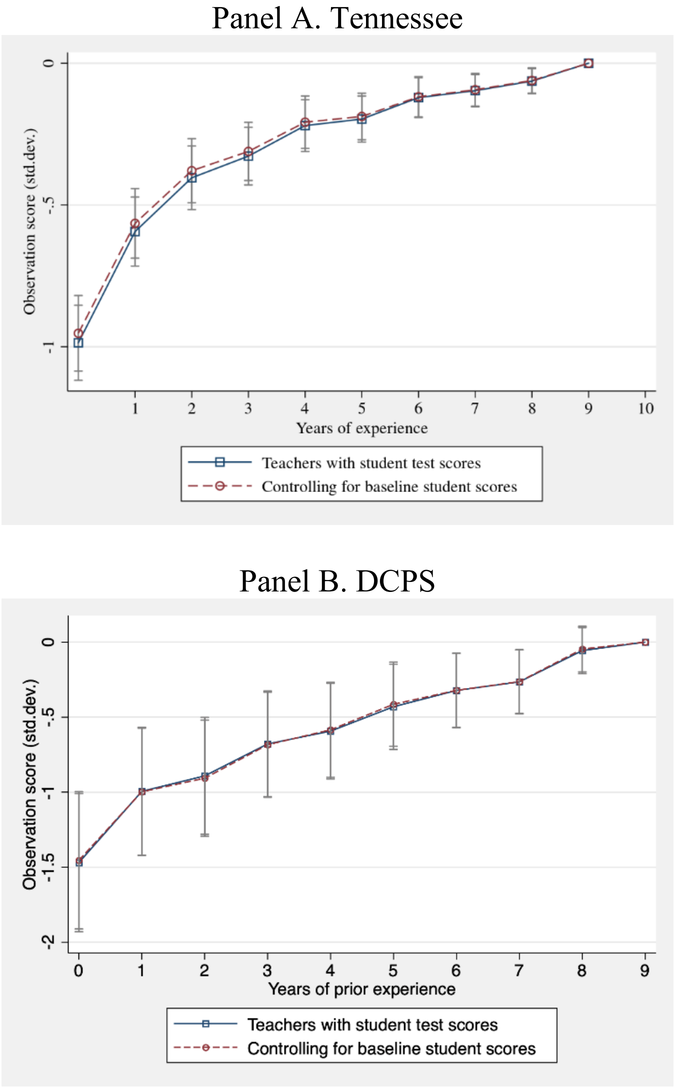
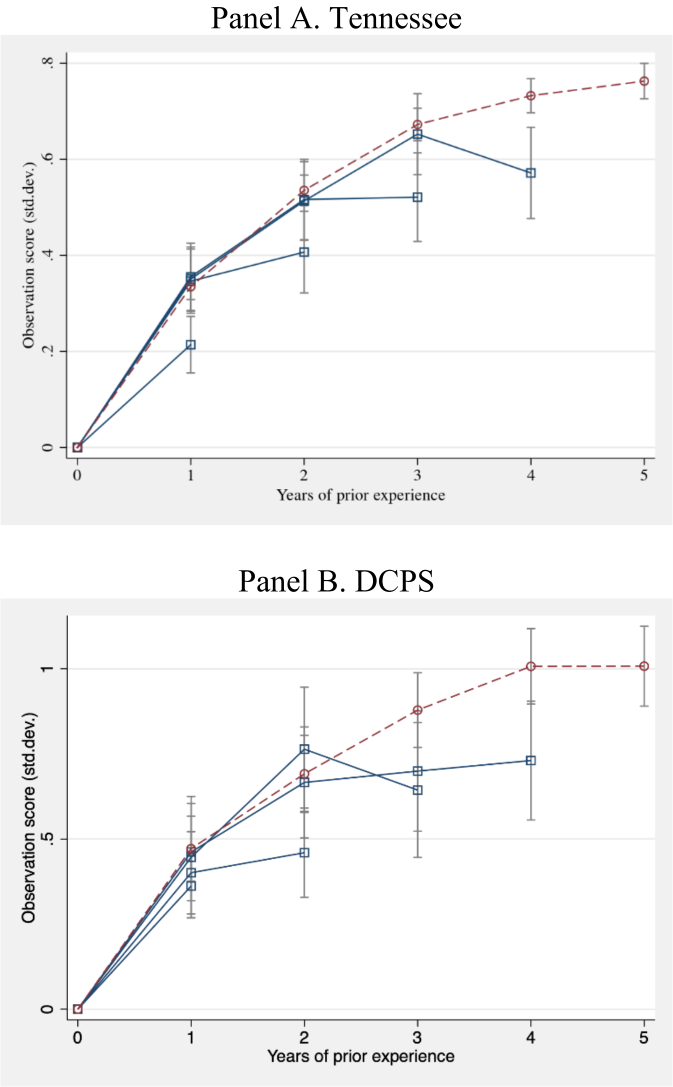


Panel A. Tennessee

Panel B. DCPS

Figure 8—Estimates controlling for student baseline test scores

Note: Both the solid and dashed lines report estimates using our preferred diff-in-diff strategy described in Section 2.1. Both use the same identical sample of teacher-by-year observations. The vertical lines mark the 95 percent confidence intervals which are corrected for clustering (teacher). For the solid line "teachers with baseline test scores" estimates, the details of estimation are identical to the solid line in Figure 1 except that we restrict the estimation sample. The solid line sample includes only teacher-by-year observations where we have both an average observation rating, $\bar{s}_{jt}$, and baseline test scores, $A_{i(t-1)}$, for the students i assigned to teacher j in year t. For the dashed line "controlling for baseline test scores" estimates, the details of estimation are identical to the solid line except that we first residualize the outcome, $\bar{s}_{jt}$, using the mean baseline test score, $A_{i(t-1)}$, among teacher j's students. The sample size the same for the two lines; in Tennessee 3,076,946 student-by-subject-by-year observations and 65,750 teacher-by-year observations for 25,017 unique teachers, and similarly in DCPS 250,377, 5,369 and 2,258.


Panel A. Tennessee

Panel B. DCPS

Figure 9—Estimates by year of exit

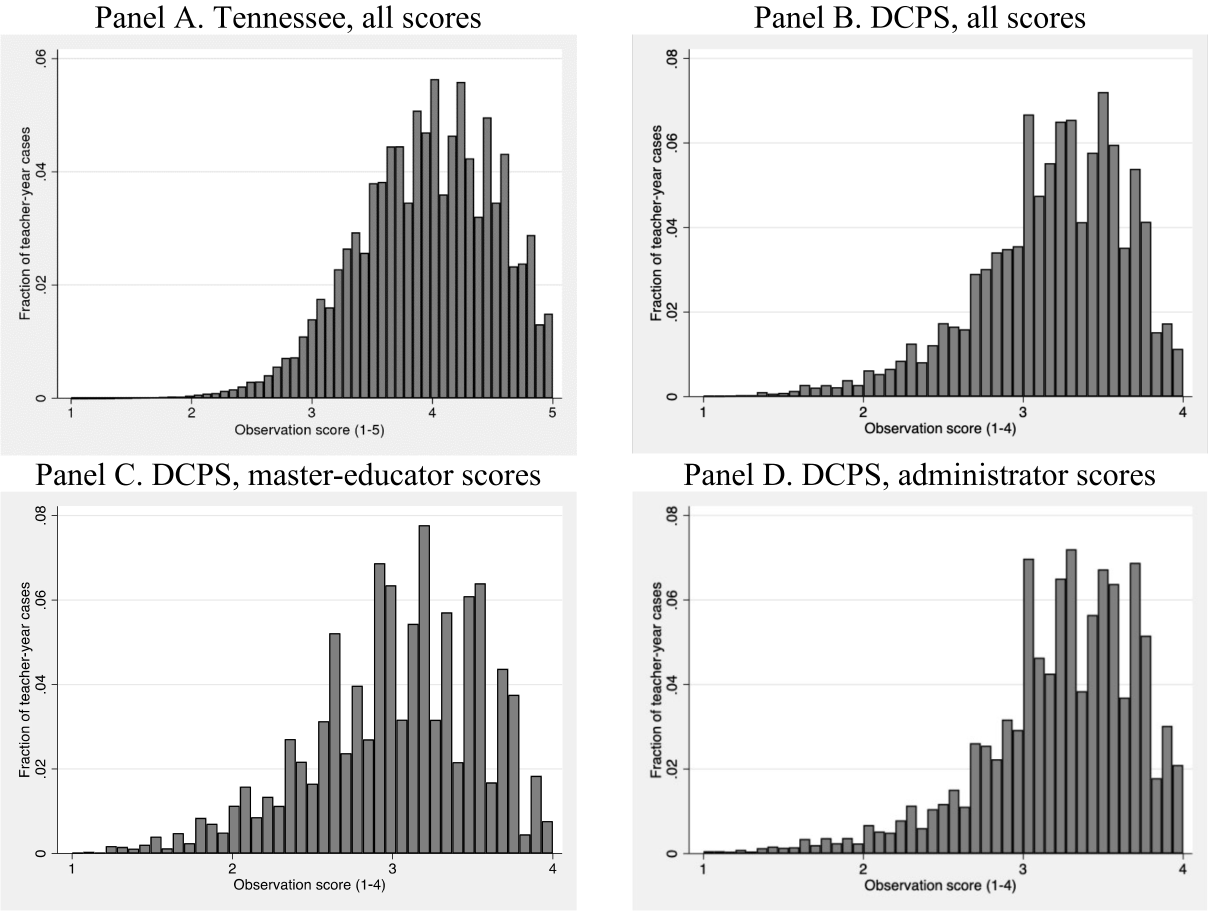
Note: All lines report estimates using our preferred diff-in-diff strategy described in Section 2.1. The vertical lines mark the 95 percent confidence intervals which are corrected for clustering (teacher). For each line in the figure, the details of estimation are identical to the solid line in Figure 1 except for the estimation sample. The sample for each of the four solid lines is defined by how many years the teacher taught in the jurisdiction (Tennessee or DC). Each teacher is observed for exactly 2, 3, 4, or 5 consecutive years and then not observed in the data subsequently. The dashed line includes teachers observed for 6 or more consecutive years. The sample size the same for the two series; in Tennessee 27,853 teacher-by-year observations for 6,613 unique teachers, and similarly in DCPS 31,785 and 8,931.

Table 1—Characteristics of the two samples

	Tennessee	DCPS
	(1)	(2)
(A) Students		
At or above proficiency on NAEP		
Math, grade 4	0.39	0.31
Math, grade 8	0.30	0.18
Reading, grade 4	0.34	0.27
Reading, grade 8	0.32	0.20
Race/ethnicity		
Black	0.22	0.64
Hispanic	0.09	0.18
White	0.64	0.13
Other or multiple race or ethnicity	0.05	0.04
Urbanicity		
City	0.34	1.00
Suburb	0.25	0.00
Town	0.14	0.00
Rural	0.27	0.00
Share of school-age population in poverty	0.22	0.28
English language learner	0.04	0.10
Special Education	0.13	0.17
(B) Teachers		
Observation score (original units)	3.94	3.17
	(0.57)	(0.47)
Observation score, administrators	3.94	3.22
	(0.57)	(0.49)
Observation score, master educators		3.02
		(0.53)
In student test score sample	0.23	0.15
Female	0.79	0.74
Race/ethnicity		
Black	0.06	0.51
Hispanic	0.00	0.05
White	0.86	0.32
Other or multiple race or ethnicity	0.08	0.04
Graduate degree	0.55	0.69
Years of experience		
Mean	11.83	10.86
Standard deviation	(9.61)	(8.25)
Categorical		
1st year teaching	0.06	0.07
2nd	0.06	0.07
3rd	0.06	0.07
4th	0.05	0.06
5th	0.05	0.06
6th	0.05	0.05
7th	0.04	0.05
8th	0.04	0.04
9th	0.04	0.04
10th or more	0.55	0.48

Note: Panel A: National Assessment of Educational Progress (NAEP) scores are the simple mean of NAEP tests which occurred during the years in our analysis sample. Descriptive statistics for students are form the from National Center for Education Statistics' Common Core of Data. The exception is the "in poverty" statistic which comes from US Census Bureau Small Area Income and Poverty Estimates. Panel B: Authors calculations using administrative data.

Appendix A. Additional figures and tables


Panel A. Tennessee, all scores

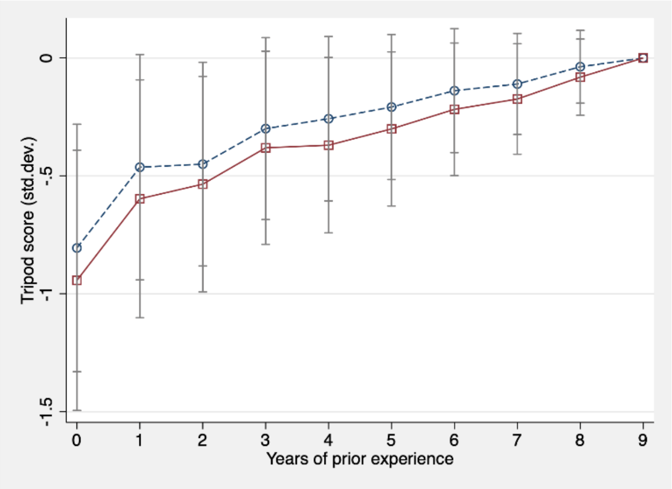
Panel B. DCPS, all scores

Panel C. DCPS, master-educator scores

Panel D. DCPS, administrator scores

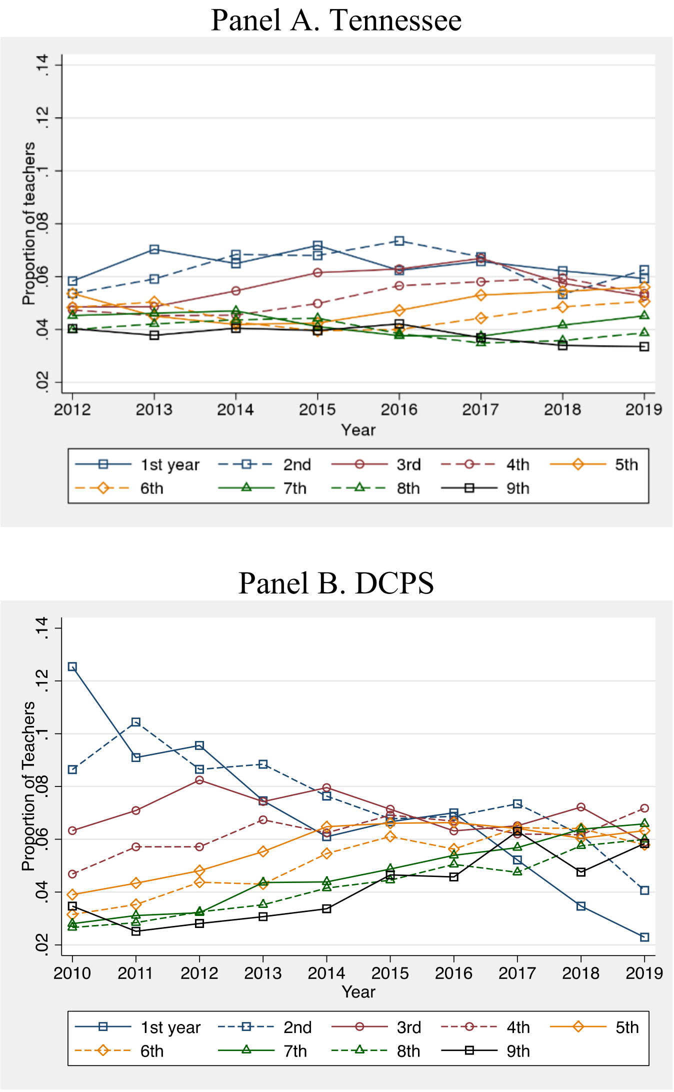
Appendix Figure A1—Distribution of observation scores

Note: Histograms of teacher-by-year observations. The x-axis is a teacher's annual observation score, which is an average of scores for different items or tasks, in the original rubric-scale units. Data are from the Tennessee TEAM rubric 2011-12 through 2018-19, and DCPS TLF rubric 2009-10 through 2015-16. The sample size for Tennessee in Panel A is 375,072 teacher-by-year observations; and similarly for DCPS 35,672 in Panel B, 34,898 in Panel C, and 21,086 in Panel D.



Appendix Figure A2—Returns to experience measured in scores from student surveys (DCPS)

Note: The dashed line reports estimates using our preferred diff-in-diff strategy described in Section 2.1. The dashed line reports estimates using the conventional two-way fixed effects approach described in Section 2.4. The vertical lines mark the 95 percent confidence intervals which are corrected for clustering (teacher). The details of estimation are identical to Figure 1 except that the outcome variable in this figure is based on student survey responses to the Tripod survey. The dependent variable is the teacher j's Student Surveys of Practice (SSoP) score for school year t. SSoP scores are standardized (mean 0, s.d. 1) by school year using the distribution for all teachers in DCPS. The survey was administered to all DCPS students in grade 3 and above from 2016-17 to 2018-19. The sample size for the solid line is 4,406 teacher-by-year observations for 1,687 unique teachers, and similarly 4,312 and 1,640 for the dashed line.

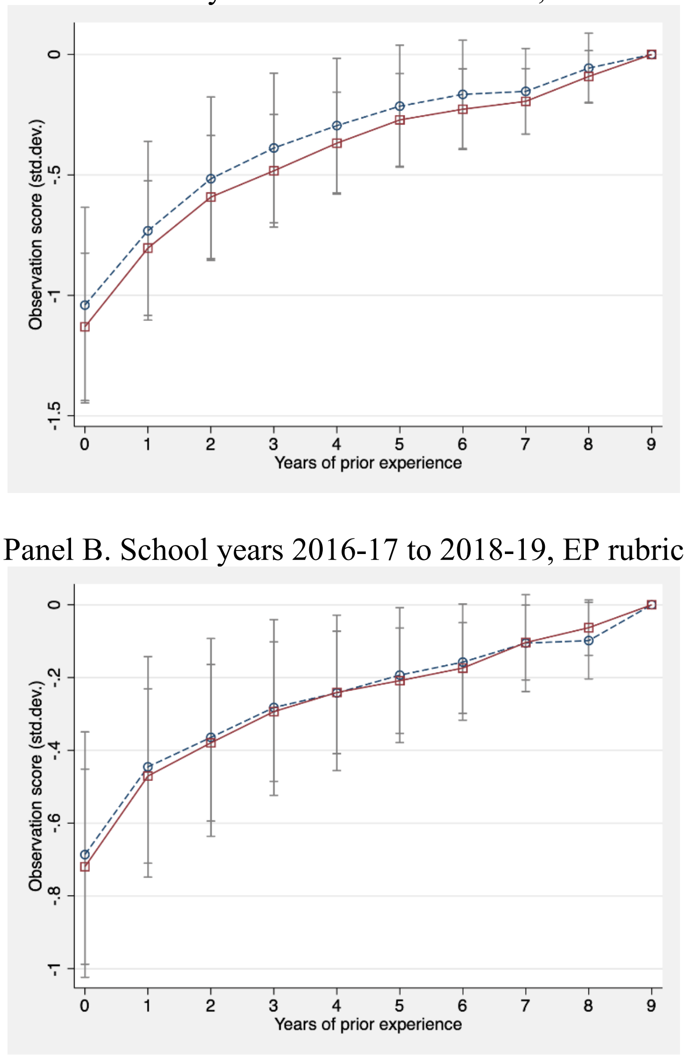


Panel A. Tennessee

Panel B. DCPS

Appendix Figure A3—Distribution of teacher experience over time

Note: Each line measures the proportion of teachers (y-axis) in a given school year (x-axis) who are in their *e*th year of teaching. The estimation sample is the same as Figure 1. The estimation sample for Tennessee includes 375,072 teacher-by-year observations for 81,847 unique teachers, and similarly for DCPS 35,672 and 9,455.

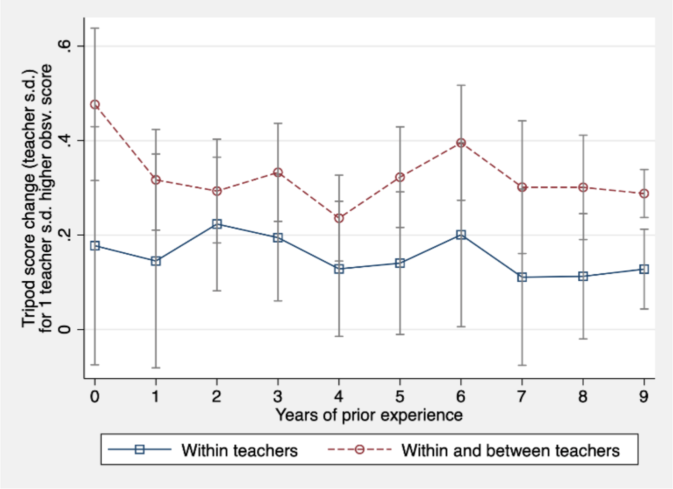


Panel A. School years 2013-14 to 2015-16, TLF rubric

Panel B. School years 2016-17 to 2018-19, EP rubric

Appendix Figure A4—Estimates when the distribution of experience is relatively stable (DCPS)

Note: The solid line reports estimates using our preferred diff-in-diff strategy described in Section 2.1. The dashed line reports estimates using the conventional two-way fixed effects approach described in Section 2.4. The vertical lines mark the 95 percent confidence intervals which are corrected for clustering (teacher). The details of estimation are identical to Figure 1 except that the estimation samples here are each a subset of Figure 1's estimation sample. Panel A uses only data from 2013-14 to 2015-16, and panel B only 2016-17 to 2018-19. Starting in 2016-17 DCPS switched from the TLF rubric to the new EP rubric. The sample size for the solid line in panel A is 24,125 teacher-by-year observations for 7,726 unique teachers; and similarly 21,558 and 5,452 for dashed line in panel A, 11,547 and 5,083 for solid line in panel B, and 10,116 and 3,689 for dashed line panel B.



Appendix Figure A5—Predicting student survey scores with teacher observation scores by years of teacher experience (DCPS)

Note: The solid and dashed lines each report estimates from a separate linear regression. The vertical lines mark the 95 percent confidence intervals which are corrected for clustering (teacher). In both cases the outcome variable is teacher j's Student Surveys of Practice (SSoP) score for school year t. SSoP scores are standardized (mean 0, s.d. 1) by school year using the distribution for all teachers in DCPS. In both cases the specification includes (a) indicators for years of prior experience 1 through 8 individually, with ≥ 9 years the omitted category; (b) classroom observation score, $\bar{s}_{jt}$; and (c) the interactions of (a) and (b). Each plotted point is sum of the coefficient on the (a)*(b) interaction for e years of experience (x-axis) plus the main-effect coefficient on (b). The solid line specification includes year and teacher fixed effects. The dashed line includes only year fixed effects, omitting the teacher fixed effects. The sample size for both lines is 5,362 teacher-by-year observations for 2,643 unique teachers.

Appendix Table A1—Predicting student test scores with teacher observation scores

	(1)	(2)	(3)	(4)
(A) Tennessee				
Observation score (st.dev.)	0.166	0.081	0.009	0.005
	(0.003)	(0.001)	(0.002)	(0.002)
(B) DCPS				
Observation score (st.dev.)	0.196	0.098	0.029	0.025
	(0.012)	(0.006)	(0.007)	(0.008)
Student prior test score controls		√	√	√
Teacher experience controls				√
Teacher fixed effects			√	√

Note: Each column within panels reports results of a separate least-squares regression. Standard errors in parentheses are corrected for clustering (teacher). The dependent variable is student i's test score, A_{ijst}, in subject s (maths or English language arts pooled) and school year t. Test scores are standardized (mean 0, s.d. 1) within each grade-by-subject-by-year cell using the distribution for all students in the jurisdiction, Tennessee or DCPS respectively. The key independent variable is teacher j's classroom observation score, $\bar{s}_{jt}$, which is an average of several item-level scores recorded during a given school year t. Observation scores are standardized (mean 0, st.dev. 1) by school year using the distribution of all teachers in the jurisdiction, Tennessee or DCPS respectively. The "student prior test score controls" are a quadratic in prior-year test score, where the parameters are allowed to differ across grade-by-subject-by-year cells, $b\left(A_{is(t-1)}\right)$. The "teacher experience controls" are a set of indicators for years of experience 1 through 9 individually, with ≥ 10 years the omitted category. The sample size the same across columns; in Tennessee 4,222,939 student-by-subject-by-year observations and 92,403 teacher-by-year observations for 34,395 unique teachers, and similarly in DCPS 252,400, 5,429, and 2,274.

Appendix B: Details of Estimates Involving Teachers' Value-Added Contributions to Student Test scores

B.1 Estimates for Figure 2 Solid Line

The solid line in Figure 2 plots returns-to-experience estimates where the performance measure is a teacher's value-added contributions to student test scores. We first obtain value-added scores, $\hat{\mu}_{jt}$, following the procedure described in the next two paragraphs, then we apply the estimator in Equation 1 substituting $\hat{\mu}_{jt}$ for $\bar{s}_{jt}$. In Figure 2, the y-axis, $\hat{\mu}_{jt}$, is measured in student standard deviation units, and the sample is limited to teachers of grades 4-8 in math and English language arts.

To estimate $\hat{\mu}_{jt}$ we first fit the following regression specification, separately for Tennessee and DCPS data:

$$A_{ijst} = b\big(A_{is(t-1)}\big) + \lambda_j + u_{ijst} \tag{B.1}$$

where A_{ijst} is the end of year t test score for student i in subject s taught by teacher j. Test scores are in student standard deviation units (mean 0, s.d. 1 within jurisdiction-by-year-by-subject-by-grade cells, where jurisdiction is either the state of Tennessee or the DCPS district). The function $b\big(A_{is(t-1)}\big)$ is a flexible function of student i's prior year test score in subject s, specifically, a quadratic in $A_{is(t-1)}$ where the parameters are free to differ across grade-by-school-year cells. Finally, the λ_j term represents teacher fixed effects.[1]

After fitting Specification B.1, we calculate the modified residuals: $A^*_{ijst} = A_{ijst} - \hat{b}\big(A_{is(t-1)}\big)$ or equivalently $A^*_{ijst} = \hat{\lambda}_j + \hat{u}_{ijst}$. Then our estimate of value added, $\hat{\mu}_{jt}$, is the average residual, A^*_{ijst}, averaging over all students i assigned to

[1] Years 2015-16 and 2016-17 are excluded for Tennessee because students were not tested in 2015-16. In Tennessee if the student had two or more teachers in a given subject and year, we include one observation per teacher and weight each observation by the proportion of responsibility allocated by the state to the teacher. Three quarters of students had one teacher in a given subject. If the student's prior year test score is missing, we replace it with zero and include an indicator for missing in the function b.

teacher j in year t, and averaging over subjects s (math and reading) if the teacher taught both. This average residual, $\hat{\mu}_{jt}$, version of a "value added measure" is the same average residual as in step one of the Chetty, Friedman, and Rockoff (2014) or Kane and Staiger (2008) approaches. In the current application we do not "shrink" the estimates because $\hat{\mu}_{jt}$ is the outcome in our analysis.

B.2 Estimates for Figure 2 Dashed Line

The dashed line in Figure 2 plots returns-to-experience estimates where the outcome is also teacher value added, but the estimation methods follow the conventional strategy instead of our preferred strategy. That conventional strategy is described in the next paragraph.

For these estimates we fit a version of the regression specification in Equation 3, but a specification fit with student-level data:

$$A_{ijst} = h\left(expr_{jt}\right) + b\left(A_{is(t-1)}\right) + \lambda_j + \pi_t + v_{ijst} \qquad \text{(B.2)}$$

where the function $h\left(expr_{jt}\right)$ is specified just as it is for the classroom observation outcomes. We repeat Equation 4 here for convenience:

$$h\left(expr_{jt}\right) = \sum_{e=0}^{\bar{e}-1} \beta_e \times \mathbf{1}\left\{expr_{jt} = e\right\} \qquad (4)$$

$$\text{and } \delta_e = \beta_e - \beta_{e-1}.$$

with the omitted category is veterans, $\mathbf{1}\left\{expr_{jt} \geq \bar{e}\right\}$. All other details of estimation for B.2 are the same as for fitting B.1. We continue to estimate standard errors using a cluster (teacher) correction.

B.3 Estimates for Figure 3

Figure 3 shows the relationship between observation ratings and test-score value added, and how that relationship changes with teacher experience. The x-axis is years of prior experience. The y-axis is the predicted increase in value added if we increase the teacher's observation score by one standard deviation.

To obtain the estimates in Figure 3 we fit the regression specification in Equation B.2, except that the function $h\left(expr_{jt}\right)$ is replaced with:

$$h\left(expr_{jt}, \bar{s}_{jt}\right) = \alpha_{\bar{e}}\bar{s}_{jt} + \sum_{e=0}^{\bar{e}-1} \beta_e \mathbf{1}\left\{expr_{jt} = e\right\} + \alpha_e\left(\mathbf{1}\left\{expr_{jt} = e\right\} \times \bar{s}_{jt}\right) \quad \text{(B.3)}$$

which interacts experience and observation ratings on the right-hand side. Figure 3 plots $(\hat{\alpha}_e + \hat{\alpha}_{\bar{e}})$ for each level of experience, e. The solid line in Figure 3 uses only within-teacher over-time variation, by including teacher fixed effects just as in Specification B.2. The dashed line in Figure 3 uses both within- and between-teacher variation by omitting the teacher fixed effects from the regression specification. As throughout the paper, we estimate standard errors using a cluster (teacher) correction.

References

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