TN EDUCATION RESEARCH ALLIANCE

Will Mentoring a Student Teacher Harm My Evaluation Scores?

Effects of Serving as a Cooperating Teacher on Evaluation Metrics

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Abstract

Growing evidence suggests that preservice candidates receive better coaching and are more instructionally effective when they are mentored by more instructionally effective cooperating teachers (CTs). Yet, teacher education program leaders indicate it is difficult to recruit instructionally effective teachers to serve as CTs, in part because they worry that serving may negatively impact district evaluation scores. Using a unique dataset on over 4,500 CTs, we compare evaluation scores during years these teachers served as CTs to years they did not. In years they served as CTs, teachers had significantly better observation ratings and somewhat better achievement gains, though not always at significant levels. These results suggest that concerns over lowered evaluations should not prevent teachers from serving as CTs.

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Introduction

A growing body of evidence suggests that certain characteristics of teachers' preservice training, including aspects of their student teaching experiences, are related to better workforce outcomes (Boyd, Grossman, Lankford, Loeb, & Wyckoff, 2009; Krieg, Theobold, & Goldhaber, 2016; Ronfeldt, 2012; Ronfeldt, 2015; Ronfeldt, Schwartz, & Jacob, 2014). Of particular relevance to the present study, two new studies have found recent graduates to be more instructionally effective when they learned to teach with more instructionally effective cooperating teachers (CTs) during their preservice training (Ronfeldt, Brockman, & Campbell, 2018; Ronfeldt, Matsko, Greene Nolan, & Reininger, 2018).

Yet, many teacher education program leaders and state policymakers suggest that, despite their best efforts, teacher candidates are often still placed with CTs who are not all that instructionally effective (Greenberg, Pomerance, and Walsh, 2011). As we describe in more detail below, there are a number of possible explanations for why this might be the case. Importantly, at least one possible explanation, commonly cited in Tennessee, where our study takes place, is that instructionally effective teachers are concerned that hosting teacher candidates will negatively impact their teacher evaluations. Given substantial evidence that new teachers are far less effective than more experienced teachers, these concerns may be warranted. The only existing analysis of the effects of hosting a student teacher on instructional performance also suggests that these concerns are justified. In Washington, Goldhaber and colleagues (2018) found that hosting a student teacher had a small, negative impact on math achievement, although, these negative effects were concentrated among the lowest performing CTs.

However, more studies in different labor markets and policy environments are needed – like our present study in Tennessee – in order to test if these findings are specific to the Washington context. Additionally, in Tennessee, student achievement gains are only one aspect of the teacher evaluation system. This study also tests whether teachers' observation ratings, which receive equal weight in state evaluations, are also impacted by hosting a student teacher. We also contribute to the existing empirical base by testing whether serving as a mentor also impacts teacher evaluations in years after hosting a student teacher. We investigate this, in part, because some existing literature suggests that mentoring can function like a form of professional development also for the mentor (Spencer, 2008). Finally, we test whether the effects of serving as a CT are concentrated among teachers who are more or less instructionally effective or among teachers who work at specific school levels (elementary, middle, secondary).

Results from this study suggest that, compared to other years, teachers receive better observation ratings and similar achievement gains in years that they serve as cooperating teachers. We find positive effects on observation ratings for teachers across quartiles of instructional effectiveness, though effects are most positive for teachers in the bottom quartile. When considering achievement gains, we detect small, positive effects for top-quartile teachers and small, negative (but non-significant) effects for bottom-quartile teachers; this is somewhat inconsistent with Goldhaber and colleagues (2018), who found negative effects across quartiles and significantly negative effects in the bottom quartile. On the other hand, because Tennessee requires that teachers must meet performance standards in order to serve as a CT, it is possible these differences in results are driven by these contextual factors, as their results are concentrated amongst teachers who might not be eligible to serve in Tennessee. We also find positive effects of serving as a CT on observation ratings to be concentrated among elementary teachers and the

effects on achievement gains to be similar across school level. In years after serving as a CT, teachers perform similarly on observation ratings and slightly worse on student achievement gains, though the latter results may be explained by regression to the mean (Atteberry, Loeb, & Wyckoff, 2015).

The results of this study suggest that concerns that serving as a CT may harm teacher evaluations seem unwarranted; in fact, hosting a student teacher may benefit observation ratings. Any negative effects on achievement gains appear to be concentrated among the least instructionally effective CTs. An implication is that teacher education programs and policymakers should continue to target instructionally effective teachers to serve as CTs and that these teachers should consider serving as CTs since it will likely benefit the next generation of teachers and may even benefit their own evaluations.

Literature Review

Recent evidence suggests that new teachers are more instructionally effective in their first year if, during their preservice preparation, they received mentoring from more instructionally effective CTs. In a study evaluating statewide data from Tennessee, Ronfeldt, Brockman, and Campbell (2018) found that preservice candidates who completed their student teaching or residency in a classroom with cooperating teachers who received observation ratings of 5.0 (the highest score on Tennessee's ratings scale) performed as if they had an additional year of teaching experience when they began teaching as compared to peers whose cooperating teachers received ratings of 3.0. They also found the student achievement value-added scores of candidates and their CTs to be significantly and positively correlated. Likewise, in a study using data from Chicago Public Schools (CPS), Ronfeldt, Matsko, Greene Nolan, et al. (2018), found that each additional point on a cooperating teachers' observational ratings (on a scale of 1-4) was associated with a 0.16 point gain for their preservice candidates' observational rating during their first year, an amount that is comparable to the average difference on observation ratings between teachers in their first year and teachers with between two and five years of experience in Chicago (Jiang & Sporte, 2016).

Such similar findings, from different labor markets and based upon different measures of instructional effectiveness, suggest that these relationships may be real. Yet, both studies were based upon correlational evidence, and so could potentially be explained by various forms of selection, instead of by CTs causing mentees to perform better. In particular, instructionally effective candidates might sort to more instructionally effective CTs even before clinical experiences begin, or more effective teacher education programs might recruit more effective CTs. The only foolproof way of adjusting for this possibility is through random assignment.

Addressing this concern, Ronfeldt, Goldhaber, Cowan et al. (2018) randomly assigned candidates in one large program in Tennessee to two possible kinds of student teaching placements. Specifically, the researchers asked the program to over-recruit CTs. They then used information from administrative data on the characteristics of these potential CTs and schools – which prior research has shown to be correlated with future instructional performance – to predict which would be more and less promising placements. Critical to the present study, the instructional effectiveness of the CT, measured by observational ratings and value-added scores, largely determined whether a placement was classified as promising or not. Those candidates who were assigned to the field placements predicted to be more promising reported receiving

more frequent and better coaching from their CTs than did those assigned to less promising placements. These candidates also reported receiving more opportunities to practice various aspects of teaching and feeling somewhat better prepared to teach at the end of their preservice preparation. Due to the random assignment to placement, the results provide preliminary evidence that the relationship between CTs' instructional effectiveness and higher quality clinical preparation is indeed causal. They also suggest that instructionally effective CTs may cause mentees to be more instructionally effective, at least in part, by providing more and better coaching; that is, teachers who provide better instruction to P-12 students, on average, appear to also provide better coaching to preservice candidates. In other words, the effect of instructionally effective CTs on teacher candidates' performance is not solely through modeling more effective instruction but also through providing better quality coaching (Matsko, Ronfeldt, Greene Nolan et al., 2018).

The importance of receiving high-quality coaching is also well-established in literature on in-service teacher induction and support. In a recent meta-analysis, Kraft, Blazar, and Hogan (2018) examined literature that demonstrated causal or plausibly causal effects of coaching on teachers' instructional performance, including studies of professional development or induction programs. They found that the effects of receiving teacher coaching to be significant and meaningful; they combined results from 60 studies on teacher coaching and found pooled effect sizes of 0.49 standard deviations on instruction and 0.18 standard deviations on achievement. While the meta-analysis, and the studies on which it was based, suggest that coaching makes a difference on instructional practice, they say less about the effects specifically of being coached by an instructionally effective mentor. This distinction is important because it is possible that mentors are capable of providing high quality coaching to teacher peers without themselves being effective teachers of P-12 students.

There are no studies, to our knowledge, that link the instructional performance of in-service mentors to the instructional performance of in-service mentees. Other literature on in-service teachers, though, suggests that informal mentoring through collaboration with instructionally effective peers can promote instructional performance – whether through coaching, modeling, or some combination of the two. Papay and colleagues (2016) tested whether low-performing teachers matched to work with colleagues who excel in those areas of weakness demonstrate growth over the year, as a result of this partnership. They found that students in the low-performing teachers' classrooms scored 0.12 standard deviations higher than students in classrooms where their low-performing teacher was not paired with a stronger colleague. Similarly, Jackson and Bruegmann (2009) have found evidence that improving the quality of other teachers in the same grade level and school improves student performance across the grade. Using value-added measures to estimate teacher quality, they find that teachers with more instructionally effective peers have better achievement gains. In their preferred model, they find that improving the instructional quality of a teacher's peers by one standard deviation is associated with an effect on that teacher's students between one-tenth and one-fifth of the effect of improving his or her own quality by one standard deviation.

Taken together, the literature reviewed thus far suggests that being assigned an instructionally effective CT is likely to cause teacher candidates to become more instructionally effective themselves. Yet, existing qualitative literature and anecdotal evidence suggests that teacher candidates are often assigned to cooperating teachers who are not the most instructionally effective teachers in their schools or districts (Greenberg, Pomerance, and Walsh, 2011). There

are many possible explanations for this. First, there is some evidence that teacher education programs often privilege recruiting cooperating teachers who are known to provide good coaching to preservice candidates over recruiting the most instructionally effective teachers of P-12 students (Mullman & Ronfeldt, in preparation). Recent research conducted on student teaching placements also suggests that proximity to the program or the pre-service candidate's home might be the most influential factor in selection of CTs, rather than instructional quality (Krieg et al., 2016; Maier & Youngs, 2009). Program leaders must consider that their candidates often do not own cars or have the financial resources to incur daily travel costs, so typically consider candidate preferences for placement location. Prior research also suggests that different stakeholders – including program staff, district leader, school administrators, and candidates themselves – in different programs take primary responsibility for making placement decisions (Grossman, Hammerness, McDonald, and Ronfeldt, 2008; Matsko et al. 2018), and these stakeholders likely differ in terms of how much they prioritize CT instructional effectiveness as a selection criterion. Related, not all of these stakeholders have access to instructional performance information about potential CTs. Even states where these data exist, they are not typically available for program leaders or teacher candidates to review, given FERPA restrictions and the highly sensitive nature of teacher evaluation information. States like Tennessee and Florida have minimum teacher evaluation requirements to serve as a CT; in Tennessee, for example, a teacher must have at least a score of 3.0 (on a 5.0-point scale) to serve as a CT. This means that programs leaders and teacher candidates must depend upon school and district leaders – who have access to performance data -- to ensure CTs are instructionally effective teachers. District and school leaders, though, are sometimes hesitant to select their most instructionally effective CTs to serve as cooperating teachers because this means rookie teachers will take over instruction for their best teachers, which they fear may have negative short-term effects on student learning and achievement, especially given the rise of high-stakes testing (Mullman & Ronfeldt, in preparation; St. John, Goldhaber, Krieg, and Theobold, 2018). Indeed, some of the TEP staff that Mullman and Ronfeldt spoke to about placement procedures indicated that principals occasionally want to put PSTs in the classrooms of struggling teachers so that they can help out and serve as "an extra set of hands." Similarly, St. John and colleagues (2018) find that principals sometimes make these matches "with the hope of either supporting or motivating a [CT's] practice."

Most relevant to this study, though, are reports by teacher education program leaders, and the district and school leaders with whom they collaborate, that teachers can be hesitant to serve as CTs for concerns ear that their annual evaluation scores may suffer. We initially heard about these concerns anecdotally, during conversations with TDOE policymakers and EPP leaders. These concerns were subsequently confirmed during interviews -- as part of a research study on the variation in clinical preparation -- by EPP leaders responsible for designing and implementing clinical experiences across Tennessee (Mullman & Ronfeldt, in preparation). When we presented these findings to former Tennessee Commissioner Candice McQueen (November 2, 2018), she indicated that this had been a concern she had encountered as a former dean and in her current role; she also explained that a likely source had been an unpublished report by the SAS Institute (2014) from a pilot study in Tennessee which concluded:

> For most grades and subjects, supervising student teachers had no significant difference in terms of teacher effectiveness, particularly for teachers who are considered average or high performing. However, the initial findings do suggest that that low performing

> teachers might have a small negative impact in their effectiveness in Mathematics and Science when supervising student teachers as compared to not supervising. This finding has potential implications for the assignment of student-teachers to licensed teachers (p.2).

There are good reasons to believe these kinds of concerns are prevalent, especially in states and districts where student achievement is used in teacher evaluations and factors heavily into decisions about teacher tenure. Serving as a cooperating teacher inevitably means handing over a substantial amount of instructional responsibilities to rookie teachers so that they can learn to take on these responsibilities. Even though prospective teachers learn to teach under the guidance of the more experienced cooperating teacher, the evidence is clear that inexperienced teachers are, on average, less effective and often flounder during their student teaching experiences. On the other hand, we might hypothesize that instructional quality could improve in classrooms that take on a student teacher, given the higher student-to-teacher ratio, opportunities for collaborative teaching, and potentially the introduction of new knowledge and pedagogy by the novice.

We are aware, though, of only one, recent study that has directly tested the impact of hosting a student teacher on teachers' instructional performance. In Washington, Dan Goldhaber and colleagues (2018) tested whether hosting a student teacher affected student achievement, and whether effects were heterogenous across levels of CT instructional effectiveness, as measured by teachers' value-added scores. Using data from 14 TEPs in Washington state, they found that hosting a student teacher has, on average, a small negative impact on students' math performance, and no significant impact on ELA achievement. When they divided teachers into quartiles based on their value-added measures, they found that the effects in math achievement were driven by the lowest performing CTs. This suggests, as the authors argue, that more effective CTs are able to "mitigate" the impact of letting an inexperienced pre-service teacher take over instruction in the classroom; this finding adds to the growing body of evidence that the most instructionally effective teachers should be the ones to serve as cooperating teachers (Ronfeldt et al., 2018).

In keeping with recent calls for more replication studies in educational research (Makel & Plucker, 2014), the present study replicates the Goldhaber et al. study in a different teacher labor market and state context. Like Goldhaber and colleagues (2018), we are interested in the effect that hosting a student teacher has on teachers' evaluation metrics. The present study, though, also extends prior research in important ways. First, we consider both value-added measures and observational ratings as our outcomes of interest. While Goldhaber and colleagues only considered value-added measures, in many states, including Tennessee, observation ratings carry equal, and sometimes more, weight in final evaluations. Especially given prior evidence that observation ratings may be prone to rater tendencies, biases, and subjectivities (Campbell, 2014; Campbell & Ronfeldt, 2018; White, 2018), it may be that the effects of hosting a student teacher on observation ratings differ from the effects on value-added measures. For example, the elevated status of being a CT may cause raters to inflate scores of teachers hosting student teachers. Second, we use an analytic sample from Tennessee state administrative data, a state with a labor market and cultural context that differs from Washington. We also approach this question from the perspective of teacher learning and professional growth, and interrogate whether serving as a CT changes future performance. The literature suggests that effective professional development include long-term, active learning (Desimone, 2009), and it is possible

that mentoring a novice teacher meets these requirements. In fact, in a recent survey of CTs in Chicago, almost one-fifth of CTs indicated that their primary reason for serving as a CT was because it helped them to improve as a teacher (Matsko et al., under review). This is consistent with Papay and colleagues (2016) who found that, in pairing lower-performing teachers to collaborate with higher-performing peers, both parties demonstrated improved performance. This belief is further supported by a UK review of mentoring programs for novice teachers, where Shanks (2017) finds that mentors, in coaching novices, sometimes engage in the same kinds of critical inquiry and reflection as mentees, creating opportunities for learning for both parties. Thus, we go beyond prior literature to test whether there is any evidence for lagged effects of serving as a CT on teachers' instructional performance in years after they hosted student teachers.

Research Questions

RQ 1: Do teachers perform differently in years that they serve as CTs?

RQ 2: Are the effects different for different groups of CTs?

RQ 3: Do teachers perform differently in years after they serve as CTs?

Data

Data for this paper comes from a unique dataset of CTs collected by the Tennessee Department of Education. This dataset includes information from seventeen teacher education programs[1] in the state and identifies the teachers who served as CTs for these programs between the 2010-2011 and the 2013-2014 school years. We merge these data onto Tennessee's teacher and school databases. The teacher database includes information about teachers' work experience, licensing status, and evaluation scores. School-level data comes from Tennessee school universe files which include information about student body characteristics, average attendance, and school improvement status.

Descriptive Statistics

Our analytic dataset includes all teachers in Tennessee from the 2010-2011 school year through the 2016-2017 school year. Our sample includes 458,717 teacher-by-year observations. Table 1 presents descriptive information about types of evaluation data we have for teachers, including observation ratings and their value-added measures (Teacher Value-Added Assessment System, or TVAAS. See, Voster, Guranio, and Woolridge, 2018, for more information on how these scores are calculated). Similar to other states' use of value-added measure, TVAAS is calculated using state test data, and intends to capture an individual teacher's effect on student achievement; teachers receive scores for specific tested subjects as well as composite scores. TVAAS was piloted in the 2010-2011 school year and fully implemented the following year, so we report value-added measures starting in 2011. TVAAS scores are available for about half of the teachers in our sample because of variation in testing requirements across grade levels and school settings. Observation ratings are available starting from the 2011-2012 school year. We have a total of 4,522 teacher-by-year observations for teachers who served as CTs between the

[1] These seventeen educator preparation programs graduated about 40% of the teacher candidates prepared in Tennessee during our period of observation.

2010-2011 to 2013-2014 school years[2]. Teachers in Tennessee are assessed multiple times per year using the Tennessee Educator Acceleration Model (TEAM), a rubric that includes four domains and multiple indicators within each domain. The four domains are instruction, environment, planning, and professionalism. Professionalism is only assessed one time, at the end of the school year. Multiple domains and indicators are scored at the same time, and teachers receive scores on a scale from 1 (significantly below expectations) to 5 (significantly above expectations). For this paper, domain and overall ratings are an average of indicator scores and domain scores, respectively.

Table 2 presents summary statistics comparing those teachers who served as CTs to those who did not. Reading the table from left to right, we present the average statistics for our entire analytic sample of teachers, CTs, all other teachers, and the difference between CTs and other teachers. Teachers who served as CTs are, on average, statistically different from other teachers when it comes to their observation ratings, TVAAS scores, teacher covariates, and school covariates. On average, we find that CTs are more likely to be White (7.6 percentage point difference) and female (3.6 percentage point difference), have 1.88 years more experience, are more likely to hold an advanced degree, and work in schools with a greater proportion of students who are White and meet proficiency levels on state exams and with a smaller proportion of students who quality for free or reduced-priced lunch (FRPL). CTs also tend to have higher observation ratings and TVAAS scores, a fact that should not be surprising, as Tennessee policy sets minimum requirements on both of these measures to serve as a CT. In our sample, the average observation rating for a CT was 4.04, compared to 3.88 for teachers who did not serve. The average TVAAS score for CTs was 0.061 student standard deviation units higher than other teachers. These findings are consistent with other prior research which has found CTs to have stronger evaluation scores, on average, than non-CTs (Goldhaber et al., 2018; Matsko et al., 2018; Ronfeldt et al., 2018).

CTs Blocks

In order to conduct a more appropriate comparison of those who serve as CTs to those who do not, we constructed blocks of all eligible teachers for a student teaching placement in a given year. We identified teachers who served each year and then grouped them with all other teachers in their districts with the same teaching endorsement (e.g. secondary math, elementary, secondary ELA, etc.). This allowed us to create a hypothetical pool of all teachers who could have served for a particular student teacher.[3] We merged Tennessee's Personnel Information Reporting System (PIRS) and teacher assignment data onto our analytic sample and then compared the courses they taught that year and assigned them an endorsement. If for example, a seventh-grade social studies teacher in district *D* served as a CT, we create a block with that CT with all other secondary social studies teachers in that district, in order to build a sample of all possible CTs for that year.

[2] In most of our model, we restrict the evaluation data to cover the same time-span as the CT dataset. As a robustness check, we use the full evaluation data. Our results are robust against the dataset that we use to estimate the effects of serving as a CT on evaluation scores.

[3] It is common for EPPs to ask candidates for their preferences in terms of districts in which they are willing to complete their student teaching and to then select placements in the requested districts (Maier & Youngs, 2009; Krieg et al., 2016). One reason for this is that student teachers often have geographic and travel constraints.

Table 3 presents descriptive differences between blocks with and without eligible CTs, as well as differences between CTs and the rest of the teachers in their block. Compared to blocks without CTs, on average, blocks with CTs have lower observation ratings and years of experience but higher TVAAS scores. Blocks with CTs have a higher share of elementary and middle school teachers and lower share of secondary teachers. They also tend to have more female and Black teachers but fewer White teachers.

When we look within blocks, we find that CTs outperform non-CTs. In our sample, CTs have average observation ratings of 4.03, on a scale of 1 to 5. Non-CTs in the same blocks have average observation ratings 0.19 lower than CTs. CTs also have higher TVAAS scores than non-CTs (a 0.08 standard deviation difference for teachers not in blocks and a difference of 0.06 for those in blocks). CTs were more likely to be female, White, and hold a graduate degree but were less likely to be Black. When compared to the rest of their block, CTs were also more likely, on average, to teach in schools with higher proportions of White and higher-achieving students.

Methods

Research Question 1

To investigate the effects of serving as a CT on evaluation metrics, we use a fixed-effects model. This model allows us the estimate the within-teacher changes on years during which they serve as a CT as compared to the other years during which they did not host a teacher candidate.

Our preferred model is:

$$Y_{it} = \beta_{0i} + \beta_1 CT_{it} + \delta Exp_{it} + \lambda_t + \epsilon_{it} \quad (1)$$

where Y_{it} is the outcome of interest. β_{0i} is the individual-level fixed effect. CT_{it} is an indicator variable taking the value of 1 for all years t during which teacher i is reported as serving as a CT. Exp_{it} is a set of indicators for years of work experience that we add to the model to increase efficiency and account for the timing of being selected to be a CT. λ_t is the year fixed effect. We use these fixed-effects to account for any secular variation in evaluation scores. ϵ_{it} is the stochastic error term adjusted for clustering of teachers at the school level.

Our coefficient of interest is β_1. This term captures the causal effect of serving as a CT on evaluation scores and teacher value-added estimates. Our causal claim rests on two identifying assumptions. First, any individual-level characteristics that lead to selection to be a CT are constant over time and can be accounted for by an individual-level fixed effect. Second, these characteristics have a linear and additive functional form to the model's intercept (Angrist & Pischke, 2008).

Research Question 2

We modify our preferred model to answer the second research questions. We use this model:

$$Y_{it} = \beta_{0i} + \beta_1 CT_{it} + \beta_2 CT_{after_{it}} + \delta Exp_{it} + \lambda_t + \epsilon_{it} \quad (2)$$

where we divide the counterfactual for serving as a CT in equation (1) into two parts using the $CT_{after_{it}}$ indicator. This indicator takes the value of 1 for all teachers who were reported as being a CT for at least one year and for all years following serving as a CT. This

allows us to separately estimate the effects of serving as a CT on evaluation metrics for the years during which a teacher serves as a CT and for the years following serving as a CT[4].

The coefficient of interest for these analyses is β_2. This captures the effects of serving as a CT in the period following this experience as compared to evaluation scores during the period preceding serving as a CT.

Research Question 3

Heterogeneity by Quartile. Goldhaber, Krieg, and Theobald (2018) suggested that the effects of serving as a CT vary for teachers in different effectiveness quartiles, with most of the negative effect concentrated in the lowest quartile of teachers.[5] We calculate effectiveness quartiles using a two-step approach. First, we estimate the teacher fixed-effect from this model:

$$Y_{it} = \tau_i + \pi_1 CT_{it} + \epsilon_i \quad (3)$$

where τ_i is the teacher fixed-effect for teacher i. It captures the evaluation score averages for teacher i over all observation years, controlling for effects of serving as a CT on evaluation scores. We use these teacher fixed-effects to calculate the quartile of effectiveness for each teacher or, more formally, $Q_i \mid \tau_i$. We use these quartiles to estimate the effect of serving as a CT for teachers across the quality distribution using the model

$$Y_{it} = \beta_{0i} + \beta_1 CT_{it} \cdot \mathrm{Q_i} + \delta Exp_{it} + \lambda_t + \epsilon_{it} \quad (4)$$

where $CT_i \cdot Q_i$ is the interaction term between the CT indicator and the quartile of effectiveness for each Y. β_1 is a vector of four estimates, one for each quartile of effectiveness, that allow us to test whether the effects of serving as a CT are different for teachers at different points of the teacher performance continuum.

Heterogeneity by School Type. A major difference between elementary and secondary teachers is that the former are typically with the same group of students throughout the day while the latter tend to work with different students (often in different subject areas) across the day. These differences also afford different opportunities for student teachers placed at different school levels that could have implications for their mentors' evaluations. For example, elementary student teachers likely have more opportunities to build relationships with students which could differentially benefit evaluations of elementary CTs over secondary CTs. Alternatively, secondary CTs may be able to strategically place student teachers in courses or sections that could minimize potential costs and maximize potential benefits. Assuming school leaders will schedule observations during classes/periods in which teachers are not hosting a student teacher,

[4] We observe that 83% of CTs are reported to serve only once during our observation period. 14% of CTs serve twice. 3% serve three times or more.

[5] Goldhaber et al. (2018) also found evidence of regression to the mean in their sample. We test for this issue using a Monte Carlo simulation described below. We do not find evidence that evaluation scores regress to the mean in our sample. This fact could be due to differences in the way that we calculated the effectiveness quartile for teachers and the way in which Tennessee calculates teacher value-added scores. First, we calculate quartile of effectiveness using all evaluation data available for each teacher. This is because we do not have access to evaluation data for the period preceding serving as a cooperating teacher. Second, TVAAS models differ from traditional value-added models insofar that scores for each teacher are calculated separately for each student cohort and that teacher value-added are calculated using empirical Bayes' estimates (Vosters, Guranio, & Woolridge, 2018).

CTs could place student teachers in their most difficult classes/periods. We felt an important first step was to investigate whether or not the effects of serving as a CT vary by school level.

Thus, we divide schools into four categories, elementary schools (grades K-5), middle schools (grades 6-8), high schools (grades 9-12), and other schools (other grade configurations, for example K-8). We estimate a model similar to (4) where we interact the CT indicator with indicators for school type. This allows us to test whether the effects of serving as a CT are concentrated in a specific instructional setting.

Robustness Checks

We run several robustness checks to test whether our results are sensitive to our model specification, to the sample of teachers that we use, and to our estimation strategy. We find that the results from our preferred model are robust against all these robustness checks. Moreover, our preferred model provides the most conservative estimates of the effects of serving as a CT on evaluation scores.

First, we test whether our results are sensitive to the inclusion of teacher experience. Papay and Kraft (2015) argued that the experience coefficients could be biased when used in a fixed-effects model that includes year terms. We address this concern by estimating our preferred model without the experience terms and by adjusting the experience coefficients using the technique described by Papay and Kraft (2015).

Second, we include school-level covariates to control for possible unobserved differences among workplaces that could confound selection to be a CT and evaluation scores. For example, researchers have found teachers' evaluation ratings to be related to the characteristics of their students (Campbell & Ronfeldt, 2018; Jiang & Sporte, 2016; Steinberg & Garrett, 2016).

Third, we submit our preferred model to progressively more restrictive samples of teachers in order to account for various forms of likely selection We restrict our sample to teachers who teach in the same school district and subject area as the teachers that we observe serving as a cooperating teacher, to teachers who teach in the same school and subject, and to teachers who are reported as being CTs at least once.

Last, we use difference-in-differences and matched-sample model specifications to check whether our results are sensitive to model specification. In detail, we use the equation

$$Y_{it} = \beta_{0i} + \beta_1 CT_{ever\,it} + \beta_2 CT_{it} + \delta Exp_{it} + \pi_{it} + \lambda_t + \epsilon_{it}$$

where CT_{ever} is an indicator variable that takes the value of 1 for any teacher who served as a CT at least once, CT is the indicator variable taking the value of 1 during all years t for which teacher i is reported as serving as a CT, π_{it} is a school fixed effect, and λ_t is a year fixed effect term. Conceptually, this model compares teachers who serve as CTs to teachers who did not serve within the same school. The first difference is between teachers who ever serve as CT and teachers who never serve as a CT. This difference accounts for average differences on evaluation scores between the group of teachers that is ever selected to serve as CT and the group of teachers that is never selected to serve as a CT. The second difference is within the group of ever CTs and compares the evaluation scores for the years during which these teachers serve as CT

and years during which they do not. This second difference estimates the effect of serving as a CT on evaluation scores.

This difference-in-differences specification relies on more permissive assumptions than our preferred model - that the evaluation scores of teachers who were ever selected to be CTs and the ones for teachers who were not followed parallel trends before CT selection. Evaluation and CT data availability make it difficult to formally assess the degree to which evaluation scores for CTs and non-CTs were following parallel trends before serving as CT. In particular, when teachers served as CTs early in our observation window, we sometimes have no data on pre-trends or only a single year. Thus, we limit our analysis to the 2013 and 2014 CT cohorts since they have at least two years of pre-trend data, and present an event study (see Appendix Figure 1). While we observe parallel trends between CTs and non-CTs in terms TVAAS, we observe that OR seem to increase the year prior being selected to serve as a CT. These results could hint to CTs being selected using prior year observation score data. Given parallel trends seem to exist for TVAAS and that data limitations make it difficult to be certain that the parallel trends assumption has been violated for sure, we include difference-in-difference results in this paper. Nevertheless, we recommend caution when interpreting these results, as relaxing our model assumptions could introduce bias when the parallel trend assumptions are not met. In fact, we find that our difference-in-differences estimates have greater magnitude than our fixed-effects estimates. Two possible sources of bias can explain these results. First, if this model does not meet the parallel trend assumption, the estimates will be biased, in our case upwards. Second, another possible source of bias comes in in the form of unobservable variables that lead to an increase in, for example, observation ratings that is unrelated to serving as a CT. An example of this could be a teacher taking a leadership role (e.g., curriculum coordinator) at the school that gets rewarded with hosting a student teacher. We can expect that taking on that role could lead to higher ORs and that is increase unrelated from serving as a CT.

In part because of possible concerns that, prior to serving, teachers who become CTs may be increasing on observation ratings at relatively greater rates than other teachers, we also included a matched-sample robustness check. The matched-sample model allows us to construct a comparison group that is similar on observed characteristics, including pre-trends on evaluation data, to teachers who serve as a CT. We do this in a two-step process. First, we identify a sample of teachers that have similar characteristics to our CT sample. Second, we use this matched to calculate the effect of serving as a CT on evaluation scores. Specifically, we match CTs and non-CTs using a nearest neighbor matching algorithm that use an exact match on teacher demographic characteristics (i.e., race/ethnicity and gender), highest level of education completed (i.e., bachelor's, post-bachelor's, or master's degree), school level (i.e., elementary, middle, or high school), and CT block. We fuzzy match using Mahalanobis distance on up to two prior years of evaluation data and years of experience at time of serving as a CT. We remove from these analyses two CTs who did not match with other teachers in the state on background characteristics. Appendix Figure 2 reports the density distributions for the fuzzy matched variable pre- and post-matching. We note that the matching procedure was able to identify similar teachers across these three variables for all four outcomes of interest and that the common support assumption appears to have been met. We also note that we were not able to have quality matches on TVAAS mathematics scores two years prior to serving as a CT. This could introduce some bias in the matched estimates for this particular measure.

This matched-sample specification relies on the assumption that we match teachers who are reported as being CTs to teachers similar to them in all observed characteristics included in the model except for being selected to be a CT. These estimates could be biased if selection to be a CT is driven by unobserved teacher characteristics.

We find that the results of these two alternative model specifications have generally the same sign and are larger in magnitude than the estimates from our preferred model. The results confirm that our preferred model provides the most conservative estimates of our outcomes of interest.

Results

RQ1: Do teachers perform differently in years that they serve as CTs?

We present the main results for the four outcomes of interest – observation ratings, average TVAAS, mathematics TVAAS, and ELA TVAAS – in Table 4. We begin by summarizing results from our preferred models with teacher fixed effects. Across the first row, we notice that the effects of serving as a CT on evaluation metrics is either small and positive, in the case of observation ratings, or not significantly different from zero, in case of all three TVAAS estimates. Regarding observation ratings, estimates suggest that teachers' observation scores increase by 0.04 points in years that they serve as cooperating teachers as compared to other years; this is roughly equivalent to about one-fifth of the expected growth in observation ratings for a first-year teacher (Ronfeldt, Brockman, & Campbell, 2018). It is worth noting that CTs have, on average, almost 14 years of experience, a point in teachers' careers when their observation ratings tend not to increase substantially (i.e., after the 10-year mark, see Papay and Kraft, 2015, for an in-depth analysis).

The even-numbered columns in Table 4 display the estimates from the difference-in-differences models. We note that the point estimates for our coefficient of interest tend to be greater in magnitude in these models than in the teacher-fixed effects ones.[6] In fact, the estimate on models for TVAAS (all subjects), is now positive and statistically significant at the 5% level, suggesting that teachers have greater achievement gains in years that they serve as CTs. These models also allow us to estimate the difference in evaluation scores (across years) between teachers who are reported as serving as CTs at least once in our dataset (See row "Ever Cooperating Teacher") and teachers who are not reported as serving as CTs during our observation period. We interpret this coefficient as the baseline difference in evaluation scores that might have led specific teachers to be selected as CTs. Across all four outcomes, we note that teachers who serve as CTs at least once have significantly and meaningfully higher evaluation scores than their peers. In other words, teachers who serve as CTs are, on average, higher-performing teachers and seem to be positively selected on their evaluation scores.

As described above (see Table 2), we find that teachers in the same districts and subject areas as our CT sample seem to differ from teachers in other districts/subjects. While our teacher fixed

[6] As we discussed in the methods section, these results rely on a different set of assumptions than the teacher fixed-effects estimates. Namely, we are assuming that the evaluation scores for CTs and non-CTs follow parallel trends during the pre-CT period. Our results could suggest that this assumption is not met. That is, CTs have different returns to experience than non-CTs. While our analysis of parallel trends is partial, we find some potential evidence that pre-trends are not parallel for observation ratings (see Methods and Appendix Figure 2). However, the results that we report in Appendix Tables 1 and 3 seem to suggest that that our estimates are well-specified.

effects models effectively compare a teacher's performance in years in which s/he serves as a CT to performance in years in which s/he does not, above we use the full sample of teachers – including those teachers in non-CT blocks – to estimate coefficients for teaching experience and for the intercept term. Thus, we wondered whether our estimates could be sensitive to our choice for the estimation sample. To test this, we constrain our analyses to successively more restricted samples – (1) to teachers who teach in the same districts, same subject areas, and years as CTs in our sample; (2) to teachers who teach in the same school, subject areas, and years as CTs in our sample; and (3) only to teachers that served as CTs at least once. For all outcomes, the estimates for our preferred models have qualitatively similar estimates over the different estimation samples (see Appendix Table 1). However, the estimates for observation ratings decrease by about a quarter when we restrict the sample to teachers in the same blocks or same schools. This might be in line with descriptive statistics, described above, indicating that the blocks and schools where we observe CTs are different on baseline characteristics than other blocks and schools in the state. This will lead to a mechanical change in the coefficients for the covariates that we include in the model.[7] Alternatively, this might indicate the presence of positive selection bias that is not fully accounted for in models that restrict the sample to teachers in the same block or school.

As an additional robustness check for our sample choice, we use a nearest neighbor matching algorithm construct a sample of teachers who have similar observed characteristics to CTs but that were not picked to serve as CTs. This selection process happens in two steps. First, we select teachers who did not serve as CTs that have the same observed characteristics as CTs (e.g., same gender and race/ethnicity) or that are the closest on other characteristics (e.g., years of experience or prior evaluation data) to be part of the matched-sample comparison group. We use this matched-sample to calculate the average treatment effect on the treated for teachers who serve as a CT. Appendix Table 2 reports the extent to which this matching process was able to identify non-CTs with similar observed characteristics to our sample of CTs. We observe that the nearest neighbor matching process was able to identify a sample of non-CTs who had similar characteristics to CTs as the standardized means and variances for the fuzzy matched variables are close to zero and one respectively.[8]

We report these estimates in Table 5, alongside the estimates for the teacher fixed effects and difference-in-difference models. Overall, we observe that the estimates for observation ratings have the same sign and magnitude across the different estimation models. It is notable that our matching algorithm matches on two years of prior evaluation trends, including on observation ratings. Thus, we are matching CTs with non-CTs that have similar patterns of returns to experience preceding the service years. Where differences in pre-trends could explain the positive effects on observation ratings in CT service years for our difference-in-difference specifications, they are unlikely to explain observed effects in our matched-sample models. Results for TVAAS appear to be significant and greater in magnitude for the matched-sample

[7] To test whether the change in covariate coefficients could explain our results, we adjust the year fixed-effects using the method that Papay and Kraft (2015) describe. These models' results are qualitatively identical to our preferred model estimates.

[8] We also note that this test is not a formal balance test but rather an informal test that compares the matched comparison and CT samples on the fuzzy matched variables. We visually inspected the distributions of these variables by plotting box plots for the comparison and treated groups. The distributions appear to be similar between the two samples suggesting that analyses meet the common support assumption.

model. This might indicate that the matched-sample models, and to an extent the difference-in-differences models, fail to account for self-selection bias. Said in another way, teachers that were selected to be CTs could be different from other teachers in unobserved ways from teachers who were not selected. The teacher fixed-effects models account for these unobserved differences by leveraging the within-teacher variation in evaluation scores for CTs. Failing to account for these unobserved differences could lead to estimates that are biased upwards.

RQ2: Are the effects different for different groups of CTs?

In this section, we investigate whether the effects of serving as a CT differ for different groups of teachers. We begin by examining heterogeneity for different quartiles of effectiveness. When we began this study, we suspected that the effects of serving as a CT would vary by level of effectiveness, but were unsure about the direction. On the one hand, a teacher who is in the top quartile of effectiveness might be better equipped to support a teacher candidate than a teacher in the bottom quartile of effectiveness. On the other hand, replacing instruction from a top-performing teacher with a teacher candidate might negatively impact student learning, thus leading to lower TVAAS scores. As described in our review of the literature above, Goldhaber et al. (2018) found that the negative effect of hosting a student teacher on math achievement appears to be concentrated among the lowest-performing CTs; they suggest that more instructionally effective mentors are better able to support student teachers or buffer against the possibly negative (or, at least, likely less positive) effects of rookie instruction.

We also consider differences in estimates for teachers who work in different school types. We might expect that serving as a CT could have different effects for an elementary school teacher who teaches all subjects during the school day than a secondary teacher who teaches only one subject over multiple periods. Because student teachers are typically with the same group of students all day in elementary classrooms, it might be that the stronger relationships they are likely to form with students can benefit achievement more than in secondary student teaching placements. On the other hand, if student teachers are taking over more lead teaching across the day and across subjects in elementary classrooms, then it is also possible TVAAS scores in elementary classrooms suffer from more exposure to rookie teaching.

Another possibility is that differences between elementary and secondary teaching allow CTs different options for minimizing effects on their own teaching evaluations. For example, a secondary teacher could assign the teacher candidate to teach a challenging class/section, thus ensuring that his or her own evaluations would happen with a more favorable class. Alternatively, elementary school teachers could strategically ask their candidates to take over more lead teaching responsibilities in subjects that are not assessed in order to buffer their own evaluations or subjects that are particularly challenging for them.

Heterogeneity by Effectiveness Quartile. Table 6 reports the estimates that include an interaction term between the CT indicator and quartile indicator. We interpret the estimate for the CT indicators as the effect of serving as a CT for teachers in the various quartiles of effectiveness. We find positive effects of serving as a CT for teachers in all four quartiles of observation ratings. Moreover, we find that teachers in the lowest quartile benefit the most from serving as a CT compared to teachers the other quartiles. A possible explanation for this pattern of results is that the ceiling effect built into the observation score rubric negatively biases the effects of serving as CT for teachers in the upper quartiles of effectiveness. In this case, the

observation scores of more effective teachers do not have as much room for improvement as the scores of less effective teachers.[9]

Results for TVAAS tell a different story. We find that the effects of serving as a CT increase along the instructional effectiveness continuum. We observe positive effects on TVAAS scores only for teachers in the 4th quartile of effectiveness and possibly negative, but imprecisely estimated and nonsignificant, effects for teachers in the 1st quartile of effectiveness. Finding effects to be more negative for lower-performing teachers is consistent with what Goldhaber et al. (2018) found in their sample of teachers from Western Washington state, though they found significant, negative effects overall (across quartiles), with the most negative effects concentrated in the lowest quartile; we find no significant effect overall (across quartiles) and find small positive effects among teachers in the top-quartile of instructional effectiveness.

One possibility is that our results are entirely driven by regression to the mean in evaluation scores (see Goldhaber et al., 2018). In this case, we might conflate year-to-year variation in evaluation scores with effects of serving as a cooperating teacher. Specifically, if teachers' service (as CT) years coincide with years in which they also happen to be at their peak performance then they will tend to regress to their mean performance in post-CT years; this could lead to estimates like the ones that we observe for observation ratings in Table 6.[10] We check whether our results are sensitive to the way that we calculated the quartile of effectiveness by conducting a Monte Carlo simulation of the effects of serving as a CT on a placebo sample of CTs. Using the CT blocks described earlier, we randomly select 1,000 cohorts of teachers who were not actually selected as CTs during our observation period; these cohorts serve as a placebo for serving as a CT. We then calculate the effects of serving as a placebo CT for these 1,000 cohorts. This allows us to test the extent to which our results are sensitive to regression to the mean as Goldhaber et al. (2018) found. Appendix Table 3 shows the results from this Monte Carlo simulation. If regression to the mean were at play then we would expect placebo CTs at the ends of the distribution in performance to have non-zero estimates. We find that that all the point estimates for the placebo CT sample are all close to zero and their 95% credible intervals are centered at zero. Said in another way, we find that the placebo effect of serving as a CT on evaluation scores is centered around zero for teachers along the effectiveness continuum. This suggests that our estimates for the effects of serving as a CT for each quartile of effectiveness are robust against teachers' evaluation scores regressing to the mean.

Heterogeneity by School Type. Table 7 displays the results for the effects of serving as a CT by school type. We find that the positive results on observation ratings are driven by CTs who teach in elementary and middle schools and that the evaluation scores of high school or other school teachers do not change when serving as a CT. Though we are not entirely sure why we observe these differences by school level, we discuss possible explanations below (see Discussion).We also find that estimates for serving as a CT on TVAAS scores are mostly similar across the different school settings. However, the results seem to suggest that high school mathematics

[9] Observation scores averages are 3.36 [s.d. = .36] for CTs in quartile 1, 3.73 [s.d. = .30] for teachers in quartile 2, 4.01 [s.d. = .28] for quartile 3, and 4.39 [s.d. = .38] for quartile 4.

[10] In detail, regression to the mean could explain the improvement in observation scores that we observe for teachers in the lower quartiles of effectiveness by suggesting that our CT estimates are based on a "good evaluation" year and that these teachers' observation ratings regress back to the mean for years following serving as CT.

teachers' scores increase the year they serve as CTs but that these point estimates are imprecisely estimated.

RQ3: Do teachers perform differently in years after they serve as CTs?

Based on our most conservative (i.e., teacher fixed effects) estimates, we find small and positive effects on observation ratings and null effects on TVAAS scores for years in which teachers serve as CTs. One possibility, though, is that the effects of serving as a cooperating teacher are not immediate, but instead are observed in subsequent years. For example, if serving as a cooperating teacher functions as a form of professional development then we might not expect to observe increases in performance during the year a teacher serves, but perhaps in following years. In the next section we turn to Research Question 3, where we estimate different effects for the years during which teachers serve as CTs and for years following that experience.

Table 8 reports the results of the teacher fixed-effects and difference-in-differences estimates of the effects of serving as a CT in years following serving as CTs. This allows us to compare performance while serving as a CT and after serving as a CT to the evaluation scores during the time before serving as a CT. For observation ratings, CTs' evaluations do not increase, on average, in years following serving as a CT (see, Columns 1 and 2). We note that the point estimate from the difference-in-differences model changes sign and remains non-significant. This suggests that the point estimate for the period after serving as a CT is zero. That is, both specifications indicate that serving as a CT does not have a lasting impact on observation ratings beyond the years during which teachers serve as CTs.

On the other hand, results for TVAAS scores show that CTs' scores decline in the period after serving as a CT. TVAAS scores for the years in which teachers serve as CTs are similar to their scores for years before serving as a CT. However, scores in years after serving are lower than scores in years prior to serving. These results might highlight unobserved differences between CTs and non-CTs that we are not able to control in our main models. In fact, the negative effects for TVAAS scores disappear once we restrict our analyses to only teachers who ever serve as a CT (see Appendix Table 4 column 3). This could suggest that CTs might have differential returns to experience on TVAAS scores (Atteberry et al., 2015, find differential returns to experience by quartile of performance). Specifically, CTs experience relatively higher growth on TVAAS in years leading up to service years. This performance may increase the likelihood that teachers are tapped to serve as CTs; given a bump in performance during years leading up to serving. A post-serving decline may be expected if non-CTs close the TVAAS gap during the post-CT period.

Similar to RQ1, we explore whether our results are sensitive to sample selection. Appendix Table 4 presents the results for the teacher fixed-effects models on restricted samples of teachers. The estimate directions and magnitudes are similar across the estimation samples for the main effect of serving as a CT. The estimates for the period following serving as a CT appear to somewhat change depending on the sample that we use. For observation ratings, we find that the positive but insignificant estimate for the years following serving as CT appears to move towards

a null but imprecise estimate. For TVAAS scores, we find that the negative effect on the years following serving as a CT appears to move towards zero.[11]

Discussion and Conclusion

There is growing evidence that recruiting more instructionally effective teachers to serve as CTs is a promising approach to improving the preparation that teacher candidates receive and, subsequently, the instructional effectiveness of the incoming supply of new teachers. So why are program leaders reporting that it can be difficult to get our most instructionally effective teachers to serve as CTs? The challenge appears to be multifaceted, and this study investigates one factor: that teachers are hesitant to mentor a teacher candidate for fear that they may receive lower evaluations. Our results suggest that any concerns over declining evaluations are not warranted. Rather, we find observation ratings may increase while TVAAS scores are unaffected. The implications are that instructionally effective teachers who are considering becoming CTs should not let fears over evaluation scores deter them; moreover, program and district leaders charged with recruiting these teachers to serve can assure recruits that such fears are likely unwarranted.

The results of this study diverge somewhat from the findings of the only existing research on the effects of serving as a CT (Goldhaber et al., 2018). While we found no effects of serving as a CT on teachers' achievement gains in any subjects, Goldhaber and colleagues found math achievement to significantly decrease for CTs in Washington; ELA achievement was unaffected. Moreover, we found that teachers' observation ratings may actually benefit by hosting a student teacher; to our knowledge, our study is the first to investigate effects on observation ratings.

Goldhaber et al. (2018) also found that the effects of serving as a CT on math achievement were negative across quartiles of instructional effectiveness, but largest in magnitude and significant for the least effective teachers. They conclude that more instructionally effective teachers are likely better able to buffer against the negative effects of hosting a teacher candidate. Like Goldhaber and colleagues, we find that the coefficients on serving as a CT decreases as CT effectiveness also decreases. However, we find positive and significant effects for teachers in the top quartile, and negatively trending but non-significant effects for teachers in lower quartiles. These results do not seem to be consistent with an explanation that higher-performing teachers are mitigating the negative effects of hosting a student teacher; rather, our results seem to suggest that higher-performing teachers actually benefit from hosting a student teacher.

We are uncertain why our results diverge from those reported by Goldhaber and colleagues. Differences in study design and methods could possibly explain the different findings. One possibility is that both studies detected real effects because effects are heterogeneous. Since the

[11] A possible explanation for these unstable estimates could be collinearity between the CT and following CT indicators, the experience fixed effects, and the years fixed effects. This would lead to unstable point estimates that are sensitive to the estimation sample that we use to identify the main effects. To address this concern, we use the two-stage adjustment strategy for year fixed-effects described in Papay and Kraft (2015). We first estimate the year fixed effect using a model that does not include teacher fixed effects. We then use the year-specific coefficients estimated in stage one in our preferred model. The results from these models are consistent with the estimates from our preferred models (see Appendix Table 5), confirming a null effect on observation ratings for years following serving as a CT and a possible small and negative effect on TVAAS scores.

studies occurred in different states with different preparation requirements, evaluation systems, and labor market constraints, it is possible that the studies detected different results because the effects of serving in fact differ by context. If the effects of serving as a CT vary across contexts, then future research should also investigate how and why.

Another possibility, though, is that limitations in study design and methods explain the differing results. There are potential limitations in our research that must be considered. For example, we depend upon teacher-level TVAAS measures that were provided by the state, that we did not construct ourselves. By contrast, Goldhaber and colleagues used student-level data to construct teachers' value-added measures. Both sets of measures adjust for students' lagged achievement, but the measures in the Goldhaber et al. study also adjust for other student and school characteristics. Differences in how these measures of teachers' achievement gains were estimated could potentially explain the differences in findings.

Another limitation of our study is that we do not have comprehensive data identifying all teachers who served as CTs across the state and across the years included in our study. Rather, our CT data come from only those EPPs that had kept and were willing to share these data, and only for years that were included in their records. Thus, our coverage across EPPs and years was uneven. It is possible that the CTs for the particular EPPs and years in our sample may respond differently to serving than the CTs we do not observe. Though unlikely, it is possible that teachers in our sample improved on observation ratings when they served but teachers outside our sample declined in performance, which we don't observe. The study by Goldhaber and colleagues also did not have full coverage of programs in Washington state, and so may be subject to similar limitations. We are currently in negotiations with the TDOE to see if we can access comprehensive data on CTs across all programs in Tennessee for future cohorts.

More research is needed to understand the mechanism by which teachers may get a boost in observation ratings during the years in which they serve as a CT. One possibility is that serving as a CT does indeed boost the quality of instruction. It might be, for example, that, in years they are serving as CTs, having 'two hands on deck' helps with instruction by increasing the amount of independent instructional time each student has with a teacher. In years they serve as CTs, teachers also might invest more in instructional planning as a result of needing to onboard another teacher and ensure they are modeling good practice.

Another possibility is that teachers who serve as CTs must schedule evaluations on days or during sections/periods when their student teachers are not lead teaching. This could mean that unscheduled observations (for evaluation) are less common in years that teachers mentor student teachers. It also could mean that CTs are able to be more strategic about when they schedule observations/evaluations – e.g., during easier periods/classes or during subjects in which they especially excel. In these ways, teachers could effectively boost their evaluations, possibly explaining the bumps in performance we observe during years they serve as CTs. These explanations are consistent with finding that lowest-quartile teachers benefit most on observation ratings when serving as CTs, as one might expect strategically planning evaluations to benefit less effective teachers most. They are also consistent with finding little to no effects of serving as a CT on TVAAS scores, where student achievement, rather than scheduled observations by raters, dictate performance.

One additional consideration is that we find the positive effects of serving as a CT on observation ratings to be concentrated in elementary schools. There are many reasons why this might be true. It could be, for example, that elementary teacher candidates are better able to form personal relationships with students because they are with them all day or because younger students are more willing to connect with new teachers in their classrooms; this, in turn, likely translates into a stronger instructional environment. If this were the case, though, we would expect teachers' TVAAS, and not just observation ratings, to increase in years they serve as CTs.

Alternatively, one of the main differences between elementary and secondary teachers is that the former are tasked with teaching all subjects, even ones in which they are less knowledgeable or effective. It is possible that elementary teachers who host student teachers are more inclined to hand over lead teaching responsibilities in subjects in which they feel less proficient. If so, this could result in evaluators being more likely to evaluate CTs when teaching their stronger subjects and, thus, to rate them higher than in other years. If this were the case then we would expect hosting a student teacher to likely benefit lower performing elementary CTs the most, which is what we observe (see Appendix Table 6).

It is true that secondary teachers also often have multiple preps – e.g., a science teacher may teach biology, advanced biology, and chemistry in the same term. Secondary teachers could also then assign their teacher candidates to the subjects or preps that are their weakest. However, we believe that secondary teacher candidates are often more specialized in their subject matter focus, and more likely to request a specific class or prep that is a match. Compared to elementary teachers, this would likely place more constraints on secondary CTs in terms of which parts of the school day that they would be able to hand over lead teaching responsibilities to candidates; in other words, we suspect that secondary CTs may have somewhat less flexibility than elementary CTs how they assign their candidates.

Another possibility is that student teachers in elementary school placements are more likely than student teachers in secondary placements to stay in their placements throughout the course of the school day. If this is the case then school administrators may find it more difficult to do unscheduled observations of CTs, as they may drop in on a class only to discover the student teacher is taking on lead teaching responsibilities at any given time. Thus, it might be that elementary CTs need to do more scheduled observations which likely would benefit their evaluation scores more (as we argue above); moreover, the lowest performing elementary teaches would also likely benefit most, as we observe in Appendix Table 6. In order to move beyond such speculation, though, more research is needed to understand what explains how the effects of serving as a CT may differ across school levels.

If boosted performance among CTs is explained by having opportunities to somehow game the evaluation system, then we expect that some will argue that it seems inequitable for teachers who host a student teacher to gain such an advantage. Though we understand this perspective, we also recognize that mentoring a student teacher is a tremendous amount of additional work for classroom teachers, work that is often unrecognized, unappreciated, and not rewarded. One recent study found that cooperating teachers typically received about $300 for mentoring a student teacher, and that many were not compensated at all (Matsko et al., under review). Especially given that CTs can have meaningful, positive impacts on the instructional effectiveness of the incoming supply of teachers, they may be deserving of advantages during the years in which they mentor. In fact, one consideration might be to relieve teachers of being

evaluated in the years in which they mentor a student teacher. Doing so would remove any concerns about cooperating teachers gaming the evaluation system and would offset fears that hosting a student teacher might harm evaluation scores, even though our results suggest that these fears may be unwarranted.

Finally, our research extends prior work by testing whether or not serving as a CT has a longer-term effect evaluation during post-serving years. A positive effect in post-serving years could suggest that mentoring serves a professional development function. For observation ratings, we find post-service performance to decline back to pre-service levels. For TVAAS, we find post-service performance to actually be somewhat worse than preservice levels. However, when we constrain models only to individuals who ever served as CTs then the post-serving estimates are similar to pre-serving estimates; this may suggest that CTs are not actually doing worse in post-CT years but that, instead, non-CTs tend to have stronger relative returns. Either way, while we find potentially some boost to evaluations during the years in which teachers serve as CTs, we find no evidence that serving as a CT makes individuals better teachers in post-serving years. Thus, serving as a mentor does not appear to function as a form of long-term professional development.

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Table 1. Number of Teachers Valid Evaluation Data

	2011	2012	2013	2014	2015	2016	2017	Total
All Teachers	21,708	74,512	71,756	73,906	71,966	72,807	72,062	458,717
Observation Ratings	0	70,616	65,894	57,637	60,873	70,523	69,681	395,224
TVAAS Scores	21,291	21,843	30,551	31,714	25,523	8,879	21,158	160,959
Did Serve as Cooperating Teacher	417	1,163	1,561	1,381				4,522
Observation Ratings	0	1,151	1,507	1,291				3,949
TVAAS Scores	417	472	983	786	*Cooperating Teacher Data Unavailable*			2,658
Did Not Serve as Cooperating Teacher	21,291	73,349	70,195	72,525				237,360
Observation Ratings	0	69,465	64,387	56,346				190,198
TVAAS Scores	20,874	21,371	29,568	30,928				102,741

Note. The Cooperating Teacher database is available for a subset of Educator Preparation Programs in the state for school years 2010-2011 through 2013-2014. The Tennessee Teacher Value-Added Assessment System was piloted during the 2010-2011 school year and fully implemented in the 2011-2012 school year. Observation ratings are available starting from the 2011-2012 school year.

Table 2. Comparing Observable Characteristics of All Teachers to Those of Cooperating Teachers

	All Teachers	Cooperating Teachers	Other Teachers	Diff	p-value
Outcomes of Interest					
Observation Ratings	3.888	4.042	3.879	0.163	***
TVAAS - All Subjects	0.042	0.098	0.038	0.061	***
TVAAS - Mathematics	0.083	0.158	0.078	0.081	***
TVAAS - ELA	0.022	0.054	0.019	0.035	***
Teacher Covariates					
Percent Female	0.799	0.833	0.797	0.036	***
Percent White	0.870	0.941	0.866	0.076	***
Percent Black	0.122	0.055	0.125	-0.070	***
Percent Other	0.005	0.004	0.005	-0.002	***
Percent Bachelors Degree	0.408	0.326	0.414	-0.088	***
Percent Masters Degree	0.503	0.547	0.500	0.047	***
Percent PhD	0.009	0.012	0.009	0.002	*
Age	42.55	42.95	42.53	0.42	***
Years of Teaching Experience	11.96	13.74	11.86	1.88	***
School Assignment					
Elementary School	0.433	0.498	0.429	0.068	***
Middle School	0.185	0.193	0.185	0.009	**
High School	0.278	0.230	0.280	-0.051	***
School Covariates					
Percent White	0.678	0.759	0.673	0.086	***
Percent Black	0.216	0.140	0.221	-0.081	***
Percent Hispanic	0.075	.074	.075	-0.001	
Percent FRPL	0.587	0.567	0.589	-.0022	***
Percent Proficient	0.514	0.537	0.513	0.024	***
N	241,882	4,522	237,360		

Note. The Cooperating Teacher database is available for a subset of Educator Preparation Programs in the state for school years 2010-2011 through 2013-2014. The Tennessee Teacher Value-Added Assessment System was piloted during the 2010-2011 school year and fully implemented in the 2011-2012 school year. Observation ratings are available starting from the 2011-2012 school year. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Table 3. Descriptive Statistics by Block

	Blocks Without CTs	Blocks with CTs		
		All	Non CTs	CTs
Outcomes of Interest				
Observation Ratings	3.911	3.855	3.838	4.026
TVAAS	0.033	0.051	0.045	0.109
TVAAS - Mathematics	0.063	0.100	0.093	0.171
TVAAS - ELA	0.019	0.024	0.020	0.058
Teacher Covariates				
Percent Female	0.781	0.824	0.822	0.845
Percent White	0.878	0.857	0.849	0.940
Percent Black	0.110	0.137	0.145	0.056
Percent Other	0.005	0.005	0.005	0.004
Percent Bachelors Degree	0.401	0.419	0.429	0.330
Percent Masters Degree	0.509	0.493	0.487	0.548
Percent PhD	0.010	0.008	0.008	0.011
Age	42.87	42.10	42.05	42.69
Years of Teaching Experience	12.13	11.72	11.57	13.41
School Assignment				
Elementary School	0.39	0.50	0.50	0.53
Middle School	0.17	0.21	0.21	0.19
High School	0.33	0.20	0.20	0.21
School Covariates				
Percent White	0.693	0.657	0.648	0.752
Percent Black	0.207	0.230	0.238	0.145
Percent Hispanic	0.072	0.079	0.080	0.074
Percent FRPL	0.588	0.586	0.589	0.564
Percent Proficient or above	0.517	0.509	0.507	0.537
N	102,560	118,562	114,037	4,222

Note. Blocks were calculated according to whether a CT served in particular district in a given year. We group them with all other teachers in that district with the same teaching endorsement, so blocks represent all eligible CTs for a pre-service teacher that year.

Table 4. Effects of Serving as a Cooperating Teacher on Evaluation Metrics

	Observation Ratings		TVAAS - All Subjects		TVAAS - Math		TVAAS - ELA	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Fixed Effects	Diff-in-Diff	Fixed Effects	Diff-in-Diff	Fixed Effects	Diff-in-Diff	Fixed Effects	Diff-in-Diff
Cooperating Teacher	0.040***	0.053***	0.008	0.014*	-0.001	0.005	-0.003	0.005
	(0.007)	(0.007)	(0.006)	(0.006)	(0.012)	(0.013)	(0.007)	(0.007)
Ever Cooperating Teacher		0.108***		0.040***		0.075***		0.028***
		(0.007)		(0.006)		(0.011)		(0.005)
Mean Outcome	3.885	3.885	0.063	0.040	0.133	0.081	0.037	0.021
Standard Deviation	0.572	0.582	0.358	0.379	0.495	0.515	0.238	0.252
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Teacher Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Experience Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
N	174,214	242,339	91,726	127,669	40,943	61,943	46,771	69,642
R-Squared	0.771	0.292	0.689	0.125	0.702	0.194	0.619	0.131
Adjusted R-Squared	0.639	0.286	0.541	0.110	0.537	0.167	0.412	0.105

Note. Robust standard error clustered by teacher in parentheses. Cooperating teacher is a time-varying indicator taking the value of 1 during the school year in which a teacher is reported as serving as a cooperating teacher. Experience is included as single indicators for years 0-30 and as a pooled indicator for experience above 30 years. We drop singleton observations from models with teacher fixed effects. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Table 5. Coefficient Sensitivity to Estimation Method

	(1) Observation Ratings	(2) TVAAS All Subjects	(3) TVAAS Math	(4) TVAAS ELA
Fixed Effects Model	0.040***	0.008	-0.001	-0.003
Difference-in-Differences Model	0.053***	0.014*	0.005	0.005
Matched Sample	0.059***	0.039**	0.092**	0.007

Note. This table reports the sensitivity of the cooperating teacher coefficient to various model specifications. The fixed effects models include controls for years of experience, year and teacher fixed-effects. The difference-in-differences models include controls for years of experience, year and school fixed effects. The matched sample models report the Average Treatment Effects on the Treated (ATET) on teachers who serve as Cooperating teachers. We fuzzy match using Mahalanobis distance on up to two prior years of evaluation data and years of experience at time of serving as a CT. We exact match on teacher background characteristics. We remove two CTs who do not match with other teachers in the state on background characteristics. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Table 6. Heterogeneity by Quartile for RQ1

	(1) Observation Ratings	(2) TVAAS All Subjects	(3) TVAAS Math	(4) TVAAS ELA
Cooperating Teacher # Quartile 1	0.108***	0.001	-0.049+	-0.031
	(0.023)	(0.013)	(0.029)	(0.024)
Cooperating Teacher # Quartile 2	0.026*	-0.003	-0.028	-0.020+
	(0.013)	(0.009)	(0.019)	(0.011)
Cooperating Teacher # Quartile 3	0.030**	0.008	-0.004	0.004
	(0.011)	(0.008)	(0.019)	(0.010)
Cooperating Teacher # Quartile 4	0.023**	0.031*	0.069*	0.027+
	(0.009)	(0.014)	(0.027)	(0.015)
Mean Outcome	3.885	0.040	0.081	0.021
Standard Deviation	0.582	0.379	0.515	0.252
Year Fixed Effects	Yes	Yes	Yes	Yes
Teacher Fixed Effects	Yes	Yes	Yes	Yes
Experience Fixed Effects	Yes	Yes	Yes	Yes
N	233149	117034	52933	60082
R-Squared	0.748	0.662	0.671	0.585
Adjusted R-Squared	0.643	0.527	0.521	0.398

Note. Robust standard error clustered by teacher in parentheses. Cooperating teacher is a time-varying indicator taking the value of 1 during the school year in which a teacher is reported as serving as a cooperating teacher. Experience is included as single indicators for years 0-30 and as a pooled indicator for experience above 30 years. We drop singleton observations from models with teacher fixed effects. Quartile are calculated for each outcome using the teacher fixed effect from a regression that includes an indicator for being a cooperating teacher, time-varying school characteristics, teacher fixed effects, and year fixed effects. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Table 7. Heterogeneity by School Type for RQ1

	(1) Observation Ratings	(2) TVAAS All Subjects	(3) TVAAS Math	(4) TVAAS ELA
Cooperating Teacher # Elementary School	0.052***	0.002	-0.017	-0.002
	(0.009)	(0.008)	(0.014)	(0.011)
Cooperating Teacher # Middle School	0.032*	0.002	-0.006	-0.004
	(0.016)	(0.009)	(0.022)	(0.011)
Cooperating Teacher # High School	0.022	0.021	0.079	-0.016
	(0.014)	(0.016)	(0.058)	(0.015)
Cooperating Teacher # Other School	0.031	0.033	0.009	0.016
	(0.025)	(0.023)	(0.045)	(0.035)
Mean Outcome	3.882	0.065	0.135	0.037
Standard Deviation	0.570	0.356	0.494	0.238
Year Fixed Effects	Yes	Yes	Yes	Yes
Teacher Fixed Effects	Yes	Yes	Yes	Yes
Experience Fixed Effects	Yes	Yes	Yes	Yes
N	170464	87080	38666	44243
R-Squared	0.771	0.689	0.700	0.618
Adjusted R-Squared	0.639	0.534	0.527	0.402

Note. Robust standard error clustered by teacher in parentheses. Cooperating teacher is a time-varying indicator taking the value of 1 during the school year in which a teacher is reported as serving as a cooperating teacher. Experience is included as single indicators for years 0-30 and as a pooled indicator for experience above 30 years. We drop singleton observations from models with teacher fixed effects. Quartile are calculated for each outcome using the teacher fixed effect from a regression that includes an indicator for being a cooperating teacher, time-varying school characteristics, teacher fixed effects, and year fixed effects. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Table 8. Effects of Serving as a Cooperating Teacher on Growth of Evaluation Metrics

	Observation Ratings		TVAAS - All Subjects		TVAAS - Math		TVAAS - ELA	
	(1) Teacher FE	(2) Diff-in-Diff	(3) Teacher FE	(4) Diff-in-Diff	(5) Teacher FE	(6) Diff-in-Diff	(7) Teacher FE	(8) Diff-in-Diff
Cooperating Teacher	0.046***	0.049***	-0.002	-0.004	-0.010	-0.013	-0.011	0.001
	(0.009)	(0.009)	(0.007)	(0.008)	(0.015)	(0.015)	(0.008)	(0.008)
After Cooperating Teacher	0.015	-0.005	-0.020*	-0.030***	-0.016	-0.024	-0.017*	-0.007
	(0.009)	(0.010)	(0.008)	(0.009)	(0.017)	(0.016)	(0.008)	(0.008)
Ever Cooperating Teachers		0.112***		0.058***		0.091***		0.032***
		(0.009)		(0.008)		(0.014)		(0.006)
Mean Outcome	3.885	3.885	0.040	0.040	0.081	0.063	0.021	0.021
Standard Deviation	0.582	0.582	0.379	0.379	0.515	0.502	0.252	0.252
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Teacher Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Experience Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
N	233149	242339	117034	127669	52933	61943	60082	69642
R-Squared	0.748	0.292	0.662	0.125	0.671	0.194	0.585	0.131
Adjusted R-Squared	0.643	0.286	0.527	0.111	0.521	0.167	0.398	0.105

Note. Robust standard error clustered by teacher in parentheses. Cooperating teacher is a time-varying indicator taking the value of 1 during the school year in which a teacher is reported as serving as a cooperating teacher. Experience is included as single indicators for years 0-30 and as a pooled indicator for experience above 30 years. We drop singleton observations from models with teacher fixed effects. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

APPENDICES

Appendix Table 1. Robustness Checks of Teacher Fixed Effects Models on Different Subsamples of Teachers

	(1) Main Effects	(2) CT Blocks	(3) CT Schools	(4) CT Ever
Panel A. Observation Ratings				
Cooperating Teacher	0.040***	0.028***	0.030**	0.035***
	(0.007)	(0.007)	(0.009)	(0.007)
Mean Outcome	3.870	3.868	3.878	4.022
Standard Deviation	0.578	0.570	0.534	0.485
Year Fixed Effects	Yes	Yes	Yes	Yes
Teacher Fixed Effects	Yes	Yes	Yes	Yes
Experience Fixed Effects	Yes	Yes	Yes	Yes
N	174214	81513	19880	10127
R-Squared	0.771	0.784	0.816	0.741
Adjusted R-Squared	0.639	0.638	0.671	0.603
Panel B. TVAAS – All Subjects				
Cooperating Teacher	0.008	0.008	0.008	0.007
	(0.006)	(0.006)	(0.008)	(0.006)
Mean Outcome	0.063	0.068	0.077	0.113
Standard Deviation	0.358	0.326	0.309	0.305
Year Fixed Effects	Yes	Yes	Yes	Yes
Teacher Fixed Effects	Yes	Yes	Yes	Yes
Experience Fixed Effects	Yes	Yes	Yes	Yes
N	91726	52838	11817	7042
R-Squared	0.689	0.709	0.728	0.668
Adjusted R-Squared	0.541	0.537	0.522	0.517

Note. Robust standard error clustered by teacher in parentheses. Cooperating teacher is a time-varying indicator taking the value of 1 during the school year in which a teacher is reported as serving as a cooperating teacher. Experience is included as single indicators for years 0-30 and as a pooled indicator for experience above 30 years. Models (1) and (4) include singleton observations by teacher-year. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Appendix Table 2. Nearest Neighbor Matching Quality

	Mean		Variance	
	Raw	Matched Sample	Raw	Matched Sample
Panel A. Observation Ratings				
OR - Two Years Prior	-0.847	0.000	1.874	1.000
OR - One Year Prior	0.271	0.002	0.613	1.004
Years of Experience	0.175	0.016	0.849	1.017
N	45,220	4,608		
Panel B. TVAAS				
TVAAS - Two Years Prior	-0.097	0.012	0.503	1.135
TVAAS - One Year Prior	0.255	0.013	0.777	1.180
Years of Experience	0.181	0.008	0.866	0.987
N	18,424	2,236		
Panel C. TVAAS Mathematics				
TVAAS Math - Two Years Prior	-0.127	0.015	0.427	1.088
TVAAS Math - One Year Prior	0.186	0.007	0.757	1.122
Years of Experience	0.172	0.021	0.865	0.987
N	8,753	988		
Panel D. TVAAS ELA				
TVAAS ELA - Two Years Prior	-0.036	0.009	0.704	1.081
TVAAS ELA - One Year Prior	0.265	0.008	0.823	1.126
Years of Experience	0.162	0.016	0.817	0.969
N	9,684	1,176		

Note. This table reports the standardized difference between CTs and non-CTs on the variables we used to construct the nearest neighbor matched sample. Values close to zero for the matched sample means and close to 1 for the matched sample variance indicate that the matching procedure was able to identify a similar non-CT sample to the observed CT sample.

Appendix Table 3. Monte Carlo Simulation

	(1) Observation Ratings		(2) TVAAS - All Subjects		(3) TVAAS - Math		(4) TVAAS - ELA	
Placebo CT # Q1	-0.003	[-0.031, 0.027]	-0.006	[-0.033, 0.024]	0.004	[-0.058, 0.063]	-0.019	[-0.046, 0.009]
Placebo CT # Q2	-0.008	[-0.032, 0.016]	-0.006	[-0.019, 0.009]	0.001	[-0.030, 0.030]	-0.012	[-0.029, 0.004]
Placebo CT # Q3	-0.004	[-0.024, 0.017]	0.002	[-0.011, 0.015]	0.010	[-0.017, 0.039]	-0.004	[-0.021, 0.012]
Placebo CT # Q4	0.002	[-0.016, 0.020]	0.006	[-0.015, 0.028]	0.007	[-0.033, 0.047]	-0.005	[-0.030, 0.020]

Note. This table reports the results of a Monte Carlo simulation that draws 1000 placebo CTs and calculates the placebo effect of serving as a CT on evaluation scores. The model adjusts for year-fixed effects. Quartiles are calculated using the method that we used to estimate the heterogeneity by quartile using evaluation years 2011-2015. 95% credible intervals are in brackets. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Appendix Table 4. Robustness Checks of Growth Models on Different Subsamples of Teachers

	(1) Main Effects	(2) CT Blocks	(3) CT Schools	(4) CT Ever
Panel A. Observation Ratings				
Cooperating Teacher	0.046***	0.040***	0.034*	0.032**
	(0.009)	(0.010)	(0.013)	(0.011)
Years following serving as a CT	0.015	0.027+	0.007	-0.003
	(0.009)	(0.014)	(0.019)	(0.016)
Mean Outcome	3.885	3.847	3.841	4.038
Standard Deviation	0.582	0.582	0.561	0.497
Year Fixed Effects	Yes	Yes	Yes	Yes
Teacher Fixed Effects	Yes	Yes	Yes	Yes
Experience Fixed Effects	Yes	Yes	Yes	Yes
N	233149	81807	19880	13157
R-Squared	0.748	0.784	0.816	0.707
Adjusted R-Squared	0.643	0.638	0.671	0.599
Panel B. TVAAS – All Subjects				
Cooperating Teacher	-0.002	-0.001	0.009	0.010
	(0.007)	(0.008)	(0.011)	(0.009)
Years following serving as a CT	-0.020*	-0.018+	0.005	0.008
	(0.008)	(0.011)	(0.016)	(0.013)
Mean Outcome	0.040	0.048	0.046	0.093
Standard Deviation	0.379	0.349	0.330	0.314
Year Fixed Effects	Yes	Yes	Yes	Yes
Teacher Fixed Effects	Yes	Yes	Yes	Yes
Experience Fixed Effects	Yes	Yes	Yes	Yes
N	117034	52918	11817	8468
R-Squared	0.662	0.709	0.728	0.645
Adjusted R-Squared	0.527	0.537	0.522	0.515

Note. Robust standard error clustered by teacher in parentheses. Cooperating teacher is a time-varying indicator taking the value of 1 during the school year in which a teacher is reported as serving as a cooperating teacher. Experience is included as single indicators for years 0-30 and as a pooled indicator for experience above 30 years. In addition, experience variable is interacted with the years following indicator, allowing differential returns to experience after a teacher first serves as a cooperating teacher. Models (1) and (4) include singleton observations by teacher-year. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Appendix Table 5. Two-Stage Estimation for Cooperating Teacher Growth Trajectories

	Observation Ratings		TVAAS All Subjects		TVAAS Math		TVAAS ELA	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Teacher FE	Diff-in-Diff	Teacher FE	Diff-in-Diff	Teacher FE	Diff-in-Diff	Teacher FE	Diff-in-Diff
Cooperating Teacher	0.051	0.048	-0.002	-0.011	-0.015	-0.015	-0.012	-0.008
After Cooperating Teacher	0.023	-0.005	-0.021	-0.043	-0.025	-0.038	-0.020	-0.027
Ever Cooperating Teachers		0.112		0.066		0.098		0.043

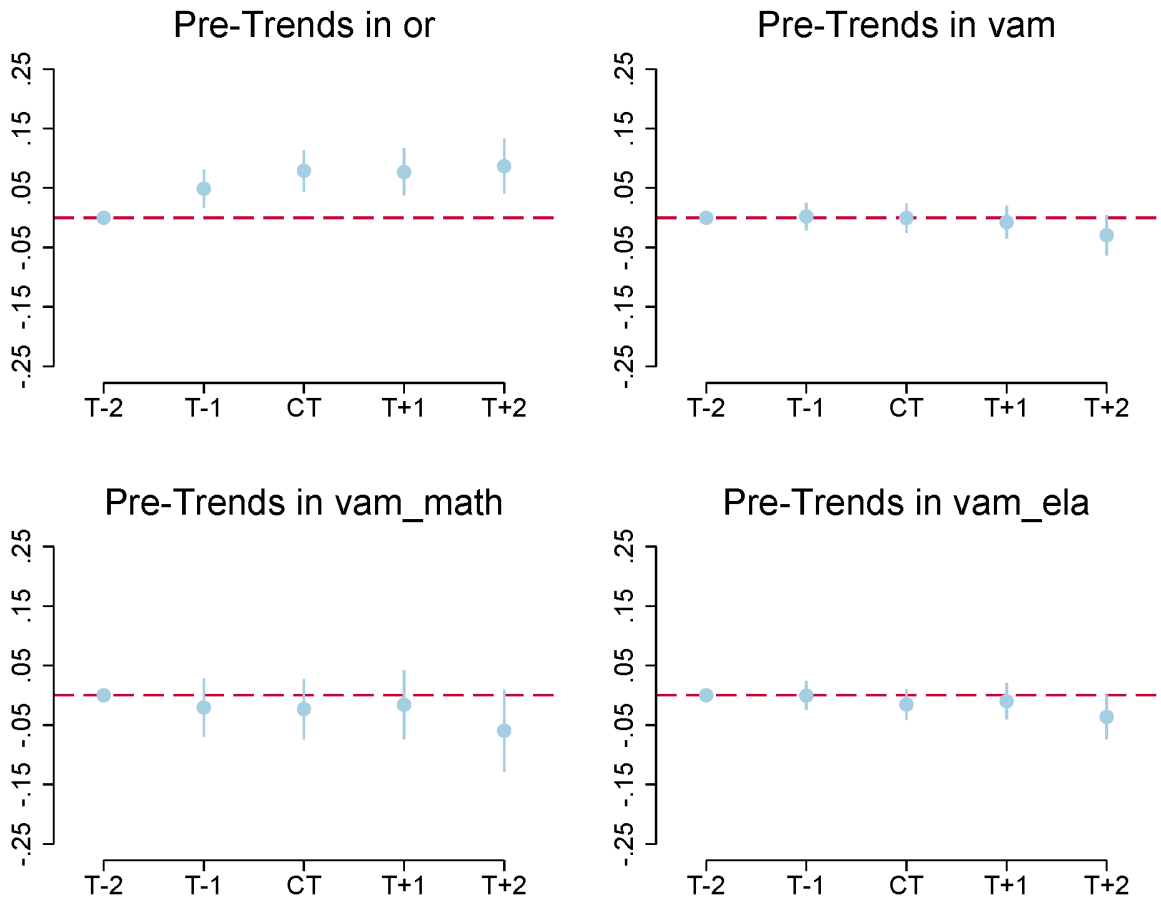
Note. Standard errors are not reported because they are not calculated for the two-stage Papay-Kraft estimation correction. Cooperating teacher is a time-varying indicator taking the value of 1 during the school year in which a teacher is reported as serving as a cooperating teacher. Experience is included as single indicators for years 0-30 and as a pooled indicator for experience above 30 years. We drop singleton observations from models with teacher fixed effects.

Appendix Table 6. Heterogeneity by School Type and Quartile

	Observation Ratings				TVAAS - All Subjects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Elementary School	Middle School	High School	Other School	Elementary School	Middle School	High School	Other School
CT # Quartile 1	0.153***	0.012	0.051	0.032	0.003	-0.011	-0.007	-0.008
	(0.030)	(0.018)	(0.035)	(0.025)	(0.020)	(0.012)	(0.013)	(0.027)
CT # Quartile 2	0.058	0.025+	0.041	0.027	0.081*	0.004	0.002	0.028
	(0.051)	(0.014)	(0.026)	(0.025)	(0.037)	(0.012)	(0.013)	(0.021)
CT # Quartile 3	0.089*	0.035**	0.029	-0.005	0.009	0.010	0.005	0.088**
	(0.037)	(0.012)	(0.024)	(0.019)	(0.038)	(0.018)	(0.024)	(0.034)
CT # Quartile 4	0.006	0.104*	0.042	-0.020	0.036	0.018	-0.039	0.077
	(0.027)	(0.048)	(0.050)	(0.109)	(0.051)	(0.018)	(0.031)	(0.052)
N	228417				112476			
R-Squared	0.749				0.663			
Adjusted R-Squared	0.644				0.523			

Note. Robust standard error clustered by teacher in parentheses. Cooperating teacher is a time-varying indicator taking the value of 1 during the school year in which a teacher is reported as serving as a cooperating teacher. Experience is included as single indicators for years 0-30 and as a pooled indicator for experience above 30 years. We drop singleton observations from models with teacher fixed effects. Quartile are calculated for each outcome using the teacher fixed effect from a regression that includes an indicator for being a cooperating teacher, time-varying school characteristics, teacher fixed effects, and year fixed effects. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Appendix Figure 1. Event Study of Serving as a CT on Outcomes of Interest



Appendix Figure 2. Density Distribution of Fuzzy Matched Covariates