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March 2025

WORKING PAPER No. 312-0325

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Acknowledgments

This study was funded by the Bill and Melinda Gates Foundation through a grant to the American Institutes for Research and we are grateful for this support. This study uses administrative data from three anonymous states. The findings from the study do not necessarily reflect the views of the Gates Foundation, the data providers, or the institutions to which the authors are affiliated. The authors wish to thank Nathaniel Marcuson and Nate Brown for research assistance and Maia Goodman Young for comments on an earlier draft.

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CALDER Working Paper No. 312-0325
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Abstract

We implemented two reforms to student teaching in randomized controlled trials designed to test improvements to pre-service preparation. Although neither reform affected overall teacher employment, we find significant effects on other labor market outcomes. The first intervention placed student teachers with more effective mentors and in more effective schools for their clinical experiences. We find that treated candidates tended to find employment in higher income and less diverse schools, an effect that appears to operate primarily among teachers not hired into their placement sites. The second intervention provided detailed reports to teacher candidates, their field instructors, and their school-based mentor teachers about performance on clinical evaluations during the clinical experience. Treated candidates in this experiment were more likely to obtain teaching positions in the schools in which they completed their student teaching. Overall, the results suggest that student teaching reforms may have important and unexpected effects on the teacher labor market.

1. Introduction

Advocates and policymakers have recently called for reforms to the student teaching experience to better develop the skills of prospective teachers.[1] These reforms are grounded in research showing relationships between a variety of characteristics of placement schools, mentors, and clinical teaching experiences and the performance of early-career in-service teachers. Changes to the selection or recruitment procedures for clinical placements may therefore improve the skill development – and hence job prospects – of prospective teachers (Ronfeldt et al., 2020). But because teacher labor markets are very localized (Boyd et al., 2005; Goldhaber et al., 2014), changes to the location of student teacher placements also have the potential to influence teacher employment. Yet we currently know very little about how reforms to the student teaching experience may affect the labor market.

We assess the effects of two reforms to student teaching implemented as part of a randomized controlled trial, the Improving Student Teaching Initiative (ISTI). The first initiative (the "Placement Initiative") changed where student teachers were placed for their clinical teaching experience and with whom they worked. We ranked potential placements using a weighted average of mentor teacher and placement school characteristics and randomly assigned candidates to placements that we predicted, based on prior evidence, to be more promising ("high-index") or less promising ("low-index"). The second intervention (the "Feedback Initiative") provided detailed feedback reports to teacher candidates, their field instructor, and their school-based mentor teacher about their performance on evaluations during the clinical experience. The reports contextualized teacher candidates' ratings and compared them to others in their same cohort, information which was not previously available to school-based officials.

[1] See, for instance, Greenberg and Walsh (2011), Klein (2017), and NASEM (2022).

The interventions were assigned separately to different groups of candidates, allowing us to identify the effects of each intervention on employment outcomes.

We find little evidence that either intervention affected overall employment of teacher candidates, but both initiatives affected *where* teacher candidates became employed. For the Placement Initiative, we find that high-index candidates were employed in higher income and less diverse schools following graduation. Given that high-index placements tended to be in communities with more affluence and fewer under-represented minority students and that many new teachers are employed by schools where they do their student teaching, one might assume that this difference in employment is driven by new teachers securing jobs at high-index placement schools. However, we demonstrate that this effect is primarily driven by candidates employed in schools *other* than their placement sites. Surveys of student teachers indicate that the placement site meaningfully changed their preferences over job characteristics.

The findings from the Feedback Initiative, by contrast, show that teacher candidates were about 6 percentage points more likely to obtain teaching positions in the schools in which they completed their clinical teaching experience. One possible explanation for the findings on being hired into internship schools is that the Feedback Initiative changes information about the quality of teacher applicants and hence the probability of being hired. But while we find that teacher candidates who receive higher evaluations are more likely to find employment overall, there is little evidence that employment effects are driven by candidates receiving positive evaluations in the treatment group, or that teacher candidates with relatively positive evaluations in the treatment group are disproportionately likely to be employed in their internship schools.

In sum, our findings suggest that reforms to the clinical experiences may have unanticipated labor market effects. Changing student teaching placements or the clinical

evaluation process may affect teacher candidates' job preferences, the networks of teachers and principals who recommend them for permanent positions, or the amount of information that is available to schools about job candidates. These findings further support the idea that policymakers may want to explicitly consider school placements as a potential lever to address staffing challenges and the equity of the teacher quality distribution (Goldhaber et al., 2021; Krieg et al., 2016; Ronfeldt et al., 2020). But they also suggest that policymakers should be mindful of the fact that the student teaching experience affects both professional development and the teacher labor market. In addition to honing their teaching skills, candidates are vying for jobs and learning about their own preferences over schools. Policies that affect where they complete their clinical experiences, whom they work with, or the informational content of clinical evaluations may also affect where candidates become employed.

2. Background and Prior Literature

2.1 Improving Student Teaching Initiative

The Improving Student Teaching Initiative (ISTI) consisted of two randomized experiments designed to test whether high quality student teaching placements and increasing the information provided during clinical evaluations affected teacher candidates' feelings of preparedness, preservice clinical evaluations, early career teaching effectiveness, and employment.

In the first intervention – the Placement Initiative – we randomized teacher candidates from two teacher education programs (TEPs) to one of two lists of placements for their clinical teaching experience. The lists were derived from a set of potential placements that our partner TEPs were considering for their teacher candidates. Before assigning candidates, we linked potential placement schools and mentor teachers to historical administrative data and generated ranks to predict, based on a weighted average of teacher and school characteristics, which

placements would be more (high-index) or less (low-index) promising; importantly, TEP leadership identified all as being strong placements aligned with program requirements. The index included five components that prior literature found to be related to graduates' instructional effectiveness: 1) mentor teachers' formal observational/summative performance ratings, 2) mentor teacher value-added, 3) mentor teacher years of experience, 4) the placement school's value-added, and 5) school-based measures of teacher retention.[2] We standardized all variables and assigned each teacher characteristic a weight of ¼ and each school characteristic a weight of 1/8. The weights were based on the authors' subjective evaluation of the relationship between mentor teacher and school characteristics and the future performance of student teachers from prior research literature (Goldhaber et al., 2020a; Ronfeldt, 2012; Ronfeldt et al., 2018a,b).[3]

We designed the placement procedure to respect candidates' preferences for region, subject, and grade level and leave programs some flexibility in making final assignments. The TEPs organized the lists of potential placements and teacher candidates into randomization blocks consisting of region, grade level, and/or subject area placements. We asked TEPs to overrecruit placements to ensure adequate backup options if one of the potential placements fell through. We then ranked potential placements within each randomization block, assigning placements at the median or above to the high placement list and placements below the median to the low placement list. We randomly assigned candidates to either the high-index or low-index placement lists and returned both lists to the programs. The TEPs made the final placement assignments by assigning candidates to one of the sites on the prescribed lists. Beyond the placement lists, we did not provide guidance to programs on individual candidate assignments.

[2] In the first program, the evaluation data is the continuous score on the state's observational teaching rubric. In the second program, the evaluation is the teacher's performance evaluation result, which assigns teachers one of four potential ratings. The stay-ratio is a measure of turnover among non-retirement aged teachers.
[3] For a fuller discussion of the literature please see Goldhaber et al. (2020c).

As we show in the Data section, there were significant differences in assigned placement characteristics between the two placement lists.

The second intervention – the Feedback Initiative – adjusted clinical evaluation procedures for providing feedback about candidate performance; clinical evaluations are completed by mentor teachers and field supervisors during student teaching. We randomly assigned candidates from six TEPs in three states to either receive tailored feedback about their student teaching performance or to a "business as usual" condition. In both conditions, candidates were evaluated several times during the clinical experience by their university-based field instructor and school-based mentor teacher using TEPs' pre-existing rubrics. In all cases, the rubrics were at least partially aligned with in-service observational rubrics used in the state.

The evaluation process differed in two ways for candidates in the treatment condition. First, after each clinical evaluation cycle, we emailed automatically generated reports to teacher candidates, mentor teachers, and field instructors based upon the clinical evaluation data (see Figure 1 for a sample report). To construct these reports, we obtained clinical evaluation scores for all teacher candidates, regardless of treatment status, after each cycle. Each report contained three mandatory sections and one optional section. The first section displayed a candidate's average performance in each domain and indicated their performance relative to others in their cohort. We used a sequence of emojis to quickly convey information about a candidate's relative performance in each domain. If a candidate performed in the 50^{th} to 75^{th} percentile on a domain relative to their evaluated peers, this domain or item received one smiley-face emoji, and if a candidate performed in the top quartile, it received two emojis. The next two sections indicated domains in which candidates were either struggling or excelling relative to their performance in other domains. Because the domains were identified relative to a candidate's own performance,

each candidate received reports with a domain identified in each section. Within their best scoring and worst scoring domains, the reports depicted how the candidate was performing on their top 5 best or worst items.[4] In addition to these sections, we included one optional section if mentor teacher and field instructor ratings differed substantially on any domain. In this case, we flagged these items and recommended additional discussion and feedback.

The second departure from the business-as-usual condition was that we requested that mentor teachers and field supervisors conduct at least one joint observation where each independently evaluated the same lesson. In some programs, this was already a required practice, in which case we requested one additional joint evaluation for candidates in the treatment condition. After the evaluation, we asked field instructors and mentor teachers to hold a joint debriefing session with the candidate to review the evaluation results and provide additional feedback and guidance. The goal of these joint sessions was to identify discrepancies between the ratings provided by field instructors and mentor teachers and reconcile them. The discussion section of the feedback report, which identified large discrepancies, was intended to support these discussions.

2.2 Prior literature

The overall objective of ISTI was to experimentally test potential improvements to the clinical teaching experience to determine if they affect teacher candidate development, employment, in-service performance, and retention.[5] One straightforward way that the interventions may have affected employment outcomes is by improving the preparation of teacher candidates. These initiatives were informed by two lines of prior research examining the

[4] Not all rubrics had the same number of domains or the same number of items. If a domain did not have 5 items nested within it, all items from the domain were listed in the report.
[5] For more information on ISTI as a whole, see Goldhaber and Ronfeldt (2020).

effects of preservice clinical teaching placements and in-service evaluation systems on teaching effectiveness. These research literatures have found that mentorship and feedback can improve teaching practice.

The clinical teaching experience is the capstone of traditional teacher preparation and is widely considered among the most foundational development experiences (Anderson & Stillman, 2013). Teacher candidates who report better experiences are more confident in their ability to assume independent classroom teaching responsibilities (Ronfeldt et al., 2014, 2020; Ronfeldt & Reininger, 2012). Teacher candidates who complete their student teaching requirement in schools with better professional climates or more effective mentor teachers also perform better when they enter the classroom as fulltime teachers (Goldhaber et al., 2020a,b; Ronfeldt 2012, 2015). The Placement Initiative generated experimental variation in these aspects of the clinical teaching experience. In prior work, we found that assignment to the high-index placement list improved candidate performance on their clinical evaluations (Goldhaber et al., 2022).

Another line of research, generally focusing on in-service teachers, has found that providing feedback on performance can improve teachers' instructional effectiveness. Taylor and Tyler (2012) study the introduction of an evaluation reform in Cincinnati that improved the quality of professional feedback in a low-stakes environment. Teachers' contributions to student test scores increased by about 0.10 standard deviations following participation in the evaluation cycle. Steinberg and Sartain (2015) provide complementary evidence from a program in Chicago. Although the Feedback Initiative was more modest than the formal teacher evaluation cycles assessed in these studies, they do suggest that improving the quality of feedback that teachers receive can meaningfully improve their practice.

If either initiative improved candidates' teaching skills, we might also expect them to affect employment prospects. Prior studies have found that teacher skills, as measured by various pre-service assessments, do predict the likelihood of employment as a teacher even when such information is not directly available to hiring officials (Bartannen & Kwok, 2020; Boyd et al., 2011; Goldhaber et al., 2017). This effect might operate through multiple channels. If hiring officials can predict teaching effectiveness before teachers take positions, teachers in the Placement or Feedback Initiatives might be better positioned to obtain teaching positions. Rockoff et al. (2011), for instance, show that the kind of information typically collected during the application process does predict classroom performance, but other studies have found mixed evidence on the extent to which school systems typically act on this information in hiring decisions (Jacob et al., 2018; James et al., 2023). Improving teacher preparation might also affect the preferences of teacher candidates. Teachers who report more self-efficacy are more satisfied with their working environments and less likely to leave the profession (Tschannen-Moran et al., 1998). Hence, candidates who had better experiences during preparation may be more likely to seek out full-time teaching positions upon graduation.

Even if the initiatives had no effect on teaching proficiency, two other aspects of ISTI may have affected teacher candidate labor market outcomes. First, the Placement Initiative affected where candidates completed their student teaching experiences and with whom they worked. Prior work has shown that these experiences predict candidates' preferences for the characteristics of the schools in which they seek employment (Ronfeldt et al., 2016). Second, the Feedback Initiative increased the salience of information about candidate performance to potential employers. Each of these reforms may have effects on teachers' employment outcomes independently of their potential effects on the development of teaching skills.

Changing the location of student teaching placements may affect where teachers obtain positions through several mechanisms. Maier and Youngs (2009) argued that preparation programs construct social networks among candidates, in-service teachers, and school administrators that may aid candidates in their job search and affect the set of schools in which they obtain permanent teaching positions. This line of argument is consistent with empirical evidence from the labor economics literature finding that job prospects propagate through social networks (Gee et al., 2017; Schmutte, 2015). The Placement Initiative affected the location of the clinical teaching site, which might have affected candidates' subsequent employment. Prior research has found that about 15-20% of teacher candidates obtain a teaching position in their placement school (Krieg et al., 2016; Matsko et al., 2022). The clinical teaching experience may also have affected candidates' preferences over job types. Although unintended, the high-index list included higher income schools than the low-index list. Some teachers appear to prefer working in schools with more advantaged students (Hanushek et al., 2004; Goldhaber et al., 2019; Jackson, 2009; Lankford et al., 2002). But exposing teacher candidates to more diverse educational environments might have changed their preferences by making them more familiar with different kinds of schools (Bornstein, 1989). Ronfeldt et al. (2016), for example, found that student teaching in field placements with more ELL students predicted candidates' preferences to work in schools with more ELL students after graduating. Candidates may be especially influenced by these experiences if their placements occur in well-organized schools with strong working conditions (Horng, 2009).

The evaluation process that occurs during the clinical experience may also provide potential employers with additional information about student teachers. In the Feedback Initiative, we provided mentor teachers with regular reports that compared the clinical

evaluations of the teacher candidates to peers in their same cohort. Although the reports were intended to improve the quality of feedback provided to teacher candidates, they may also have provided potential employers, either directly or indirectly, with additional signals about candidates' performance.[6] Studies have found that providing information to schools about teacher effectiveness affects employment decisions. Loeb et al. (2015), for instance, found that a policy providing additional performance information to principals making tenure recommendations affected tenure rates among lower performing teachers.[7] These findings are consistent with other research showing changes in teacher retention following the introduction of even low stakes teacher evaluation policies (Cullen et al., 2021).

There is relatively little evidence on the role of performance information in affecting decisions about whom to hire, but Jacob et al. (2018), who study the hiring process in the District of Columbia Public Schools, find that teachers who are assigned to recommended hiring lists based on preservice measures are more likely to obtain teaching positions. They also find that performance on the screening measures – which are made available to the district – do not strongly predict teacher employment conditional on recommendation status.[8] Hence, there is some uncertainty about whether providing additional information about teacher performance directly to hiring authorities in less formal circumstances would affect hiring decisions.

[6] We note that mentor teachers are often not the people who sit on hiring committees; however, it is possible that involving mentors in the feedback process may make them more likely to advocate on the behalf of their student teachers during hiring seasons.

[7] See also Rockoff et al. (2012), who evaluated a random assignment pilot experiment where some principals received estimates of value added for teachers in their school and found that principals' assessments of teacher performance became more aligned with value added in the treatment schools and that separations increased among low performing teachers.

[8] A few studies have found that the information typically available to principals, such as employment screeners, sample lessons, and reference letters, do predict teacher performance (Bruno and Strunk, 2019; Goldhaber et al., 2017; Jacob et al, 2018; Rockoff et al., 2011; Sajjadiani et al., 2019). There is less information about the effects of these on hiring policies.

Compared to a centralized hiring process with formal review criteria, the intervention we study is relatively low touch.

3. Data and Sample Description

3.1 Experimental design

We conducted ISTI as a randomized controlled trial with eight participating TEPs in three states during the 2016-17 and 2017-18 academic years. We randomized within each TEP, which is key to estimating causal effects on early career workforce outcomes (Goldhaber & Ronfeldt, 2020). In a non-experimental setting, estimates of the effects of evaluations or placements might be biased if more effective teacher candidates sort to particular TEPs, mentor teachers, or placement schools. The randomization was carried out slightly differently in each of the TEPs depending on participation in the two initiatives, although we stratified assignment of the feedback treatment by placement status when programs participated in both initiatives.

For the TEPs and cohorts that participated in the Placement Initiative, we first randomized candidates to the placement lists within placement blocks. In the first program, the placement blocks were defined by school district, grade level, and field. In the second program, where school districts are significantly larger and all candidates were enrolled in an elementary education program, we randomized by region of the surrounding school district. The randomization procedure operated slightly differently in the first and second years of the experiment. In the first year of the study, for a placement block of size n, we randomly assigned $n/2$ (or $(n+1)/2$ if n were odd) candidates to the high placement list; in the second year of the study, the probability of assignment to the high placement list was 0.5 for each candidate, regardless of the number of candidates in their block. At the first program, 26 candidates initially registered for the residency but dropped out before they were assigned a placement by the TEP; these candidates were dropped from the study.

We did not identify which list was the high-index list when we returned them to the programs; additionally instructional effectiveness information on mentor teachers is not publicly available so program leaders could not have used this kind of information to infer low- or high-index placements. Importantly, neither the candidates, field instructors, nor mentor teachers were aware of the research design and would not have known their treatment status for the Placement Initiative. They were informed only that the university was working with research partners on some revisions to the student teaching assignment process. Once we completed randomization of the Placement Initiative, we conducted randomization to the feedback conditions within placement blocks and placement lists to ensure balance in the feedback condition across treatment status.

For the programs participating only in the feedback experiment, we randomized candidates by TEP and cohort to either the high feedback group or business as usual. Because we did not intervene in the placement process, the assignment of candidates by grade level or field was not an impediment to randomization. For most programs, therefore, the randomization blocks consist of cohorts of teacher candidates. One TEP requested that we balance the additional workload across university supervisors. We therefore treated supervisors as the randomization block for this program in both the assignment process and for the analyses.

3.2 Data and experimental balance

Both initiatives spanned two academic years. The Placement Initiative consisted of 379 teacher candidates (of whom 95% attended the first TEP), while the Feedback Initiative consisted of 685 teacher candidates. We linked candidates to state administrative employment records in state public schools.[9] In total, 272 teacher candidates from the Placement Initiative

[9] In two states, this was done by the research team matching candidates by name against employment records. In one state this was done in accordance with a data sharing agreement and in another it was done using publicly available

and 457 candidates from the Feedback Initiative were observed as entering the public school workforce in the state in which they did their preparation within 4 years of graduating.[10] Note that we cannot determine if teacher candidates may have been employed as teachers outside of public schools or in public schools in a different state.

We then matched employment records to school level data using the Common Core of Data (CCD). We obtained data on the race/ethnicity of students and the School Neighborhood Poverty Index (SNPI) from the CCD, which provide the income-to-poverty ratio for neighborhoods around school buildings.[11] The index indicates the relative percent of the poverty line for the average household in the neighborhood. For example, a value of 300 means that the average household income in the neighborhood is three times the poverty line.[12] We also merged school level information to internship schools. In cases where teacher candidates had multiple student teaching experiences at different schools, we used the average of these school characteristics.[13]

We also conducted surveys of candidates before and after the clinical teaching experience. Given our limited ability to reach students during the summer, the pre-internship survey was launched after randomization of candidates to the Feedback and Placement Initiatives

data. The Department of Education in the third state matched candidates to state teacher employment data by name and preparation program and returned the matched list to the research team.

[10] As we describe in the methods section, different cohorts of teacher candidates have more or fewer years of post-graduating data available. The employment rates for two of the states for which the authors have administrative data for larger samples (64% and 69%) are similar to rates for all teachers in the respective states (70% and 60-80%, respectively).

[11] Across the two experiments, 24 teacher candidates could be merged to a district, but not to an individual school. For these cases, the district average was used in place of school-level data.

[12] The advantage of SNPI over eligibility for free-or-reduced-priced lunch is that recent changes to school lunch eligibility due to the Community Eligibility Program introduced measurement error into the free-or-reduced priced lunch indicators (Koedel & Parsons, 2021).

[13] Because teacher candidates in the Placement Initiative were randomized in a way that included mentor teacher characteristics, all teacher candidates have matching internship school characteristics. Some teacher candidates in the Feedback Initiative, 4%, due to spelling mistakes, abbreviations, or multiple distinct schools with the same name could not be reliably matched to an internship school and are thus excluded from models that leverage these data.

but before the beginning of the clinical teaching experience. In some cases, we kept surveys open during the first few weeks of the internship to increase response rates. The post-internship survey was conducted over the last few weeks of the internship. The response rate was approximately 60% on each survey across all participating TEPs. The surveys covered many topics including satisfaction with student teaching, aspects of the relationship with mentor teachers and field instructors, preparedness to begin teaching, work plans for the next academic year, and preferences for job characteristics. We use information about future work plans and job preferences from the post-internship survey as additional outcome measures and responses from the pre-internship survey as control variables in some specifications.

Table 1 presents summary statistics for teacher candidates involved in the two initiatives. About 60% of teacher candidates ended up employed in each of the years following graduation. Similar proportions of candidates were employed in the first year after graduation. The probability of employment in the placement school does appear to vary by treatment status. Overall, about 10% of teachers were employed in their placement school in each of the years after graduation. This includes about 10% of high-index and 9% of low-index candidates and 11% of high-feedback and 7% of low-feedback candidates. Turning to the placement school characteristics, we see little difference in the school demographics across either treatment condition. Candidates in the high-index list completed their student teaching requirements in schools that were higher income and less diverse, but these differences are generally quite small (about 7% of a standard deviation on underrepresented minority enrollment and less than 1% of a standard deviation on the school poverty index). Differences between treatment and control groups are considerably smaller for the Feedback Initiative, which is unsurprising given that it did not influence placement assignments.

Table 2 presents balance tests of the teacher candidate characteristics for each of the initiatives. As evidenced from Panel A, the pre-experiment teacher candidate characteristics do not appear to systematically vary by high- or low-index status, suggesting that randomization was successful. However, to more formally test this, we conducted an omnibus test across all pre-experiment teacher candidate characteristics. We fail to reject the null hypothesis of differences in candidate characteristics across assigned placement lists (p = 0.20). Panel B presents the pre-experiment characteristics of teacher candidates by low or high feedback statuses. The omnibus balance test yields p-value of 0.42, suggesting that there was no systematic sorting of teacher candidates to the high or low feedback conditions.

4. Methods

Given the experimental design, we estimate intent-to-treat effects for the interventions by regressing employment outcomes on treatment assignment and randomization block indicators:

$$Y_i = \beta_0 + D_i\beta_1 + B_i\beta_2 + \varepsilon_i \qquad (1)$$

In Eq. (1), Y_i is an employment outcome for candidate i, D_i is a binary indicator for whether the teacher candidate i was assigned to the high index list ("Placement Initiative") or the high feedback condition ("Feedback Initiative"), and B_i is a vector of randomization blocks (these vary across the participating TEPs, but include: subject region and grade level; geographic area; and field instructor).[14] We pool data across the available years of data and cluster standard errors by teacher candidate. Because not all TEPs participate in both initiatives and the sample for each initiative differs, we estimate Eq. (1) separately by initiative and specify D_i as a binary indicator for treatment assignment in the relevant intervention. Given the small samples

[14] Roughly 22% of teacher candidates in the Feedback Experiment had two student teaching placements at different schools. For the employment in the same school outcome, we consider employment at either student teaching schools. In one program, assignment to the Feedback Initiative was stratified by the university field instructor to ensure a balanced workload for each supervisor.

participating in both initiatives, we do not include a full factorial specification of the treatment vector in our preferred specifications, but the interaction effects (not significant) are available on request.

Our baseline models include no additional variables other than the randomization block indicators. To increase precision and remove biases from sampling differences in observables, we include a limited set of control variables in some specifications that includes proxies for several of the outcomes. Because we pool data from multiple years after graduation, we include indicators for each year after graduation.[15] We include an indicator for participation in the other initiative, although the randomization procedure ensured that treatment assignment in the other initiative was balanced across treatment groups, so this has little effect in practice. Finally, to increase precision, we include controls for a set of pre-treatment proxies for the outcomes using information from the pre-internship survey.[16] The pre-internship survey included one question about candidates' employment plans in the following year. We include an indicator for whether the candidate intended to work full-time as a teacher. The surveys also asked teacher candidates to rank up to three preferences for school characteristics in a full-time teaching position among a list of 14 options. We construct three indicators related to the outcome variables from this list. First, we indicate whether a teacher ranked their placement school among the top three choices ("The school in which I completed my residency or other field experience"). To match the percentage of underrepresented minority students, we combine preferences to work with African American students, Hispanic students, and English language learners. We also indicate whether candidates identified schools with many low-income students among their top three choices.

[15] For example, one candidate graduated in the Fall of the 2017, and another in the Winter of 2018, both would have their first full school year be the 2018-2019 school year.
[16] If teacher candidates did not fill out the survey, we dummy out the missing data.

Finally, in some analyses of the demographic characteristics of candidates' schools of employment, we include the same characteristic of the placement school. In the Placement Initiative, this amounts to adjusting for a post-treatment variable that is specifically targeted by the intervention and the estimated treatment effects should not be interpreted as an intent-to-treat effect in this case. Nonetheless, they are informative about the extent to which school placements directly mediated the effect of the intervention. For instance, to the extent that changes in employment school characteristics were driven by candidates hired into their placement schools, adjusting for placement school characteristics would tend to attenuate the estimated treatment effect. We discuss these results in more detail in Section 6.

5. Results

5.1 The Placement Initiative

We present estimates of the intent-to-treat effect of the Placement Initiative (that is, assignment to the high index list) on employment outcomes in Table 3. In columns (1) through (3), we show effects on overall employment. The first two columns show results for all available years, while the second column shows results for the first year after program completion. Taken at face value, the estimated treatment effects suggest that candidates on the high placement list were about 1 percentage point more likely to obtain employment after graduation, but none of the estimates is statistically significant and we can rule out effects larger than a 10-percentage point increase in employment. By comparison, about 60% of the sample is employed in public schools during each year in the sample window and nearly 70% is ever employed in public schools during this period of our data.[17]

[17] Beyond the initial year after the internship, we focus on employment in public schools rather that retention for employed teachers because the former is observable for every participant in our sample. One could instead examine effects on retention; however, this outcome is only available for those employed in public schools and is subject to sample selection biases if the initiatives affect employment outcomes. Thus, it is difficult empirically to separate

Columns (4) through (6) show estimates for models where the outcome is employment in the same school as the student teaching placement. In columns (4) through (5), we conduct this analysis using the full sample of teachers. The results in column (6) are limited to the first year of teaching. In all specifications, the difference in placement-school employment is about 3 percentage points and not statistically significant. The estimated effect is slightly larger during the first year after a teacher candidate's graduation, but again, these differences are not statistically significant. Note also that the effect on placement-school employment is larger than the effect on *any* employment, which suggests the Feedback Initiative caused some teacher candidates who would have become employed in other schools to take positions in their placement school instead.

In Table 4, we consider the effects of the Placement Initiative on employment school characteristics. Treated candidates in the Placement Initiative were somewhat more likely to serve in schools with higher values of the poverty index (that is, they worked in schools in higher income neighborhoods) and fewer underrepresented minority students during their internship. Employed candidates on the high-index lists worked in schools with lower enrollment of underrepresented minorities (URM) by about 5 percentage points (column 1), which is a relatively large difference given that the average URM enrollment is about 24% among control candidates in the sample. High-index candidates also were employed in lower poverty schools (column 4): the average neighborhood family income was higher by about 24% of the poverty threshold as compared to the schools of low placement list candidates. Results are not sensitive to the inclusion of controls for candidates' stated preferences from the pre-internship survey (columns (2) and (5)). Finally, in columns (3) and (6), we focus on the first year of employment.

effects on employment that originate from changes in initial employment from those that originate through reduced attrition.

The coefficient estimates from these models are similar to the comparable specifications for the full sample, suggesting that the findings we see across all years of employment are largely driven by the school in which teacher candidates initially find employment.

5.2 The Feedback Initiative

Table 5 provides estimates of the intent to treat effects of the Feedback Initiative (that is, assignment to the high feedback condition) on teacher candidate employment. In columns (1) through (3), we show effects on overall employment in the years following the intervention. The first two columns use all available years of data, while the third restricts the sample to the first year after the internship. Depending on the specification, we estimate effects between -2 and 2 percentage points, none of which are significant. These effects are relative to a baseline employment of about 60% across the sample window and we can rule out employment effects of about 8 percentage points. In columns (4) through (7), we estimate the effects of the Feedback Initiative on employment in the placement school. Overall, participation in the feedback experiment increased employment in the placement school by 5 percentage points, which is statistically significant at the 5 percent level. The estimated effects are slightly smaller and insignificant in the first year after graduation, but generally consistent across specifications. The discrepancies between the two sets of results are quite small but may reflect the fact that many candidates obtain part-time or temporary teaching positions before landing a full-time job (Goldhaber et al., 2022). Among teacher candidates in the control condition, approximately 7% were employed in their internship each year following graduation. The effect is thus approximately 70% of the baseline incident of same-school employment. When we limit the sample to employed teachers only (column 7), the Feedback Initiative increased employment in the placement school by 8 percentage points.

In Table 6, we show the effects of the Feedback Initiative on the characteristics of employment schools. We find little evidence of any effect on school characteristics. Columns (1) through (3) show that the high feedback condition appears to have no impact on the likelihood of candidates being employed in schools with high URM enrollment; the coefficients are small and not directionally consistent across specifications. Columns (4) through (6) show that while the high feedback condition is associated with employment in schools with lower income students, the difference is not significant. Although not significant, the direction of the effect is at least consistent with the observed effect on employment in the placement school found in Table 5: schools that serve as placement sites tend to be lower income on average than other schools in the state. Finally, effects do not appear to differ when we restrict the analysis to the year after graduation (columns 3 and 6).

6. Exploration of Mechanisms

In Section 2.2, we suggested several explanations for potential employment effects of the Placement and Feedback Initiatives. Either initiative may have led to general improvements in teaching practice, which could make candidates more desirable to potential employers. The interventions could also affect employment through more direct channels. These include changes in employment school characteristics for candidates hired directly into their placement schools through the Placement Initiative, changes in the preferences or referral networks of candidates, and changes in the performance information available to potential employers through the Feedback Initiative. In this section, we combine the data on employment outcomes with survey data on teacher candidates' job search to consider these potential mechanisms.

6.1 Employment location and school characteristics

The Placement Initiative directly affected where teacher candidates completed their clinical teaching experiences. Given that roughly 10% of candidates in our sample obtain

teaching positions in their placement school, it is possible that changes in the characteristics of placement schools could explain differences in the student demographics of the schools where teachers on the high placement list obtained employment. Although the differences in placement school characteristics in Table 1 do not appear significant enough to explain the differences in employment school characteristics in Tables 4 and 6, we test this more thoroughly in Table 7. Specifically, in columns (2) and (6), we add controls for underrepresented minority enrollment and the school poverty index for the candidate's placement school. Given the small differences in placement school characteristics between the lists, this adjustment has little impact on the estimated treatment effect. The estimated intent to treat effect becomes statistically insignificant for the percentage of underrepresented minority students, but the coefficient is only slightly attenuated from the corresponding estimate in Table 4. The estimated intent to treat effect on the school poverty index is slightly larger after adjusting for placement school characteristics.

In the next two columns, we investigate whether the effect of the Placement Initiative on employment school characteristics varies if the candidate was hired into their placement school. Specifically, we add an interaction between placement school employment and treatment status. These models provide evidence on whether the findings differ for candidates employed in or outside the schools in which they did their student teaching; the coefficient on treatment assignment provides the estimated effect for candidates *not* employed in the school in which they did student teaching, and the sum of this coefficient and the interaction term provides the effect for those who are employed in their student teaching school; note, however, that placement school employment is a post-treatment outcome, so these estimates do not have a clear causal interpretation as effects for recognizable candidate subgroups. Nonetheless, we find that the overall differential in the type of school in which teachers are employed is driven largely by

candidates *not* employed in the same school in which they did student teaching. High-index candidates not employed in their placement school are working in schools with lower URM enrollment (6 percentage points) and higher average family income (28% of the poverty threshold) than low-index candidates employed in their placement schools.

The estimated effect for candidates hired into their placement schools (the sum of the treatment assignment and placement school hire interaction) is close to zero for the main specifications (columns 3 and 7). Adding controls increases the effect somewhat, but it is still statistically insignificant and smaller than the main effect of placement list assignment. Put differently, school demographics are quite similar across treatment arms for candidates employed into their placement schools. Overall, the effect on employment school characteristics does not appear to operate through changes in the placement school directly.[18]

6.2 Employment effects by clinical evaluation results

The Feedback Initiative increased the likelihood that teacher candidates were employed in their placement school. One potential explanation is that the provision of information on teacher candidate performance changes the school's perception of the teacher candidates. We test this by estimating treatment effects by candidate performance on the clinical evaluations administered during the student teaching experience. An important caveat is that performance on the evaluations is a post-treatment outcome and could be influenced by candidates' feedback status, meaning that candidates in the treatment group might receive higher evaluations than those in the control because of the Feedback Initiative. Thus, changes in employment outcomes

[18] In both programs, randomization occurred within school districts, so it is not likely that the observed effects on school characteristics could be explained by differences in districts in which candidates were placed. Nonetheless, as an empirical check on this possibility, we conduct a chi-square test of the independence of Placement Initiative treatment status and district assignment. Consistent with the research design, we find little evidence that the initiative affected the districts in which candidates completed their student teaching ($p = 0.99$). Similarly, we find little evidence that placement status affected the district poverty rate of the clinical internship school.

among higher achieving teacher candidates could be driven by changes in the composition of the high achieving group. To partially mitigate these concerns, we identify candidates who received an average evaluation score above the median in their randomization block and treatment assignment. Nonetheless, even if the initiative had no effect on evaluation results, it may have provided additional information about how the candidates compared to others in their cohorts.

We then estimate a model with interactions between receiving a high evaluation score ($HiEval_i$) and treatment assignment:

$$Y_i = \beta_0 + D_i\beta_1 + B_i\beta_2 + HiEval_i\beta_3 + D_i \times HiEval_i\beta_4 + \varepsilon_i \qquad (2)$$

We focus on the employment and employment at placement school outcomes given the hypothesis that such outcomes might be driven by the information received by hiring authorities during the evaluation experiment. The coefficient β_1 provides the effect for candidates at the bottom of the evaluation distribution while $\beta_1 + \beta_4$ yields the effect for high achieving candidates.

We present the results of this analysis in Table 8. We find little evidence that employment effects are driven by candidates receiving positive evaluations in the treatment group. Teachers who receive higher evaluations are more likely to find employment overall, although the coefficients are not statistically significant. But we do not find any evidence that teacher candidates with positive evaluations in the treatment group are disproportionately likely to obtain teaching positions in their placement school or elsewhere. The estimated interaction terms are generally small and not statistically significant. It does not appear that the Feedback Initiative caused schools to hire more higher-performing candidates from among their student teachers.

6.3 Stated employment preferences

Finally, we investigate whether either intervention affected teachers' stated preferences over job types. In Table 9, we estimate intent to treat effects on candidates' reported preferences for employment and for particular school characteristics. The information on preferences comes from the post-internship survey administered at the end of the student teaching experience. The surveys included two questions about candidates' plans for the upcoming school year and their preferences in a job placement. The work plans question asked about plans for employment in various educator and non-educator roles in the following school year.[19] We use responses from this question to construct an indicator for whether teacher candidates planned to work as a classroom teacher in the following school year. The placement preferences question included a list of several school characteristics and asked candidates to rank up to three that were important in their job search.[20] We use this question to construct three indicators for teachers who included the following in their preference list: (1) the school in which they completed their clinical teaching experience; (2) schools with many low income students; or (3) schools with many African American students, Hispanic students, or English language learners. The school preference indicators roughly correspond to the characteristics of employment schools considered in Section 5.

In the first two columns, we consider the effects of the Placement Initiative on job preferences. We do not find that assignment to the high placement list affected plans to teach in

[19] The work plans question included the following options: teaching in a classroom; working in a classroom but not teaching; working in a field other than education; no plans to work; enroll in a graduate program focused on education; enroll in a graduate program in another field; unsure.

[20] The list of options included: schools with many lower income students; many African American students; many Hispanic students; many students with diverse learning needs; many English language learner students; many students with learning disabilities; many higher achieving students; many lower achieving students; an atmosphere of strong collaboration among teachers; few classroom management challenges; strong school leadership; strong family involvement; strong faculty mentorship; and the school in which I completed my clinical teaching experience.

the following school year. Teachers on the high placement lists were about 9 percentage points more likely to report a preference for working in their placement school, but these effects are not statistically significant. We also find that the effect of the initiative on employment in higher income schools is reflected in candidates' stated job preferences: teacher candidates on the high-placement list were 16 percentage points less likely to report an interest in working in "a schools with many low-income students." The effects on reported interest in working with racial minority students or English language learners are directionally consistent with the findings on school characteristics, but they are not statistically significant.

In columns (3) and (4), we find little evidence that the Feedback Initiative affected candidates' preferences for job types. We find no effect on preferences for school characteristics, which is consistent with the findings on employment outcomes. We also find no effect on preferences for working in the student teaching placement school. Although the confidence intervals are not precise enough to rule out effects of a similar magnitude as the effects on observed employment outcomes, the coefficients are close to zero. The findings suggest that the observed effect on placement school employment does not operate through changes in the preferences of teacher candidates.

Taken together, the findings suggest that the two interventions might have operated on different ends of the labor market. We find that the Placement Initiative appeared to affect candidates' preferences over school types. By contrast, we find little evidence that changes in employment school characteristics can be explained by job opportunities at their clinical placement school or district. On the other hand, the Feedback Initiative does not appear to have influenced candidates' self-reported preferences to work in their clinical placement school.

Although somewhat speculative, it appears that the employment patterns we observe may reflect changes in the preferences of employers.

7. Discussion

In this study, we consider the teacher labor market consequences to two potential reforms to the preservice clinical teaching experience. We find that a policy that changed the pool of mentor teachers and the location of placement sites affected the characteristics of schools in which teacher candidates ended up employed. Those assigned to placements that we predicted, based upon prior literature, to be more promising placements were more likely to obtain teaching positions in high income and low minority schools. These effects appear to have operated primarily through teacher candidates who were not employed in the school in which they completed their student teaching. Although there were differences in the characteristics of placement schools across treatment assignments, these differences were significantly smaller than the observed differences in employment schools and adjusting for these differences made little difference in the treatment effect.

The effects of the Placement Initiative on employment school characteristics are somewhat puzzling. The initiative did not affect the school district in which candidates completed their clinical teaching experience, and the effect cannot be explained by teachers hired into their placement school. These patterns suggest that effects on the employment school are not likely to have resulted from changes in referral networks caused by the assignment mechanism. Instead, the high-index schools had fewer low-income students, and we find that the assignment process also affected candidates' preferences to work in schools with lower income students. The results suggest that states attempting to improve student teaching placements should also be mindful of how such policies affect the teacher pipeline in high needs schools. But improving the quality of the student teaching experience and ensuring diverse placements are not necessarily in

conflict: Work by Ronfeldt (2012) suggests many low-income schools with high effectiveness or school climate are potentially available to serve as placement sites. States may want to consider placing students intentionally in these schools. On the other hand, if preservice clinical placements do influence candidates' preferences, then state efforts to change where student teachers complete their preservice training could also serve to expand supply to high needs schools. One of the states involved in this study requires all student teachers to complete some training in a diverse classroom setting.

We also find that our Feedback Initiative, which aims to improve the kinds of feedback that candidates receive based upon their clinical evaluations, increased the likelihood that candidates took positions in their placement school but had no effect on the overall employment rate. The findings are consistent with research on in-service personnel policies that suggest providing administrators with information about teaching effectiveness can affect schools' retention decisions even when there are not formal stakes involved (Loeb et al., 2015; Rockoff et al., 2012). Furthermore, employment in the student teaching placement school may be especially desirable given evidence that candidates perform better in schools similar to where they completed their clinical teaching experience (Goldhaber et al., 2017).

More broadly, the findings from this study demonstrate that policymakers should consider the labor market implications of student teaching requirements. We present evidence on two potential mechanisms that may be relevant for other policy initiatives. We show that placement sites potentially have follow-on consequences for where students are employed. Although there is other evidence for this hypothesis from descriptive studies (Krieg et al., 2016), the present study provides some empirical validation from a randomized trial. We also show that providing information about skill acquisition during clinical evaluations leads to higher

employment rates in placement schools. We find no overall effect on employment and no effect on teacher candidates' preferences to work in the placement school. The findings are suggestive that employers, and colleagues who may influence employment decisions, learn about teacher candidates when they conduct their practice teaching in their schools. The feedback reports we developed provided evidence on how candidates compared to the rest of their cohort, and this may have provided schools with an additional datapoint to use in hiring decisions. Given the current interest in improving placement practices and the evaluation of teacher candidates, these lessons may be relevant for a variety of reforms.

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Tables and Figures

Table 1. Summary Statistics

	Placement Initiative		Feedback Initiative	
	Treatment	Control	Treatment	Control
Outcomes				
Hired	.607	.590	.590	.565
Hired in First Year	.613	.611	.594	.609
Hired into Placement School	.104	.087	.113	.072
Placement School Characteristics				
School %URM	15.964	17.099	26.865	27.042
	(13.117)	(15.034)	(20.966)	(20.873)
School Poverty Index	232.952	232.703	302.413	302.879
	(87.280)	(91.288)	(152.583)	(139.704)
Hiring School Characteristics				
School %URM	21.365	24.559	31.990	32.478
	(20.175)	(22.987)	(24.803)	(25.916)
School Poverty Index	259.470	241.622	289.577	312.162
	(104.334)	(79.699)	(134.512)	(164.262)

Notes: Summary statistics for initiative participants. Placement school characteristics indicate characteristics of student teaching placement school. Hiring school characteristics indicate characteristics of school of employment following graduation. Treatment status indicates random assignment.

Table 2. Balance Tests

		Treatment Status				
	Overall	Control	Treatment	Difference	Effect Size	p-value
Panel A. Placement Initiative						
Female	0.859	0.832	0.883	0.052	0.148	
Male	0.138	0.162	0.117	-0.046	0.131	
White	0.965	0.977	0.954	-0.022	0.121	
Non-White	0.035	0.023	0.046	0.022	0.121	
Inclusive GPA	3.447	3.424	3.468	0.044	0.125	
Overall						0.20
Panel B. Feedback Initiative						
Female	0.842	0.838	0.845	0.007	0.019	
Male	0.149	0.148	0.151	0.003	0.009	
White	0.914	0.934	0.894	-0.041	0.145	
Non-White	0.086	0.066	0.106	0.041	0.145	
Inclusive GPA	3.509	3.498	3.522	0.024	0.075	
Overall						0.42

Notes: Omnibus test of balance across treatment status for Placement (Panel A) and Feedback (Panel B) Initiatives. The final column indicates the joint test of significance across treatment and control conditions. The student characteristics are derived from pre-internship surveys.

Table 3. Effect of the Placement Initiative on Employment Outcomes

	Employed			Employed in Placement School		
	(1)	(2)	(3)	(4)	(5)	(6)
Placement Initiative	0.011	0.006	-0.001	0.028	0.027	0.048
	(0.042)	(0.043)	(0.053)	(0.025)	(0.026)	(0.030)
Covariates		X	X		X	X
First Year			X			X
N	1407	1407	406	1407	1407	406

Notes: Estimated effects of Placement Initiative "high placement" list assignment on employment outcomes. Intent to treat effects obtained from regression of employment outcomes on randomized treatment assignment and randomization block effects. Covariates include indicators for year after graduation and plans to enter teaching derived from pre-internship surveys. First Year indicates sample is restricted to year after graduation. Standard errors clustered by teacher candidate in parentheses. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$.

Table 4. Effect of the Placement Initiative on Employment School Characteristics

	Percent URM			School Poverty Index		
	(1)	(2)	(3)	(4)	(5)	(6)
Placement Initiative	-4.944**	-3.989*	-6.009*	23.961**	24.829**	31.996**
	(2.307)	(2.293)	(3.423)	(10.642)	(10.811)	(12.900)
Covariates		X	X		X	X
First Year			X			X
N	785	785	230	785	785	230

Notes: Estimated effects of Placement Initiative "high placement" list assignment on employment school characteristics. Intent to treat effects obtained from regression of employment outcomes on randomized treatment assignment and randomization block effects. School Poverty Index refers to the School Neighborhood Poverty Index based on data from the Census Bureau; higher values indicate schools are located in higher income neighborhoods (see text for details). Covariates include indicators for year after graduation and plans to enter teaching derived from pre-internship surveys. First Year indicates sample is restricted to year after graduation. Standard errors clustered by teacher candidate in parentheses. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$.

Table 5. Effect of the Feedback Initiative on Employment Outcomes

	Employed			Employed in Placement School		
	(1)	(2)	(3)	(4)	(5)	(6)
Feedback Initiative	0.013	0.007	-0.027	0.044**	0.040*	0.031
	(0.033)	(0.033)	(0.037)	(0.021)	(0.021)	(0.025)
Covariates		X	X		X	X
First Year			X			X
N	1884	1884	733	1721	1721	673

Notes: Estimated effects of Feedback Initiative on employment outcomes. Intent to treat effects obtained from regression of employment outcomes on randomized treatment assignment and randomization block effects. Covariates include indicators for year after graduation and plans to enter teaching derived from pre-internship surveys. First Year indicates sample is restricted to year after graduation. Candidates missing data on placement school identifiers are dropped from the sample for regressions where placement school employment is the outcome. Standard errors clustered by teacher candidate in parentheses. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$.

Table 6. Effect of the Feedback Initiative on Employment School Characteristics

	Percent URM			School Poverty Index		
	(1)	(2)	(3)	(4)	(5)	(6)
Feedback Initiative	0.290	-0.021	-1.706	-19.209	-19.713	-15.761
	(2.201)	(2.200)	(2.336)	(15.123)	(15.276)	(15.159)
Covariates		X	X		X	X
First Year			X			X
N	995	995	407	995	995	407

Notes: Estimated effects of Feedback Initiative on employment school characteristics. Intent to treat effects obtained from regression of employment outcomes on randomized treatment assignment and randomization block effects. Covariates include indicators for year after graduation and plans to enter teaching derived from pre-internship surveys. First Year indicates sample is restricted to year after graduation. Standard errors clustered by teacher candidate in parentheses. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$.

Table 7. Placement Sites and Employment School Characteristics

	Percent URM				School Poverty Index			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Placement Initiative	-4.088*	-3.184	-5.751**	-4.144*	22.320**	23.561**	28.203**	28.601**
	(2.257)	(2.291)	(2.559)	(2.502)	(10.607)	(10.768)	(11.969)	(12.261)
Hired into Placement			-0.267	1.616			-8.254	-8.691
			(3.805)	(3.653)			(19.601)	(19.935)
PI x Hired into Placement			5.101	0.737			-24.990	-22.609
			(6.254)	(5.973)			(27.906)	(29.593)
Placement School Characteristic		X				X		
Covariates		X		X		X		X
First Year				X				X
N	785	785	785	230	785	785	785	230

Notes: Estimated effects of Placement Initiative "high placement" list assignment on employment school characteristics. Intent to treat effects obtained from regression of employment outcomes on randomized treatment assignment and randomization block effects. Covariates include indicators for year after graduation and plans to enter teaching derived from pre-internship surveys. First Year indicates sample is restricted to year after graduation. Standard errors clustered by teacher candidate in parentheses. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$.

Table 8. Clinical Evaluation and Employment Outcomes

	Employed			Employed in Placement School		
	(1)	(2)	(3)	(4)	(5)	(6)
Feedback Initiative	0.027	0.027	-0.039	0.035	0.035	0.014
	(0.053)	(0.053)	(0.059)	(0.037)	(0.037)	(0.043)
High Rating	0.044	0.044	0.082	0.018	0.018	0.026
	(0.057)	(0.057)	(0.061)	(0.040)	(0.040)	(0.045)
FI x High Rating	-0.038	-0.038	-0.025	-0.016	-0.016	0.002
	(0.075)	(0.076)	(0.083)	(0.058)	(0.058)	(0.064)
Covariates		X	X		X	X
First Year			X			X
N	1472	1472	593	1298	1298	530

Notes: Estimated effects of Feedback Initiative on employment outcomes by clinical evaluation ratings. Sample includes all candidates with clinical evaluation data. Intent to treat effects obtained from regression of employment outcomes on randomized treatment assignment and randomization block effects. Covariates include indicators for year after graduation and plans to enter teaching derived from pre-internship surveys. First Year indicates sample is restricted to year after graduation. Standard errors clustered by teacher candidate in parentheses. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$.

Table 9. Effects on Teacher Preferences for Employment Outcomes

	Placement Initiative		Feedback Initiative	
	(1)	(2)	(3)	(4)
Plans to Teach Next Year				
Plan to Teach	-0.003	-0.009	0.021	0.046
	(0.048)	(0.061)	(0.037)	(0.034)
N	182	182	359	359
School Placement Preferences				
Prefers Placement School	0.094	0.089	-0.003	0.013
	(0.067)	(0.067)	(0.051)	(0.050)
Prefers School with Lower Income Students	-0.158***	-0.160***	-0.031	-0.032
	(0.056)	(0.056)	(0.053)	(0.052)
Prefers School with URM Students	-0.051	-0.050	0.051	0.039
	(0.033)	(0.033)	(0.038)	(0.037)
Covariates		X		X
N	182	182	321	321

Notes: Effects of initiative on stated school preferences and work plans on treatment assignment indicator and randomization block effects. Covariates include pre-placement survey responses. Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$.

Figure 1. Sample Feedback Report

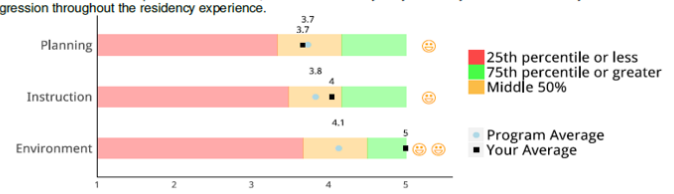
Performance Evaluation Report

Teacher Candidate: 2
Mentor Teacher: A

Date: August 25, 2017
Field Instructor: C

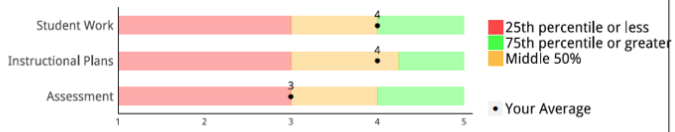
Performance Summary

The Performance Summary displays your average score and the overall TEP residency average by domain. The colors of the bars show the distribution of scores by each domain on the rubric for all of the teacher candidates in your program. The color of the bar indicates your individual average relative to your peers (e.g. having your average in orange means you fall in the middle 50th percentile of scores). Your scores may vary for many reasons but ideally should show progression throughout the residency experience.



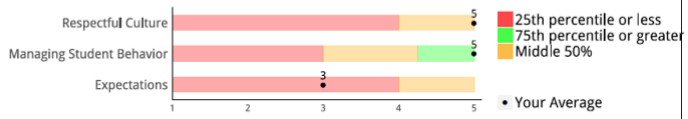
Focus Areas: Planning

Your identified focus area is a domain with indicators that have the most potential for growth. Your focus area is determined through comparison to your performance in other domains and your peers performance. The focus domain and related indicators are intended to help you identify areas for improvement and do not necessarily indicate poor performance. No matter how well you score overall, each teacher candidate is assigned areas of improvement to help achieve your full potential.
Based on your evaluations, you might want to focus on the following indicators within Planning as areas for further improvement. You are encouraged to discuss these opportunities for growth with your mentor teacher, university supervisor, and university professors so that they may provide you with actionable next steps.



Strength: Environment

In addition to your focus areas, the Strength section identifies the areas on the observation rubric where you score especially well. Nice job! Based on your results, you are performing strongest in Environment.



Discussion Areas: Instruction

Discussion areas are items where your mentor teacher and field instructor evaluations differ substantially. Disagreements between mentor teachers and field instructors may arise from fluctuations in your performance across different lessons or through genuine disagreements on how well you are meeting standards. Based on your results, you may want to further explore your performance in Instruction with your field instructor and mentor teacher.