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Effectiveness Research for Teacher Education

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Abstract

Despite calls for more evidence regarding the effectiveness of teacher education practices, causal research in the field remains rare. One reason is that we lack designs and measurement approaches that appropriately meet the challenges of causal inference in the context of teacher education programs. This article provides a framework for how to fill this gap. We first outline the difficulties of doing causal research in teacher education. We then describe a set of replicable practices for developing measures of key teaching outcomes, and propose causal research designs suited to the needs of the field. Finally, we identify community-wide initiatives that are necessary to advance effectiveness research in teacher education at scale.

Keywords: teacher education, causal research, measurement

Effectiveness Research for Teacher Education

Over the past decade, scholars have developed promising practices for teacher education, where practices are defined as the approaches, activities, and processes provided by teacher educators to develop preservice teachers' (PSTs) knowledge and skill. These promising practices include experiences that foster PSTs' ability to analyze classroom video (e.g., Santagata & Yeh, 2014; J. Sun & van Es, 2015); courses focused on preparing PSTs for addressing issues of race and racism in classrooms (e.g., Brown, 2014; Carter Andrews et al., 2019; Durden et al., 2016; Haddix, 2017; Lee, 2018); instruction in how to meet the needs of diverse learners (e.g., Bravo et al., 2014; Hernandez & Shroyer, 2017; Kang & Zinger, 2019); field experiences that join coursework to community settings (e.g., Horn & Campbell, 2015; Wasburn-Moses et al., 2015); and rehearsals and teaching simulations aimed at increasing PSTs' instructional skills (e.g., Kavanagh & Rainey, 2017; Windschitl et al., 2012). These new practices differ markedly from typical teacher education curricula and pedagogy, and often challenge the traditional separation between coursework and clinical experiences.

Promising as these practices are, researchers have seldom evaluated their effectiveness in producing more skilled and thoughtful teaching. One reason for the lack of such studies is that many of these practices are still in the development stage, necessitating careful exploration of design and initial implementation, and then adjustments based on these observations and participant feedback. Such work is necessary for refining emerging practices. The dearth of evidence on the effectiveness of these promising practices also stems from another source: the absence of consistent measurement and causal analysis among teacher education researchers.

In this paper, we address two reasons for this situation: the lack of causal research designs that can be used in the context of teacher education programs (TEPs), and the lack of

common measures that capture important proximate outcomes of teacher education. More specifically, teacher education is missing practical designs that compare otherwise similar individuals with and without the experience of the innovative teacher education practice. Such comparisons can help us identify the specific influence of a practice on outcomes. Also missing are high-quality, replicable measures of the PST outcomes we care about, including *in situ* teaching skills and the knowledge and dispositions thought to mediate improved teaching. Thus, despite calls for more effectiveness research in teacher education over the years (e.g., Cochran-Smith & Zeichner, 2005; Diez, 2010; Fallon, 2006; Grossman, 2008), barriers to doing such work remain high.

To enable studies that examine the effectiveness of new practices in teacher education, the field needs research designs and measures sensitive to the challenges inherent in teacher-educating organizations. In this paper, we review these challenges, then propose two pathways forward. First, we describe a set of replicable practices for building measures of key teaching outcomes. Second, we propose new research designs that take advantage of comparisons between groups or within individuals over time to evaluate the efficacy of new practices. Although such methods have been used widely in program evaluation, we discuss adaptations to suit the needs of teacher education. Finally, we describe a plan of community-wide work to bring together research designs, measurement, and the realities of TEPs.

Effectiveness Research in Teacher Education

For several decades, most research on educating new teachers has fallen into one of three paradigms (Borko et al., 2007): *interpretive research*, in which scholars trace PSTs' sensemaking as they engage with teaching tasks; *practitioner research*, in which teacher education scholars describe the nuances of their and/or others' practice; and *design research*, in

which scholars create blueprints for PST learning experiences, enact and critique those plans, then iteratively redesign those experiences. Research in these traditions has improved the state of practice in the field. Teacher educators now have sources of expertise in designing learning experiences for PSTs, and the process of innovation, reflection, revision, and redesign has both improved local offerings and produced strong hypotheses about effective approaches to training PSTs (e.g., Cochran-Smith et al., 2009; Draper et al., 2012; Hyland & Noffke, 2005).

Recently, a fourth strand of research has emerged on whether specific *programs* are more effective than others at preparing teachers, often using state administrative data to rank those programs (e.g., Gansle et al., 2012; Koedel et al., 2015; Ronfeldt & Campbell, 2016; von Hippel & Bellows, 2018). This line of research fails to causally identify the impact of specific program practices or elements, however, and in most cases does not address the efficacy of specific program features or practices (for an exception, see Boyd et al., 2009). For such information, we instead look to studies that fall into a fifth paradigm, one that we label *effectiveness research* in teacher education. Here, we refer to studies that attempt to arrive at a causal estimate of the impact of a specific TEP practice on either PSTs or, down the line, the students they will serve. Ideally, these studies would, to the extent possible, use common measures of valued teacher education outcomes, enabling comparisons of impacts across programs and settings. While both program-level research and effectiveness research are evaluative in nature, only the latter provides actionable guidance regarding program design and practice.

Though evaluative research in the field of education more broadly has grown sharply since the early 2000s, effectiveness research in teacher education has not. Mancenido (2020) reviewed 165 impact evaluations of teacher preparation practices—specifically, studies that sought to identify the effect of a specific practice on PSTs—published in peer-reviewed journals

between 2002 and 2019. Only 27% of these studies used a comparison group, which would have helped to identify what would have happened in the absence of exposure to the teacher education practice; only 43% of this group (11% of all studies) provided evidence that the comparison groups were equivalent before participants experienced the teacher education practice. Fewer than half of the studies (46%) used a standardized procedure for assessing PSTs' pedagogical knowledge, skills, or perceptions of program impact. These findings are similar to those arrived at by Cochran-Smith et al. (2016) in their review of science teacher education studies published between 2002 and 2015. Effectiveness research within TEPs is thus relatively uncommon. (For a limited set of exceptions, see Baylor, 2002; Baylor & Kitsanis, 2005; Bulunuz & Jarrett, 2009; Cohen et al., 2020; Ely et al., 2018; Giebelhaus & Bowman, 2002; Sayeski et al., 2015.)

One reason for the paucity of causal research in teacher education is that schools of education, like other professional schools, have long had an ambivalent relationship with research. As schools of education grew in the early 1900s, leading scholars aligned themselves with the disciplines, which often governed universities and, by extension, education schools; this in turn shored up education schools' legitimacy (Clifford & Guthrie, 1990; Fallon, 2009; Labaree, 2004). The result was that research tended to address questions that were important for the disciplines but that had less practical application to teachers and teaching. In turning to disciplinary theory and questions, many education schools shunted teacher education to the side; educating teachers remained a "duty carried out obligingly in the background" (Fallon, 2009, p. 13). In modern schools of education, most TEPs are not staffed in ways that assume teacher educators will be conducting research; teacher educators typically have more teaching responsibilities, less released time for research, and fewer resources (e.g., internal or external grants) with which to conduct research (Holmes Group, 1995; Labaree, 2004; Schneider, 1987).

Another reason for the paucity of effectiveness research in teacher education is that conducting such research within schools of education is, to put it simply, difficult given regulatory and practice constraints. The hypothetical example we describe next illustrates these difficulties.

An Illustrative Example

Imagine Gabrielle, a mathematics teacher educator and researcher at a mid-sized college of education. Gabrielle wants the 20 PSTs in her math methods course to be able to launch cognitively demanding mathematics tasks, but with sensitivity to learners' prior knowledge—a topic of research and concern in STEM education (e.g., González & Eli, 2017; Jackson et al., 2012; Kang et al., 2016). In thinking about this goal, Gabrielle relies on theories and evidence developed as part of Universal Design for Learning (Rose & Meyer, 2006), which urges teachers to think about and plan for variability in student responses to instruction. Gabrielle has also become interested in approximations of practice, such as rehearsal—an activity in which PSTs "publicly and deliberately practice with their peers how to teach rigorous content to particular students using particular instructional activities" (Lampert et al., 2013, p. 227).

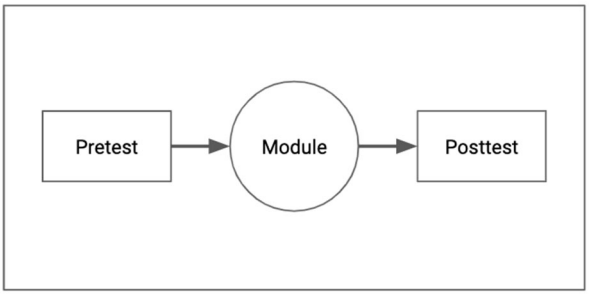
Gabrielle quickly builds a short module—about 3 hours of instruction—that takes PSTs through the process of task selection and task analysis and then a micro-teaching simulation similar to a rehearsal of practice. Specifically, Gabrielle designs this module to help PSTs to (a) select cognitively demanding mathematics tasks that offer multiple entry points to students, so that students of all levels of prior knowledge and abilities can productively work on the task, ensuring equitable access for all students; (b) identify the contextual features of mathematics problems, such as unknown vocabulary words and unfamiliar settings (see Jackson et al., 2013), that may affect the accessibility of the task for students; and (c) launch tasks with sensitivity to

student prior knowledge and background, but without devolving the cognitive demand of the task (Jackson et al., 2012). Gabrielle then wonders whether and how to collect data about her module's effects on PSTs' skills and practice. Mindful that she has yet to achieve tenure, she hopes to publish a manuscript describing her results.

Some of Gabrielle's colleagues have promoted the idea that she can save time by doing research on her own class. She would design a pretest and posttest on the skills she hopes to improve, and then collect and analyze data to show that PSTs improved their performance on the test during the class (Figure 1).

Figure 1

Typical Teacher Education Evaluative Research Design



However, Gabrielle has concerns about this design (see Table 1 for a summary). She knows that students will learn some content naturally, as they mature and gain life experience. She also knows that her PSTs have concurrent experiences in other courses and in clinical settings, and that these may also improve their performance on selecting, analyzing, and launching tasks. A research design that lacks a comparison group cannot distinguish these effects—broadly known as *history* and *maturation effects*—from actual learning from her module.

Gabrielle also knows that pre–post studies of innovations in intact methods courses cannot disentangle the effects of (a) a new practice; (b) the specific instructor; and (c) the class composition (e.g., peer knowledge and skill; propensity to collaborate productively). She worries most about *instructor effects*. Any estimate of the impact of her module will also include some assessment of her basic effectiveness as a teacher: If she is particularly effective, the module will appear more successful; if she is particularly ineffective, her module will fail to show effects. Gabrielle also worries about simultaneously teaching and studying her PSTs and the conflict of interest and potential bias in her data that might result, particularly if she uses course assignments as an outcome measure (i.e., *instrumentation effects*).

Finally, Gabrielle worries about whether students who opt into her class section might be different from those who enroll in other instructors' sections. She knows she has a reputation for paying attention to equity, a focus of her new module. As such, any gains she observes among her PSTs may result in part from having students who are well positioned to learn this content, in terms of motivation and prior knowledge, rather than from her module alone (i.e., *selection effects*).

Table 1

Common Threats to Validity in Evaluations of Teacher Preparation Practices

Threat to validity	Description	Possible strategies to address threat
History and maturation effects	- Effects of events concurrent to treatment may be captured in study outcome measures (e.g., if Gabrielle's students also learn how to select,	- Use a comparison group that does not receive the intervention, or that receives an alternative intervention aimed at a different outcome

	analyze, and launch tasks in other courses or fieldwork) - Effects due to natural growth	-Use multiple assessments to gauge growth on the outcome measure (a) prior to and (b) during the intervention
Instructor effects	- Effects due to the designer of the treatment delivering it in a way that cannot be standardized/replicated	- Develop standardized training and/or a manual for delivering the treatment - Collect implementation fidelity measures to check for standardization across groups that receive the intervention - Have research assistants deliver the intervention
Instrumentation effects	- Effects of bias due to who rates the outcome measure (e.g., Gabrielle rates those who received treatment systematically higher) - Effects of bias if the outcome measure is a graded course assignment	- Use independent measures for course assignments and research outcomes - Use multiple raters to check the reliability and replicability of the scoring procedure - Double-blind the scoring procedure*

Selection effects	- Effects due to selection into treatment group - Effects due to selection into study (e.g., some of Gabrielle's students are highly motivated to participate in the study)	- Randomize participants to the treatment versus comparison group - Randomize offers to participate in the study from the broader population*

* While we do not explicitly discuss these strategies in this paper, we note them because they are good research practices. Discussions of some can be found in Shadish et al. (2001).

Gabrielle's doctoral training included several classes in research design, including both positivist approaches focused on identifying causal impacts and more interpretivist approaches focused on uncovering how research participants make meaning of social experiences and interactions. Gabrielle thus begins to consider some of these designs, including randomized experiments and analytic approaches that attempt to mimic experiments using observational data (e.g., regression discontinuity designs). Yet when Gabrielle begins to think about using such designs in the context of teacher education, she identifies numerous challenges.

Teacher education in the United States is widely dispersed, with almost 1,500 separate institutions educating teachers for initial licensure; as a result, most programs are small, like Gabrielle's (U.S. Department of Education, 2019). Most support one or (as in Gabrielle's case) two sections of each course. Further, similar to at other universities, her students' schedules are often determined partly by preference and partly by external scheduling constraints. This prevents Gabrielle from taking advantage of random assignment to classes. Even if she were able to randomly assign students to math methods sections, students would still be taught by different

instructors, and Gabrielle would not be able to disentangle the effect of any new practice from her effect as an instructor. A further problem is that, as in most other TEPs, all her students follow the same or very similar pathways through coursework, so approaches that take advantage of differences in these pathways are not possible.

For some perspective, Gabrielle consults her school's program evaluation expert, who first recommends launching an experimental trial of her new module in multiple universities, an idea with some interest in teacher education (e.g., Grossman & McDonald, 2008). Yet Gabrielle knows that working across multiple institutions would be logistically difficult and that a large sample of teacher education institutions (potentially in the dozens) would be necessary to achieve statistical power for detecting effects. She also knows the cost of recruiting and training teacher education faculty would be beyond what she could bear.

Hearing this, the expert recommends Gabrielle offer a mini-course featuring her module, then track students who take this new course into their first years of teaching, using district administrative data to compare them to students who do not take the course. But Gabrielle knows that results from PSTs' performance as teachers of record are typically not available for at least a year, and possibly several years, after they graduate. This kind of performance data—from state-mandated classroom observations or from student test scores—can also be difficult to obtain and to interpret, given the nonrandom sorting of PSTs into schools and districts and the nonrandom attrition of PSTs between course enrollment, program graduation, and actual teaching assignments.

The expert's comment also makes Gabrielle think about how to measure her PSTs' outcomes. Even if district administrative data from teachers' first years of teaching were easily available, it seems unlikely that a distal measure, such as her PSTs' eventual value-added or

classroom observation scores, would capture outcomes from her module. These measures can be insensitive to teacher learning (Sussman & Wilson, 2019), and her 1-week intervention on task selection and launch is unlikely to move the needle on either (Diez, 2010). Specifically, these distal measures capture many different aspects of teaching in addition to potential outcomes from the specific practice under study, making them less sensitive to that practice.

Finally, Gabrielle is discouraged by her broader organizational context and how it disincentivizes the sort of research she hopes to undertake. Because of the manifold requirements that her program must navigate (e.g., organizing district partners, managing state credentialling requirements, navigating bureaucratic university structures), Gabrielle knows she is unlikely to have flexibility in course scheduling (e.g., she can't reschedule some students to take a parallel version of the class in a different semester). And, her TEP lacks organized and standardized data collection on PSTs and has not instituted the kinds of partnerships with districts and states that would help her follow up with PSTs in their eventual classrooms.

Given Gabrielle's concerns and challenges, what kinds of measures and designs are feasible? We explore possible answers to this question below. We first discuss how Gabrielle could approach the challenge of measuring her outcomes of interest, noting that she will need to balance the goal of conserving her limited time and research funds with using instruments that return accurate and valid scores for her outcomes of interest. We then discuss Gabrielle's potential research designs. While not every design satisfies all of her concerns, we explain how each enables her to progress toward making stronger inferences about PST learning.

Measurement

Key to improving researchers' ability to evaluate a new practice in teacher education is work that locates or develops outcome measures that: (a) assess the focal teaching knowledge or

skill; (b) provide reliable and replicable scores; (c) can be feasibly implemented in the context of teacher education; and, ideally, (d) predict future valued outcomes, such as teaching quality and student learning. These new measures should not double as course assignments, which are problematic as outcome measures for two reasons: because incentives in grading can distort PST performance (e.g., when PSTs game an assignment) and, when researchers teach a course they also evaluate, they may unknowingly bias grades in favor of finding an effect of the new practice.

Recall that Gabrielle hopes to develop PSTs' skills in (a) selecting cognitively demanding tasks that allow multiple entry points for students; (b) identifying features of tasks that may present barriers to student work; and (c) launching tasks with sensitivity to learners' knowledge and background while maintaining high cognitive demand. She breaks down these skills into five distinct constructs (see Table 2) and observes that she can potentially group them in a way that allows her to collect two kinds of data: (a) from written assessments, in which PSTs select high-cognitive-demand tasks with multiple entry points and identify features that affect the accessibility of tasks to students; and (b) from a simulation of PSTs actually launching a task in front of peers or graduate assistants playing the role of students (e.g., Shaughnessy & Boerst, 2018).

Gabrielle begins by searching EdInstruments (https://edinstruments.com/), hoping to locate measures that require minimal adaptation for her purposes, thus controlling costs, including the cost of her own time. Locating existing instruments would also make her findings comparable to those from other research teams, facilitating later research syntheses. Gabrielle finds two relevant instruments: the Mathematics Scan (M-Scan; Berry et al., 2010) and the Instructional Quality Assessment (IQA; Boston, 2017). Both instruments contain an item that

captures the potential of a task to result in cognitively demanding work (1a in Table 2 below), and an item that captures the enacted cognitive demand of tasks (2b). Both instruments define each construct carefully and provide raters guidance on how to assign score points associated with different levels of the construct, something that Gabrielle knows is important given her aim of having reliable and replicable scores. After a close analysis of the items in the M-Scan and IQA, Gabrielle chooses to use the items from the M-Scan; they are more aligned with how she conceptualizes task launch, and she knows that other faculty in her department use the M-Scan, allowing for comparisons across courses.

While Gabrielle is lucky to find measures for two of her constructs, she cannot find an instrument that assesses whether selected tasks have multiple entry points (1b), whether PSTs can identify contextual features that affect task accessibility to students (1c), or whether PSTs are sensitive to students' prior knowledge when launching tasks (2a). As such, she decides to develop these from scratch. Because Gabrielle has read several papers describing measure development, she knows that this process follows a relatively straightforward set of steps. First, she writes prompts that will enable PSTs to demonstrate their knowledge, reasoning, and skills, including prompts that will yield data that can be scored by the M-Scan.

Table 2

Measurement Plan

Construct	Potential assessment	Existing instruments that measure the construct	Plan of action

1a. Selecting a task with potential for high cognitive demand	Task selection and analysis of written assessment	IQA (Boston, 2017) M-Scan (Berry et al., 2010)	Use "Cognitive Demand: Task Selection" item from M-Scan
1b. Selecting a task with multiple entry points	Task selection and analysis of written assessment	Not found	Develop from scratch
1c. Identifying contextual features that affect task accessibility for students	Task selection and analysis of written assessment	Not found	Develop from scratch
2a. Launching a task with sensitivity to students' prior knowledge	Simulation of task launch	Not found	Develop from scratch
2b. Launching a task in a way that maintains high cognitive demand	Simulation of task launch	IQA (Boston, 2017) M-Scan (Berry et al., 2010)	Use "Cognitive Demand: Teacher Enactment" item from M-Scan

Because Gabrielle has read several papers describing measures development, she knows that this process follows a relatively straightforward set of steps. First, she writes prompts that will enable PSTs to demonstrate their knowledge, reasoning, and skills, including prompts that will yield data that can be scored by the M-Scan. She remembers from her reading of the literature and her courses with Dr. Schilling, her measurement professor, that it is important that the prompts gauge each construct independently. For instance, a student's score on identifying contextual features that affect task accessibility (1c) cannot be contingent on them already getting a high score on selection of a task with potential for high cognitive demand (1a). In light of this, Gabrielle decides to standardize parts of her assessment. For task selection (1a and 1b), PSTs will select tasks themselves, so she can measure whether these tasks have the potential for high cognitive demand and multiple entry points; all PSTs will then analyze the same task for contextual barriers that affect accessibility (1c).

Gabrielle decides to similarly standardize the simulation, asking all PSTs to launch the same task rather than launch the tasks they find, which may vary in quality. Her end result is a three-part assessment: (a) a prompt that asks PSTs to select a task that has both high cognitive demand and multiple entry points; (b) a prompt that asks PSTs to analyze a previously unseen task to identify contextual features that affect task accessibility for students; and (c) a prompt that asks PSTs to adapt and then launch a second previously unseen task to two research assistants posing as elementary students.

She pilots these prompts with several PSTs similar to those in her population of interest, because doing so helps her understand whether they will yield the data she needs to evaluate her module. For example, she wants to make sure that she sees some variation in responses to the measures across PSTs. If all PSTs respond the same way or if they all perform very well on the

tasks, it is unlikely that Gabrielle will be able to find an effect of her intervention. As well, Gabrielle must see whether any features of the prompt lead respondents to interpret it the wrong way, or to provide off-base or idiosyncratic answers. Based on her analysis of data returned from these pilots, Gabrielle iteratively revises and pilots the prompts.

Next, Gabrielle must develop a procedure for scoring the data she collects. The goal of this scoring system is to distill responses to the task into easy-to-use units, like scores on a rubric. The M-Scan allows her to score tasks 1a and 2b. For the multiple-entry-point measure (1b), her research team sits down and provisionally scores the pilot data, developing definitions for score points iteratively by successively scoring and discussing PST responses. When the research team feels they have arrived at a set of items and score-point definitions, they test for interrater agreement using a fresh set of responses. They repeat this process for the measures in 1c and 2a.

If project resources permit, Gabrielle can determine score reliability through a generalizability study (see, e.g., Hill et al., 2012), which can help determine the optimal number of raters required to produce desired score reliabilities. Again, if resources permit, Gabrielle can also determine whether scores have predictive validity—for instance, whether scores on the task-launch part of the assessment predict the quality of task launches during PSTs' clinical placements. If project resources are constrained, Gabrielle can simply estimate Cohen's kappa to determine rater agreement.

Gabrielle concludes this phase of measure development by documenting the prompts and scoring guidelines in a codebook. These two elements—carefully designed prompts that elicit targeted PST responses and scoring systems that compress information from responses into

easily manipulated units—help ensure that Gabrielle's measures produce high-quality data for evaluation of her module, and that these data can be scored in a reliable and replicable way.

Gabrielle's work toward measuring valued outcomes is both practical and forward-looking. Where possible, she is building on measures developed by other scholars, thereby reducing her costs and allowing for future comparisons between her studies and others. Investing in careful measurement of new outcomes, on the other hand, helps build a base for future researchers by creating measures that reflect the specific knowledge and skills targeted by teacher educators.

Research Designs

Having developed a plan for measuring outcomes, Gabrielle now turns her attention to research design. After surveying the existing causal literature in teacher education and reviewing the texts used in her research methods coursework (e.g., Mohr, 1995; Murnane & Willett, 2010; Shadish et al., 2001), Gabrielle considers research designs that use three strategies to make stronger causal inferences: (a) making comparisons between "untreated" PSTs and "treated" PSTs; (b) using randomization to the untreated and treated groups in order to increase the likelihood that PSTs in those groups are similar before the treatment begins; and (c) increasing the number of measurement occasions to facilitate within- and between-person comparisons and to increase the precision of her estimates. Given that Gabrielle's aim is to isolate the specific effects of her module on PSTs' skills, using each of these three strategies together or in combination can help rule out alternative explanations for any changes in their performance.

Extra-Treatment Designs

A design that may work particularly well in Gabrielle's situation, given that she isn't yet sure about incorporating the new module into her regular class, is an extra-treatment design. In

this design, she would deliver the module to selected participants from her class, but do so outside of regular class hours, as an extra class. Gabrielle finds this design appealing because the pulled-out students would attend class as usual with the control group, ensuring they don't miss important material.

Figure 2

Extra-Treatment Design

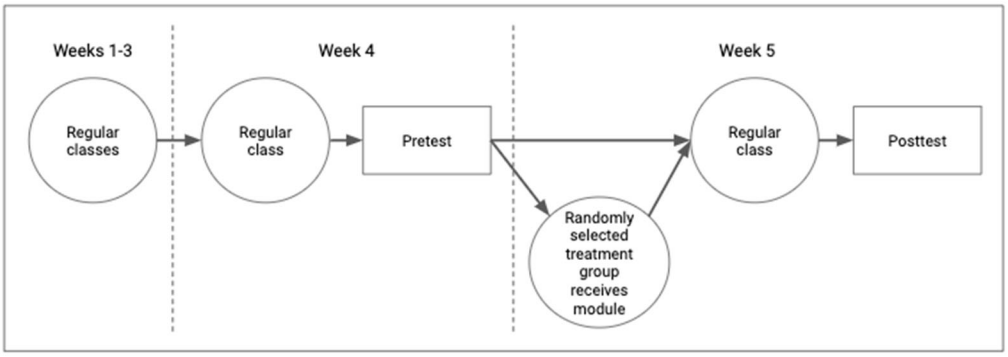


Figure 2 shows a hypothetical extra-treatment design and data collection plan. In this example, all PSTs in both groups (untreated and treated) take the first 4 weeks of a course as it is typically taught, followed by a pretest. Following the pretest, a group is randomly assigned to treatment. Randomization of PSTs to the extra-treatment group addresses selection effects by making it more likely that the treated and untreated groups will be equivalent at the outset of her study—something the pretest can help verify. But randomization to an extra class may be difficult in Gabrielle's TEP: Students take other classes, have jobs, or commute to school, and thus are not available at all times. Gabrielle also knows she will have to "count" in her treatment group the students who are assigned to but cannot attend the extra Week 5 session; once random assignment occurs, students must stay for analysis in their randomly assigned group. Although this option seems unappealing, the alternative—contending with selection effects by allowing

students to opt into the module—seems much worse. Therefore, Gabrielle decides to find times in her school's master schedule when it seems likely students will be on campus and free. She asks students at the beginning of the semester to hold this date.

After the treated group takes the new module in addition to their regular class during Week 5, both groups complete the posttest. Because of randomization, the performance of groups on the posttest can be directly compared, controlling for pretest scores, to identify whether the module has had an effect. The underlying assumption is that because those in the treated group and those in the untreated group were randomly assigned, we would expect their average outcomes to be the same if there was no treatment (or if everyone received the treatment). To undertake the comparison between groups, Gabrielle would likely use a simple regression or related statistical model.

One major challenge of implementing the extra-treatment design in teacher education settings is equity. Gabrielle knows she can offer the module to the untreated group later in the semester, but students may not be able to access it because of scheduling conflicts or the end-of-semester time crunch. She recognizes this is a particular concern for postgraduate and/or alternative TEPs, which are generally shorter and therefore have less flexibility in curriculum and scheduling.

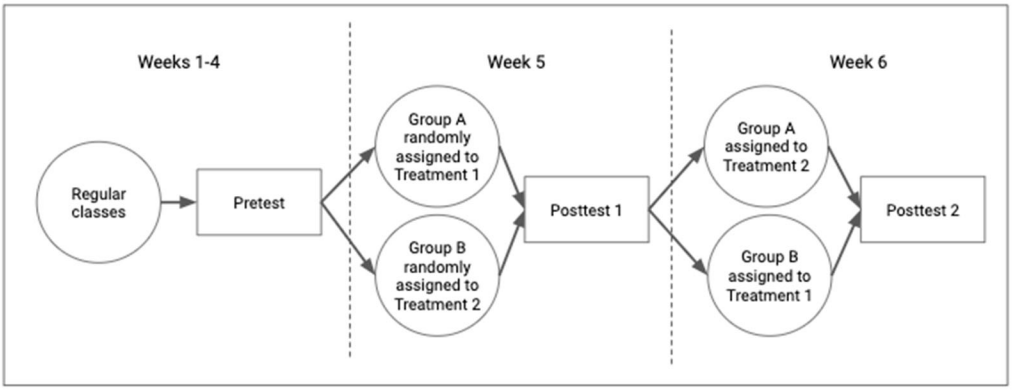
Crossover Designs

Given the equity concern in the extra-treatment design, Gabrielle might consider a crossover design (Liu, 2016; Ratkowsky et al., 1992), sometimes called a counterbalanced design. This approach is most often used in early-stage research, typically with intact classrooms of instructors and students, making it particularly appealing to teacher educators. As the name implies, the defining characteristic of this design is that participants "cross over" from one

treatment to another in the middle of the study. For example, Baylor and Kitsantas (2005) compared the effects of two different instructional planning scaffolds by having two sections cross over in an educational technology course; similarly, Bulunuz and Jarrett (2009) compared the effects of course readings, hands-on learning stations, and concept mapping using two sections of a science methods course. Likewise, in Gabrielle's case, each group of PSTs would be randomly assigned to receive each of two treatments, yet in a different sequence (Figure 3).

Figure 3

Crossover Design



Choosing the second treatment requires some thought. Luckily, while Gabrielle was developing her module, she considered two approaches: the one she chose, in which students examine a task and then conduct a simulation of its launch with peers playing the role of students (Treatment 1), and another in which students examine several written tasks and then analyze videos of expert teachers launching those tasks in classrooms (Treatment 2). Both possibilities are rooted in the work of teaching, and both feature an examination and critique of at least one task.

During development of her module, Gabrielle was not sure which approach would lead to her PSTs being better able to launch tasks. On one hand, she worried they would not take the

simulation seriously enough, and that doing a simulation with peers absent a close video analysis of expert teachers would deprive her PSTs of the opportunity to study strong practice. On the other hand, she worried that a video-based decomposition would not be a close enough approximation of practice to improve PSTs' skills. Because Gabrielle remains somewhat undecided about the benefit of one approach over another, the crossover design becomes appealing.

As Figure 3 shows, her intact class would experience the first weeks of the course as a group. During the first phase of the study, in Week 5, Gabrielle would split the class randomly, with half (Group A) examining a task then rehearsing its launch, and the other half (Group B) examining tasks and then watching experts launch it on video. At the end of this first phase, Gabrielle would collect a wave of outcome data (Posttest 1) so she can identify the initial conditions' effects on her PSTs. The groups would then cross over to the other treatment conditions, and Gabrielle would collect a second round of outcome data (Posttest 2).

This approach uses two types of comparisons. First, the initial round of experiences provides a comparison of the two treatments—specifically, Posttest 1 (in Week 5) allows for a direct comparison. The randomization of students to each group helps ensure that the posttest is similar between the two groups without the specific difference in experience. Second, Posttest 2 (after the second treatment in Week 6) allows Gabrielle to see whether the order of Treatments 1 and 2 affects the outcomes (e.g., at the conclusion of the study, PSTs in Group A on average grew 3 units more than PSTs in Group B, suggesting a benefit in videos first, then simulations). Again, these comparisons are possible because of randomization. For this design to work best, the outcome of interest would need to be something that PSTs will get better at, but not yet fully

master, during the first treatment and would need to capture performance equally well at all levels of skill, such that growth can be reliably identified.

One major challenge of implementing the crossover design in teacher education settings is that intact classes may not contain a sufficient number of students (i.e., power) to detect the effect of treatment with confidence. Class sizes of 20, for instance, may be too small for differences between treatment groups to be statistically significant, unless these differences are quite large. One solution for Gabrielle may be to join forces with another mathematics teacher educator to conduct her study across multiple classes. Doing so would open up a whole range of other challenges, however, such as ensuring treatment fidelity, guarding against instructor effects, and standardizing curriculum across courses to ensure a fair comparison.

Lab Experiments

If her class size is too small for the extra-treatment or crossover design, and the challenge of partnering with other teacher educators is too great, another option Gabrielle might consider is a lab experiment. Teacher education researchers have used this design to document how equipping cooperating teachers with a framework for evaluating practice can result in higher-quality teaching (Giebelhaus & Bowman, 2002) and how features of classroom simulations can improve PST knowledge and practice (Cohen et al., 2020; Ely et al., 2018).

Here, Gabrielle would recruit students independently of her class and randomly assign them to participate in the module or to serve as an untreated control, with a pretest and posttest for all. Similar to the extra-treatment and crossover designs, random assignment would make it more likely that the treatment and control group would be equivalent at pretest in terms of key variables, such as knowledge, skill, and motivation to learn. This would allow for the control

group's performance in the posttest to stand in for treatment group performance in the absence of treatment.

In other educational settings, such as schools, random assignment studies can be quite large, involving dozens of schools, hundreds of teachers, thousands of students and, often, millions of dollars. In Gabrielle's case, however, a much smaller randomized trial is possible. Gabrielle might conduct a power analysis—perhaps by using freeware catalogued at PowerandSampleSize.com (http://powerandsamplesize.com)—to determine an adequate study sample size given the magnitude of impact on performance she anticipates and the level of statistical significance she hopes to reach. Given an outcome measure that is well-predicted by a pretest, a measure sensitive to treatment, and a treatment with a moderate expected effect, as few as 40 PSTs—20 in the treatment group and 20 in the control—may be possible.

Gabrielle would then need to think about where she could recruit those PSTs. One option is to use all PSTs in Gabrielle's and her colleague's class, as together they may reach the number indicated by the power analysis. But Gabrielle knows that it's unlikely that everyone enrolled in these classes would take part in the study. Instead, she could think about recruiting from the intending teacher population generally. In her school, for instance, PSTs take 2 years of general education and discipline-based coursework before enrolling in math methods; this population would thus be good candidates for her study. Engaging PSTs who are not enrolled in her own class would also alleviate Gabrielle's concern about studying her own students.

Once Gabrielle decides whom to enroll in the study, she would need to finalize other parts of her design. One decision involves how the module gets administered to those in the study (i.e., the unit of assignment to treatment). The treatment could be delivered individually to each PST—in Gabrielle's case, as a tutorial. She finds this idea appealing, because it enhances

the argument that she has a well-defined treatment that can be replicated across settings and in her absence. However, Gabrielle also wonders whether conducting her task-launch module in groups, simulating a regular class, would enhance the external validity of her experiment. Delivery in groups is important because the module is supposed to be completed in a class setting, and because Gabrielle expects students to learn from one another. Further, delivering the module to small groups instead of one big group is appealing because it would allow Gabrielle to understand whether there are group-level effects in her data (e.g., the extent to which one group performs better than another because of group composition, day of the week, or some other variable). Whatever she decides, analysis of randomized trial data requires relatively simple methods—typically, regression models comparing treatment impact while controlling for pretest performance.

Lab experiments can be implemented quite flexibly by teacher educators. For instance, a study on feedback to PSTs by clinical supervisors could randomize the feedback to be self-reflective or more directive in nature. Clinical supervisor impacts could be controlled with a series of binary variables indicating each supervisor and thus representing their "effect" (i.e., fixed effects), and treatment fidelity could be ensured through logs and video recordings. Or, a program piloting a new community-based teacher education experience may choose to randomly assign half of a cohort to that experience and half to the more traditional course it replaces. Because most lab experiments need to occur outside class settings, this design is also flexible with regard to what is taught and how intensively; interventions outside the typical curriculum and that last for as long as investigators desire (and for as long as PSTs will participate) are possible.

Lab experiments still come with some challenges, however. For Gabrielle, one drawback centers around the fact that instead of using students in her class as the sole participants in the study, she would need to recruit, treat, collect data from, and compensate some number of additional PSTs. Another drawback centers around balancing the length and cost of the treatment—while shorter treatments may not well represent experiences offered in teacher education coursework, longer experiences (e.g., three 2-hour "class meetings") may be more difficult to fund and may bring other, substantial drawbacks, like participant attrition.

Other Designs

Gabrielle briefly considers two other designs that can be used in situations in which randomization is not feasible or is objectionable for ethical reasons.

Nonequivalent Dependent Variables. Designs using nonequivalent dependent variables can help reduce internal threats to validity, even in the absence of a comparison or control group (see Coryn & Hobson, 2011). In these designs, researchers assess a range of similar outcomes before and after participants receive treatment. Some outcomes are specifically targeted by the treatment; others (i.e., the nonequivalent dependent variables) are not. The effect of the treatment is identified by comparing gains in targeted outcomes (A–C in Figure 4) versus nontargeted outcomes (D in Figure 4). The underlying assumption is that without the treatment, gains in the target outcomes would have been the same as the nontargeted outcomes.

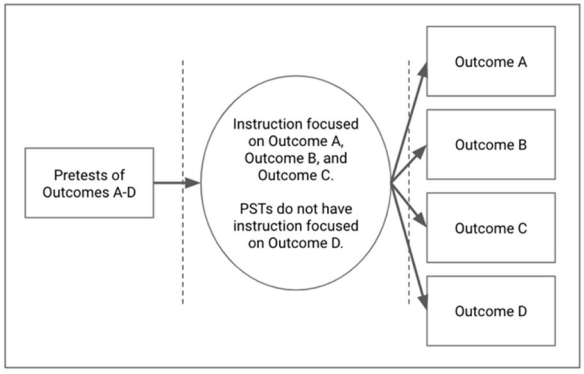
The primary challenge of this design is choosing the right nonequivalent variables. They cannot be so similar to the outcomes of interest that any learning is transferred. For instance, Gabrielle cannot use PSTs' knowledge of what makes a complex task or clarity of instruction-giving as a nonequivalent dependent variable, because each is too close to the target outcomes listed in Table 2. Nor can the variable be learned elsewhere during the study period, such as in a

practicum or concurrent coursework. Either situation would understate the magnitude of any effects, as the nonequivalent variable would be inflated. Alternatively, the nonequivalent variable cannot be so different—for example, in Gabrielle's case, a list of words to translate into Russian or PSTs' knowledge of professional ethics—that it does not serve as an adequate baseline or capture the effects of other possible influences on the outcomes. Using nonequivalent variables like these would likely lead to overstating the magnitude of any effects.

Nevertheless, this design may be useful to teacher educators who have a large number of potential course topics but must choose a narrow set for focus during a semester-long course. For instance, Morris and Hiebert (2017) and Kavanagh and Rainey (2017) both compared PST performance on knowledge and skills taught and not taught in their TEP courses, finding that students performed better on the content taught. Critically, in both cases, the not-taught content could have been taught in the course. Morris and Hiebert compared topics taught (operations with whole numbers and fractions) with a topic included in state standards but excluded from the course due to time constraints (finding the mean). Kavanagh and Rainey took a similar approach, comparing PST growth in taught content (supporting students' discussions with text) and content that was not taught but could have been covered in the course (supporting students' use of disciplinary practices to analyze text). Gabrielle could take a similar tack, comparing measures that capture PST task selection and launch with measures of other content not included in her course (e.g., using redirections to manage student behavior).

Figure 4

Nonequivalent Dependent Variable Design



Nonequivalent Comparison Groups. Given her students' or institution's scheduling constraints or equity concerns, Gabrielle may think about using one of the above designs but without random assignment, or she may choose to run the module in her class and compare her students' outcomes with those from other classes. These nonequivalent comparison group designs make many analysts' hearts sink: Even with a strong pretest on the outcome of interest, there are few ways to ensure that the two groups are similar enough—in prior skills and knowledge, motivation to improve, or other key characteristics—to serve as fair comparisons to one another.

That said, when a nonequivalent comparison group design is necessary, investigators can try to identify, investigate, and eliminate as many threats to validity as possible (see Table 1). Specific features of TEPs may be useful in mitigating such threats. One opportunity stems from the fact that students typically enroll in TEPs over multiple years. PSTs drawn from the cohort prior to (or ahead of) those treated may serve as adequate comparisons. For example, Santagata and Yeh (2014) compared scores on the Performance Assessment for California Teachers and videos from two cohorts of students: one that participated in their video-analysis course, and one that preceded the development of the course. Investigators can also take advantage of the course enrollment process, which in some places does not allow PSTs much choice in classes and

sections. Investigators can gather information about the enrollment process and, if possible, make the case that assignment to treatment is not contingent on PSTs' preferences for experiences.

Another opportunity stems from the fact teacher education researchers typically have access to students before they become research participants. Thus, prior to the introduction of the new practice, researchers can either directly assess the outcome targeted by the study or use a proxy measure (e.g., grade in a foundation course). Pretests of this sort have several advantages. They help establish baseline equivalence between comparison groups. In the case of attrition from either the treated or untreated group, they also allow investigators to examine the nature of that attrition, determining whether higher or lower performers have dropped out, and qualifying conclusions appropriately. Finally, because pretests and posttests are typically correlated, including pretests in analytic models helps explain posttest variability and can make identification of treatment effects more efficient, meaning a smaller sample size is needed. Teacher education researchers can also collect multiple pretests from study participants, allowing for comparison of pretreatment and during-treatment learning trajectories.

Other. Finally, investigators can create new research designs that combine the basic elements discussed above. For inspiration, we refer readers to the section in Shadish et al. (2001) titled "Untreated matched controls with multiple pretests and posttests, nonequivalent dependent variables, and removed and repeated treatments" (p. 153). Clearly, design possibilities are endless. However, in keeping with the ideas we have presented here, teacher educators should gravitate toward designs that apply key principles of rigorous evaluative research design: establishing comparison groups to guard against history and maturation effects, using

randomization to increase the likelihood that comparison and treatment groups are similar, and using pretests to ensure baseline equivalence and adjust for pre-intervention statuses or trends.

External and Internal Validity

Regardless of the design she selects, Gabrielle knows that she still needs to consider a number of threats to validity. Specifically, she must address concerns that the effects she identifies have limited generalizability to other treatments and other settings (external validity), and that the effects she identifies are not just due to her module alone (internal validity).

One issue involves the extent to which results from the studies described above would generalize to other settings. For instance, Gabrielle knows that evaluating a discrete practice like the simulations in her module may provide direct information about its efficacy in this particular context; however, it offers little information about the use of simulations more broadly. This is because simulations differ in their design across teacher education settings (e.g., Kavanagh et al., 2020; Stroupe & Gotwals, 2018). Moreover, even if they were replicated, Gabrielle's simulations would be offered in different contexts by different instructors with different sets of students—variations that may affect its efficacy. Many fields address this issue by repeatedly studying new practices and programs across a wide range of settings, building not only general evidence of efficacy but also, ideally, a sense for what adaptations produce learning.

A second issue is instructor-by-treatment effects, which occur when a single teacher educator provides the treatment under consideration, and which are not solved by our research designs. For instance, threats to the external validity of Gabrielle's study results could arise if her enthusiasm for her module means that she unwittingly provides better instruction for those in the treatment condition than for those in the control condition, and/or if her expertise means that other instructors in other contexts are unlikely to deliver the intervention with equivalent skill.

We see several ways to mitigate bias associated with instructor-by-treatment effects. First, instructors can document the quality of instruction, describing the delivery of the new practice and any teaching done in a comparison condition. Documentation may involve a close description of practice or coding for a set of practice-specific and generic indicators (e.g., teacher educator questioning and modeling; PST engagement). When carefully used, this can serve as evidence for the equivalence of instructional quality across different conditions. A second (and better) strategy to mitigate this issue involves having other instructors, such as graduate students, deliver the new practice to PSTs; doing so would show that it can be replicated by individuals who are not its designers, albeit with close training and supervision. Finally, similar to how both IES and NSF fund progressively larger and more complex studies, teacher educators can replicate their designs with different samples of PSTs, different instructors, and even in different settings.

A third issue is spillover effects—essentially, when treated students share the knowledge, practices, and skills with untreated students—which can affect the internal validity of results. Several recent studies have identified effects on a teacher's practice when the teacher's peers attend professional development (Gonzalez, 2020; M. Sun et al., 2013). In an extra-treatment design, for instance, treated students may share knowledge gained in the extra class time with students in the untreated condition; this would result in decreased observed treatment impacts. Investigators who choose designs with comparison groups may want to take steps to limit spillover and to understand the extent to which they occur. For instance, if Gabrielle were to use an extra-treatment design, she could ask the extra-treatment group not to discuss the treatment with the rest of the class until the conclusion of data collection; she could also survey her

students to determine how much social interaction between treatment conditions occurred during the study period and which study-related topics students discussed during those interactions.

A fourth issue relates to random assignment—in particular, equity concerns regarding PSTs who may not be selected for treatment but, given their background or interests, may need or want to participate. In these cases, Gabrielle could consider pulling these PSTs out of the official study prior to random assignment, allowing them to experience the treatment but not using their data to evaluate the efficacy of the new practice. The key is making this determination prior to the random assignment process. Similarly, to preserve the integrity of random assignment, once students are assigned to one group, they cannot be switched to another, even if the reasons are relatively neutral (e.g., scheduling difficulties).

Conclusion

This paper has introduced approaches to measurement and research design that meet the needs of teacher education researchers interested in assessing the effectiveness of promising practices. These methods are complementary to other methods already used in the field, where a range of research approaches, from interpretive to design research, provide insights for understanding teacher education practices. To date, however, a paucity of teacher education studies have provided compelling causal evidence about the effectiveness of promising practices in the field. We argue for rebalancing the teacher education research portfolio to include more causal studies; doing so will help build credible evidence about which practices contribute to improved PST knowledge and skills. To conclude, we offer some thoughts on how these approaches might fit into the larger field of teacher education research.

First, the evaluative studies we describe above can only answer a narrow set of questions in teacher education: questions focused on identifying which new practices work to improve

certain teacher-level outcomes, and therefore, hopefully, related student-level outcomes. There is clearly a larger universe of questions—for instance, those investigating PSTs' and teacher educators' beliefs, thinking, and experiences, and the interplay between them. These questions call for other methodologies, including critical race theory, ethnography, surveys, or case studies. We see the research designs proposed above as complementary to others already established in the field, expanding options for researchers interested in questions that involve formal assessments of the efficacy of new teacher education practices.

Second, we see three additional shifts that must take place within the field before we can improve the research designs in evaluative studies. The first relates to how research is conceptualized and designed. Similar to harder sciences (Becher, 1989), teacher education must establish relatively more linear and durable lines of research, work that addresses common questions, uses common measures, and thus accumulates knowledge (Grossman & McDonald, 2008). For instance, teacher education researchers may propose a central challenge for PSTs—for instance, responding to students' ideas (Kavanagh et al., 2020)—and then investigate different methods by which program experiences can prepare PSTs to engage in this skill. Or, researchers may choose to focus on a promising practice, like rehearsals, and provide evidence about its efficacy across many contexts, teacher educators, and potential designs. Whatever the case, establishing durable lines of research means that, over time, teacher education researchers may establish robust and nuanced evidence regarding the effectiveness of specific practices. This is not possible in a situation in which teacher education research tends toward entropy in its foci and aims.

The second shift relates to the publishing process. When authors seek to make inferences about PST learning from teacher education experiences, reviewers and editors must become

more demanding of their measures and research designs, preferring those with fewer threats to the validity of the conclusion. Reviewers and editors should ensure that each intervention is defined in enough detail that it is replicable by others wishing to take up the line of research. Further, authors must describe what happens in the "business as usual" condition, so the effects of treatment can be better interpreted. Finally, a common template for reporting methods and results would be useful, for we have noticed that studies inconsistently report several key study characteristics, including how investigators have selected PSTs to join the study, PST attrition, and the reliability and validity of PST scores on the study's outcomes of interest.

The final shift relates to expectations and support for causal research in teacher education. Instead of assuming that teacher education researchers can conduct their research "on the side"—often alongside busy teaching schedules—we must support them with the time and resources necessary to carry off these more labor-intensive designs. Faculty engaged with this effort need time (such as through course releases) to complete the work. Newly minted faculty need start-up packages that fund master's and doctoral student researchers. And the field needs funders—and in particular, federal and foundation grant-makers—willing to invest in better research designs and better measures. Moreover, institutions can facilitate this kind of research by collecting data about PST learning—including preprogram surveys, course grades, and records from clinical placements—that faculty can easily access for research purposes. Doctoral training institutions must encourage their graduate students to apprentice with faculty carrying out causal research, similar to how STEM graduate students often apprentice in the labs of established scholars. Doctoral training institutions must also examine their curricula to ensure that coursework focuses on producing scholars who can be active in different epistemological traditions, from interpretivism to positivism, and who can use both quantitative and qualitative

data to achieve their goals (see also Wilson, 2006). Changes like these will support more effectiveness research in teacher education and, in turn, improve the preparation of PSTs more broadly.

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