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A Meta-Analysis of the Impact of Professional Development on Teachers' Knowledge, Skill, and Self-Efficacy in Data-Based Decision-Making

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Abstract

The purpose of this review was to synthesize research on the effect of professional development (PD) targeting data-based decision-making processes on teachers' knowledge, skills, and self-efficacy related to curriculum-based measurement (CBM) and data-based decision-making (DBDM). To be eligible for this review, studies had to (a) be published in English, (b) include in-service or pre-service K–12 teachers as participants, (c) use an empirical group design, and (d) include sufficient data to calculate an effect size for teacher outcome variables. The mean effect of DBDM PD on teacher outcomes was $g = 0.57$ ($p < .001$). This effect was not moderated by study quality. These results must be viewed through the lens of significant heterogeneity in effects across included studies, which could not be explained by follow-up sensitivity analyses. In addition, the experimental studies included in this review occurred under ideal, researcher-supported conditions, which impacts the generalizability of the effects of DBDM PD in practice. Implications for research and practice are discussed.

Keywords

curriculum-based measurement, CBM, data-based individualization, DBI, data-based decision-making, DBDM, K–12 teachers, meta-analysis

Curriculum-based measurement (CBM; Deno, 1985) is a set of standardized assessment procedures developed in the 1970s by Deno and colleagues. The development of CBM aligned with the passing of Public Law (PL) 94-142 in 1975. Historically, this was the first congressional law that protected the educational right to a free and appropriate public education for children and adolescents with disabilities. PL 94-142 mandated the use of frequent assessment of student progress toward Individualized Educational Program (IEP) goals. Consequently, this legislation provided the impetus for developing assessment systems that supported special education teachers as they monitored student progress and made instructional changes aligned with data.

Deno's early work developing CBM procedures represented one approach to assessment that provided reliable and valid data on students' academic growth across time and allowed teachers to individualize instruction using data-based decision-making (DBDM; Deno, 2003). The CBM approach to assessment systematized the individualization process and helped school systems navigate the legal responsibilities outlined in PL 94-142. Deno and Mirkin (1977) conceptualized this process in their early work on data-based program modification (DBPM) and experimental teaching. These processes link the instruction and assessment portions of a child's education, under the assumption that data on student progress could inform intervention services and thereby improve the quality of instruction provided to each student in special education.

In this way, the goal of DBPM was to help teachers closely monitor student progress and use data to inform intervention, increasing compliance with legislative requirements. PL 94-142, currently enacted as the Individuals with Disabilities Education Improvement Act (IDEIA), has been amended a number of times to expand the federal protection of educational services for children with disabilities. Similarly, the DBPM model for intensifying intervention

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has evolved and expanded to account for the growing understanding of evidence-based practices for students with disabilities. Most recently, the model for intensive intervention has developed into data-based individualization (DBI; see Danielson & Rosenquist, 2014).

The DBI model built upon the DBPM model to include an explicit set of essential components. These components include (a) a validated intervention program foundation, (b) ongoing progress monitoring, (c) diagnostic assessment of need for students whose data indicate inadequate response, and (d) data-based adaptations used to individualize and intensify the intervention for these students (Danielson & Rosenquist, 2014). Using these components iteratively, educators are able to systematically assess the relative effectiveness of interventions and instructional adaptations to provide intensive interventions to students with the most persistent academic or behavioral difficulties.

Through the evolution of PL 94-142 and special education, many similar terms have been used to categorize DBDM processes. For this meta-analysis, we will use the term DBDM to categorize any systematic process of collecting student data to inform instruction, including DBPM, experimental teaching, and DBI. DBDM provides a process through which teachers may provide students more than de minimis special education services and "offer an IEP reasonably calculated to enable a child to make progress appropriate in light of the child's circumstances" (*Endrew F. v. Douglas County School District*, 2017, p. 15). Furthermore, this process may be used within a school's existing Response to Intervention (RTI) framework (Danielson & Rosenquist, 2014). Given that RTI provides one approach to identifying learning disabilities (i.e., instructional discrepancy hypothesis; Fletcher et al., 2019), DBDM is particularly relevant for students with learning disabilities before and after identification.

The Effect of CBM and Data-Based Decisions on Student Outcomes

Forty years of evidence support the use of CBM/DBDM frameworks to positively affect the academic outcomes of students with disabilities. The DBDM research base is largely rooted in the early CBM work of L. S. Fuchs and colleagues. This early work began with CBM training that involved administering assessments and evaluating data by hand (e.g., Fuchs et al., 1984) and via software that automatically collected, scored, graphed, and evaluated the data using decision rules (e.g., Fuchs, Fuchs, & Stecker, 1989). In some studies, computer-generated reports included dynamic goals (e.g., Fuchs et al., 1989b) and guided questions to improve teachers' interpretation and data evaluation (e.g., Fuchs et al., 1989a).

The computer reports improved teachers' use of CBM decision rules, but often teachers struggled to identify the specific changes to instruction they should make. Therefore, the research team began evaluating the effect of additional computer report components to make the nature of instructional changes more explicit, such as an analysis of student skills (e.g., Fuchs et al., 1991a) and instructional recommendations (e.g., Fuchs et al., 1991b). These additions improved teachers' use of instructional adaptations and the diversity of instructional programs. Later, Fuchs and colleagues began examining general education teachers' use of data to inform small group instruction (e.g., Fuchs et al., 1995). The research that has been published since the 1990s builds off of the foundational research program of L. S. Fuchs and colleagues.

Stecker et al. (2005) conducted a narrative review of the CBM literature to examine the effect of these CBM/DBDM systems on achievement. The authors found that students of teachers who adapted instruction based on CBM data showed greater growth across academic areas (e.g., reading, mathematics, spelling) than students of teachers who did not engage in the DBDM process. Stecker et al. (2005) noted that changes in student outcomes depended upon the entire DBDM process (i.e., a priori decision rules, data evaluation, error analysis, and instructional adaptations). In addition, the use of technology increased the efficiency and the acceptability of these practices for teachers. Finally, Stecker et al. (2005) noted that many CBM interventions with skills analysis or instructional recommendation components added value to intervention effects on student outcomes when administered in vivo or embedded within existing computer programs.

Similarly, Jung et al. (2018) conducted a meta-analysis of the effect of DBI on student achievement. The 14 studies in this meta-analysis compared a DBI-only condition (i.e., implementers received CBM data) to a business-as-usual (BAU) control. Six studies also included a DBI Plus condition, in which DBI implementers received additional information (e.g., skills analysis or instructional recommendations). The DBI-only versus BAU and the DBI Plus versus BAU overall average effect sizes (ESs) were $g = 0.37$ and $g = 0.38$, respectively. Effects did not differ across academic area, but were larger for students of teachers who received individual consultant and collaborative supports compared with less intensive implementation supports.

Current State of Improving Teachers' DBDM Expertise

Despite the evidence supporting the use of CBM/DBDM, the majority of the studies reviewed in Stecker et al. (2005) and Jung et al. (2018) occurred in the context of high levels of researcher support to teachers as they engaged in DBDM. Without such support, many teachers do not consistently and reliably use CBM data to make instructional adjustments that appropriately intensify interventions for students

(Deno, 2014). In their narrative review, Stecker et al. (2005) noted challenges related to teachers using CBM data to inform instruction. For example, many teachers failed to make instructional or goal-based changes based on CBM data, even when additional supports were provided to assist in CBM administration. In addition, Stecker et al. (2005) noted that teachers struggled to plan and enact instructional changes on their own, often requiring additional supports such as consultation or recommendation systems.

There has been an increased focus on DBDM frameworks in schools, such as the work of the National Center on Intensive Intervention (NCII; www.intensiveintervention.org). Lemons et al. (2019) reported lessons learned from NCII's technical assistance (TA) work to build effective DBDM systems with partner schools. The results of these interviews indicated similar challenges to DBDM implementation in practice. Although the school professionals interviewed remained resilient and positive, they noted challenges in implementing the full DBDM model and reported slow DBDM integration despite intensive TA.

Fuchs et al. (2015) argued that specialized interventions, such as those provided in the DBDM process, are likely to reduce achievement gaps by emphasizing individualized, intensive instruction. The focus of previous reviews (Jung et al., 2018; Stecker et al., 2005) was on student outcomes. Increasing teachers' DBDM expertise may be an important first step to improving student achievement. Additional information is needed to better understand how to structure professional development (PD) to support teachers' implementation of DBDM and elicit a cascading effect on student achievement (Deno, 2014).

Examining teacher-level interventions could provide future directions to make the use of DBDM more effective and feasible. In a recent literature review, Brownell et al. (2020) explored teacher education research published in *Teacher Education and Special Education* since 2010. The authors identified studies that aimed to measure or improve teacher outcomes across a range of teaching competencies. The authors found that teacher PD often targets teacher outcomes across three constructs: knowledge, instructional strategies (i.e., skills), and beliefs (i.e., self-efficacy). Teacher knowledge (i.e., teacher understanding of PD concepts, as assessed through outcome measures such as direct tests) and skill (i.e., teacher application of knowledge in practice, as assessed by measures such as fidelity) are malleable factors that can be improved through PD, though often PD targets discrete skills such as use of specific feedback (Brownell et al., 2020). In addition, self-efficacy, or teachers' belief in their abilities, may be a critical factor related to teachers' knowledge and skills, as well as student outcomes (Graham et al., 2001; Varghese et al., 2016). Finally, researchers have considered the extent to which teacher-level interventions met Council for Exceptional Children's (CEC, 2014) quality indicators (QIs). For example, Sweigart et al. (2016) examined the effect of performance feedback to increase teacher praise. Evaluating quality is important because assessing the quality of scientific research is a necessary step to accurately identifying evidence-based practices. Although CEC QIs are typically used to assess interventions for students with or at-risk for disabilities, there is a precedent for considering these QIs for teacher interventions such as PD as well (e.g., Sweigart et al., 2016).

To date, there has not been a review of teacher PD aimed specifically at DBDM. The purpose of this review was to build upon previous reviews of the effect DBDM on *student* outcomes, synthesize the impact of DBDM-focused PD on *teachers'* DBDM-related outcomes, and consider the influence of study quality on the effect of PD. We had the following research questions:

Research Question 1 (RQ1): Does DBDM-focused PD increase teachers' DBDM knowledge, skills, reported behaviors, and/or self-efficacy?

Research Question 2 (RQ2): Is the effect of DBDM PD moderated by study quality?

Method

Included studies were written in English and met the following inclusion criteria. First, participants were in-service or pre-service general or special education teachers of K–12 students. Second, the authors included a treatment condition in which teachers were explicitly trained to implement DBDM (e.g., data collection, analysis, data-based adaptations) in an academic area. Third, the authors measured teachers' DBDM knowledge, skill, and/or self-efficacy (*not* DBDM acceptability/usefulness or self-reported time estimates). Fourth, the authors used a randomized control trial (RCT), quasi-experimental, or single case design (SCD). No SCD studies met all criteria. Finally, we required sufficient data to calculate an ES.

Search Procedures

We searched databases for key terms related to: (a) participants *(teach* OR educat*)*, (b) independent/dependent variables *("curriculum based measure*" OR "progress near/2 monitor*" OR ["data based" near/2 [individualization OR decision OR modification OR instruction]] OR "experiment* teach*")*, and (c) study design *([random* OR RCT] OR [quasi-experiment* OR QED] OR ["single case" OR "single subject" OR SCD OR "case study"] OR [experiment* OR impact OR effect OR effectiveness OR caus*] OR [posttest OR post-test OR pretest OR pre-test] OR "efficacy trial")*. We used the "AND" Boolean term to connect each parenthetical category.

We conducted an electronic search of the *PsycINFO, ERIC*, and *ProQuest Education* databases to identify published literature (dissertations/theses excluded; no limiters for search term location) and a separate search of the *PsycINFO, ERIC, ProQuest Education, ProQuest Dissertations Global*, and *Dissertations & Theses @ Vanderbilt University* databases to identify potential gray literature (dissertations/theses as publication type; required key terms in abstract). We conducted an initial search in February 2018 and an updated search in September 2019.

We applied four additional search methods. First, we screened the studies in previous syntheses (Jung et al., 2018; Stecker et al., 2005). Second, we backward searched the references included studies. Third, in Google Scholar, we forward searched articles citing included studies. Finally, we hand searched the Online First and 2016–2019 issues of *Exceptional Children, School Psychology Review, Remedial and Special Education, Journal of Learning Disabilities, Teacher Education and Special Education*, and the *American Educational Research Journal*.

Screening Procedures

These search procedures yielded 6,491 records (see Figure 1). We conducted title/abstract screening, followed by full text screening of 252 records. Screening yielded 31 included studies from database searches. During full text coding, we excluded three studies that did not have sufficient data to calculate an ES and one study (Lembke et al., 2018) that reported data which were later published in another included study (McMaster et al., 2020; author confirmed this interpretation). This left 27 included studies from the original database searches. The additional search methods yielded one other study. The final set of 28 studies included data representing 26 unique teacher samples due to two sets of overlapping participants of (a) Fuchs, Allinder, et al. (1990) with Fuchs et al. (1991a) and (b) Fuchs and Fuchs (1993) with Fuchs et al. (1989).

Screening training included a review of the inclusion criteria and independent practice until agreement reached at least 90% agreement with the first author's screening decisions for the practice items. The first author screened all title/abstracts and full text records. A trained PhD student double screened a randomly selected 23.8% of title/abstracts and 27.38% of full text records. Screeners discussed discrepancies and reached consensus for final decision and reason for exclusion. Agreement was high for title/abstract (95.9%) and full text screening (91.5%).

Coding Procedures

Our codebook included six sections (Context/Setting, Participants, Intervention Agent, Treatment Conditions/Fidelity, Measures/Data, and Design/Data Analysis) based on the categories of quality delineated in CEC's QIs for group comparison research (2014). See supplemental material (Table S1) for an overview of each section of the codebook, including examples of coded variables. Each section of the codebook included descriptive variables (e.g., location, teacher participant certification, teacher trainer qualifications, teacher intervention components, posttest scores on outcome measures, data analysis used) and culminated with the CEC QIs for that category and those addressing related threats to validity. The CEC QIs are typically applied to student-level interventions; consequently, we adapted some standards to reflect the nature of this review's scope (i.e., teacher-level interventions). To receive credit for a QI, the authors of the study needed to report the information required by the QI. We calculated an overall quality rating by calculating the percent of applicable QIs the study met.

We also categorized features of treatment conditions, control comparison conditions, and dependent variables. We coded treatment conditions across characteristics related to context (i.e., opportunity for applied practice and use of a computer to display CBM/DBDM data), report components (i.e., use of dynamic goals, computer- or researcher-provided decision related to the adequacy of student response, analysis of student skills, and recommendations for instruction), and additional supports (i.e., expert guidance, self-monitoring forms or guided questions, and collaboration). Treatment conditions often had more than one of these characteristics. We categorized control comparison conditions across three categories: (a) BAU with no CBM, (b) BAU with CBM, and (c) attention control.

We categorized teacher-level outcomes across five relevant outcome types: (a) knowledge, (b) skill, (c) self-efficacy, (d) computer-report data of teacher behaviors, and (e) self-report measures. We categorized outcomes as knowledge outcomes if they assessed teachers' knowledge of DBDM components or procedures separate from classroom application (e.g., multiple choice tests). In contrast, skills outcomes included direct measurement of teachers' classroom behaviors (e.g., CBM or DBDM fidelity). The self-efficacy outcome category reflected measures of teachers' belief in their ability to engage in DBDM processes. Although different than the other areas of teacher DBDM expertise, we believed it would be important to include self-efficacy in the review of the studies because teachers' beliefs in their ability to perform tasks are associated with teachers' knowledge and skills, as well as student outcomes (Graham et al., 2001; Varghese et al., 2016). Finally, reported teachers' behaviors reflected frequency counts or estimated percentage of time spent engaging in DBDM processes, as measured by computer system reports of teacher behavior (e.g., computer-generated, auto-calculated number of CBM measurements based on data inputted) or teachers' self-reports of their classroom behaviors

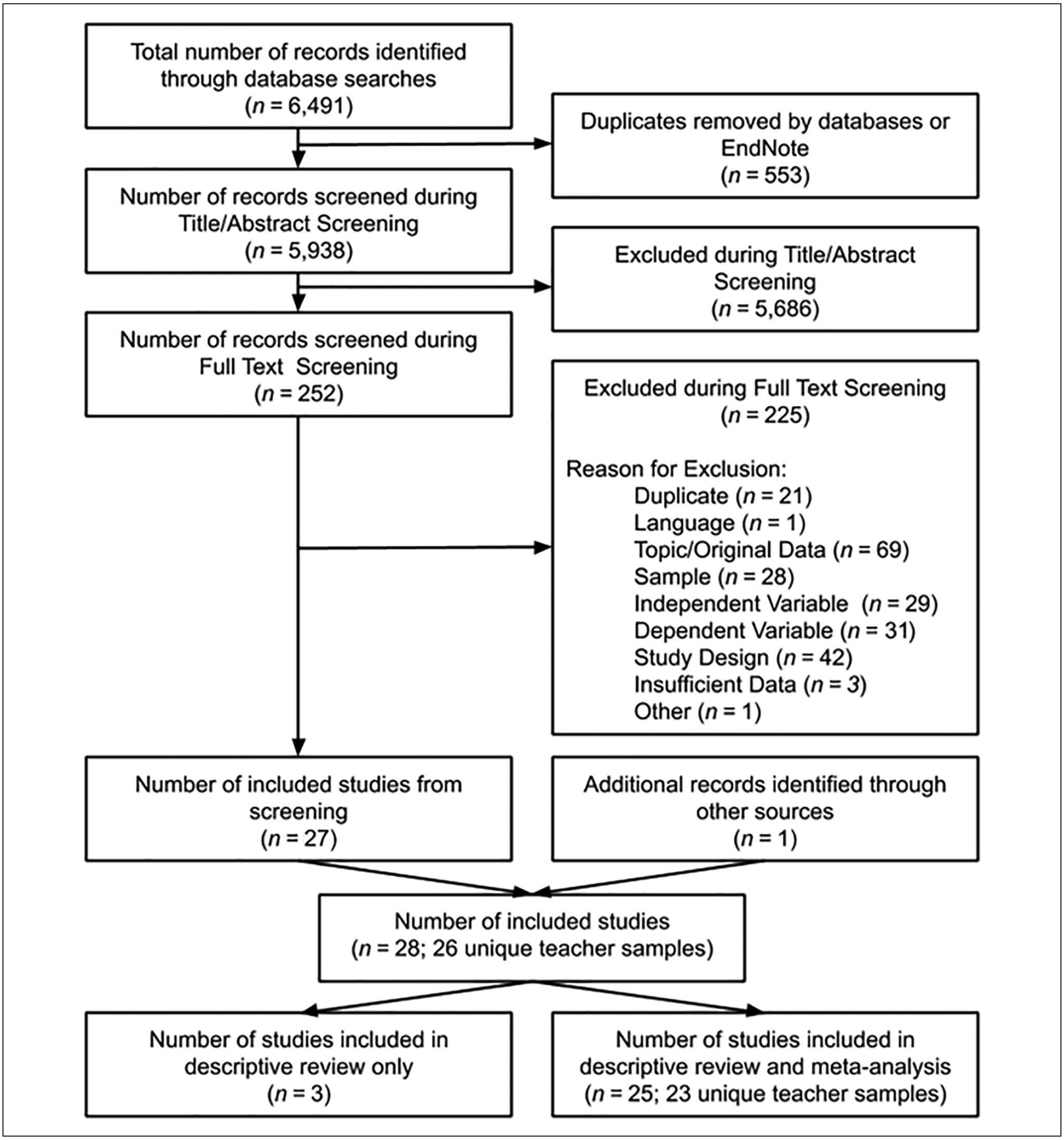

Figure 1. Screening procedures to determine study eligibility.

(e.g., teachers' self-reported use of systematic monitoring). We hypothesized that these categories reflected aspects of DBDM expertise as a broad construct, captured by our primary research question.

Codebook training included a review of the variables and independent coding practice until reaching 90% agreement with the first author's coding of practice articles. A second data enterer confirmed the accuracy of all descriptive data entered and independently entered outcome measure data into the ES database to ensure reliable data entry. ES data entry reliability was 100%. A second rater also blindly and independently coded the treatment condition characteristics, control comparison conditions, dependent variables, and QIs for seven (25%) randomly selected studies. Inter-rater agreement was sufficiently high for treatment characteristics (M = 81.48%), control condition category (80%), dependent variable category (100%), and QIs (94.64%). Coders discussed discrepancies and arrived at consensus.

Data Analysis

To be included in the synthesis of variables, the reported outcomes needed to have sufficient data to calculate an ES (i.e., Hedges's *g*; Hedges, 1981) and have a clear therapeutic direction of change. If a study reported an outcome measure with a negative therapeutic direction of change, we reverse-coded (such that higher numbers represented improved scores) before computing ESs when possible. Finally, in the case that researchers described teacher-reported use of different types of assessment to inform instructional decisions, we only included teacher-reported use of systematic monitoring, given that this method of data use was the only directly targeted outcome measure in the PD.

Three studies were included in the descriptive review, but not in the meta-analysis. First, Mathes et al. (1998) provided each condition's *M* and *SD* for teacher-level measures, but reported these values based on the number of students in each group. Therefore, there was no way to capture the PD's effect on individual teacher participants. Second, Wilson (2013) used a Mann–Whitney *U* test to determine treatment effect. Although online calculators provide an ES estimate from these values, it was not possible to synthesize the Eta-squared estimates with the remaining ES estimates. Finally, Fuchs et al. (1988) included measures that did not have a clear therapeutic direction of change (i.e., time in measurement and evaluation).

For the majority of outcome measures, authors reported mean values and standard deviations for data extraction. We included data for all relevant condition comparisons. For Casey et al. (1988), we included data for only the treatment and *volunteer* comparison conditions in ES calculations. The remaining studies in this review included two types of comparisons. In some studies, authors compared a treatment with a control condition. In other studies, authors compared the effect of two CBM/DBDM PD conditions. Studies in which authors collected teacher data for treatment conditions only—even if a control condition existed—fell into the latter category of comparison. If authors included data for two treatments *and* a control condition, we only calculated ESs for the treatment versus control comparisons.

For studies that did not report *M*s and *SD*s for relevant outcome measures, but reported data from which we could calculate ESs (e.g., means/*SD* with subgroups, or F test results), we used the Campbell Collaboration's ES Calculator (https://bit.ly/2VX0YS9) to calculate and extract Cohen's *d* and V_d. We then transformed all ESs from *d* to *g*, and used Stata/SE 14.0 (StataCorp, 2015) for all analyses. We included multiple ESs per study to assess the effect on all relevant measures. For all analyses, we used random-effects models and used random-effects robust variance estimation (RVE) to account for ES dependency (Hedges et al., 2010; Tanner-Smith & Tipton, 2014). We set $\rho = 0.80$ and conducted follow-up sensitivity tests that assessed the effect of different levels of correlation ($\rho = 0.80$, 0.50, and 0.20); results indicated that varying ρ values did not change results, suggesting the present findings are robust.

We used a moderator analysis to analyze the influence of study quality on treatment ES. We tested whether the study quality, as defined by the percent of applicable CEC QIs the study met (i.e., reported information required by the QI), significantly predicted individual-study ESs. Prior to moderator analyses, we assessed overall heterogeneity using the Q, τ^2, and I^2 statistics. The Q statistic identifies whether there is significant between-study variability in the meta-analytic sample. The τ^2 statistic provides an estimate of the distribution of the individual-study ESs. The I^2 is the proportion of observed variance of ESs that is true variance, rather than variance accounted for by sampling error (Borenstein et al., 2009).

We tested for potential outliers within the original forest plot output by looking at the ends of the ES distribution. Because studies with larger sample sizes and larger ESs have a greater chance of being published than studies with small samples and/or null effects (Chow, 2018), we tested for publication bias in three ways: (a) funnel plots of the ESs for visual analysis of the relation between ESs and their SE (Sterne et al., 2011), (b) Egger's linear regression method (Egger et al., 1997) to assess potential publication bias via small-study effects, and (c) calculated the ES for the published versus unpublished set of studies and reported these values separately to capture the potential publication bias further (Chow & Ekholm, 2018).

To assess the robustness of our analyses, we conducted sensitivity analyses to assess if treatment and study-level characteristics influenced the overall mean ES. There were several factors that were important to consider and capture through sensitivity analyses: (a) overlapping study participants across studies; (b) condition comparison type (i.e., two treatments compared to a treatment vs. control); (c) outcome type (i.e., knowledge, skill, self-efficacy, computer reports, or self-reported behavior); (d) subject area (i.e., reading, math, or spelling); (e) study duration (in weeks); (f) teacher type (i.e., pre-service vs. in-service); (g) research lab (i.e., L. S. Fuchs lab vs. not); (h) primary study goal (teacher vs. teacher *and* student) and (i) year of publication.

Results

There were 28 included studies, but only 26 unique teacher samples. To ensure that data from these studies did not have an undue influence on outcomes, we analyzed study, participant, and treatment condition characteristics using the 26 unique samples and reported data based on the study with the most complete sample. In the case of discrepancies, we relied on data reported in the most recent publication. For these reasons, we used descriptive data from Fuchs et al. (1991a) and Fuchs, Fuchs, and Stecker (1989) for the sets of overlapping participant samples. We used data from all studies (n = 28) when analyzing measures and quality. Table 1 provides an overview of study characteristics. Table 2 provides a summary of the experimental conditions and outcomes included in the meta-analysis across studies.

Study Characteristics and Setting

Most studies were published in peer-review journals (n = 22; 84.62%) and used an RCT design (n = 24; 92.31%) that randomized teachers to conditions. Mathes et al. (1998) used a stratified RCT to assign schools to conditions. The remaining two studies (7.69%) used a quasi-experimental design (Casey et al., 1988; McCullum, 1999). Study duration ranged greatly, from a single training session to 25 weeks of intervention. The majority of the studies lasted 15 to 20 weeks. The majority of the studies took place in the southeast (n = 14; 53.85%) and midwest (n = 6; 23.08%) portions of the United States and occurred at least partially in an urban setting (n = 20; 76.92%) within K–12 schools (n = 24; 92.31%). The 28 total studies focused on mathematics (n = 8; 28.57%), reading (n = 12; 42.86%), spelling/writing (n = 6; 21.43%), or a combination (n = 2; 7.14%). Finally, L. S. Fuchs was an author on 17 (60.71%) of the 28 total included studies.

Teacher Participants

The 26 samples included 1,193 teachers (12–270 per study; median = 30). Researchers in only 15 studies (57.69%) reported gender, with 10 to 215 females per study (70.97%–100% of each study's sample). Researchers in fewer studies (n = 9; 34.62%) reported race/ethnicity, with a reported total of 27 non-White teacher participants (0%–20.83% of each study's sample). Studies included pre-service (n = 332; 27.83%) and in-service (n = 807; 67.64%) teachers. The in-service teachers included 311 general educators (26.07% of all participants) and 496 (41.58%) special educators, with varying levels of degree attainment. In-service teachers averaged 10.44 years of teaching experience (M = 3.68–16 years). The majority of the studies included elementary and middle school teachers. See supplemental material (Table S2) for participant information.

Conditions

Researchers in six studies (23.08%) compared a DBDM treatment condition with a control condition, eight (30.77%) compared two DBDM conditions, and 12 (46.15%) compared two DBDM conditions and a control condition. This amounted to 47 conditions involving DBDM PD. See supplemental material (Table S3) for experimental condition information.

Most treatment conditions included opportunities for applied practice across days (n = 38; 80.85%); CBM software that automatically graphed data, applied decision rules, and provided reports of student progress (n = 27; 57.45%); and individual student CBM reports (n = 40; 85.10%). Researchers in two studies (four conditions) included technology to train teachers (i.e., content acquisition podcasts in Kennedy et al., 2016; digital modules in van den Bosch et al., 2019). For each treatment condition, researchers used one or more additional report component or support. CBM report components included the addition of (a) dynamic goals (n = 27; 57.45%); (b) providing correct decision related to adequacy of student growth (n = 23; 48.94%); (c) student or class-wide skills analysis (n = 15; 31.91%); and (d) instructional recommendations for individual, small group, or whole class instruction (n = 11; 23.40%). Additional supports included expert guidance through consultation or mentoring (n = 34; 72.34%), self-monitoring forms or guided questions (n = 8; 17.02%), and collaboration with other teachers (n = 3; 6.38%).

There were 19 control conditions across 18 studies (69.23%). Control conditions included 12 (63.16%) BAU control conditions (no CBM), four (21.05%) BAU control conditions with regular teacher practices including CBM data collection, and three (15.79%) attention control conditions that included support related to assessments broadly or general instructional practices.

Teacher-Level Outcome Measures

There were 100 dependent variables included in the meta-analysis. These dependent variables were reported across 25 studies. As noted in the "Method" section, three studies (Fuchs et al., 1988; Mathes et al., 1998; Wilson, 2013) were included in the descriptive review, but not in the meta-analysis. See supplemental material (Table S4) for outcome measures by study. By outcome type, these variables included measures of (a) DBDM skill (44 variables [44%] measured in 18 studies [72%]), (b) computer-based reports of teacher behaviors (26 variables [26%] in eight studies [32%]), (c) self-report of teacher behaviors (13 variables [13%] in eight studies [32%]), (d) DBDM knowledge (12 variables [12%], in four studies [16%]), and (e) self-efficacy (five variables [5%] in four studies [16%]). The specific measures used varied across studies. The most common outcome measures, accounting for 30 of the dependent variables, were versions of the *Modified Accuracy of Implementation Rating Scale* (MAIRS), a fidelity rating tool that includes mathematics, reading, and spelling versions

Table 1. Study Characteristics and Setting.

Author (year)	Type	Design	*N*	Focus	Location	Setting	Role/Grade	Area	Duration	QIs Met (%)
Allinder & BeckBest (1995)	PRJ	Strat. RCT	18	T & S	US: MW	Rural	SE; 2–8	Math	24 weeks	81.80
Allinder et al. (2000)	PRJ	RCT	30	T & S	US: MW	Urban	SE; M = 4	Math	NR	79.20
Capizzi & Fuchs (2005)	PRJ	RCT	35	T	US: SE	Urban	GE/SE; 1–5	Read	40 min	91.70
Casey et al. (1988)	PRJ	QED	61	T	US: MW	Urban	SE; K–6	Read	24 weeks	66.70
Fuchs (1988)	PRJ	RCT	18	T & S	US: SE	Urban	SE; NR	Spell	15 weeks	95.50
Fuchs, Allinder, et al. (1990)*	PRJ	RCT	27	T	US: SE	Urban	SE; 3–9	Spell	15 weeks	83.30
Fuchs et al. (1984)	PRJ	Strat. RCT	39	T & S	US: NYC	Urban	SE; M = 5	Read	18 weeks	83.30
Fuchs & Fuchs (1993)**	PRJ	RCT	20	T	US: SE	Urban	SE; NR	Read	12–15 weeks	95.50
Fuchs et al. (1989a)	PRJ	RCT	27	T & S	US: SE	Urban	SE; M = 4	Spell	15 weeks	87.50
Fuchs et al. (1989b)	PRJ	RCT	30	T & S	US: SE	Urban	SE; 2–9	Math	15 weeks	87.50
Fuchs et al. (1991a)*	PRJ	RCT	30	T & S	US: SE	Urban	SE; 3–9	Spell	15 weeks	95.80
Fuchs et al. (1991b)	PRJ	RCT	30	T & S	US: SE	Urban	SE; 2–8	Spell	18 weeks	83.30
Fuchs et al. (1992)	PRJ	RCT	33	T & S	US: SE	Urban	SE; 1–9	Read	17 weeks	91.70
Fuchs et al. (1994)	PRJ	Strat. RCT	40	T & S	US: SE	Urban	GE; 2–5	Math	25 weeks	91.70
Fuchs et al. (1995)	PRJ	Strat. RCT	20	T & S	US: SE	Urban	GE; 2–4	Math	25 weeks	95.50
Fuchs, Fuchs, et al. (1990)	PRJ	RCT	30	T & S	US: SE	Urban	SE; 1–9	Math	15 weeks	87.50
Fuchs, Fuchs, Hamlett, & Stecker (1991)	PRJ	RCT	33	T & S	US: SE	Urban	SE; 2–8	Math	20 weeks	83.30
Fuchs, Fuchs, & Stecker (1989)**	PRJ	RCT	30	T	US: SE	Urban	SE; 2–9	Read	12–15 weeks	87.50
Fuchs et al. (1988)	PRJ	RCT	20	T	US: SE	Urban	SE; M = 4	Mix	15 weeks	86.40
Garnes (2004)	DIS	Strat. RCT	17	T	US: Utah	NR	PST; K–8	Read	1 semester	90.90
Kennedy et al. (2016)	PRJ	Strat. RCT	270	T	US: Varied	NR	PST; K–12	Read	30–45 min	90.90
Mathes et al. (1998)	PRJ	Strat. RCT***	24	T	US: SE	SU, Urban	SE; 2–6	Read	NR	87
McCullum (1999)	DIS	Matched QED	12	T & S	US: NW	SU, Urban	GE; 2–3	Read	12 weeks	95.50
McMaster et al. (2020)	PRJ	Strat. RCT	21	T	US: MW	Urban	SE; 1–5	Write	20 weeks	100
Tichá (2008)	DIS	Strat. RCT	34	T & S	US: MW	Rural, SU	SE; 2–11	Read	15 weeks	83.30
van den Bosch et al. (2019)	PRJ	RCT	184	T	NL	NR	GE/SE; 3–6	Read	25–45 min	100
van der Scheer & Visscher (2016)	PRJ	RCT	62	T	NL	NR	GE; 4	Math	1 year	87.50
Wilson (2013)	DIS	RCT	45	T	US: MW	Urban	PST; NR	Mix	30–105 min	95.80

Note. PRJ = peer-reviewed journal; DIS = dissertation; Strat. = stratified; RCT = randomized control trial; QED = quasi-experimental design; T = teacher; S = student; MW = midwest; SE = southeast; NW = northwest; NL = The Netherlands; SU = suburban; SE = special educator; GE = general educator; PST = pre-service teacher; NR = not reported; M = mean; QI = quality indicator. *Fuchs et al. (1991a) and Fuchs, Allinder, et al. (1990) used same teacher participants for treatment conditions. **Fuchs, Fuchs, and Stecker (1989) and Fuchs and Fuchs (1993) used same data for treatment conditions (Fuchs & Fuchs, 1993 only included two treatment groups). ***Randomized schools, not teachers.

Table 2. Experimental Conditions and Outcome Measures Included in Meta-Analysis.

Author (year)	Experimental conditions	Number of outcomes by outcome type: Knowledge	Skill	Self-efficacy	Computer Bx	Self-report Bx
Allinder & BeckBest (1995)	T1: Consultant; T2: SM	0	3	0	4	0
Allinder et al. (2000)	T1: SM; T2: CBM Only; C1: BAU (No CBM)	0	1	0	0	0
Capizzi & Fuchs (2005)	T1: Diagnostic Feedback; T2: CBM Only; C1: No CBM	0	3	0	0	0
Casey et al. (1988)	T1: DBI; C1: BAU (w/CBM)	2	0	0	0	0
Fuchs (1988)	T1: Computer; T2: Pen & Paper	0	2	0	0	0
Fuchs, Allinder, et al. (1990)*	T1: Performance + Skill; T2: Perf Only; C1: BAU (No CBM)	0	1	0	0	0
Fuchs et al. (1984)	T1: CBM only; C1: Attention (No CBM)	0	1	0	0	1
Fuchs & Fuchs (1993)**	T1: Computer; T2: Pen & Paper	0	3	0	0	0
Fuchs et al. (1989a)	T1: Enhanced; T2: Unenhanced; C1: BAU (No CBM)	0	3	0	2	0
Fuchs et al. (1989b)	T1: Dynamic Goal; T2: Static Goal; C1: BAU (No CBM)	0	3	0	2	0
Fuchs et al. (1991a)*	T1: Performance + Skill; T2: Perf Only; C1: BAU (No CBM)	0	4	0	4	0
Fuchset al. (1991b)	T1: Expert System; T2: No Expert; C1: BAU (No CBM)	0	3	0	4	2
Fuchs et al. (1992)	T1: Expert System; T2: No Expert; C1: BAU (No CBM)	0	3	0	4	0
Fuchs et al. (1994)	T1: Instructional Recs; T2: CBM Only; C1: BAU (No CBM)	0	3	0	0	0
Fuchs et al. (1995)	T1: ADAPT; T2: CBM only	0	2	0	0	1
Fuchs, Fuchs, et al. (1990)	T1: Performance + Skill; T2: Perf Only; C1: BAU (No CBM)	0	4	0	4	0
Fuchs, Fuchs, Hamlett, & Stecker (1991)	T1: Expert System; T2: No Expert; C1: BAU (No CBM)	0	5	0	0	1
Fuchs, Fuchs, & Stecker (1989)**	T1: Computer; T2: Pen & Paper; C1: Attention (No CBM)	0	1	0	0	5
Garnes (2004)	T1: Mentoring; T2: DBI Only	0	1	0	0	0
Kennedy et al. (2016)	T1: Podcasts; T2: Text-Only	2	0	0	0	0
McCullum (1999)	T1: Collaborative; T2: Solo	0	0	1	0	1
McMaster et al. (2020)	T1: DBI; C1: BAU (No CBM)	1	0	2	0	2
Tichá (2008)	T1: DBI; C1: BAU (with CBM)	0	0	0	0	1
van den Bosch et al. (2019)	T1: Basic; T2: Interpretation; T3: Interpretation + Linking; C1: Attention (No CBM)	7	0	0	0	0
van der Scheer & Visscher (2016)	T1: DBI (2013–2014; T1–T2) C1: BAU (No CBM)	0	0	2	0	0

Note. T = treatment; C = control; BAU = business as usual; CBM = curriculum-based measurement; DBI = data-based individualization; SM = self-monitoring; Bx = behavior. The count of measures for each study reflects the number of outcome measures that were included in the meta-analysis, which depended on there being sufficient information to calculate an effect size comparing at least two conditions. *Used same teacher participants for treatment conditions. **Used same data for treatment conditions (Fuchs & Fuchs, 1993, only included T1 and T2).

(M-MAIRS, R-MAIRS, and S-MAIRS, respectively) to assess teachers' setup/structure for data use, their adherence to measurement protocols, and their evaluation/utilization of the data.

Overall, the majority of the outcomes were technically adequate. The authors reported interobserver or inter-rater agreement for 79 outcome measures, with a mean agreement of 94.92% (range: 80.9%–100%). The authors also reported Cronbach's alpha for five of those measures. In total, the authors reported Cronbach's alpha for 15 measures, with a mean value of .77 (range: .69–.88). The authors reported test–retest reliability (.68) for one measure. The authors did not report reliability for 10 outcome measures; eight of those measures were self-reports of behaviors.

Study Quality

Table 1 includes an overview of each QI. For each QI, authors needed to report information required by the QI to receive credit. The 28 included studies met an average of 88.77% (SD = 7.09; range: 66.67%–100%) of the applicable QIs, with two studies (McMaster et al., 2020; van den Bosch et al., 2019) meeting 100% of the indicators. All 28 studies met 10 of the QIs, if the QI was applicable (QI 1.1, 4.2, 6.1, 6.3, 6.8, 7.1, 7.2, 7.4, 7.6, 8.1). The majority of studies met 11 additional QIs (QI 2.2, 3.2, 4.1, 5.2, 5.3, 6.2, 6.4, 6.9, 7.3, 7.5, 8.3), when the QI was applicable. Fewer studies met three QIs (46.43% of studies met QI 2.1 [teacher participant demographics]; 46.43% met QI 3.1 [role and background of intervention agent training the teacher]; 25% met QI 5.1 [implementation fidelity of teacher training adherence]).

Meta-Analysis Results

The overall ES for CBM/DBDM PD on teacher-level outcome measures was moderate and statistically significant (g = 0.57, SE = 0.13, $p < .001$, 95% CI = [0.292, 0.846]; range = −1.25 to 3.53). This result suggests a significant main effect of teacher-level CBM/DBDM PD on teachers' knowledge, skill, and self-efficacy related to CBM/DBDM procedures. By outcome type, ES ranges varied (Knowledge: −0.02 to 2.28; Skill: −1.25 to 1.96; Self-Efficacy: −0.08 to 0.78; Computer Report: −0.54 to 2.01; Self-Report: −0.160 to 3.53). This suggests heterogeneity of effect that the overall average ES fails to capture. The moderator analysis that examined whether study quality was associated with ES magnitude was not statistically significant ($\beta = .007$, $p = .966$, 95% CI = [−0.042, 0.043]).

Additional Analyses

Heterogeneity. The Q statistic was statistically significant ($p < .001$), which suggests that effects varied across studies. The τ^2 value was 0.57, which suggests a meaningful distribution in the individual-study ESs estimates across studies. Similarly, the I^2 statistic indicated that 95.7% of the observed heterogeneity in the sample could be accounted for by true, between-study variability. These analyses suggest a need for additional exploratory moderator analyses to explain variance not captured by our planned moderator analysis.

Post hoc exploratory moderator analyses. We conducted a series of exploratory moderator analyses in an attempt to explain variation in ES magnitude in the present corpus of studies. We had categorized outcomes into five outcome types. Through our exploratory moderator analyses, we hypothesized that the effect of DBDM PD on teacher outcomes may vary as a function of outcome type. To this end, we fit a meta-regression model using type of outcome as predictors of the overall mean ES. We elected to conduct meta-regression analyses in an effort to include as much data as possible. Although some researchers address moderator analyses by separating out the data into individual subgroup analyses, we followed current best practices in moderator analyses (Tipton et al., 2019). In addition, including the other predictors in the meta-regression models accounted for additional variance and provided a more precise estimate for each of the included moderators. We included three outcome type moderators for which we had sufficient data: knowledge, skill, and computer report. Results of this analysis indicated that skill-based outcomes were significantly associated with a smaller ES magnitude ($\beta = -.86$, $SE = 0.35$, $p = .035$), while knowledge and computer report were not ($\beta = .45$; $p = .976$ and $\beta = .09$; $p = .071$, respectively).

Outliers and publication bias. There were no outliers that needed to be accounted for in statistical analyses. We conducted three assessments of publication bias risk to assess bias across multiple types of information (Chow, 2018). First, visual analysis of the funnel plot indicated relative symmetry across ESs and SEs. However, the results of the Egger's test of asymmetry in the distribution of ESs and SEs were significant ($p = .04$), which indicates possible bias due to small-study effects (i.e., positively biased ESs for smaller versus larger studies; Ekholm & Chow, 2018). Finally, we examined average effects for published versus unpublished studies to assess the potential effect of publication. These results demonstrated that published articles had higher ESs than unpublished ($g = 0.54$ vs. $g = 0.39$, respectively); however, the small number of unpublished studies ($n = 2$) limits conclusions that can be drawn from these results.

Sensitivity analyses. To assess the robustness of the findings, we tested the influence of overlapping participant samples, condition comparison type, subject area focus, teacher type (in-service vs. pre-service), L. S. Fuchs research lab, teacher role (teacher vs. teacher and student), and year of publication on the results. None of these factors significantly predicted the individual-study ES and CI, nor did they not change the significance of any results reports. These results suggest our findings in this meta-analysis were robust.

Discussion

The purpose of this meta-analysis was to descriptively and meta-analytically synthesize the effect of teacher-level

DBDM PD on teachers' DBDM expertise. Previous reviews examined the effect of teachers' use of CBM/DBDM on *students'* achievement. The authors found that CBM/DBDM had a positive effect on student outcomes in both descriptive (Stecker et al., 2005) and meta-analytic reviews (g = 0.37; Jung et al., 2018). These results support the assertion that specialized interventions are likely to positively affect student achievement (Fuchs et al., 2015). Stecker et al. (2005) descriptively found that, with researcher support, teachers appropriately implemented CBM/DBDM procedures; however, Deno (2014) reported that these practices are less likely to occur in practice. DBDM PD may be one way to improve teacher outcomes, which may, in turn, have a cascading effect on student achievement (Deno, 2014). This meta-analysis extends the work of previous reviews through a statistical analysis of the effect of DBDM PD on *teacher* outcomes. To our knowledge, this study is the first meta-analysis to assess the effect of DBDM PD on teachers' DBDM expertise.

In all, 28 studies met our inclusion criteria, representing 26 unique teacher samples. The results indicated a significant effect of DBDM PD on teacher-level outcomes (g = 0.57). This effect was not moderated by quality or two outcome types (knowledge and self-reported outcomes). Skills-based outcomes were significantly associated with smaller ESs. This result may indicate that it is more challenging to influence teacher scores on skills-based measures. Alternatively, this result may be due to the fact that skills-based measures were more likely to be assessed in treatment conditions only, rather than control conditions. Therefore, the smaller ESs may be confounded by the tendency for ESs related to treatment versus treatment comparisons to be smaller than ESs for treatment versus control comparisons (Borenstein et al., 2009).

Limitations

There are limitations to the findings of this meta-analysis. First, analyses indicated a possible presence of publication bias. Consequently, the ES may be inflated. Second, there was significant heterogeneity across studies. It is possible that there are alternative characteristics of the study set that are contributing to these differences in effect across studies. It is also possible that this set of studies is underpowered for detecting sources of heterogeneity even if it were a significant factor (Borenstein et al., 2009). Third, there was variability in the types of outcome measures used across studies. We included all dependent variables that met criteria for inclusion, regardless of technical adequacy, and condensed outcome measures into one DBDM expertise construct to discuss the effect of PD on this construct broadly. We could not assess effects across all outcome types, specifically those without enough data to be included in exploratory moderator analyses (i.e., self-efficacy, self-reported behaviors). Although we believed it would be important to include self-efficacy because teachers' belief in their abilities is associated with teacher knowledge, teacher skills, and student outcomes (Graham et al., 2001; Varghese et al., 2016), it is possible that the inclusion of this construct contributed to the challenges associated with outcome measure variability and heterogeneity of effect. Finally, we required the authors of the studies to report information required by each QI to receive credit for that QI. However, many of the included papers were published prior to publication of the QIs. Therefore, it is possible that the original authors of studies may not have reported all information required for QIs, even if they appropriately addressed the QI when conducting the study.

Implications for Research and Practice

Previous research has shown that when teachers use DBDM processes under ideal conditions (i.e., intensive researcher support), their students show greater academic growth than students of teachers who do not engage in such processes (Jung et al., 2018; Stecker et al., 2005). The findings of this meta-analysis indicate that DBDM PD increases teachers' DBDM expertise under ideal, researcher-supported conditions. These results suggest directions for future research.

First, the authors of the set of studies in this meta-analysis did not assess the extent to which teachers showed continued use of DBDM after the removal of researcher support, nor did they evaluate the effect of DBDM PD on teacher outcomes in less ideal conditions (i.e., less intensive supports that more closely align to typical PD contexts). Future research should examine the effect of DBDM PD on teacher outcomes in these more naturalistic settings, with follow-up assessments of continued use of DBDM practices after study completion. By examining the effect of DBDM PD in these contexts, researchers may begin to build an empirical rationale supporting the hypothesized theory of change that DBDM PD can positively impact teachers' DBDM expertise and, in turn, improve student achievement. To robustly build this empirical rationale, researchers should implement DBDM PD with broader samples of teachers, including those who work with students with wider ranges of disabilities and those who work in middle or high schools. Collectively, this line of research could begin to address concerns of feasibility and external validity by highlighting barriers to implementation and identifying factors that contribute to continued use of DBDM. This is a first step to addressing the research-to-practice gap and implementation science challenges (Klingner et al., 2013).

Second, although insufficient reporting can be a challenge in many reviews, it was especially problematic in this review due to the teacher focus. Only a small proportion of studies met QIs related to participant (i.e., teacher) demographics, intervention agent (i.e., deliverer of the PD), and PD implementation fidelity. Future research should use more rigorous reporting standards when reporting teacher-level characteristics, interventions, and intervention agents, given that there is so much to learn about how to positively impact teachers' DBDM expertise.

Third, the presence of significant heterogeneity of effects in our findings (unexplained by any follow-up analyses) may reflect a challenge in synthesizing teacher outcomes. This characteristic contrasts with student outcomes, which are arguably more clearly defined and regularly studied. For example, although Jung et al. (2018) found significant heterogeneity in their meta-analysis, they resolved this heterogeneity issue by a follow-up moderator analysis that accounted for different types of measures (researcher-developed vs. standardized). Special education researchers do not yet consistently operationalize or measure teacher-level outcomes in the same way. It is possible that the variability in outcomes significantly contributed to the heterogeneity. Although we tried to evaluate different categories of outcomes (e.g., skill, knowledge), the measures included within each category were still wide-ranging. If special education researchers worked toward operationalizing critical aspects of teachers' DBDM implementation and came to an agreement on a set of common measures to assess PD-related change, the field would be able to build a stronger evidence-base for DBDM implementation.

Fourth, future DBDM PD research should reflect current technological advances (e.g., current CBM software applications) and DBDM practices. The field needs updated empirical studies focused on teachers' use of DBDM processes within these new contexts (Espin et al., 2017; Fuchs, 2017). It would be valuable to operationalize the taxonomy of intervention intensity presented by Fuchs et al. (2017) to create a hierarchical structure of intensification options based on intensification decision rules. This could streamline the instructional decision-making process and make it more feasible for teachers attempting to implement adaptations within current educational contexts.

This research could examine the continued use of DBDM practices in the absence of high levels of researcher support. Developing PD with these goals in mind may elucidate the core features of DBDM PD and address potential barriers to implementation at the front end. This type of work aligns with Klingner et al.'s (2013) recommendations for research aligned with implementation science. In addition, this line of research could help determine whether DBDM PD can create sustained changes in teacher behaviors, which, in turn, may lead to significant student academic growth. This is important given that evidence-based practices will not impact student outcomes if teachers are not consistently using them outside of research contexts.

Despite the need for future research, our findings have implications for practice. Our results indicate DBDM PD increases teachers' DBDM expertise. In addition, Stecker et al. (2005) and Jung et al. (2018) showed that when teachers are provided support to implement DBDM with intensive researcher support, there is a positive impact on *student* outcomes. We believe that the findings of these three reviews underscore the importance for school leaders to support teachers in implementing DBDM. This is a necessary first step for school improvement. Through increased expertise, teachers may be more likely to use DBDM processes and positively impact student outcomes. Because the studies in this review included high levels of researcher support, the interpretation of the results can only be generalized to these contexts, which may include a greater level of support than what is typically provided in schools. Although future research needs to address teacher use of DBDM practices with less researcher support, there is still a need to build teacher DBDM expertise in practice in the interim. Given that student outcomes improve when teachers use DBDM (Jung et al., 2018; Stecker et al., 2005), school leaders should prioritize providing DBDM PD to teachers in their district to meet student needs and maximize compliance with IDEIA. Resources provided by NCII (intensiveintervention.org) are available for school leaders who would like to implement DBDM in their schools.

Conclusion

The recent *Endrew F.* Supreme Court decision requires that schools engage in a reasonable, defensible process to monitor the progress of students with disabilities and implement data-based adaptations when students demonstrate insufficient response (Prince et al., 2018). The DBDM process represents a framework that is clearly aligned with this expectation and is particularly relevant for students with or at-risk for learning disabilities, given the ability to use DBDM within RTI frameworks (Danielson & Rosenquist, 2014). This review demonstrated that DBDM PD has a significant effect on teachers' DBDM expertise. Additional work is necessary to understand how to ensure that the change in teachers' expertise results in improved student outcomes and to understand the core features of DBDM PD to maximize teachers' continued use of DBDM in practice. This line of continued work maintains a strong potential to positively impact the lives of many students with disabilities.

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Supplemental Material

Supplemental material for this article is available online.

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