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Daily Report Card Intervention and Attention Deficit Hyperactivity Disorder: A Meta-Analysis of Single-Case Studies

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Abstract

The daily report card (DRC) is a commonly employed behavioral intervention for treating attention deficit hyperactivity disorder (ADHD) in schools. Much of the support for the DRC comes from single-case studies, which have traditionally received less attention than group studies. This lack of attention to single-case studies results in an incomplete review of the literature for this intervention. The present study utilized meta-analytic techniques to examine the DRC as used in single-case studies, with moderating variables explored through hierarchical linear modeling. Fourteen articles, including data on 40 single-subject cases, were included in the analyses. Effect sizes generally illustrated improvement with use of the DRC, with some differences across methods of effect size estimation. Study quality and class type moderated outcomes. Overall, the present study supports the use of the DRC with students who have ADHD, and it provides guidance for using single-case studies in meta-analyses of intervention effects.

Students with attention deficit hyperactivity disorder (ADHD), a prevalent and chronic mental health disorder, comprise the majority of students receiving special education services under the categories of emotional disturbance and other health impairment in the United States (Schnoes, Reid, Wagner, & Marder, 2006). The adverse outcomes of ADHD include severe disruptions in relationships (McQuade & Hoza, 2015) and academic problems throughout the school year (McConaughy, Volpe, Antshel, Gordon, & Eiraldi, 2011), which may lead to the poor academic, social, and school completion outcomes commonly seen for students with ADHD (Kent et al., 2011).

To address these significant difficulties within school settings, numerous behavioral interventions have been developed and evaluated for youth with ADHD. One of the most commonly employed behavioral interventions for children with ADHD is the daily report card (DRC; Kelley, 1990; O'Leary, Pelham, Rosenbaum, & Price, 1976; Volpe & Fabiano, 2013). The DRC is an operationalized list of a child's target behaviors (e.g., interrupting, noncompliance, academic productivity), and it includes specific criteria for meeting each behavioral goal (e.g., "interrupts three or fewer times during math instruction"). Teachers provide immediate feedback to the child regarding target behaviors on the DRC, and typically some reward is provided contingent on the child's performance. DRCs are commonly employed and acceptable interventions for school settings (Chafouleas, Riley-Tillman, & Sassu, 2006).

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Although there are numerous examples of the efficacy of the DRC when used as a component of a multimodal treatment package (e.g., MTA Cooperative Group, 1999; Owens, Murphy, Richerson, Girio, & Himawan, 2008), fewer studies have investigated the efficacy of the DRC as a stand-alone intervention for ADHD (e.g., McCain & Kelley, 1993). Assessing the efficacy of the DRC as a stand-alone intervention is important, as it will begin to elucidate whether the DRC is a contributor to the positive effects within multimodal studies—which fits within recent initiatives to identify the effective components of treatment (e.g., National Center on Intensive Intervention, What Works Clearinghouse [WWC]).

The DRC is an operationalized list of a child's target behaviors [and] includes specific criteria for meeting each behavioral goal.

Whereas group designs are most likely to include multimodal interventions, single-case designs more commonly employ stand-alone interventions. Further, single-case designs comprise a large proportion of the ADHD psychosocial treatment literature (Fabiano et al., 2009). A recent review of meta-analyses of ADHD treatment by Fabiano, Schatz, Aloe, Chacko, and Chronis-Tuscano (2015) revealed that single-case designs often generate large effect sizes for youth with ADHD, with some notable exceptions (DuPaul & Eckert, 1997; DuPaul, Eckert, & Vilardo, 2012; Fabiano et al., 2009). Thus, single-case designs should be subjected to the same scrutiny as between-group designs (i.e., see guidelines for the WWC for group and single-case designs; WWC, 2014) to better understand the efficacy of interventions such as the DRC.

In a recent meta-analysis, Vannest, Davis, Davis, Mason, and Burke (2010) examined the efficacy of the DRC as a stand-alone intervention across 17 single-case studies and showed variable but generally positive support for the intervention, with effect sizes ranging from −0.15 to 0.97 and an average effect of 0.61. To account for this range, the study examined several moderating variables and found that greater home-school communication and greater use of the DRC (using it for >1 hr per day) produced significantly stronger effect sizes. One limitation of this meta-analysis, however, was that the focus was on the DRC as an intervention, and not necessarily the presenting problems of the students. Thus, the students included in the studies demonstrated a variety of symptom profiles and impairment, making it difficult to generalize results to a particular group of students, such as those with ADHD.

The present study aimed to expand these results by examining those DRC effects specific to children diagnosed with ADHD. Although behavioral interventions such as the DRC have been identified as best practice for children with ADHD (DuPaul & Eckert, 1997; Evans, Owens, & Bunford, 2014), single-case studies implementing the DRC with children who have ADHD have never been examined as a whole. In addition, despite the commonalities of the DRC—which include setting clear goals, providing contingent feedback, and establishing contingent rewards for goal attainment—there are many different parameters of the DRC that can be varied across students and settings. These differences include changes to the amount of home–school communication, the age and gender of the students with which it is used, and the class setting in which it is implemented (e.g., special vs. general education). These factors may change the efficacy of the DRC, and further examination of their moderating influence is needed.

To date, there have been six between-group and one within-group design studies that have investigated the efficacy of the DRC as a stand-alone intervention, but the diversity among the aims of these studies precludes a meta-analysis (Blechman, Taylor, & Schrader, 1981; Fabiano et al., 2010; Leach & Byrne, 1986; Murray, Rabiner, Schulte, & Newitt, 2008; O'Leary et al., 1976; Owens et al., 2012; Palcic, Jurbergs, & Kelley, 2009). Although single-case studies of DRC efficacy are relatively more numerous, they have not been systematically reviewed in a meta-analysis as a stand-alone intervention for individuals with ADHD (Chronis, Jones, & Raggi, 2006; DuPaul et al., 2012; Evans et al., 2014; Fabiano et al., 2009; Fabiano et al., 2015; Pelham & Fabiano, 2008). Thus, case studies that used the DRC as a stand-alone intervention for students with ADHD were the focus of the current investigation.

Approaches to Quantifying Single-Case Study Results

It is important to acknowledge that the quantification of effects across single-case studies in a meta-analysis is an evolving area within the field of intervention research (WWC, 2014). To measure effects within and across single-case studies, scholars have focused on examining graphed time-series data, visually and quantitatively. These procedures reveal how effective the intervention has been at improving outcomes, and they demonstrate how these outcomes may be moderated by student- or study-level characteristics. Although there is currently no "gold standard" for calculating effect sizes in single-case research, there have been several recommendations to use nonparametric and parametric methods in tandem (Gage & Lewis, 2014; Kratochwill et al., 2010; Wolery, Busick, Reichow, & Barton, 2010). Nonparametric methods include nonoverlap-based effect sizes, such as the percentage of nonoverlapping data (PND; Scruggs, Mastopieri, & Casto, 1987) and the improvement rate difference (IRD; Parker, Vannest, & Brown, 2009), whereas parametric methods include regression (Allison & Gorman, 1993) and hierarchical linear modeling (HLM; Raudenbush & Bryk, 2002; Van den Noortgate & Onghena, 2007).

In the interest of expanding the literature on effect sizes in single-case research with a clinically relevant sample, the present study utilized several nonparametric effect size approaches in addition to HLM. Effect sizes included in the present study were selected per their use in previous research on the DRC (Owens et al., 2012; Vannest et al., 2010) and their ability to address unique concerns, such as baseline trend (Tau-U; Parker, Vannest, Davis, & Sauber, 2010). HLM was chosen over regression due to the hierarchical structure of single-case data (data points are nested within treatment phases, which in turn are nested within participants and separate studies) and because HLM analyses can account for complex data structures (missing data, varying intervention lengths) likely to be found in single-case studies (Gage & Lewis, 2014; Raudenbush & Bryk, 2002; Van den Noorgate & Onghena, 2007, 2008).

Efforts to increase the yield and precision of single-case study outcomes are crucial, as these studies have been marginalized in systematic reviews and determinations of research evidence for particular interventions, including the DRC. Indeed, in contrast to WWC (2014) evidentiary standards, the most recent criteria for determining evidence-based child and adolescent treatments (Southam-Gerow & Prinstein, 2014) no longer include single-case studies as appropriate empirical evidence for determining the strength of evidence for a child treatment. These modified recommendations effectively remove the majority of studies on interventions such as the DRC from further consideration (see Fabiano et al., 2009; Fabiano et al., 2015). Further development of appropriate methods for quantifying single-subject results may allow researchers and policy makers to include evidence from these designs in decision making, which will help bridge the gap between more traditional research designs (i.e., large randomized controlled trials) and applied practice.

Summary and Research Questions

Although scholars have identified classroom contingency management as an evidence-based intervention for ADHD (Evans et al., 2014; Pelham & Fabiano, 2008; Pelham, Wheeler, & Chronis, 1998), at the present time a systematic review of the DRC as a specific intervention is needed. Reasons for this include the following: (a) The majority of studies in the literature that utilized a DRC did so as a part of a multicomponent intervention (e.g., MTA Cooperative Group, 1999; Owens et al., 2008), (b) prominent groups have stated that interventions such as the DRC should be utilized as second-line intervention for children with ADHD in elementary school (American Academy of Pediatrics, 2011), and (c) only a handful of controlled trials exist with the DRC alone (Fabiano et al., 2010; Murray et al., 2008). In addition, prior systematic reviews and meta-analyses of the DRC as an intervention in general yielded support for the DRC, with some differences in levels of support across moderators (e.g., Vannest et al., 2010). Given that the ADHD group may be one contributor to heterogeneity of effect within the studies examined to date, there is a need to investigate the DRC as a stand-alone intervention for students with ADHD, synthesizing single-case research.

Based on the group literature supporting the DRC as a stand-alone intervention, the present study specifically hypothesized that (a) the DRC would show large treatment effects, as measured by several nonoverlap-based effect sizes; (b) effect sizes would be strongly correlated with one another; and (c) student- and/or study-level variables—including age, gender, diagnostic criteria, level of home-school communication, study quality, and class type—would moderate the effectiveness of the DRC.

Method

In conducting this meta-analytic search and synthesis, we followed recommendations made in standard texts on research synthesis (Cooper & Hedges, 1994; Schmidt & Hunter, 2014), meta-analytic reporting standards criteria from the APA Publications and Communications Board Working Group on Journal Article Reporting Standards (2008), and articles written specifically for meta-analytic examination of single-case design (Gage & Lewis, 2014; Wang, Parrila, & Cui, 2013; Wolery et al., 2010). First, we conducted literature searches with the databases PsycInfo, EBSCO, and ERIC; the search criteria included the following: *DRC, daily behavior report card, home-school note, home school note, school home note*, and *school-home note*. There was no specific date range selected. Peer-reviewed journal articles and dissertations were examined and selected for the present study. Following this literature search, we also systematically analyzed each identified article's reference section for additional articles. We also reviewed studies within several meta-analyses of behavior modification interventions for ADHD (DuPaul & Eckert, 1997; DuPaul et al., 2012; Evans et al., 2014; Fabiano et al., 2009). The literature search was terminated January 2016.

Inclusion Criteria

A study was included in the initial collection per specified search criteria: (a) The participants must have been identified as having ADHD, either through prior diagnosis or the collection of diagnostic information through standardized ADHD rating scales (e.g., Conners Teacher Rating Scale; Conners, Sitarenios, Parker, & Epstein, 1998); (b) the participants must have been <18 years old; (c) the study must have included information that would permit the calculation of effect sizes (i.e., graphed time-series data across baseline and intervention phases); (d) studies must have used a DRC as a stand-alone intervention; and (e) the DRC must have been used in a school or primarily academic (e.g., after-school education program) setting.

In the first stage, 132 articles and dissertations met initial search criteria. In the second stage, we reviewed the abstracts of these articles to identify those that used a single-case research method. Based on this criterion, 94 articles were excluded, and 38 were kept for more detailed analysis. Fourteen of these 38 articles met all of the inclusion criteria outlined. Of these 14 articles, fewer than half (Cottone, 1998; Cowart, 1999; Jurbergs, Palcic, & Kelley, 2007; Kelley & McCain, 1995; McCain & Kelley, 1993, 1994) were previously examined in a meta-analysis of the DRC (Vannest et al., 2010), which underscores the unique nature of the present collection of studies. When an article examined more than one participant, each participant was counted as an independent case study. In total, 40 student participants were identified from 14 separate studies, with publication dates ranging from 1975 to 2013. The second author conducted a reliability search with the same search terms, databases, and meta-analysis reference sections, which yielded 100% reliability with the original search. Of note was a single dissertation, identified in the primary and reliability searches (Kraemer, 1994), that could not be obtained through interlibrary loan or direct contact with the author and was therefore not included in the present analyses.

Coding

All studies were coded at three levels, including individual data points, student-level characteristics, and study-level characteristics. All individual data points were also coded for phase (whether they were data points in baseline or intervention) and, if a reversal design was used, order (whether they came before reversal or after). Student- and study-level variables were coded to examine possible moderation of treatment effects. Student-level variables included age and gender, whereas study-level variables included the level of home–school communication, classroom type, and quality of the research design.

Outcomes. Perhaps due to the nature of ADHD or the common utilization of the DRC to manage disruptive behavior, almost all studies included in this meta-analysis examined observations of disruptive or on-task behaviors as the primary outcome. In total, five outcome variables were identified: percentage of time on task, percentage of time disruptive, number of activity changes, percentage of time spent exhibiting hyperactive symptoms, and percentage of homework completed. To allow for a common interpretation of effect, all outcomes were either kept as, or converted to, percentages. The number of activity changes was converted to a percentage by dividing the total number of activity changes by the time of the observation period (50 min). Thus, if the student changed activities 10 times, the resulting percentage would be $10/50 \times 100 = 20\%$. In addition, all outcomes were categorized as "disruptive" or "on task" targets. On-task outcomes included time on task and percentage of homework completed, while disruptive outcomes included time spent disruptive, number of activity changes, and percentage hyperactivity. Table 1 provides a summary of the outcomes for each study.

To minimize the confounding effects of medication on the DRC, data from phases that intentionally manipulated medication were excluded. Specifically, Atkins, Pelham, and White (1990) and Ayllon, Layman, and Kandel (1975) both manipulated medication. In the Atkins and colleagues study, medication was implemented in the last phase of treatment in conjunction with the DRC. Data from this final phase were excluded. In the Ayllon and colleagues study, medication was given in a phase prior to implementing the DRC, with a 3-day "washout" period between the medication and DRC phases. Data from the medication phase were excluded, with data in the DRC phase kept and assumed to be free of medication effects due to the washout period.

Graphs depicting outcome data were scanned and imported into UnGraph 5 (Version 5.0.1; Biosoft, 2015) to accurately read the values of the data points from the figures. All data points included in the graphs were coded. In cases where a reversal ABAB design was used, the first AB pair (baseline–intervention) and the second AB pair were coded. A special code was assigned to each pair to determine order, where 0 = first AB pair and 1 = second AB pair. No studies used more than one reversal. In all, 1,570 data points were coded.

Quality. We created an aggregate measure of quality—based on three broad WWC recommendations for single-case studies, supplemented with two external indicators of validity—to examine how rigorously each study designed and implemented the DRC. The WWC lists specific guidelines for single-case designs to meet evidence standards

Table 1. Summary of Studies.

Study	Outcome[a]	Method	Age (Years)	Male, *n*	Female, *n*	Homeschool[b]	Quality[b]	Class type
1. Atkins, Pelham, & White (1990)	1	Alternating	9	1	0	5	2	General
2. Ayllon, Layman, & Kandel (1975)	3, 4	MB	8, 9, 10	2	1	2	2	Special
3. Cottone (1998)	1, 2, 3	MB	7, 11, 12	3	0	5	4	Special, general
4. Cowart (1999)	1, 2	MB	8, 11	2	0	3	3.5	General
5. Fabiano & Pelham (2003)	1, 2	MB	8	1	0	2	2	General
6. Grady (2013)	1, 2	MB	5, 6, 6	2	1	5	3	General
7. Jurbergs, Palcic, & Kelley (2007)	1	ABAB	6, 6, 8, 8, 8, 8	5	1	5	3.5	General
8. Kelley & McCain (1995)	1	Alternating	6, 6, 7, 7, 9	2	3	5	3.6	General
9. LeBel, Chafouleas, Britner, & Simonson (2013)	2	MB	4, 4, 4, 4	3	1	4	3.5	General
10. McCain & Kelley (1993)	1, 2, 5	ABAB	5	1	0	5	3	General
11. McCain & Kelley (1994)	1, 2	Alternating	11, 11, 11	3	0	5	3.33	General
12. McCorvey (2013)	1, 2	MB	9, 9, 9	1	2	3	2.33	General
13. Miller & Kelley (1994)	1, 3	MB, ABAB	9, 10, 11, 11	2	2	4	3.75	Unknown
14. Weakley (2012)	1	MB, alternating	14	1	0	1	3	General

Note. Alternating = alternating treatment design; ABAB = reversal design; MB = multiple-baseline design.
[a]1 = percentage of time spent on task; 2 = percentage of time spent disruptive; 3 = percentage of homework completed; 4 = percentage of time engaged in hyperactive behaviors; 5 = number of activity changes made. [b]Quality and home–school communication scores represent an average across all participants.

(Kratochwill et al., 2010; WWC, 2014). For the present study, three of these criteria were chosen and coded as 1 = meets criterion and 0 = does not meet criterion: (a) interobserver agreement reported for at least 20% of the data points, with at least 80% agreement; (b) at least 5 data points within each phase; and (c) data within the baseline phase providing a sufficient demonstration of a clearly defined pattern of responding (e.g., small day-to-day differences vs. large peaks and valleys), as determined through visual analysis. Therefore, for each WWC criterion, the study could receive a score of 1 or 0, with higher scores (up to 3) indicating greater quality.

In addition to these four WWC criteria, the present study used two external indicators of study internal validity, including (a) treatment integrity reported (0 = no, 1 = yes) and (b) observers blind to treatment conditions (0 = no, 1 = yes). Scores for the five indicators were added, and a total quality score was found, with higher scores indicating greater quality. As some participants evinced certain criteria (e.g., five data points in each phase) whereas others did not, quality scores were initially calculated at the individual level and then averaged to provide a study-level quality score (see Table 1).

Level of home–school communication. Home–school communication was coded following a similar strategy to that of Vannest and colleagues (2010) in their meta-analysis of the DRC. Specifically, an aggregate score was calculated with three criteria: (a) reinforcement, where 0 = no reinforcement planning, 1 = reinforcement determined by the researcher, and 2 = reinforcement determined collaboratively; (b) home training, where 0 = no home training, 1 = indirect training (e.g., with a handout), and 2 = direct parent training (e.g., in-person meeting); and (c) feedback, where 0 = feedback on school behavior given at only one location (home or school) and 1 = feedback on school behavior given at both home and school. These scores were combined to yield a study-level communication score (see Table 1).

Classroom type. Differences between special education versus general education settings, such as the presumed greater availability of resources and supports in special education classrooms, may influence the efficacy of the DRC. Student's classroom placements (when available) were coded (0 = general education, 1 = special education).

Age. Age may be related to DRC effectiveness. For instance, older children attending middle school tend to have a highly varied schedule, with a number of teachers. These changes may lead to less consistency. This speculation needs to be evaluated empirically, as other studies have not documented a moderating effect for age on behavioral treatment (Pelham & Fabiano, 2000). Age was coded numerically for all participants (see Table 1 for summary).

Gender. The moderating effect of gender on DRC effectiveness is in need of exploration, given that girls may exhibit different profiles relative to boys (Gaub & Carlson, 1997; Pelham & Bender, 1982). All participants were coded for gender (0 = female, 1 = male; see Table 1 for summary).

Reliability

Data points from graphs and all moderator variables (predictors) were coded twice (once by the main author and once by a trained graduate assistant blind to the previous coding) to ensure reliability. Training was held in an hour-long meeting with the main author, in which all articles and operational definitions for codes were reviewed. The reliability of the data point coding was examined with an intraclass correlation, whereas the reliability of all predictor-level coding was found with the following formula: (agreements) / (agreements + disagreements).

Analysis

The analysis for the present study was conducted in two stages. First, well-supported effect sizes were calculated—including the standard mean difference (SMD; Busk & Serlin, 1992), PND (Scruggs et al., 1987), percentage of all nonoverlapping data (PAND; Parker, Hagan-Burke, & Vannest, 2007), percentage exceeding the median (PEM; Ma, 2006), IRD (Parker et al., 2009), and Tau-U (Parker et al., 2010). For each goal type (disruptive or on task), a separate effect size was calculated. Following the calculation of these effect sizes, the relationships among effect sizes were examined with Pearson correlations.

The second part of the analysis used HLM to examine the moderating influence of several student- and study-level variables on the efficacy of the DRC. In addition to these moderating effects, HLM was used to estimate an overall effect size (Hedges's *g*) across all studies included in the meta-analysis.

Standard mean difference. The SMD is sometimes referred to as the "no assumptions effect size" (Busk & Serlin, 1992), and it is calculated by subtracting the mean of the baseline from the mean of the intervention data and dividing by the standard deviation of the baseline.

Percentage of nonoverlapping data. The PND is calculated by identifying the most extreme baseline point (highest if an increase is desired, lowest if a decrease is desired) and determining how many intervention data points fall above or below that extreme, depending on the effect desired (Scruggs et al., 1987).

Percentage of all nonoverlapping data. PAND is the percentage of data remaining after removing the fewest data points that would eliminate all overlap. PAND takes into account all data points within both treatment and baseline phases, rather than a single extreme data point, as in PND. PAND is scaled from 0 to 100, with greater values being more desirable (Parker et al., 2007).

Percentage exceeding the median. PEM is calculated by locating the median of the baseline phase and determining the percentage of intervention data points above or below that point (depending of the effect desired). PEM is advantageous in that it is not necessarily affected by extreme baseline values and may therefore give an estimate of intervention efficacy less influenced by outlier values (Ma, 2006).

Improvement rate difference. The IRD examines the difference in improvement rates between the baseline and intervention phases. It was modeled after the "risk difference" concept used in medical research, and it reflects visual nonoverlap well. To calculate the IRD, data points in the intervention phase that overlap with data points in the baseline phase are identified and counted. This number is considered the "minimum removed" needed to eliminate all overlap between the intervention and baseline phases. The minimum is then divided in half, and the intervention and baseline "rates" are found. The difference between the intervention and baseline rates is the IRD (Parker et al., 2009).

Tau and Tau-U. Tau and Tau-U examine the percentage of data that shows improvement across phases by comparing pairs of data points. By comparing the amount of nonoverlap (desired) to the amount of overlap (not desired), a conservative effect size can be calculated. Tau-U has the added benefit of controlling for positive baseline trend, when present. Both tests show more statistical power than other nonoverlap-based effect sizes (Parker, Vannest, & Davis, 2011) and allow for the calculation of *p* values and confidence intervals.

Hierarchical linear modeling. All statistical analyses were conducted with HLM 7 (Bryk, Raudenbush, & Congdon, 2011). In the present study, we used a 3-level linear growth model to explore the treatment effects from baseline to intervention and to examine the impact that certain student- and study-level predictors had on this treatment effect. In these models, Level 1 represents the data points, or repeated measures within persons. There were 1,570 data points. Level 2 represents the students and those characteristics, such as age and gender, that may influence the mean of their data points, or the way in which their behavior changes from baseline to intervention. There were 40 student cases at Level 2. Level 3 represents study characteristics, such as classroom type and study quality, that may affect treatment outcomes. There were 14 study cases at Level 3. To allow for a common interpretation of effects, all outcomes were coded so that higher percentages were always considered "more desirable" regardless of whether the goal related to disruptive or on-task behavior. HLM models were created sequentially to address four major goals: order and measure-type effects, treatment effect, student-level variables, and study-level variables.

Data considerations for HLM. Initial examination of the data revealed that the Cottone (1998) dissertation acted as a major outlier in our analyses, driving effects at the student and study levels. These results were due in large part to the "disruptive" goal included in the dissertation, which suffered from significant floor effects at baseline (where the most common amount of disruptive behavior was 0) and intervention. To create a more parsimonious model that better reflected the data as a whole, rather than an individual study, we removed the Cottone dissertation data from all analyses.

Results

Results for each outcome—including reliability of data and moderator coding, effect size calculations, estimates of publication bias, and HLM—are explored in turn.

Reliability

With regard to the data points coded from UnGraph, a high degree of reliability was found, as indicated by an intraclass correlation of .97, with a 95% confidence interval from .96 to .98, $F(609, 610) = 70.16$, $p < .001$. Codes for the predictor variables were created separately and then compared. These codes ranged in reliability from 87% to 100%, with the greatest discrepancies in consistent baseline trend (quality; 87% agreement) and feedback at home and school (home–school communication; 87% agreement).

Effect Sizes

Overall, effect sizes generally illustrated improvement from implementing the DRC, with some differences across methods. Of the methods used, the most varied effect sizes were produced with the SMD (–0.27 to 54.45; at the individual level). Effect sizes calculated with the PND, PAND, PEM, IRD, and Tau-U methods were generally similar, with average effect sizes across all studies ranging from 0.59 to 0.94. Average effect sizes across participants for each study are listed in Table 2. Pearson correlations demonstrated that all effect sizes were significantly related, with the strongest relationships with PND, PAND, PEM, IRD, and Tau and the weakest relationships with SMD. All correlations are listed in Table 3.

Publication Bias

The present study sought to limit errors based on publication bias by incorporating published and unpublished studies (dissertations). In addition, we calculated a fail-safe *N* (Cooper, 1979) for each effect size and chose a criterion effect size (*d*) of 0.10 to represent a "null" effect. For the smallest average effect size found in the present study (PND, disruptive = 0.59), at least 68 additional studies would need to find a null effect to reduce the effect size to an insignificant level. For the largest average effect size (SMD, on task = 4.31), >500 studies would need to find a null effect to reduce this effect size. These results suggest that publication bias is unlikely to have distorted the reported findings.

Hierarchical Linear Modeling

Several initial models were created to examine the data. These models demonstrated (a) the relative magnitude of variance among students versus that among studies, (b) the differences in the treatment effect between first and second AB pairs in reversal studies, and (c) the differences in the treatment effect between outcomes (on task vs. disruptive). These models were designed

Table 2. Effect Sizes Across Studies.

Authors	Goal type	SMD	PND	PAND	PEM	IRD	Tau/Tau-U	*p*
1. Atkins, Pelham, & White (1990)	On task	1.32	0.30	0.68	0.78	0.59	**0.47**	<.01
2. Ayllon, Layman, & Kandel (1975)	On task	8.89	1.00	1.00	1.00	1.00	1.08	<.001
	Disruptive	9.90	1.00	1.00	1.00	1.00	**1.00**	<.05
3. Cottone	—	—	—	—	—	—	—	—
4. Cowart (1999)	On task	2.78	0.77	0.94	1.00	0.84	0.91	<.001
5. Fabiano & Pelham (2003)	On task	1.62	0.15	0.76	1.00	0.88	0.86	<.001
	Disruptive	1.34	0.31	0.77	1.00	0.75	0.90	<.001
6. Grady (2013)	On task	2.29	0.74	0.78	0.86	0.75	0.78	<.001
	Disruptive	3.26	0.71	0.80	0.85	0.67	0.76	<.001
7. Jurbergs, Palcic, & Kelley (2007)	On task	2.87	0.91	0.96	0.98	0.93	0.94	<.001
8. Kelley & McCain (1995)	On task	9.86	0.92	0.97	0.99	0.94	0.93	<.001
9. LeBel, Chafouleas, Britner, & Simonson (2013)	Disruptive	3.61	0.91	0.95	1.00	0.94	0.96	<.001
10. McCain & Kelley (1993)	On task	3.30	1.00	1.00	1.00	1.00	1.00	<.001
	Disruptive	4.03	0.79	1.00	1.00	0.89	1.00	<.001
11. McCain & Kelley (1994)	On task	10.17	0.95	0.97	1.00	0.95	0.98	<.001
	Disruptive	1.08	0.56	0.67	0.92	0.61	**0.73**	<.001
12. McCorvey (2013)	On task	0.79	0.35	0.71	0.72	0.54	0.44	<.05
	Disruptive	0.62	0.22	0.67	0.61	0.31	0.17	>.05
13. Miller & Kelley (1994)	On task	2.40	0.75	0.88	0.96	0.82	0.84	<.001
14. Weakley (2012)	On task	1.45	1.00	1.00	1.00	1.00	1.00	<.001
Average Across Studies	**On task**	**4.31**	**0.76**	**0.89**	**0.94**	**0.84**	**0.84**	
	Disruptive	**3.14**	**0.59**	**0.81**	**0.87**	**0.69**	**0.72**	

Note. The effect sizes shown in this table represent the average effect sizes across all participants or phases. The only exceptions to this rule are the Tau-U effect size, which represents a weighted average, and its related *p* value, which represents the significance of improvement across phases. In addition, all Tau effect sizes shown in bold are Tau-U effect sizes and have been corrected for positive baseline trend. SMD = standard mean difference; PND = percentage of nonoverlapping data; PAND = percentage of all nonoverlapping data; PEM = percentage exceeding the median; IRD = improvement rate difference.

in a similar manner to those outlined by Gage and Lewis (2014).

Initial models. We first examined a fully unconditional model, in which no predictors were entered. This model demonstrated that approximately 25% of the variance in behavioral outcomes lay among students, while 20% lay among studies. These results supported our interest in examining the moderating effects of student- and study-level variables. Next, we examined the effect of the Level 1 order predictor (0 = first AB pair, 1 = second AB pair). This model demonstrated that, after reversal, students may show faster change from baseline to intervention, speeding up the change by approximately 6 percentage points, $\beta = 6.09$, $t(13) = 3.12$, $p < .01$. Finally, we examined whether there were differences in the treatment effect due to the goal type (on task vs. disruptive). No significant differences were found between the goal types, $\beta = 7.58$, $t(13) = 1.41$, $p = .18$.

Partially conditional model. The partially conditional model examined the effect of the Level 1 phase predictor (0 = baseline, 1 = treatment) on the data points. This model indicated that the average treatment effect across studies was significant, with participants gaining approximately 30 percentage points from baseline to treatment, $\beta = 30.32$, $t(12) = 8.82$, $p < .001$. The partially conditional model also indicated that there was significant variability among students in their scores at baseline, $r = 67.91$, $\chi^2(23) = 130.05$, $p < .001$, and among students in their response to the

Table 3. Correlation Analysis of All Nonparametric Effect Sizes.

	SMD	PND	PAND	PEM	IRD
PND	.36**				
PAND	.35**	.90**			
PEM	.26*	.69**	.71**		
IRD	.35*	.86**	.83**	.82**	
Tau-U	.31*	.81**	.78**	.90**	.95**

Note. SMD = standard mean difference; PND = percentage of nonoverlapping data; PAND = percentage of all nonoverlapping data; PEM = percentage exceeding the median; IRD = improvement rate difference.
*p < .05. **p < .01.

intervention, r = 23.04, $\chi^2(23) = 41.19$, $p <$ .05. At the study level, there was significant variation in the baseline scores of participants, u = 135.38, $\chi^2(12) = 68.63$, $p < .001$, and the treatment effects, $u = 128.53$, $\chi^2(12) = 89.21$, $p <$ .001. These results suggest that there may be student- and study-level characteristics that moderate the treatment effect (see Table 4). Hedges's g was calculated for the partially conditional model and was found to be 2.19.

Fully conditional model. Age (n = 37; range, 4–14) and gender (n = 37, 25 male) showed no significant impact on baseline or the change from baseline to intervention (see Table 5).

Due to these findings, we used a more parsimonious model, in which age and gender were excluded, to examine the effects that study-level variables—including quality (n = 13), home–school communication (n = 13), and class type (n = 13)—had on outcomes (see Table 1 for study-level details). In this fully conditional model, quality and class type moderated the treatment effect, but home–school communication did not. On average, higher-quality studies demonstrated significantly greater change across the phases by approximately 13 percentage points, γ = 13.17, $t(9) = 3.04$, $p = .01$. Changes in class type yielded a similar increase, with studies completed in special education classrooms gaining approximately 33 percentage points more across phases, $\gamma = 32.99$, $t(9) = 3.23$, $p =$ .01. This result should be interpreted with caution, given that only one study was included in these analyses that examined students in special education classrooms (Ayllon et al., 1975). Home–school communication was not significantly related to outcome, $\gamma = -2.21$, $t(9) = -1.02$, $p = .34$ (see Table 6).

Discussion

Overall, the results of the present study support the DRC as an effective stand-alone intervention for students with ADHD based on the results of single-case studies. The implementation of the DRC significantly changes behavior, increasing desirable behavior by almost 30 percentage points from baseline to intervention. Based on HLM, the moderating effects of class type and study quality were illustrated, with higher-quality studies and special education classroom settings associated with greater gains. The effects of the DRC are consistent and large, as indicated by nonoverlap-based effect sizes that range from 0.59 to 0.94 and an overall Hedges's g of 2.19. In addition, the present study demonstrates that evidence for an intervention can be shown with a meta-analysis of single-case studies, particularly with the advent of statistical techniques such as HLM. These findings support the utility and continued inclusion of single-case studies in meta-analyses of treatment effects. Inclusion of these studies is especially important for the ADHD treatment literature, where the majority of studies are single-case studies (DuPaul & Eckert, 1997; DuPaul et al., 2012; Fabiano et al., 2012).

The implementation of the DRC significantly changes behavior, increasing desirable behavior by almost 30 percentage points from baseline to intervention.

Although HLM is relatively new, it shows promise for addressing many of the criticisms levied against statistical analysis of single-subject designs (Kratochwill et al., 1974; Parsenson & Baer, 1992; Salzberg, Strain, & Baer, 1987; White, 1987), and it meets proposed criteria for meta-analysis of single-subject designs (Wolery et al., 2010). In the

Table 4. Hierarchical Linear Modeling: Partially Conditional Model Showing Average Change Across Phases.

Fixed effect	Coefficient	*SE*	*t*	*p*
Mean at baseline, γ_{000}	50.25	3.67	13.69	<.001
Mean growth rate (treatment effect), γ_{100}	30.32	3.43	8.82	<.001
Random effect	Variance	*df*	χ^2	*p*
Variability among participants (Level 2)				
Baseline, r_0	67.91	23	130.05	<.001
Treatment effect, r_1	23.04	23	41.19	.01
Level 1 error, *e*	233.75			
Variability among studies (Level 3)				
Baseline, u_{00}	135.38	12	68.63	<.001
Treatment effect, u_{10}	128.53	12	89.21	<.001

Note. All coefficient values are presented as percentage points. The average treatment effect refers to the average change that students showed from baseline to intervention (their average improvement).

present study, HLM analyses demonstrated that students with ADHD who were given a DRC showed a mean improvement of approximately 30 percentage points from baseline to intervention. Given the initial baseline average of 51%, this shift resulted in students who were >80% on task and disruptive <20% of the time. This mean shift is consistent with the results of Gage and Lewis (2014), who used HLM to demonstrate that functional behavior assessment-based interventions increased desirable behavior by 34 percentage points from baseline to intervention for students with emotional and behavioral disorders.

Although the benefits of the DRC were considerable, significant variability remained not only among students but also among studies in terms of the treatment effect, suggesting that there were student- and study-level moderators. In the present study, age, gender, class type, home–school communication, and study quality were examined as potential moderators of the DRC. Neither age nor gender moderated the treatment effect. These results are positive, suggesting that students from different genders and age groups will equally benefit from the DRC intervention.

Although age, gender, and home–school communication did not moderate outcomes, class type and study quality significantly moderated outcomes. As anticipated, higher-quality studies were associated with greater gains from baseline to intervention. This result lends support to the use of certain guidelines (e.g., WWC; Kratochwill et al., 2010) in conducting single-case research. Although class type was also anticipated to moderate outcomes, this result should be interpreted with caution, as only one study included in these analyses was conducted in special education classrooms (Ayllon et al., 1975). The nonsignificant moderation of home–school communication on outcomes was not anticipated and deserves a more thorough investigation.

Although age, gender, and home–school communication did not moderate outcomes, class type and study quality significantly moderated outcomes.

Greater home–school communication is theorized to be one of the linchpins of the DRC, allowing teachers and parents to work collaboratively to improve a student's behavior (Fabiano et al., 2010; Kelley, 1990). Indeed, in a prior meta-analysis of the DRC, Vannest et al. (2010) demonstrated that those with the highest home–school communication showed significantly stronger effect sizes when compared with those with the lowest home–school communication.

Table 5. Fully Conditional Model Showing Effects of Age, Gender, and Diagnosis of Attention Deficit Hyperactivity Disorder.

Fixed effect	Coefficient	*SE*	*t*	*p*
Effects on baseline behavior averages (intercepts)				
Age	1.07	1.02	1.05	.32
Gender	−1.28	4.54	−0.28	.78
Effects on treatment effect (slopes)				
Age	−0.30	1.48	−0.20	.85
Gender	0.29	3.24	0.09	.93

Note. All coefficient values are presented as percentage points. Negative values indicate a decrease in percentage points associated with a 1-point increase in the moderating variable. The average treatment effect refers to the average change that students showed from baseline to intervention (their average improvement).

Table 6. Fully Conditional Model Showing Effects of Home–School Communication, Study Quality, and Class Type.

Fixed effect	Coefficient	*SE*	*t*	*p*
Effects on baseline behavior averages (intercepts)				
Communication	−3.09	2.89	−1.07	.31
Quality	−0.23	5.95	−0.04	.97
Class type	−28.06	13.54	−2.07	.07
Effects on treatment effect (slopes)				
Communication	−2.21	2.18	−1.02	.34
Quality	13.17	4.33	3.04	.01
Class type	32.99	10.22	3.23	.01

Note. All coefficient values are presented as percentage points. Negative values indicate a decrease in percentage points associated with a 1-point increase in the moderating variable. The average treatment effect refers to the average change that students showed from baseline to intervention (their average improvement).

Although the results of the present study appear to contradict these findings, they may suggest something unique about the population of students with ADHD. For instance, it is possible that an increased amount of communication between the home and the school may not always be beneficial to the student and may in fact represent a more severe impairment that requires a greater concerted effort (e.g., home and school rewards) to address the problem. It is clear that there is a need for more research in this area to examine the influence of home–school communication on student behavioral outcomes. In particular, future studies should endeavor to operationalize and clearly report the level and type of home-school communication used, as this will help future meta-analyses determine the moderating influence of changes in this variable.

The present study used values from the partially conditional HLM model to calculate an overall Hedges's *g* of 2.19, which suggests that the DRC is very effective at increasing desirable behavior in students with ADHD. Although this effect size was very large, it is consistent with the significant changes demonstrated by the nonparametric effect sizes, which ranged, on average, from 0.59 to 4.31. This large range was due to the use of the SMD effect size, which is not based on percentage of overlap from baseline to intervention (Busk & Serlin, 1992). Although the SMD yields effect sizes that are not interpretable by current standards (e.g., Cohen, 1992), research continuing to use this effect size and compare it with other effect sizes is greatly needed, especially to create new standards for judging the magnitude of these effect sizes, which are often very large (Gage & Lewis, 2014).

Although there was some variability in the nonparametric effect sizes, all methods were significantly correlated, suggesting that they largely agreed in illustrating improvement with the DRC. Although there are no firmly established standards for these nonoverlap-based effect sizes, suggested criteria list effect sizes of 0.70 to 0.90 as denoting moderately effective interventions and effect sizes >0.90 as highly effective (Ma, 2006; Parker et al., 2011; Scruggs & Mastropieri, 2001). By these criteria, the DRC intervention is supported as a moderate to highly effective stand-alone intervention for children identified as having ADHD. In addition, although Vannest and colleagues' (2010) meta-analysis demonstrated conflicting support for the DRC with the IRD effect size, the present study did not find the same variability in the IRD, suggesting that the DRC is a particularly effective intervention for youth with ADHD.

The present study has several limitations. First, although efforts were made to select statistical models that addressed the sample size issue inherent in single-case studies, the number of studies and participants included in this meta-analysis is still small. The sample size may limit the generalizability of these findings, especially with regard to the moderating effects of study-level variables.

The study was also limited by small sample sizes of subgroups, particularly with regard to girls (n = 11) and older children (age, >10 years; n = 9). This lack of diversity in gender and ages may also limit the generalizability of the present findings. In addition, the present study was not able to account for the severity of ADHD symptoms, the ethnicity of participants, the types of services offered to students within special education, or the presence of comorbid conditions. These factors deserve further exploration in future studies of the DRC.

Conclusion

The present study supports the use of the DRC as an effective intervention for students with ADHD. Although higher-quality designs and special education classroom settings led to more rapid behavioral change, greater home–school communication was not associated with outcomes. School psychologists, special educators, and clinicians are encouraged to use the DRC to address on-task and disruptive behaviors with students who have ADHD (e.g., Volpe & Fabiano, 2013). Future research is needed to address the elements of home-school communication as they relate to the DRC, particularly identifying the type and degree of home-school communication that influences outcomes.

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