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COMPARING, CONTRASTING, AND EVALUATING SINGLE CASE RESEARCH DESIGNS AND DESCRIBING HOW TO APPLY COACHING TECHNIQUES IN AN APPLIED SETTING

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COMPARING, CONTRASTING, AND EVALUATING SINGLE CASE RESEARCH
DESIGNS AND DESCRIBING HOW TO APPLY COACHING TECHNIQUES IN AN
APPLIED SETTING

THESIS

A thesis submitted in partial fulfillment of the
requirements for the degree of Master of Science in the
College of Education
at the University of Kentucky

By

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2020

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ABSTRACT OF THESIS

COMPARING, CONTRASTING, AND EVALUATING SINGLE CASE RESEARCH DESIGNS AND DESCRIBING HOW TO APPLY COACHING TECHNIQUES IN AN APPLIED SETTING

During the Spring 2020 semester, students within the applied behavior analysis/special education/interdisciplinary early childhood education program were conducting applied thesis projects within typical contexts as part of their fulfillment of the requirements of a master's degree program. Due to the coronavirus disease 2019 (COVID-19), public schools and related facilities closed with no plans to reopen within the time frame to allow for graduation for students in the last semester of their graduate program. Students were allowed to complete an alternative thesis assignment in the form of responding to writing prompts followed by an oral defense of the written products, along with questions related to their field of study. The following written prompts were assigned as an alternative to an applied thesis project:

Alternate Thesis Project

Spring 2020

The deadline for submitting responses is April 6. Responses should be emailed to all members of your committee. You will complete an oral defense on the date that you have already scheduled, and you will answer questions about your written questions, as well as answer questions from any content that you have learned during your Master's program.

1. *Compare and contrast the multiple baseline design with a multiple probe design. I expect the product of your work to provide sufficient information that would demonstrate your understanding of each design.*
2. *I have attached a single-case article in your area of interest. You will use the handout you were given and practiced in EDS 633 to analyze the article (attached) – write a summary of the findings that evaluates the rigor, quality, and potential bias in the article.*
3. *You will write an article, designed for a practitioner, about the independent variable (coaching practices) you chose for your original thesis including a rationale for why this IV is important, how to implement the IV, an application vignette or scenario, and supporting references. I have attached examples of such papers (not in your topic area and longer than you are expected to write – BUT should serve as a guide in this process).*

*Each response must be 4 double-spaced pages and adhere to APA 6th edition guidelines and include references (this section **does not** count toward page requirements). When reviewing and editing your work, make sure your responses are analytical, technical, and your own original ideas/work (plagiarism is not worth failing;*

<https://apastyle.apa.org/style-grammar-guidelines/citations/plagiarism>).

KEYWORDS: Multiple-Baseline, Multiple-Probe, Rigor, Quality, Bias, Coaching

Kasey Jo Waddell

05/08/2020

Date

COMPARING, CONTRASTING, AND EVALUATING SINGLE CASE RESEARCH
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APPLIED SETTING

By
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05/08/2020

Date

DEDICATION

To Brian, you have supported me and my dream without question and have been there for me through the good and bad times. To my family, without whom I would not have the ability to be where I am. Finally, to God, for without whom I would be eternally cursed and lost without the guidance and strength needed to carry on with my passions to make a difference.

ACKNOWLEDGMENTS

The following thesis, while an individual work, benefited from the insights and direction of several people. First, my Thesis Chair, Dr. Justin D. Lane, who exemplifies the high-quality scholarship to which I aspire. In addition, he provided timely and instructive comments and evaluation at every stage of the thesis process, allowing me to complete this project. Next, I wish to thank the complete Thesis Committee: Dr. Melinda Ault, and Dr. Collin Shepley. Each individual provided insights that guided and challenged my thinking, substantially improving the finished product.

In addition to the technical and instrumental assistance above, I received equally important assistance from family and friends. My fiancé, Brian Morrow, provided on-going support throughout the thesis process which was critical in completing this project.

CHAPTER 1. MULTIPLE-PROBE AND MULTIPLE-BASELINE DESIGNS

Single-case research methodologies allow for the evaluation of the effectiveness of an intervention through repeated measurement of an individual's performance under a baseline condition and an intervention condition. This notion is rooted in baseline logic where the participant serves as their own control. Single-case designs are used to produce quality and rigorous data to answer current research questions, to add information for data-based decision making, and to fuel future research and further benefit the field (Ledford & Gast, 2018). Selecting a single-case design depends on many factors but for the sake of this article, two designs will be discussed (Cooper, Heron, & Heward, 2020). These designs are the multiple-baseline (MB) design and the multiple-probe (MP) design. MB and MP designs are both similar in how they are used, formatted, and evaluated (Ledford, Lloyd, & Gast, 2018; What Works Clearinghouse, 2013).

1.1 Similarities

Both MB and MP designs can be used across participants, behaviors, and contexts (e.g., settings, formats, arrangements), both are visually analyzed and formatted vertically (stacked A-B designs), and they share almost the same design standards according to What Works Clearinghouse (2013) (Ledford et al., 2018; WWC, 2013). Each of these individual components are explained in depth in the following sections.

1.1.1 Basic Design

These designs should, therefore, be considered for use when withdrawing the IV is not ethical (e.g., for self-injurious behaviors, severe aggression, etc.). Each individual graph within these designs also looks like an A-B design; where A stands for the pre-

intervention condition and B stands for intervention (Cooper et al., 2020; Ledford et al., 2018). The DV must also be functionally independent and functionally similar but need not be reversible (“[DVs]... that are likely to revert to baseline levels if an intervention is removed”; Ledford et al., 2018. p. 99).

Functionally similar means that the DVs are likely to be influenced by the IV similarly across tiers. Functionally independent means that the DVs in other tiers will not change upon the introduction of the IV in previous tiers (Ledford et al., 2018). These requirements are made in order to decrease the likelihood of covariation, which occurs when the IV in a previous tier is introduced and the DV in the other tiers indicate therapeutic improvements without the introduction of the intervention (Ledford et al., 2018). Any variation or fluctuation across tiers when the IV is introduced, is an indication that there is a lack of experimental control.

1.1.2 Basic Design

The next key component to describe is data collection. After identifying functionally similar and independent behaviors, people, and contexts, the criterion for when to introduce the IV in each tier is established (Ledford et al., 2018). This usually consists of collecting data points in the pre-intervention condition, across all tiers, before the introduction of the IV in the first tier (Ledford et al., 2018). This helps establish a predictable pattern in responding which is the first step in identifying a functional relation (i.e., “[when] the change in the [DV] is causally (functionally) related to the implementation of the [IV]” which is demonstrated and replicated at least two times (p. 4)) (Gast & Ledford, 2018. p. 4; Ledford et al., 2018). Pre-intervention data will be taken concurrently in all other tiers until the pre-established criterion for introducing the IV to

the next tier is met. This is why this design is considered time lagged. Data is staggered based on the criterion set in other tiers making the introduction of the IV lagged. Once the IV is introduced in the second tier and responding is visually similar to that in the first tier, the second step in identifying a functional relation occurs. Verification “verifies” that what occurred in Tier 1 as due to the IV based on the replication of the results in tier two. After this, the process is repeated until the end of the treatment or study. If replication of the results occurs in further tiers, replication has been obtained which is the final step in identifying a functional relation. The concurrent collection of data in each tier also aids in the identification of any covariation or threats to internal validity that may occur (Ledford et al., 2018).

1.1.3 Threats to Internal Validity

MB and MP designs also share the same possible threats to internal validity. These threats are history (i.e., when other things in the environment influence responding), maturation (i.e., changes in data are due to the passage of time), testing (i.e., being exposed to the IV repeatedly influences future responding– this is more of a threat for MB design), attrition bias (i.e., when participants leave the study), sampling bias (i.e., how the choice of participants affects the design), instability (i.e., “the amount of variability in the data”), covariation (i.e., when the IV in a previous tier is introduced and the DV in the other tiers indicate therapeutic improvements without the introduction of the intervention), and inconsistent effects (i.e., when the IV appears to work in some instances but not in others) (Gast & Ledford, 2018. p. 22; Ledford et al., 2018). These possible threats are more likely with these designs due to the fact there is an extended

baseline condition. This means that some tiers remain in the baseline condition for longer periods of time, increasing the likelihood for a threat to internal validity to occur.

1.2 Difference

The major difference between a MB and MP design is how often data are taken in the pre-intervention condition. With a MB design, data are taken continually and concurrently in each condition in each tier. In the MP design, data is collected intermittently in the pre-intervention condition and, because of this, there are additional design standards that need to be met since there are fewer data points.

WWC (2013) standards state that baseline sessions must overlap vertically and probe trials must occur within the first three sessions in order to meet standards without reservations. Three consecutive points also must occur immediately prior to introducing the IV in an untreated tier. In all other tiers at least one probe trial needs to be recorded just before the introduction of the IV or once criteria is met to meet design standards without reservations. In order for design standards to meet with reservations, at least one probe trial will need to be recorded within the first three sessions of the pre-intervention condition and at least one probe trial must occur prior to the introduction of the IV. Probe trials can also be recorded using two different formats: conditions or days.

When collecting data under the conditions format, the graph will include condition lines where only baseline/probe trials are collected across tiers. When collecting probe trial data under the days format, days may be staggered between baseline trials and IV trials, however, there are no additional condition changes (Ledford et al., 2018). These are the main differences between the MB and MP designs. MP requires

additional design standards, MP contains two additional types of data collection, and visually they look different. Certain considerations do, however, need to be made when choosing which design to use.

1.3 Considerations for Use

When deciding on which design to consider for use, Horner and Baer (1978) discussed reasons for why using a MP design may be advantageous over a MB design. They mentioned that if the data collector finds that the use of continuous measurement is “impractical, unnecessary, or reactive [at risk for validity issue]” then MP designs may be a better option for that individual opposed to a MB design (Horner & Baer, 1978. p. 196; Cooper et al., 2020). Ledford and Gast (2018) extended these considerations by explaining that MB may be more advantageous because the user is more likely to visually analyze threats to internal validity faster and it allows a better opportunity for making data-based decisions since there are more data to analyze. They continue their list of considerations by supporting Horner and Baer’s (1978) claim that MP designs can be more practical and decrease the likelihood of testing threat because baseline is not as extended compared to the MB design (Horner & Baer, 1978; Ledford et al., 2018). A final recommendation is to consider ethical guidelines when choosing the MB design over the MP design given that it is unethical to withhold effective treatment (Bailey & Burch, 2016). If it is foreseen that an extended baseline may be keeping treatment from a participant, then a probe design should be considered due to the fact that probe designs have shorter conditions because fewer data points are required (Bailey & Burch, 2016; Ledford et al., 2018).

1.4 Conclusion

In conclusion, both the MB and MP designs are feasible for research and for informing treatment decisions (Ledford et al., 2018). There are hundreds of articles using both methods effectively and that meet design standards set by WWC (Ledford & Gast, 2018; WWC, 2013). When choosing which design to use, resources, practicality, and ethics should be considered in order to ensure the best and most ethical design is chosen.

CHAPTER 2. EVALUATING FOR RIGOR, QUALITY, AND BIASES

Within the field of Applied Behavior Analysis, there are six attitudes and assumptions and seven characteristics that inform each practitioner's behavior (Cooper, Heron, & Heward, 2020). These attitudes, assumptions, and characteristics establish a need to evaluate studies based on rigor, quality, and biases. Rigor, quality, and biases are terms explained by Ledford, Lane and Tate (2018) to describe to what extent the results of a study or intervention "are due to planned differences between conditions, and not to any other facts" (Ledford, Lane, & Tate, 2018. p. 366). Rigor, quality, and biases are all evaluated differently based on their individual properties. The purpose of this review is to evaluate the article written by Barton, Rigor, Pokorski, Velez, and Domingo (2019) for rigor, quality, and biases.

2.1 Rigor

Both MB and MP designs can be used across participants, behaviors, and contexts (e.g., settings, formats, arrangements), both are visually analyzed and formatted vertically (stacked A-B designs), and they share almost the same design standards according to What Works Clearinghouse (2013) (Ledford et al., 2018; WWC, 2013). Each of these individual components are explained in depth in the following sections.

2.1.1 Design Standards

Barton et al. (2019) utilized a multiple baseline single case design to assess the independent variable (IV) across behaviors. These standards state that there must be a systematic manipulation of the IV and multiple attempts to demonstrate the effectiveness of the IV over time. In addition, a minimum of three data points in each condition must

be recorded along with inter-observer agreement (IOA) across 20% of sessions in each condition with mean agreement of at least 80% across each condition (What Works Clearinghouse, 2013; Ledford, Lane, & Gast, 2018). Cooper, Heron, and Heward, (2020) continue these standards by requiring procedural fidelity (PF) data to be recorded across 25% of sessions in each condition with a score of at least 80%. Considering these standards, Barton et al. (2019) met design standards with reservations.

In their article, Barton et al. (2019) systematically manipulated the IV and displayed several attempts at demonstrating the effectiveness of the intervention. This was accomplished by introducing the IV at different times based on set criterion and recruiting four participants with whom effectiveness could be demonstrated (Barton et al., 2019). There were also three tiers of behaviors for three participants and four tiers of behaviors for one participant with which effectiveness could also be demonstrated (Barton et al., 2019). The primary reason for why their study met standards with reservations was due to the fact there was not a minimum of five data points recorded in each condition to meet standards without reservations (WWC, 2013). Instead, a minimum of three data points was recorded in each condition.

2.1.2 IOA and PF

In terms of IOA and PF, Barton et al. (2019) adhered to design standards by collecting IOA data across all conditions and participants for a minimum of 20% of sessions while also meeting the minimum agreement of 80% (Barton et al., 2019). PF data was collected for 93% of sessions across all conditions and coaches which resulted in the minimum of 80% PF being met (Barton et al., 2019).

2.1.3 Evaluation of the Outcomes

The final component that is addressed under rigor is the evaluation of outcomes. This category evaluates whether or not there was a functional relation. Based on the standards set by WWC (2013), Barton et al. (2019) demonstrated strong evidence of a functional relation with one of their four participants and limited evidence with three of their participants (WWC, 2013). This was determined based on the fact that for the participant that demonstrated a functional relation, there were zero non-effects. This means that there was no difference in level or trend when comparing baseline to intervention. The other three participants did not demonstrate a difference in level or trend when comparing baseline to intervention in more than one tier leading to zero evidence of a functional relation (WWC, 2013). Something to note, however, was that there was variability within conditions and minimal overlap across conditions, however, this does not hinder the rating of strong evidence (Barton et al., 2019).

2.2 Quality and Biases

2.2.1 Quality

Quality “refers to whether the study includes components that are considered to be important for generality or applicability” (Ledford et al., 2018. p. 366). In their article, Barton et al. (2019) utilized several systems for improving the quality of their study. These systems are explained in the following sections.

2.2.1.1 Randomization

Barton et al. (2019) did not use randomization in their study and, upon further evaluation, this would have been appropriate since the behaviors targeted were not a part of a chain of behaviors. Based on this fact, the quality of the study could decrease,

however, because Barton et al. (2019) utilized other methods for increasing quality. Based on this fact, choosing to not randomize their participants did not hinder the generality and applicability of the study.

2.2.1.2 Blinding

Barton et al. (2019) used single blinding of the data collectors in their study. This was done through training their data collectors to collect IOA but not revealing other components or involving them further in the study (Barton et al., 2019).

2.2.1.3 Ecological Validity, Social Validity, Generalization, and Maintenance

Based on the participants, behaviors, and contexts that this article included, the study proved to be ecologically valid (Barton et al., 2019). This is due to the fact that the behaviors utilized were intended to be used by practitioners, in the classroom, thus, fitting the definition of ecological validity (Barton et al., 2019; Ledford et al., 2018). Barton et al. (2019) also conducted a survey evaluating the social validity of the study's purpose, goals, and outcomes. This was appropriate for evaluating if the dependent variable was valued by the participants who are intended to use this in the future. According to Barton et al. (2019), "social validity was high across target behaviors given all received an average rating of 4.45 out of 5 or higher" (Barton et al., 2019. p. 93). Generalization and maintenance also were assessed in this study by observing the teachers at a 3 week and 3-month follow-up; however, occurrences of the target behaviors did not occur (Barton et al., 2019). While these are important quality indicators, biases must also be observed when evaluating quality.

2.2.2 Biases

Ledford et al. (2018), described biases as "the likelihood that the outcomes of a study are *biased* due to some methodological decision made by the researchers, resulting in potential overestimation of effects" (Higgins et al., 2011; Ledford et al., 2018. p. 366).

Based on the fact that randomization participants consisted of doctoral candidates from a laboratory pre-school, there was a high risk for selection bias however, because blinding was used, risk was mitigated (e.g., detection bias)(Higgins et al., 2011).

2.3 Conclusion

Based on the standards set by Higgins et al. (2011), Ledford et al. (2018), and WWC (2013), Barton et al. (2019) produced a quality and rigorous study. Though there are limitations, future research will be able to replicate and continue developing the procedures addressed in their study because these standards were followed. This aids the field in growing and developing effective strategies and interventions for researchers and practitioners alike.

CHAPTER 3. TRAINING STRATEGIES FOR EARLY CHILDHOOD EDUCATORS

Ms. Fernandez is the lead teacher in an early childhood education classroom. The five students that comprise her class vary in age and level of need (e.g., diagnosed disability; socioeconomic status). Two paraprofessionals (paras) assist Ms. Fernandez in the classroom and both completed an intensive two-day training session that focused on a new intervention for two of the students. Ms. Fernandez recently noticed, however, that the paras no longer implement the interventions as they were taught. As a result, the students' behaviors are not changing and education goals are not being met. Ms. Fernandez is perplexed to identify the root cause of the intervention shortfall. After all, she personally led the training session of her paras and provided thoroughly detailed plans for each student. Currently, Ms. Fernandez struggles to retrain and refocus her paras on proper techniques. This struggle affects her students by having a negative impact on their overall success.

Early childhood educators who only receive professional development in the form of workshops, lectures, or one day trainings may lack adequate training to fully support their students. Artman-Meeker, Fettig, Barton, Penny, and Zeng (2015) indicated that, “there is a growing recognition that these forms of professional development (PD) are insufficient if the goal is to influence teachers’ sustained use of evidence-based intervention practices in [Early Childhood] settings” (p. 183). This could be due in part to professional development being viewed as only occurring in the form of a one-time or brief training. The effect of this method of PD is that students may not be receiving effective treatment which could directly impact their growth and development. A solution to this issue is comprised of multiple steps and considerations that are shown to improve

student and practitioner outcomes (Artman-Meeker et al., 2015; Ledford et al., 2019). This solution is provided by Dunst and Trivette (2009) and is intended to be used when training or teaching, and it optimizes the acquisition of knowledge. The *Adult Learning Theory* is described by Dunst and Trivette (2009) as the “collection of theories, methods, and approaches for describing characteristics of and conditions under which [the] process of learning is optimized” (p. 2). Dunst and Trivette described four learning methods: coaching, guided design, just-in-time training, and accelerated learning. Their article, though, focused primarily on coaching and what the individual components are that make-up effective coaching.

3.1 Coaching

Coaching is supported in early-childhood literature as an evidence-based practice for the sustained use of interventions (Artman-Meeker et al., 2015; Joyce & Showers, 1980; Trivette, Dunst, Hamby, & O’Herin, 2009). Hargreaves and Dawe (1990) described coaching as, “a method of transferring skill(s) and expertise from more experienced and knowledgeable practitioners to less experienced ones” (p. 230). It is oftentimes required for practitioners, along with additional training and supports, to improve their practice and skill repertoire past its initial learning phase and can be done by peers in the working environment (Artman-Meeker et al., 2015; Hargreaves, & Dawe, 1990; Joyce & Shower, 1980). There are six key components of coaching that increase potential effectiveness. These components are introduction, illustration, practice, evaluation, reflection, and mastery (Ledford et al., 2019; Trivette et al., 2009).

3.1.1 Introduction

Introduction is the action of providing information, materials, and practices that “are related to the target skill” at the beginning of training (Ledford et al., 2019, p. 235). Introduction can be in the form of a pamphlet, a hand-out, or online modules that introduce information relevant to the target skills. This provides the information and rationale for why you are asking the trainee to engage in the target skill.

3.1.2 Illustration

Illustration refers to modeling/demonstrating the target skills by using whatever materials are necessary to replicate the target skill. Illustration can occur through the use of video modeling (e.g., watching one’s self perform the target skills, watching someone else) and live modeling (Trivette et al., 2009).

3.1.3 Practice

Practice involves the coach providing the trainee the opportunity to perform multiple repetitions of the new skills. This is followed by the coach delivering performance-based feedback. Practice should occur in the environment the target skills are intended to be used and can occur in the form of role-playing. Performance-based feedback should include supportive feedback which informs the trainee what was done correctly and corrective feedback, which is used, as needed, to improve the trainee’s use of the new skill (Ledford et al, 2019).

3.1.4 Evaluation

Evaluation is the practice of reviewing “the outcomes of the use of target skills” (Ledford et al., 2019, p. 235). Evaluation can happen through taking data and interpreting the outcomes. This can be done by completing the following steps.

1. Create a simple list of all the steps needed to engage in the target skills correctly.
2. Place checks next to each step completed correctly.
3. Calculate the percentage of steps completed by dividing the number of steps completed by the total number of steps and multiply the quotient by 100.
4. Over time, calculate the average number of steps completed by adding together the percentages over time and dividing the sum by the number of times data was collected.

It is important to consistently record data in order to know when to adapt coaching. For instance, the data may inform the coach that they need to repeat the steps in the coaching process in order to execute the target skills as intended because the trainees are, on average, are only completing 75% of the steps. Evaluation also includes providing performance-based feedback; the feedback is delivered when the trainee is engaging in the target skills (e.g., in person feedback, bug-in-ear technology) and/or after (e.g., in person, through e-mail, using a checklist; Ledford et al., 2019). The method for delivery should be considered based on the trainee and the target skills. For example, if the skills being targeted are complex, feedback is delivered in the moment opposed to at the end of the session (Ledford et al., 2019).

3.1.5 Reflection

Reflection is the component of coaching where the coach and trainee collaborate and assess their progress and establish goals for future steps (Trivette et al., 2009).

3.1.6 Mastery

Mastery is the sixth component and consists of providing supports for the trainee in order to conceptualize how the target skills can be utilized across settings and people and how to maintain these skills over time (Dunst & Trivette, 2009; Ledford et al., 2019).

3.2 Conclusion

Artman-Meeker et al. (2015) demonstrated in their article that all six components of adult learning are critical and lead to more “positive learner outcomes” (p. 9). They also showed that practices that “actively involve learners to [use], [process], and [evaluate] their knowledge and skills as part of the learning and mastering of new information or practices are most effective” (Artman-Meeker et al., 2015. p. 9). These practices are effective with practitioners of varying backgrounds and abilities and can be utilized in the work environment. This makes coaching a feasible and effective strategy for promoting professional development and skill acquisition in practitioners.

Determined to ensure the success of both students and paras, Ms. Fernandez began following the six components of coaching. Soon after identifying which format was most appropriate for her paras, she began collecting data by creating a list of all the steps that need to be completed in order to perform the target skills and placing a check next to each completed step. She then realized that she needed to repeat the practice, evaluation, and reflection components in order to reach mastery of the target skills based on the fact that the paras were only completing, on average, 50% of the steps needed to engage in the target skills. Both paras have since communicated with Ms. Fernandez that they feel more comfortable with the intervention they were taught and now demonstrate further understanding by implementing the procedures with other students and completing the target skills as intended. Ms. Fernandez is impressed with her paras and continues to deliver feedback on their performance in order to promote maintenance of the new skills.

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