2020

Alternative Thesis Project for Spring 2020 Semester

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Elaine Murner, Student

Dr. Justin Lane, Major Professor

Dr. Melinda Ault, Director of Graduate Studies

ALTERNATIVE THESIS PROJECT FOR SPRING 2020 SEMESTER

THESIS

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

By

Elaine Murner

Lexington, Kentucky

Director: Dr. Melinda Ault, Associate Professor in Early Childhood, Special Education, and Counselor Education

Lexington, Kentucky

2020

ABSTRACT OF THESIS

ALTERNATIVE THESIS PROJECT FOR SPRING 2020 SEMESTER

Overview of Alternative Thesis Project

During the Spring 2020 semester, students within the special 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 (behavior skills training for social behaviors) 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 Design, Multiple Probe Design, Quality, Rigor, Behavior Skills Training, Social Behaviors

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(Name of Student)

05/08/2020

Date

ALTERNATIVE THESIS PROJECT FOR SPRING 2020 SEMESTER

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CHAPTER 1. ALTERNATIVE THESIS PROJECT PART 1: COMPARISON OF MULTIPLE BASELINE DESIGN AND MULTIPLE PROBE DESIGN

Single case designs (SCDs) are commonly utilized in applied research conducted in educational and clinical settings (Gast & Ledford, 2018). SCDs can be used to evaluate the effectiveness of an intervention on a dependent variable (DV). Depending on the research question, a SCD may be used to answer a demonstration or comparison question by introducing and withdrawing an intervention multiple times (withdrawal design), time-lagged introduction of an intervention, or rapidly or slowly alternating between conditions to compare interventions (Gast & Ledford, 2018). The specific research design for each study is selected based on the research question and the target behavior. For the remainder of this paper, the multiple baseline (MB) design and multiple probe (MP) design will be discussed in terms of similarities and differences, procedural guidelines, and advantages and limitations.

The MB design and MP design can be used with reversible and non-reversible behaviors, both designs can be visualized as multiple A-B graphs stacked into one figure, and both designs are useful in demonstrating the effectiveness of an intervention on a DV. Experimental control for both designs is demonstrated when data in all tiers remain stable until the independent variable (IV) is introduced for that tier, and a change is observed only in the tier in which the IV is introduced. Each tier is introduced to the IV in a time-lagged manner, and each tier represents a principal variation (i.e. behaviors, contexts, or stimulus condition) that the design can be conducted across (Gast, Lloyd, & Ledford, 2018).

MB or MP designs can be conducted across behaviors, contexts or stimulus conditions, and participants. MB or MP across behaviors assesses the effectiveness of an intervention on multiple behaviors for one participant. A design across contexts examines the effectiveness of an intervention on one behavior emitted by one participant across multiple contexts, such as materials, settings, time of day, instructional agents, or other changes in stimuli. Lastly, a design across participants assesses the effects of an intervention on one target behavior emitted by multiple participants (Gast et al., 2018).

When setting up an MB or MP design across any variation, the first step is to identify and define the variations (hereafter referred to as "tiers"). The dependent variable for each tier should be similar but functionally independent to reduce the chance of covariation. If the tiers are too similar, the intervention in one tier may affect the behavior in a different tier, but if the tiers are not similar enough, the intervention may not be effective for each tier. The next step is to identify a measurement system for data collection and to set criterion levels for when the IV should be introduced to each tier. Additionally, the order in which the tiers will receive the IV and the method for collecting interobserver (IOA) and procedural fidelity (PF) data should be specified (Gast et al., 2018). After all the above steps have been completed, data should be collected and the intervention should be introduced following the unique procedural guidelines for each individual design type.

The guidelines regarding frequency of data collection are the only procedural difference between the MB and MP design. In an MB design, data are collected continuously and concurrently across all tiers. The intervention is introduced to the first tier when pre-intervention data are stable across all tiers. When data in the first tier

reaches the pre-specified criterion, and if data in all subsequent tiers remain stable, the intervention is introduced to the second tier. Each tier after the second tier receives the intervention when the prior tier demonstrates mastery while data in all subsequent tiers remain stable (Gast et al., 2018).

In MP designs, data are collected intermittently rather than continuously. Data collection frequency for an MP design is determined by the design variation: MP design days (MP-D) or MP design conditions (MP-C). The MP-D variation involves intermittent data collection in the form of single session occurrences. When data in all tiers are stable during the pre-intervention condition, the IV is introduced to the first tier. Data in subsequent tiers continue to be measured intermittently, and when data in the first tier approaches criterion levels, three consecutive data points are collected for subsequent tiers. When the subsequent tiers demonstrate stable levels of responding while the first tier reaches criterion levels, the second tier is introduced to the intervention, and the pattern continues for all remaining tiers (Gast et al., 2018).

The MP-C variation involves intermittent data collection in the form of three or more consecutive sessions that make up their own condition. Again, data are collected in the pre-intervention probe condition until stable levels of responding are ensured, and then the IV is introduced to the first tier. When the first tier reaches criterion levels, all instruction stops and a probe condition is implemented across tiers. When the probe condition data are stable across all tiers, the IV is introduced to the next tier, and the pattern is repeated until all tiers have received the intervention (Gast et al., 2018).

In both variations of MP designs, data are to be collected across all tiers at the start of a study. It is required that all tiers have at least one data point during the first

three initial sessions, and it is recommended that data be collected across all tiers on the first session. It is also required that data be collected at least once prior to introducing the IV to a tier, but three data collection sessions are recommended. Additionally, data must be collected at least once every eight sessions (Gast et al., 2018).

The MB and MP design both have practical use in applied settings since an effective intervention does not have to be withdrawn in order to demonstrate an effect. However, both designs are susceptible to threats to internal validity, such as procedural infidelity, due to the extended baseline conditions. The specific advantages for using an MP design are that less time and effort are required since data are collected intermittently as opposed to continuously, and testing threats are reduced since participants are not exposed to data collection procedures as often. However, MP designs have a higher chance that certain threats to internal validity may be undetected since continuous measurement is not used (Gast et al., 2018). The advantage of using a MB design is that continuous measurement allows for quicker detection of data instability, but a disadvantage is a higher risk of testing effects due to continuous measurement.

Overall, MB design and MP design have more similarities than differences. Both designs use time-lagged procedures to determine the effectiveness of an intervention, and both designs can be used with a variety of behaviors. The only major difference between MB designs and MP designs is the frequency in which pre-intervention data are collected. Aside from that, MB and MP designs are both flexible, relatively easy to implement, and useful for practitioners.

CHAPTER 2. ALTERNATIVE THESIS PART 2: RIGOR AND QUALITY ANALYSIS OF A SINGLE CASE DESIGN RESEARCH ARTICLE

Assessing rigor, quality, and bias in experimental studies is important to determine the extent to which we have confidence that the intervention was effective and appropriate for use in typical contexts. A frequently used tool when assessing the rigor of SCDs is What Works Clearinghouse (WWC, 2020) design standards. Contemporary guidelines for evaluating rigor, quality, and bias were created based on the WWC design standards, the Council for Exceptional Children's (CEC, 2014) quality indicators (see Appendix), and related assessments (cf. Ledford, Lane, & Tate, 2018). The remainder of this paper is an analysis of a SCD study in terms of rigor, quality, and bias using the contemporary guidelines.

An article by Chazin, Barton, Ledford, and Pokorski (2018) described two studies that addressed the need for teacher supports when working with students with complex communication needs. The first study used a multiple probe across participants design to evaluate the effectiveness of behavior skills training (BST) on teacher fidelity when implementing a student's behavior intervention plan (BIP). In addition to teacher fidelity, the frequency of adult modeling, the student's unprompted correct usage of his augmentative and alternative (AAC) device, and the student's engagement in self-injurious behavior (SIB) were also measured. The results of the first study indicated that BST, particularly the coaching component, led to an increase in teacher fidelity of BIP implementation (Chazin et al., 2018). Adult modeling, student usage of an AAC device, and SIB data all provided informative correlational data, but since the research question

did not directly target these behaviors, only the multiple probe design for the primary behavior was assessed for rigor.

The authors stated the first study met WWC design standards without reservations (Chazin et al., 2018). However, the WWC design standards for a multiple probe design require a minimum of five data points be collected in every condition across tiers for design standards to be met without reservations. When a design has at least three but less than five data points in all conditions across tiers, the design can still meet design standards, but it will be with reservations (WWC, 2020). Although the authors intentionally implemented the intervention in the first tier after only three data points due to the severity of the student's SIB (Chazin et al., 2018), the minimum requirement of five data points in all conditions across tiers was not met. Since there were at least three data points in all conditions, the other design standards were assessed to determine the overall rigor of the design.

The first study met all other primary design standards without reservations. The IV was systematically manipulated through a time-lagged procedure, there were four attempts to demonstrate the effectiveness of the intervention, and both IOA and PF were collected for a minimum of 33% of sessions. The average IOA data was 94.8%, and the average PF data was 98% (Chazin et al., 2018). In addition to the primary design standards, there were four multiple-probe specific standards that were assessed. Each tier had at least one data point collected during the initial three sessions of the pre-intervention condition, data were collected at least every eight sessions, each tier had at least one data point collected immediately prior to introducing an intervention, and untreated tiers had probe data collected when the tier receiving intervention approached

the mastery criterion. Since all design standards were met except for the minimum number of data points required in each condition, the first study met design standards with reservations.

The effectiveness of the intervention was determined through visual analysis of the data. Data were assessed in terms of level, trend, and variability within each condition, as well as the immediacy of effect, the amount of overlapping data points between conditions, and the consistency of change across tiers were considered when making the decision of whether or not there was basic demonstration of effect present in the data for each tier. The overall effectiveness of the intervention was based on the number of effects and non-effects within the design. In the first study, there were four basic demonstrations of effect and no non-effects across participants. Although data were overlapping from baseline to intervention for Claire, there was a clear decelerating, contra-therapeutic trend in the probe condition and an immediate increase in level with a therapeutic trend when the intervention began, therefore indicating a basic demonstration of effect. Based on the number of effects and non-effects in the design, the study had a strong demonstration of effect between BST and teacher fidelity of BIP implementation.

Bias and quality were analyzed using contemporary guidelines for SCDs (Ledford et al., 2018). Bias was assessed in terms of randomization, blinding, and the appropriateness of participants, and quality was assessed in terms of ecological validity, social validity, generalization, and maintenance. The first study did not directly discuss randomization or blinding. Although it is possible that participants were randomized across tiers, this was not specifically mentioned; therefore, there was a potential risk of bias in regards to randomization. Blinding of secondary data collectors could have

occurred since data were collected through video recordings. If the data collectors were unaware of which condition they were collecting data for, there would have been less risk of bias; but since blinding did not occur, there was a risk of bias. The participants selected for the study were appropriate given the research question since the adult participants worked with the child throughout the school day and were required to implement his BIP; therefore, there was not a risk of bias in terms of participants.

As for quality measures, the first study directly assessed social validity and maintenance, but it did not directly assess ecological validity or generalization. Social validity was directly measured through naïve coders ratings of video recordings from pre- and post-intervention sessions. Ratings showed that naïve observers saw increases in teacher fidelity from pre- to post-intervention. Maintenance sessions were collected after coaching was withdrawn, and maintenance measures could potentially serve as an additional social validity measure since maintained skills would indicate a lasting effect of the intervention after training was removed (Chazin et al., 2018). Generalization could have occurred across settings, and ecological validity could have been assessed through teacher questionnaires. Since neither generalization nor social validity were measured, the quality of the study was not as high as it could have been. Overall, the first study had risk of bias due to the lack of randomization and blinding, and there were quality measures that were not included.

The second study used an A-B-A-B withdrawal design to analyze the effects of a teacher's modeling behavior on a student's AAC use (Chazin et al., 2018). The second study found an increase in unprompted student use of his AAC device when adult modeling was provided (Chazin et al., 2018). Since a withdrawal design was used, it was

not necessary to assess the additional standards associated with the multiple probe design; therefore, only the primary WWC design standards (2020) were used to assess the level of rigor in the second study.

The IV of the second study was systematically implemented within the context of a withdrawal design, and condition changes were based on the data from the student's unprompted AAC use. There were three opportunities for demonstrations of effect across four conditions, and a minimum of five data points were plotted in each condition. Interobserver agreement data were collected for a minimum of 45% of sessions across conditions with an average of 94% IOA, and PF data were collected for 100% of sessions with an average of 93% PF (Chazin et al., 2018). Since all standards were fully met, the second study met design standards without reservations.

The effectiveness of the intervention was assessed using the same visual analysis components that were used when assessing the first study. In the second study, there were three demonstrations of effect between conditions and there were no non-effects. Overall, this indicates that there was a strong effect between adult modeling and increased student use of an AAC device.

The second study did not utilize randomization or blinding. Randomization would not have been appropriate given the context of the study, but secondary coders could have been blind to which condition they were collecting data for in video recordings. The student participant from the first study was selected for the second study, and he was an appropriate choice because he required instruction in AAC use to increase his communication skills. There was a potential risk of bias in the second study due to the lack of blinding of secondary coders.

In terms of quality measures, only generalization was assessed in the second study. Ecological and social validity were not explicitly discussed, and maintenance was not assessed. Generalization was measured across implementers in all conditions by having a secondary implementer periodically conduct sessions, and generalization results mirrored the primary results. Maintenance was not assessed, but it would not have been appropriate to remove the intervention since the student was showing increases in AAC communication. Although there are ecologically and socially valid reasons for increasing a child's use of an AAC device through modeling, these validity measures were not directly assessed; therefore, the quality of the study was not as high as it could have been.

Overall, both studies had potential risks of bias and both were missing quality components. Both studies demonstrated strong effects between the intervention and the dependent variable, and both studies were rigorous in their design. The second study fully met all design standards while the first study met design standards with reservations. The findings from each study could be useful for practitioners when creating adult trainings on how to implement classroom procedures or when creating interventions to increase student communication.

CHAPTER 3. ALTERNATIVE THESIS PROJECT PART 3: PRACTITIONER ARTICLE ON THE USE OF BEHAVIOR SKILLS TRAINING TO TEACH SOCIAL BEHAVIORS

Social behaviors, such as helping, sharing, and communicating with peers, are often learned in early childhood (Lane, Gast, Ledford, & Shepley, 2017). Appropriate social behaviors are necessary for social development and have been linked to increased levels of play skills (Ergin & Ergin, 2017); however, some children require systematic instruction to learn social behaviors. Although a variety of methods have been used to teach children social behaviors (i.e. prompting hierarchies and progressive time delay; Lane, Gast, Shepley, & Ledford, 2015; Kaminski, Fisher, & Akers, 2018), an alternative method to teach social behaviors is behavior skills training (BST). Behavior skills training is a training package consisting of instruction, modeling, rehearsal, and feedback that has been used to teach a variety of skills (Dibs & Sturmey, 2012; Parsons, Rollyson, & Reid, 2012). Behavior skills training is a data-based practice that can be cost-effective, efficient, and easy to implement (Parsons et al., 2012; Graudins et al., 2012). Practitioners should consider using BST when teaching children social behaviors due to the associated benefits and potentially positive effects.

Mrs. Brown is a preschool teacher in an inclusive preschool classroom. There is a child in Mrs. Brown's class who struggles with engaging in appropriate social behaviors. The child, Nick, is a four-year-old who has difficulty sharing his toys and playing with other students, and he prefers to play with toys alone or have an adult read a story aloud to him during free-choice activities. Mrs. Brown observes and collects frequency data on Nick's social behavior and other students' social behavior during afternoon recess over the course of a few weeks, and she notices that Nick engages in a

lower number of social behaviors than his peers. She decides to teach Nick how to engage in appropriate social behavior using BST.

When creating a BST intervention, it is important to first determine what skill will be taught, when and where the training will occur, and what each component of the training will include (Parsons et al., 2012). To determine the target skill, the practitioner should identify an area that a student or group of students need additional support, and a behavioral definition for the skill should be created. To determine the location and time for intervention to occur, the practitioner should consider what works best for their setting and their students in regards to practical considerations (e.g. scheduling, staffing, available space and resources, etc.). Due to the time constraints that educators often face in the classrooms, the training should be planned at a frequency that is feasible for the teacher while still allowing ample opportunities for the student to learn the target skill.

Mrs. Brown decides that the target skill she will teach to Nick will be sharing one toy from a selection of many similar toys. She defines sharing as Nick giving or attempting to give one toy from a selection of many similar toys to a peer. She decides sharing is an appropriate skill to teach Nick since he rarely engages in sharing with his peers and since an increase in sharing behavior could lead to an increase in opportunities for Nick to engage in other social behaviors. She then decides that intervention should occur at least three times a week between snack time and afternoon recess. She believes three times a week will be sufficient enough for the intervention to take effect, while allowing some days to be missed due to practical considerations and time constraints, and she believes that conducting the intervention immediately prior to afternoon recess will allow Nick an opportunity to practice any learned sharing behavior

in the naturally occurring setting. Next, Mrs. Brown will plan each component of the BST intervention.

When planning the instructional component of BST, practitioners should consider the learner's skills and deficits. Typically, a written description of the target behavior is presented to the learner during the instructional component (Parsons et al., 2012), but a four-year-old may not be able to read a written description. It might be more appropriate to provide a visual of the target behavior or only provide verbal descriptions depending on the particular student's needs. In a study conducted by Ervin, Wilson, Maynard, and Bramblett (2018), the instructional component of a BST intervention was delivered as a conversational script, and all participants learned how to appropriately respond to disruptive behavior from the training. Practitioners should create or utilize instructional strategies that are most likely to be effective with their individual student or students.

When planning the modeling component, the implementer of the intervention should provide a demonstration of the target skill (Parsons et al., 2012). Demonstrations of the target skill can look different depending on what skill is being taught. For example, a study conducted by Johnson et al. (2005) successfully taught 13 preschool children abduction prevention skills through BST where the modeling component was delivered through adult demonstration of the target skills. Other successful methods of modeling in BST have included the use of peer models or video models (Ervin et al., 2018; Day-Watkins, Pallathra, Connell, & Brodkins, 2018). Again, the individual student's strengths and areas of need should be considered when providing a model of target behavior. Prior to determining the modeling component of BST, the student should be assessed for any necessary pre-requisite skills they may need, such as the ability to visually attend to a

model, the ability to attend to an activity for a certain period of time, or the ability to imitate a model.

Following the modeling component, a practitioner should plan for the rehearsal and feedback components. Rehearsal and feedback components are usually intertwined, since specific feedback is often based on the student's performance during rehearsal. A study conducted by Morgan and Wine (2018) successfully taught an 18-year-old student with autism four restaurant job skills through BST. Rehearsal opportunities were provided in a functioning restaurant during business hours, and either corrective feedback or behavior specific praise were given based on the participant's performance. The participant learned all four skills in under six training trials, and he maintained all skills six months after training (Morgan & Wine, 2018). By providing the student a chance to practice the target skill and by providing real-time feedback, the rehearsal and feedback components are complete.

Mrs. Brown chooses to set up her BST intervention in the following manner: First, she decides to read-aloud a short story with pictures and visual supports on the importance of sharing as her instructional component. Since Nick enjoys having stories read aloud to him, Mrs. Brown believes this will be a reinforcing activity, and she believes the pictures and visual supports in the story will allow Nick to better understand the behavior that is expected of him. For the modeling component, she decides that she will demonstrate how to share one toy from a selection of many similar toys with one of Nick's peers while narrating her actions aloud, and she determines a sampling of peers that would work well. Finally, she decides that for the rehearsal and feedback components, she will provide Nick with the selection of toys and instruct him to share

with the peer. Depending on Nick's behavior, Mrs. Brown will either provide behavior specific praise and a high-five, or she will provide the instruction again and physically prompt Nick to share a toy through hand-over-hand guidance. Now that all aspects of the intervention have been considered, Mrs. Brown is ready for the next step.

The next step in preparing for the intervention is to collect any materials that will be required, including any data collection materials. Once materials are secured, the BST intervention is ready to be implemented with the child. Implementation should occur at the pre-specified location and time, with the pre-determined materials, in the way the plan was written. Each component of the intervention will be introduced sequentially during each session, and any data collection should be graphed for visual analysis. Once the student reaches mastery criterion, the behavior should be periodically monitored for any changes.

Mrs. Brown has thought out and pre-planned all the components of the training. She gathers the storybook on sharing and visual supports, the toys for the modeling and rehearsal components, and her data collection materials. The following day, she implements the intervention as planned. If the intervention is effective in teaching Nick how to share, Mrs. Brown should observe an increase in sharing behavior over the next few weeks. If there are no changes in Nick's behavior over the next few weeks, Mrs. Brown may need to modify her intervention to better meet Nick's needs.

As the scenario shows, BST does involve pre-planning and assessment of intervention effects. Once the planning is complete though, the intervention can be implemented in fairly short durations for relatively low costs. The steps of BST are often already utilized throughout the classroom as different instructional procedures (Parsons et

al., 2012), so teachers may feel comfortable utilizing the individual components. When all components are combined into one treatment package, there is a better chance that the student will effectively learn the target behavior.

APPENDIX

Article:

Evaluation of Rigor

Code for each design within an article

Design	Systematic Manipulation of IV	Adequate attempts to demonstrate effectiveness of IV	At least 5 data points in each condition	If not, at least 3 data points in each condition	IOA collected at 20% of sessions in each condition	IOA was at least 80%	For Multiple Probe Designs: Other MP guidelines met	PF collected at least 20% of sessions in each condition	PF was at least 80%	Rating: *Meets, Meets with Reservations, Does Not Meet*

Evaluation of Outcomes

Code for each design within an article

Functional Relation Present?	Rating: *Strong, Moderate, None*

Evaluation of Bias and Quality

Code for the article

Randomization

Was randomization appropriate?	
Did randomization occur?	
Type of randomization	

Rating for Randomization

Low Risk of Bias *High Risk of Bias*

Blinding

Were any members of the research team blind to the conditions (IOA, PF, implementation) or outcomes (when visually analyzing data)?	
Type of blinding	

Rating for Blinding

Low Risk of Bias *High Risk of Bias*

Quality Measures

Was the study ecologically valid (given research question and intended audience)?	
Was social validity (goals, outcomes, procedures) mentioned and assessed? And appropriate?	
Was generalization mentioned and assessed? And appropriate?	
Was maintenance mentioned and assessed? And appropriate?	

Bias and Quality

Were participants appropriate for the study - given the research question, inclusion criteria, and descriptive information?	

Rating for Participants

Low Risk of Bias *High Risk of Bias*

Additional Considerations for MP Designs

WWC Standards for Multiple Probe Designs

	Meets Evidence Standards	Meets Evidence Standards with Reservations
Meet standards for a multiple baseline design	Meets standards	Meets standards with reservations
Overlapping initial pre-intervention sessions (Baseline)	Each case has probe points in the initial three sessions	Each case has at least one probe point in the first three sessions
Probes prior to introducing the independent variable to a given case	Three consecutive points just prior to introducing the independent variable and one probe point every eight sessions	At least one point just prior to introducing the independent variable and one probe point every eight sessions
Probes for subsequent cases when introducing the independent variable to a preceding case	At least one point placed either immediately prior to the first intervention session for the earlier case or once the intervention criterion is reached for the earlier case.	At least one point placed either immediately prior to the first intervention session for the earlier case or once the intervention criterion is reached for the earlier case.

Notes/Comments

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EDUCATION

Master of Science student in Special Education Teacher Leader at the University of Kentucky, August 2018 – present. Thesis title: "Alternative thesis project for spring 2020 semester."

Bachelor of Science (December 2017) in Special Education – MSD, University of Kentucky, Lexington, Kentucky.

PRESENTATIONS AT PROFESSIONAL MEETINGS

Lane, J. D., Murner, E. M., & Shepley, C. Building the foundation for meaningful interactions between educators and students. University of Kentucky. Council for Exceptional Children 2020 Conference, Portland, Oregon, 6 February 2020.