Evidence-Based Practice & Practice-Based Evidence Applied to Adult, Medical Speech-Language Pathology

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Description
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Evidence-Based Practice & Practice-Based Evidence Applied to Adult, Medical Speech-Language Pathology

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Abstract

This paper reviews concepts of evidence-based practice (EBP), and provides a discussion of the current limitations of EBP in terms of a relative paucity of efficacy evidence and the limitations of applying findings from randomized controlled clinical trials to individual clinical decisions. A complementary model of practice-based evidence (PBE) is offered to encourage clinical scientists to design, implement, and evaluate our own clinical practices with high quality evidence. Two models for conducting PBE are described: the multiple baseline single-case experimental design and a clinical case study enhanced with generalization and control data probes. Gathering, analyzing, and sharing high quality data can offer additional support through PBE to support EBP in speech-language pathology. It is our hope that these EBP and PBE strategies will empower clinical scientists to persevere in the quest for best practices.

NOTE: Pagination of the final published article is indicated by [p.] in the text below for ease of identifying passages in the published version. The final published version should be used for citation purposes, as final copyediting changes may not be reflected in this manuscript.
Evidence-based practice (EBP) offers a framework for clinical decision-making grounded in rationality. The evidence-based clinician will use his/her own clinical experience and expertise to integrate the best current evidence with the client’s values and preferences within the context of the local environment (DiCenso, Cullum, & Ciliska, 1998; Sackett, Straus, Richardson, Rosenberg, & Haynes, 2000). The potential benefits of EBP include increased service delivery and quality of care, heightened accountability, and a bridging of the research-practice gap (Apel & Scudder, 2005; Jette, 2001; Sackett et al., 2000). Although adopted and required as a method for enhancing clinical and research practices by ASHA in 2005, and reflected in our Code of Ethics (ASHA, 2010), there remain significant challenges to the infusion of EBP into everyday practices. EBP should guide all clinical decisions, including prevention, assessment and diagnosis, treatment or management, and discharge; however, treatment evaluation has received the most emphasis and scrutiny to date. In this paper, we will explore the limitations of EBP and describe a complementary model for practice-based evidence focusing on methods to evaluate and document treatment outcomes in everyday practice.

The Best Current Evidence

On the surface, the EBP framework demonstrates the importance of all three aspects of evidence-based clinical decision making: clinical experience, client preferences, and the best current evidence. When one scrutinizes the procedural steps for EBP, however, the emphasis on the empirical research literature (best current evidence) becomes apparent. According to the ASHA model (www.asha.org/members/ebp), implementation of EBP can be conceptualized as a four-step process: formulating a searchable question, finding the evidence, assessing (critically appraising and summarizing) the evidence, and making the clinical decision. Not until the fourth step is the clinician reminded to integrate the evidence with the client’s values and preferences. There is no doubt that high quality evidence that SLP interventions “work” is critical to demonstrating and documenting the benefits of our services, but different types of research can contribute to different aspects of clinical decisions. Robey (2004) described a five phase model of clinical research as a linear progression. According to this model, different research designs can effectively contribute to evidence of an active treatment effect, efficacy, effectiveness, or efficiency. Phase I or II research utilizes a variety of designs to demonstrate that a treatment is active—i.e., an observed effect can be elicited with this intervention—without rigorous research designs. This type of evidence is important for initial evaluation of new interventions in order to guide decisions before proceeding to larger, more expensive, scaled trials. In Phase III, the key question relates to efficacy—i.e., demonstration that the observed effect results from the intervention alone and not some other unrelated factor—and requires well-designed experimental studies, such as the randomized controlled trial (RCT), to control for threats to internal validity of the research design. After demonstrating efficacy in tightly controlled trials, researchers should then proceed to Phase IV to establish evidence of effectiveness—i.e., demonstration that similar effects can be obtained with large samples in everyday practice—and focus on generalizability (external validity) of findings. Once efficacy and effectiveness have been documented, in Phase V, the researchers proceed to evaluation of treatment efficiency—i.e., evaluation of “who benefits from the treatment protocol and at what cost” (p. 406)—utilizing cost-benefit analysis to compare treatments otherwise shown to be efficacious and effective for the target population. Progressing through these linear phases of research may take several years,
but ultimately, the evidence-based practitioner faced with clinical decisions about which intervention to apply under a unique set of individual circumstances would be most concerned with evidence of effectiveness in everyday practice and efficiency of the best treatment available. From an EBP perspective, evidence of Phase III efficacy has received the greatest emphasis. Practice recommendations, which establish an intervention as “evidence-based,” focus exclusively on evidence of Phase III efficacy by giving the greatest “value” to RCTs. We argue here that while such evidence of efficacy is critical to demonstrate the effects of an intervention and rule out potential bias in research design with tightly controlled experimental studies, the clinician struggling to implement best practices must always temper any EBP decision with the client’s values and preferences and use his/her own best judgment based on clinical expertise and knowledge of the limitations of efficacy research for individualized decision-making.

Limitations of Evidence-Based Practice

EBP promises to improve the quality of our practices by wide-spread implementation of efficacious interventions. However, there remain several barriers to such practices in SLP. Implementation challenges have been described in several surveys, and among the top cited barriers are: limited time to engage with the best current research evidence; inadequate access to the best current research evidence; lack of knowledge and skills to access, summarize, and interpret the best current research; and the relative paucity of high levels of evidence to support or refute clinical decisions (Cohen, Stavrie, & Hersh, 2004; Meline & Paradiso, 2003; Zipoli & Kennedy, 2005). We believe that SLPs can acquire the knowledge and skills necessary to locate and summarize the best current evidence through coursework, continuing education, and mentorship. Instead, we will focus on two systemic barriers to EBP implementation: the paucity of evidence and limitations of the RCT.

Paucity of High Quality Evidence

An evidence-based “clinical scientist” (Apel, 1999, p. 99) can reduce the time barrier by seeking out pre-filtered evidence in the form of practice recommendations. Such recommendations consist of a systematic review of the research evidence with a corresponding translation of this research into practice recommendations. According to the American Academy of Neurology (AAN, 2004), in order for a practice to be recommended as a standard, there must be consistent evidence from at least two large randomized controlled trials with minimal attrition and using blinded outcome measures to minimize potential for bias. A practice guideline is established by at least one RCT or two consistent, well-designed, non-randomized experimental trials. A practice option is established by at least one well-designed, non-randomized experimental trial or by non-experimental evaluation, such as case study methodology. In an ideal world, such practice recommendations would be available for all of our interventions to help guide our clinical decisions for the best management plans. Unfortunately, such recommendations are not always available.

In the adult medical SLP community, we have a relative lack of Phase III efficacy trials. In order to provide pre-filtered evidence for SLPs, the Academy of Neurogenic Communication Sciences and Disorders (ANCDS) has formed several working groups to develop practice
recommendations based on the guidelines set forth by the AAN. Practice recommendations are published in peer-reviewed journals and provide an efficient means to summarize the best current evidence as they are all available as free full-text downloads on the ANCDS Web site (www.ancds.org). To demonstrate the relative lack of efficacy evidence, the ANCDS writing group for dementia has published seven recommendations to date. Among these, none serve as practice standards (should be done), five can be classified as practice guidelines (should be considered) (Hopper et al., 2005; Kim et al., 2006; Mahendra, Hopper, et al., 2006; Zientz, Rackley, Chapman, Hopper, Mahendra, & Cleary, 2007; Zientz, Rackley, Chapman, Hopper, Mahendra, Kim, et al., 2007), and two provide practice options (may be considered) (Bayles et al., 2006; Mahendra, Kim, Bayles, Hopper, & Azuma, 2006). How is the clinical scientist to implement EBP in the absence of efficacy data? Clinical researchers have an obligation under EBP to continue to investigate our interventions for dementia and provide more high-quality evidence to inform practices.

Limitations of the Randomized Controlled Trial

When a practice guideline or standard is available, the clinician is still faced with integrating that efficacy evidence into clinical decisions for the individual client in his/her office. The RCT is considered the highest quality of research evidence (“gold standard”) because it reduces the potential for bias, and is achieved through research designs that control for threats to the internal validity of a study, such as spontaneous recovery, changes in personal history/circumstances, or the learning effect linked to repeated standardized testing (Meline, 2010). Yet when the clinical scientist attempts to rationally relate practice recommendations to the individual client, several limitations emerge.

Ylvisaker et al. (2002) and Malec (2009) presented several compelling arguments about the limitations of the RCT for individualized clinical decision-making. Ylvisaker et al. emphasized that individual clinical decisions from group experimental studies (e.g., RCT designs) require two levels of statistical inference: from the study sample to the represented population and then from the represented population to the individual client. Instead, they advocated a model of client-specific hypothesis testing [p. 17] for rationally-based, theoretically-grounded, individualized and contextualized recommendations for clients with acquired brain injury. Malec described a model for individualized, ethical decision making in cognitive rehabilitation given additional limitations of the RCT: not every intervention can or should be investigated using an RCT design (i.e., ethical implications of withholding treatment), inattention to individual differences and preferences, and dismissal of placebo and non-specific effects, which may play a significant role in behavioral interventions. Even when high-quality evidence of efficacy is available, how should the clinician make the best, informed decisions for the individual client? One option would be to summarize any available evidence of effectiveness and efficiency; however, given the paucity of efficacy evidence and the linear nature of the phases of clinical research, we have access to even fewer evaluations of effectiveness or efficiency. Another option is to consider single-case experimental designs.
Single-Case Experimental Designs in EBP

While the RCT remains the gold standard to demonstrate evidence of treatment efficacy, single-case experimental designs (SCED) offer a complementary design that can both establish evidence of efficacy through rigorous design and offer direct and specific recommendations when the client in a study matches the client in your office with similar key features. SCED are not to be confused with case study designs. A case study may provide quantitative and/or qualitative in-depth analysis of an individual client’s response to an intervention, but is non-experimental in nature because there is no evidence of research control (i.e., no control condition or group) and introduces the potential for bias given the lack of random assignment with a single case (see Figure 1). On the other hand, SCED enable researchers to systematically manipulate various conditions (e.g., when to introduce the intervention) to demonstrate experimental control and reduce the likelihood that extraneous variables affect treatment outcomes. The defining attribute of SCED is that data are collected and analyzed at the individual level (Richards, Taylor, Ramasamy, & Richards, 1999). In contrast with group designs, which aggregate data (i.e., group mean and standard deviation) for statistical analysis, SCED rely on repeated measurement of the target behavior over time. The target behavior is operationally defined (e.g., number of words produced correctly) and recorded using direct observation (e.g., perceptual ratings during therapy session).

[p. 18] There are several specific types of SCED, such as the withdrawal/reversal, multiple baseline, or alternating treatment designs (Richards et al., 1999). A powerful and clinically useful design for the SLP is the multiple baseline (MBL) design. MBL designs enable an interventionist to systematically examine treatment effects on a target behavior by varying the
timing of intervention across three or more different conditions. As the name implies, multiple—and simultaneous—baselines are observed, and the same intervention approach is evaluated for each of the varied conditions at staggered times, allowing for experimental control. The design can be strengthened by randomly assigning the order of intervention for each condition. Figure 2 depicts the general layout of MBL designs, with the staggered implementation to establish experimental control. By its very nature, the MBL designs include demonstration of an experimental effect with replication within the study design (Richards et al., 1999). Even with small sample sizes, findings can be generalized to clients with similar profiles by direct comparison (Horner et al., 2005).

[p. 19] Three variants of the MBL design include MBL across participants, across behaviors, and across contexts. The MBL across participants design evaluates the efficacy of an intervention on three or more clients with similar profiles on the same targeted outcome. For example, we could assess the potential efficacy of smart phone-delivered text reminders to attend appointments for three clients with mild prospective memory impairments. The MBL across behaviors evaluates the efficacy of an intervention on three related targeted goals for a single client. For example, we could assess the potential efficacy of smart phone-delivered text reminders to complete three procedural tasks (attend appointments, take medications, and initiate phone calls) for a single client with moderate declarative learning impairments. The MBL across contexts design evaluates the efficacy of an intervention on the same targeted outcome in three or more specific contexts for a single client; this design is appropriate when spontaneous generalization across contexts is not expected to occur spontaneously. For example, we could evaluate the potential efficacy of errorless learning techniques to respond to smart phone-delivered text reminders to take medications when delivered in the therapy office, therapy gym, and dining room for a client with severe declarative memory impairment.

In SCED, visual analysis of the graphed data evaluates for functional relations between the intervention and observed changes in outcome. Baseline and treatment data are plotted on stacked graphs (see Figure 2) and analyzed for horizontal changes on each graph in three key parameters: level, trend, and variability (Kennedy, 2004). Level refers to the amount of the target behavior (i.e., high or low) and is analogous to the “average” in each phase. Trend refers to a direction or slope of the behavior within each phase (i.e., either rising or falling). Variability refers to the stability of the target behavior over time. Low variability describes a consistent or stable pattern, whereas high variability implies fluctuation in the target behavior. The nature of the intervention will determine which data patterns (e.g., higher or lower levels of the targeted behavior) are consistent with a favorable outcome. In addition, three replications of the effect (i.e., across participants, behaviors, or contexts) are required to demonstrate experimental control; when simultaneous baselines remain stable and change only when interventions are specifically applied at staggered times can one conclude that the observed effects are directly related to the treatment and not some extraneous factor (Kennedy, 2004). Horner et al. (2005), recommend 10 data points to demonstrate a sufficiently stable baseline before introducing an intervention. Staggered interventions for condition two should only be introduced once an effect has been demonstrated in condition one, and similarly for condition three only after demonstrating the effect for condition two.
Figure 2. Multiple Baseline Design. This graph depicts experimental clinical data for a single case experimental, multiple baseline across contexts design, with experimental control demonstrated by staggering the timing of interventions across the three contexts. [appears on p. 18]

SCED can thus provide strong empirical evidence to evaluate treatment efficacy, and should be considered as best current evidence in EBP (Apel & Scudder, 2005; Horner et al., 2005; Robey
2004; Ylvisaker et al., 2002). According to Horner et al. (2005), practice standards are established using SCED when the following criteria are met: the treatment, context, and outcome measures are operationally defined; treatment is implemented with documented fidelity; clear evidence of a functional relation in valued outcomes; and replication across at least five studies by at least three researchers in three different locations, including at least 20 total participants.

Practice-Based Evidence

We have described EBP as an ethical responsibility to provide interventions with demonstrated evidence of efficacy for individual clients in order to improve the quality of speech-language pathology services. Despite best efforts to make the best current evidence accessible to clinicians in the form of practice recommendations, limitations in terms of lack of high quality evidence and challenges to direct translation of the evidence to individualized clinical decisions remain. How then should the clinical scientist proceed while [p. 20] adhering to professional and ethical standards to provide the best possible services and do no harm?

A reasonable and rational complement to EBP is practice-based evidence (PBE). Wambaugh (2007) introduced us to the concept of PBE, which had been previously described by Margison et al. (2000). PBE offers the evidence-based clinician options when EBP does not provide convincing or consistent empirical evidence to support or refute a practice. PBE can provide high quality evidence that is developed in everyday clinical practices and encourages clinical scientists to actively pose questions and collect data to evaluate management decisions (Apel, 1999).

PBE is complementary to EBP, and is not a replacement. When high-quality research evidence that pertains to an individual client is available, relevant, and matches the client’s preferences and values, then that empirical evidence should guide clinical decisions about how to proceed with demonstrated best practices. However, when this high quality evidence is lacking, conflicting, does not relate to the individual client, or does not provide one clear recommendation (i.e., other options have similar levels of evidence), then the clinical scientist must rely on other methods to guide practice decisions. “Lower” levels of evidence provide theoretical and rational support, even though they will not provide empirical support. The clinician must now use his or her clinical expertise to guide decision-making by balancing the client’s values, preferences, and goals with a rational, theoretically-guided, individualized approach (Ylvisaker et al., 2002). Lemoncello and Fanning (2011) suggested a cyclical approach to PBE, which encourages the clinician to develop, implement, and evaluate treatment systematically. This model is cyclical in nature because results of the evaluation will inform the potential need to re-develop the approach.

PBE produces evidence in the field. We propose two methods to systematically evaluate practices using the develop-implement-evaluate model: SCED using the multiple baseline design, and case studies with control data. While the clinical scientist should already be in habit of routinely collecting outcomes data in the form of pre/post case study comparisons, these two designs offer additional experimental control to strengthen the types of data gathered and analyzed in everyday practice. We described the features and design of the MBL design above.
A thorough tutorial on implementation of SCED is beyond the scope of this manuscript, and the reader is referred to Kennedy (2004) for more detail. Here, we will focus on augmenting clinical data with control data.

Case Study with Control Data for Generating PBE

The evidence-based clinician will use clinical expertise to begin with a thorough assessment of the individual client and formulation of functional and important goals, which align with the client’s values and preferences. Chabon, Morris, and Lemoncello (2011) described how a logic model provides a rational framework for linking objectives, goals, and theoretically-based, ethical practice options. To select a treatment plan to reach the established objectives, the clinician will look for the highest quality available evidence. When high-quality evidence to support practices is not available, not related to the individual client’s circumstances, or is controversial, the evidence-based clinician engages in PBE. In this case, the clinician decides to implement a case study with high-quality outcome measures.

Selecting appropriate outcome measures merits additional attention. Olswang and Bain (1994) provided a helpful tutorial to guide clinical data collection. According to their framework, the clinical scientist should consider collecting several layers of data—treatment, generalization, and control—utilizing both quantitative and qualitative methods to support clinical decisions and evaluate clinical outcomes (see Figure 3). Treatment data represent the client’s immediate response to intervention and often take the form of behavioral response tracking (e.g., percentage correct, signs/symptoms observed); such data may be collected continuously or as periodic structured probes. Treatment data represent the client’s performance in the clinic, so generalization data are also required to evaluate the client’s application of learning to a variety of untrained examples or contexts. Generalization data are critical for demonstrating the functional, everyday impact of our interventions. To add to the clinical utility for supporting EBP and PBE, a third layer of data can be added to strengthen any claims that improvements in observed treatment and generalization data are due to the intervention: control data.

Control data likely represent the biggest paradigm shift in data collection for supporting PBE, and we believe are not part of standard practice. Control data represent behavior that is not expected to change with the intervention, but rather “reflect behaviors that could change as a result of other ‘cosmic occurrences,’ but… would not be considered directly tied to treatment effects” (Olswang & Bain, 1994, p. 57). In our experience, selecting appropriate control data presents the greatest barrier. Control data should represent other impaired, observable targets that, based on best current theoretical evidence, should not change as a result of the intervention. For example, when implementing an oral strengthening exercise program for the tongue to improve base of tongue retraction during the swallow with a client status post left hemisphere CVA with dysphagia, dysarthria, and aphasia, one would not reasonably anticipate that verbal naming ability for verbs would improve as a result of such tongue exercising. If verbal expression for verbs is not simultaneously targeted in therapy, then evidence of treatment control can be established when tongue strength increases while verb expression remains at a comparable level. On the other hand, if verb expression is not directly targeted in therapy and observation of improved tongue strength and improved verb expression are both noted, then
perhaps some other factor—spontaneous recovery—may be responsible for the observed changes, and not the intervention itself.

![Figure 3. Clinical Case Study with Control Data. This graph depicts non-randomized, quasi-experimental clinical data for an enhanced case study design, with experimental control demonstrated with the addition of periodic generalization and control probe data.](image)

Careful planning, collection, and analysis of these three levels of data in an ongoing cycle allows the evidence-based clinician to engage in meaningful PBE and contributes to our knowledge and understanding of the mechanisms of therapeutic effects. Each level of data can respond to different clinical questions: [p. 22]

1. Is the client responding to the intervention? (requires treatment data)
2. Is the change functional? (requires generalization data)
3. Is the treatment responsible for the observed change? (requires control data)

By systematically implementing this layered approach to PBE, clinical scientists can be well-placed to share such data with: clients and families to describe the effects (and expectation) of our interventions, third-parties who question the efficacy of SLP interventions, and administrators to advocate for client services. Although not explicitly emphasized as a step in the EBP process by ASHA, the final step in any EBP or PBE process should be to share the findings with others and contribute to our growing knowledge base (Rosenbaum, 2005). Clinical scientists should consider tracking this layered evidence and presenting findings at local in-services or state and national conferences.
Examples of PBE in Gerontology

We have discussed the limitations of EBP and described two methods by which the clinical scientist can engage in PBE. Here we present two detailed examples of how the MBL or case study with control data can be applied to practice for SLPs working with an older adult population. The first will demonstrate application of the controlled case study to dysphagia post CVA, and the second applies a MBL design to cognitive-communication functioning in early-stage dementia.

Example 1: Dysphagia.

The SLP completes a modified barium swallow (MBS) evaluation on a 92 year old woman who survived a right frontal CVA three weeks ago. The patient demonstrates reduced attention and affect with social communication impairments, but no evidence of left neglect or overt verbal memory impairments. The MBS indicated aspiration with inadequate cough during thin liquid swallows, mild and diffuse pharyngeal residue with advanced solids (cleared with double swallow). The clinician recommends small sips of nectar-thick liquids and a ground/moist textured diet with no mixed consistencies to compensate for limited dentition and promote bolus formation, and the strategy to double swallow and alternate solids/liquids to reduce risks for aspiration. These recommendations seem rational based on the results of the MBS. However, the clinician recently attended a workshop on EBP and decides that she does not know if this theoretically-based recommendation has supporting empirical evidence.

The clinical scientist decides to search for research evidence. She begins by formulating a searchable question: Do diet modification and safe swallow strategies eliminate aspiration and promote adequate hydration and nutrition for elderly patients following a right frontal CVA? She decides to search the speechBITE database (www.speechbite.com), which is freely accessible world-wide. She searches for evidence of individual swallowing/feeding intervention for adults with dysphagia due to CVA, which results in eight systematic reviews. Based on the titles, she reviews the abstracts for four of these reviews, and two appear relevant, so she obtains the full-text report of each (Foley, Teasell, Salter, Kruger, & Martino, 2008; Speyer, Baijens, Heihnen, & Zwijnenberg, 2010). Review of each of these systematic reviews reveals methodological concerns and inadequate evidence for drawing clear conclusions about the efficacy of diet modification and swallowing strategies as direct rehabilitation for dysphagia post CVA. Based on this lack of strong evidence of efficacy—without clear evidence-based guidelines—the clinical scientist decided to know the effects of her clinical decisions before engaging in PBE.

To design the clinical case study, the clinician plans for a three-levels approach to data collection. For treatment data, she will collect data on observed signs or symptoms of aspiration during bedside evaluations with meals while systematically reducing cueing as the client learns to independently use safe swallowing strategies. For generalization measures, she will monitor the client’s temperature, white blood cell count, and lung sounds daily by checking medical records and check with nursing and dietary staff about any observed challenges with eating. As control data, she decides to track social communication affect (pitch variation and
facial expression), which she is not directly targeting in therapy at this time, using her own rating scale and based on occupational therapy (OT) report. After three weeks of individual therapy, her data reveal: no signs/symptoms of aspiration during meals with current diet textures and minimal cues needed for strategy use, no evidence of pneumonia developing or reports of challenges when the SLP is not present (client eats meals in supervised dining room), and no change in social communication affect. The clinician concludes that her management choice of diet modification and safe swallow strategies is effective, and now considers re-evaluation.

**Example 2: Cognitive-Communication in Early Dementia.**

The SLP completes a cognitive-communication evaluation on an 84 year old man diagnosed with early dementia and probable Alzheimer’s disease. He is now in subacute rehabilitation due to a recent hip fracture secondary to a fall at home. Results of the evaluation reveal moderate impairments in new declarative learning, which place him at safety risk for learning to use his walker during daily functional activities. The clinician recommends co-treatment (or collaboration, as allowed by the facility or insurance) with the OT to teach the client to effectively use his walker and increase functional safety despite new declarative learning impairments in the context of a progressive disease. This recommendation seems rational, but the physician questions the benefit of cognitive rehabilitation in dementia.

The clinical scientist decides to search for research evidence. He begins by formulating a searchable question: Is cognitive rehabilitation effective to teach functional walker safety (i.e., procedural knowledge) to an adult with early stage dementia? He decides to check the practice recommendations of the ANCDS committee (www.ancds.org), and selects the review of spaced retrieval training (Hopper et al., 2005). He finds that spaced retrieval assists adults with dementia to learn new information such as names and multi-step skills such as using a calendar; he also learns that generalization occurs in meaningful contexts when specifically trained in those contexts. Based on methodological issues and overall limited number of studies, there is insufficient evidence for a practice standard. Based on this lack of strong evidence of efficacy—without clear evidence-based standards—the clinical scientist interested to know the effects of his clinical decisions decides to engage in PBE. He decides to implement a MBL design. He begins with a task analysis of walker procedures, in consultation with the OT, and determines the seven essential steps to teach this client in a variety of functional settings.

To implement the treatment protocol using elements of SCED, the clinician decides to use a MBL across contexts design. He will measure accurate walker use in three contexts: bedroom, therapy gym, and hallways. The across-contexts design enables the clinician to plan for generalization since the training will specifically occur in various settings; he randomly assigns the order of training to minimize bias. He collects baseline data by observing walker use in the three contexts, then begins to systematically teach the patient the seven step procedure using error minimization techniques (Sohlberg & Turkstra, 2011). The patient’s level of accurate walker use improved in the first treatment context (i.e., hallway) but there was no spontaneous change in walker use in the untreated conditions. The effect was then replicated sequentially across the other two contexts, allowing the clinician to demonstrate both treatment efficacy as well as evidence of generalization to other trained contexts. The clinician concludes that his
management choice of procedural training for functional walker safety in specific contexts is effective, and plans to transfer primary rehabilitation to the OT and PT to continue training in additional contexts as needed.

[p. 24] Conclusions

In this article, we have presented information about the benefits and limitations of evidence-based practices for SLPs working with older adults. Given the limitations of EBP, practice-based evidence was presented as a complementary model to generate strong clinical data to evaluate clinical decisions. The clinical scientist should specifically engage in PBE when there is a lack of clear, strong practice standards relating to the individual client, based on a review of the best current evidence. We presented two models for planning and evaluating PBE: the multiple-baseline single-case experimental design and case studies strengthened with control data. By engaging in systematic EBP and PBE, SLPs can continue to provide ethical, rational, theoretical, individualized interventions to promote high quality care and advocate for our services while continuing to add to the growing evidence to support or refute our practices. As Schlosser (2003) reminds us, “the implementation of EBP is a matter of degree… individual clinicians even with less extensive effort can accomplish some degree of EBP” (p. 5). It is our hope that these EBP and PBE strategies will empower clinical scientists to persevere in the quest for best practices.

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[end article]
Continuing Education Questions

1. Which of the following purposes is consistent with Robey’s (2004) Phases I and II of intervention research development?
   a. Determine efficiency by conducting a cost-benefit analysis.
   b. Determine whether an intervention produces an observable effect.
   c. Determine if treatment effects are replicated in large, representative samples.
   d. Determine treatment efficacy using a randomized controlled trial.

2. One example of how practice-based evidence complements evidence-based practice is when:
   a. there is abundant published research supporting an intervention.
   b. the published research is not directly relevant for a particular client.
   c. the published research is deemed high-quality.
   d. there are clear practice guidelines for a particular intervention.

3. Single case experimental designs allow clinical scientists to demonstrate the unique impact of their intervention by:
   a. using visual analysis of data to show repeated changes in behavior over time.
   b. calculating the effect of an intervention using statistical analysis.
   c. randomly assigning participants to either an intervention or control group.
   d. measuring the effect of the intervention using just one context or target behavior.

4. Measuring targeted behavior change outside the treated context is an example of which type of data collection?
   a. Treatment data
   b. Generalization data
   c. Control data
   d. Comparison data

5. What is the minimum number of successful treatment replications required for experimental control when using a multiple baseline design?
   a. One.
   b. Two.
   c. Three.
   d. Four.

*Answer Key:*

1. B
2. B
3. A
4. B
5. C