Approaches to Speech-Language Intervention and the True Believer

Frederic A. Gruber, Ph.D.*
Lamar University
Beaumont, Texas

Scott D. Lowery, Ph.D.
The Transitional Learning Center
Galveston, Texas

Hye-Kyeung Seung, Ph.D.
University of Florida
Gainesville, Florida

Randolph E. Deal, Ph.D.
Oklahoma State University
Stillwater, Oklahoma

Treatment approaches in speech-language therapy are sometimes selected and justified on the basis of a clinician’s experience that “it works.” Moreover, it has been argued (e.g., Kamhi, 1999) that such clinical judgment is, in principle, sound and should be endorsed. Contrary to this view, five extraneous effects are presented as confounding influences that invalidate clinical judgment as the sole basis for adopting a therapy approach. Clinicians should always question whether observed changes in their patients are due to their interventions or to extraneous effects. Possible controls for these effects are double-blind and single subject repeated measures. Research design suggestions are presented, with descriptions of the five extraneous effects.

It has been argued that “because it works” is an appropriate reason to adopt a treatment approach in speech-language pathology and that failure to endorse such clinical judgments indicates a lack of trust of clinicians (e.g., Kamhi, 1999). This article reviews some of the reasons why clinical judgment alone is not a sound basis on which to evaluate therapy approaches. Factors that limit clinical judgment also need to be considered in efficacy research. Well-designed investigations control for these potential threats, but clinicians should also be aware of their existence. The factors affecting the soundness of clinical judgment presently reviewed have nothing to do with faith in such clinical judgment or that clients might achieve designated outcomes. None of the interpretive problems to be presented deny that real speech and language gains occur, nor do they question the fidelity of clini-

*In memory of Dr. Frederic A. Gruber whose inspiration provided the impetus to write this article.
ical observation. At issue is the inference that a specific therapy approach was responsible for the gains observed by the clinician. The issue is important because it is the causative attribution of efficacy that recommends a therapy approach (Hegde, 1994). In fact, Carney (1996) and Carney, Folkins, and Schwartz (1998) edited supplements to The Journal of Speech, Language and Hearing Research that addressed treatment efficacy in the management of several speech, language, and hearing disorders. In these supplements, a group of researchers and clinicians wrote eight technical papers on the existing evidence for treatment efficacy associated with a variety of speech, language, and hearing disorders. These efforts certainly demonstrate an interest by clinicians in reasons why our therapy works and whether the resulting benefits to our patients are worthwhile. The current article attempts to describe effects other than our treatment that may be causing change. What follows are descriptions of five extraneous effects that may be causing change in our patients unrelated to the selected treatment.

**EXTRANEOUS EFFECTS**

There are at least five extraneous effects to a treatment approach that need to be considered in intervention studies that can influence scores in any experimental or intervention study or situation: (a) the placebo effect, (b) the Hawthorne effect, (c) the natural history effect, (d) the experimenter effect, and (e) regression to the mean. The delivery of services to manage speech-language disorders are particularly susceptible to these effects. Any time a patient is given some “treatment,” that person is given attention (the placebo effect and Hawthorne effect), progresses along an expected path for development or disease (the natural history effect), behaves in concert with expectations of the clinician (the experimenter or Pygmalion effect) or simply becomes closer to the normal population (regression to the mean). When clinicians make judgments regarding the efficacy or effectiveness of a treatment regimen, they are presupposing that any benefit the patient receives is secondary to their treatment. Although medicine, psychology, and alternative medicine are now studying and using the beneficial results of extraneous effects (Moerman & Jonas, 2000; Weil, 1997), the field of speech-language pathology has not yet specifically addressed extraneous effects such as the placebo as an intended therapy comparison method. As licensed professionals in an outcome-oriented era, we need to know more than “because it works.” We need to know that our treatment results are because of our treatments and not because of the following.

**The Placebo Effect**

A strong putative influence on outcome measures is the placebo effect. The placebo effect is a difference in an outcome score from what that outcome score would have been had not some form of intervention (pill, treatment, therapy) been engaged. The placebo effect is routinely interpreted as being caused by a participant’s perception that she or he is being given some form of assistance, coupled with a conscious or tacit belief that the assistance will be of benefit, or to prior conditioning with treatment that a patient has experienced (Peck & Coleman, 1991; Turner, Deyo, Looser, von Korff, & Fordyce, 1994). However, the effect may well be more basic, robust, and subtle than this interpretation offers. For example, in drug trials it has been found necessary to use a placebo even when the participants are human infants born at less than 30 weeks gestation (van Wassenaer et al., 1997). In a longitudinal study of vitamins and perceived quality of life, the placebo effect alone produced significant change scores for pain as well as for reported physical and mental well-being (Bouchet, Guillemin, & Briancon, 1996). Side effects, particularly drowsiness, headaches, nervousness, insomnia, nausea, and constipation have also been attributed to the placebo effect (Pogge, 1963). Even the perceptual characteristics of a placebo are associated with differential responses. In a study of medicines, yellow capsules mimicked stimulants and antidepressants, white capsules mimicked analgesics or narcotics (Blackwell, Bloomfield, & Buncher, 1972). The number of capsules given, the size of capsules, and capsules versus injection administration have all been demonstrated to produce differential results in a direction apparently related to dosage expectations (Blackwell et al., 1972; Buckalew & Coffield, 1982).

Placebos are not limited to inert medications. Placebo effects are found with simulated or mock medical instruments, surgical procedures, electrical currents, radiation of various types, manipulations, and a wide variety of therapies (Turner et al., 1994). Goodman, Green, and Laskin (1976) found that 64% of patients with temporomandibular disorders who underwent sham tooth grinding procedures reported total or near total symptom remission, demonstrating a dramatic placebo effect.
A collection of work on placebo effect is found in White, Tursky, and Schwartz (1985). It has been well documented that in animal studies the dependent variable, no matter what the treatment condition (e.g., medicines, behavioral intervention, etc.) will be influenced by the placebo effect (Farina, Ribas, Fernandez-Acenero, & Gascon, 1996; Giraud, Morton, Davis, Paul, & Thornburg, 1993; Klinge, Matsson, Attstrom, Edwardsson, & Sjödin, 1996; Kurzman, Haffa, Kemnitz, & Macewen, 1995; Li et al., 1996; Porras, Meehan, & Cote, 1995; Tomkiewicz et al., 1995; Van de Castelee, Van Roey, Nevens, & Fever, 1996).

Intervention studies that focus on aspects of speech or language (e.g., certain speech-sounds, words, grammatical structures) during therapy would be at particular risk for confusing the placebo effect with treatment results unless adequate controls are in place. A literature search found a few speech and language studies including placebo conditions in the design (Costa & Kroll, 2000; Kahn, 1981; Kessler, Thiel, Karbe, & Heiss, 2000; McNeil et al., 1997; Ramig, Countryman, O'Brien, Hoehn, & Thompson, 1996; Stemple, Lee, D'Amico, & Pickup, 1994). However, the studies that included placebo condition compared interventional effects between pharmacological treatment and placebo condition in language performance such as reading scores (Deberdt, 1994), and the effect of Carbamazepine on stuttering (Harvey et al., 1992), effect of Piracetam on language functions and cerebrovascular flow level of the PET poststroke aphasic patients (Kessler et al., 2000), instead of comparing the intervention effects among language intervention, pharmacological intervention, and placebo condition. Nor was there mention found concerning possible placebo effects in the interpretation of any speech or language outcome study. All speech, language, and swallowing interventions could and probably are confounded by the placebo effect.

It is especially difficult to ethically examine therapy results with a "no treatment" condition. One solution to this is to create groups of subjects who are treated and compared with similar groups receiving a different kind of treatment. Also, replication can control for this effect if consistent therapy results are obtained through replications of a treatment. For example, Gierut (1998) examined the treatment efficacy for children with phonological disorders. The research reviewed was all single subject or small group designs and examined whether treatment works (treatment effectiveness), whether changes were produced in the child's phonological system (treatment effects), and whether one treatment worked better than another (treatment efficiency). Gierut's suggestion for validating treatment efficacy was to continue small n studies on similar groups of children.

Direct replications provide a demonstration that this same treatment is also effective for different children with similar presenting conditions. Systematic replications demonstrate that this same treatment is also effective for different children displaying different phonological characteristics and problems. (p. 889)

The more replications that have been conducted and the more different settings in which these replications have been carried out, the more confidence one has in the findings (Chambless & Hollon, 1998).

In children who are unintelligible, could a phonological approach to therapy group be compared to a phoneme-by-phoneme approach group to compensate for the placebo effect? With dysfluency clients could a treat/no-treat condition be developed? The psychology literature suggests the use of single-case experimental designs in which each participant serves as his or her own control, to control for the placebo and other effects (Chambless & Hollon, 1998). Single subject design can be used to demonstrate clinical efficacy. Generally, single subject design (Connell & Thompson, 1986; Gaynor, Baird, & Nelson-Gray, 1999; Gliner, Morgan, & Harm, 2000; Kearns, 1986; McReynolds & Thompson, 1986; Newman & Smit, 1989; Taylor & Adams, 1982) takes an ABAB (or reversal design) or a multiple baseline design (Jacobs & Thompson, 2000; Light, Binger, Age, & Ramsay, 1999). In the ABAB design, A is the baseline phase and B is the treatment phase. For example, the baseline (e.g., stuttering, delayed swallow, number of phonological processes) is determined during the first A period, improvement during the B or treatment period, reversal or leveling of improvement during the second A period, and resumed improvement (e.g., improved fluency, more rapid swallow, fewer phonological processes) in the second B period. In the ABABC design, a treatment (B) was introduced but the improvement was minimal compared to what was expected based on the baseline measure in A. Then the investigator/clinician can implement another treatment (C). This design is flexible in that any modification of treatment (e.g., ABABC) can be added if the treatment (B) is not effective, then treatment (C) can be added and the treatment results can be plotted. In multiple baseline design, multiple participants, multiple target behaviors within an individual, or a target behavior in multiple settings may be represented.
Some experts (Coelho, DeRuyter, & Stein, 1996) indicate that program evaluation data may be the answer for treatment efficacy. This kind of search for “functional outcomes” may be the long-term solution to examining therapy methods. The authors suggest that case studies offer more individualized and patient/client-oriented accounts of treatment benefit (p. 7). Treatment outcomes certainly justify the efficacy of our treatment. However, they note that these data cannot answer questions about causal relationships between the process (treatment) and the outcome of treatment for large patient/client populations. And, they do not tell us why our treatment may work. Part of the “why” might be the placebo effect.

The Hawthorne Effect

The Hawthorne effect is thought to occur as a psychological consequence of being given extra attention by virtue of being included in a study or a therapy (De Amici, Klersy, Ramajoli, Brustia, & Politi, 2000). A small Hawthorne effect may even be present when people receive questionnaires by mail (Bouchet et al., 1996). The Hawthorne effect and the placebo effect often have the same general impact on scores, although individual responses or performance items may be more sensitive to one or the other effect. For example, the Hawthorne effect and the placebo effect influenced some individual scores in the Short Form Health Survey Questionnaire (SF36) in opposite directions (Bouchet et al., 1996; Stewart, Hays, & Ware, 1988). The placebo effect is usually associated with smaller changes in scores than the Hawthorne effect (Bouchet et al., 1996). The Hawthorne effect can also occur in transcribing, scoring, and evaluating results. The reliability of observers’ recordings has been shown to change when observers become aware their performance is being monitored (Kent, Kanowitz, O’Leary, & Cheikhen, 1977; Reid, 1970; Romanczyk, Kent, Diament, & O’Leary, 1973). Like the placebo effect, any of our interventions could be influenced by the Hawthorne effect. Surveys of laryngectomyes; questionnaires regarding doctoral programs in speech-language pathology; or simply any extra attention given to a respondent, patient, or client may confound the results. Controls similar to those used for the placebo effect should be incorporated.

The Natural History Effect

An accurate account of the natural history of normal development, of a disease, or of a condition permits prediction in new instances and serves as a basis for better understanding the processes involved. For example, knowledge that the natural history of a certain disease results in death provides a strong motivation for intervention. In speech and language disorders, the knowledge claim that the natural course of a particular condition results in later reading, writing, general academic, and social difficulties is an argument for intervention (Aram, Ekelman, & Nation, 1984; Levi, Capozzi, Fabrizi, & Sechi, 1982; Mann, 1984; Silverman, & Paulus, 1989).

Descriptive natural history studies serve as a basis for experimental studies, including intervention research. For experimental research, the natural history of a pathology serves as a control condition. A purpose of efficacy research is to determine the effect(s) an introduced independent therapy variable has on a dependent outcome variable. To accomplish this, the natural course of events, known in biostatistics as the natural history effect, is factored out. This is typically accomplished in experimental designs by creating what is most often termed a “no treatment” control group. The efficacy of treatment is then determined by contrasting the difference between otherwise comparable treatment and no treatment groups.

In studies designed to measure the efficacy of speech therapy, the speech or language difference selected for therapy is often chosen on the basis of the putative normal development sequence (Stool-Gammon & Dunn, 1985). Without controlling for the natural history effect, speech-language therapy studies may confound natural development with assumed therapy effects. That is, gains observed during and after any therapy approach may have been caused by or at least accelerated by maturational, by the myriad of events that occur to a client during the largest part of his or her time outside the therapy situation, and by the interaction of these factors. The gains may be real, but attribution to therapy alone must be suspect. These gains can be examined by using the repeated measure single subject design in which an individual’s behavior is measured frequently over time (McReynolds & Thompson, 1986). Baseline is measured during the A period, improvement during the B therapy period, reversal or leveling during the second A period, and improvement during the second B period. In the field, this phenomenon occurs naturally as children enjoy their summer vacation. Clinicians and teachers bemoan this condition annually when, reportedly, their students lose many of the gains they have made during the school year. For clinicians, here is an example of how one might
examine the effects of natural history during a period when children are not receiving services. For researchers, here is a possible "second A" period of no treatment.

The control strategy for the natural history effect is superficially straightforward. But, in social animals, especially in humans, the term natural is opaque. One might even ask if there exists a no treatment condition in the natural history of communication disorders. Individuals with communication disorders usually experience attempts to "correct" their communication differences from ambient sources. At home, at school, and at play, children with communication difficulties are often given advice, admonishment, encouragement, and sometimes, more organized efforts intended to assist. There may exist no natural condition devoid of intervention, intended or unintended, explicit or implicit. Intervention may be a part of the natural history of many conditions.

The logic suggesting that nonprofessional intervention may be a considerable part of the natural history of a condition does not change when provision for intervention becomes formal, professional, or institutionalized. It is lamented by researchers in communicative disorders that it is not possible to find a no-treatment control because treatment has been mandated by law, by professional, or by ethical concern. Yet, in reality, it has never been possible to find a truly no-treatment control. The pure, unmitigated course for most conditions is an ideal that does not and never did exist.

An appropriate control group for the natural history effect in communicative disorders is the prevailing social, cultural, and institutionalized response to the condition. An experimental, independent variable can be introduced on the hypothesis that it will result in a difference in the change in the dependent measure compared to the prevailing course of events, however complex.

No apologies need be offered. The fact that an idealized natural course of events never existed is no reason to abandon a control group strategy. Between-group comparability may be maintained by repeating selected, key descriptive assessment measures that were originally used in participant selection at regular intervals (Elias & Robbins, 1991). This strategy ensures that groups matched on the basis of natural history factors at the beginning of a study do not drift apart as the study progresses.

Other examples of the confounding influence of natural history in speech-language pathology are spontaneous recovery in aphasia, the effects of maturation on speech and language gains, and the natural course of stuttering. Yairi and Ambrose (1992) examined the natural history of stuttering by noting there is a 65% rate of spontaneous recovery or natural remission rate in untreated preschool children in the first 2 years postonset. However, Conture (1996) noted that "withholding or delaying services for stuttering in preschoolers may be fraught with various ethical and therapeutic concerns" (p. S22). Are gains made in decreasing dysfluency then secondary to our treatment or to a natural history effect?

The Experimenter (Pygmalion) Effect

The experimenter effect is said to occur when participants behave in concert with the tacit expectations of the people managing a study, therapy, or even a class in school (Rosenthal, 1966). For the experimenter effect to occur, it is not necessary that the therapist or researcher (or others assisting) be conscious of either communicating their expectations or even be aware of their expectations. Often clinicians communicate the expectation that their clients will improve, and it is part of the clinical paradigm. However, the experimenter effect is a threat to accurate interpretation of efficacy in a clinical situation. Results gained during clinical management may be due partially to the fact that we expect our therapies to be successful.

In a well-known experiment by Rosenthal and Jakobson (1966), all the children in each of several primary school classrooms were given an IQ test. Then, a randomly selected group of these students were falsely identified to their teachers as having unusually high potential for intellectual growth. Eight months later all of the students were again given IQ tests. Those children identified to teachers as having high potential for intellectual growth showed a significantly larger gain in average IQ scores than did controls. The effect was most pronounced in the first and second grades. Moreover, teachers reported being unaware of any differential treatment they might have given students. Recently, a reanalysis of the Rosenthal and Jakobson (1966) data using more sophisticated analytic techniques suggested their results were not, in fact, significant (Snow, 1995). However, these results do not invalidate the many subsequent studies that authenticate the experimenter effect.

In another example of the experimenter effect, students in an experimental psychology laboratory course were given rats. Some of the cages were labeled maze-dull, others were labeled maze-bright. In their psychology courses, the students were
taught that the psychologist Robert Tryon had bred two different strains of rats that were either efficient or inefficient maze learners. The rats actually came from the same breeding colony and were assigned to students randomly. Rats labeled bright learned to navigate the mazes faster than the rats labeled dull (Rosenthal, 1966).

The work of Rosenthal and colleagues, coupled with subsequent research that corroborated and extended their findings, resulted in acceleration of the use of double-blind methods in psychological and medical research (cf., Behi & Nolan, 1996; Dembar & Jenkins, 1970; Kent, O'Leary, Diament, & Dietz, 1974). It was reasoned that all parties to a study must be uninformed concerning both the study hypotheses and the allocation of participants in order to prevent tacit expectation from influencing results (Sackett, Haynes, Guyatt, & Tugwell, 1991).

In communicative disorder studies, transcribers, individuals who administer and score tests and measures, those who conduct interventions and sham interventions, and even those who schedule appointments would have to be kept from knowing the nature and research purpose for their activities and any facts concerning participants associated with the study in order to establish adequate blinding. For example, transcribers would be kept from knowing which recordings were from control and which were from experimental groups as well as the purpose(s) for the study. Therapists would be kept from knowing which therapies were demonstrated to be sham and which were hypothesized as being effective.

Speech therapy efficacy studies are usually conducted by researchers who believe the therapy they are testing is efficacious (Dean, Howell, Waters, & Reid, 1995). If the design of these studies is not adequately blinded, there is a high risk that the experimenter effect and the treatment effect will be confounded. Suspicion that the researcher or clinician may have measured the experimenter effect would be increased in instances where supposed therapy gains do not generalize to other settings or are not maintained. Certainly, people either not familiar with therapy expectations or not involved with therapy could be employed to administer speech intelligibility assessments, thus controlling for the experimenter effect. Clinicians ask for validation of their results often by asking others to judge the level and quality of improvement in those they treat. Not controlling for this and other effects are well-documented problems in speech pathology (Elliott & Hammer, 1993).

**Regression to the Mean**

Regression to the mean refers to the movement of a measured sample score toward the mean for the population on subsequent measurement. The essence of regression to the mean is that the more extreme the distance from the central tendency of the population a score is found to be, the greater and more likely will be its movement toward the central tendency in any subsequent measurement. The regression effect is a function of how many variables have an influence on the measurement concerned. Two broad groups of influence often cited are those that contribute to measurement error, and those that contribute to the true score. The regression effect is caused because, the more extreme a score, the more likely it is to have been caused to be extreme by an unlikely combination of underlying variables. Such unlikely combinations occur infrequently, so the chances are that in subsequent testing the score will not be so extreme. The score will regress to the mean (Furby, 1973a, 1973b). For example, it is unlikely that a student scoring either 100 or 0 on an examination will get the same score on a retake of the exam (assuming the mean for a large class is 50).

An indefinitely large number of variables underlie speech and language scores. It is therefore likely that speech and language scores will regress to the mean when a measure is repeated. The more extreme the score, the more likely it will regress and the further it is likely to regress.

In speech and language disorders, it is a common practice to select for therapy or study a subgroup of the population who are most delayed in speech or language development. Repeated measures of the performance of this subgroup are then taken to characterize development, determine the effect that intervention may have had, and so forth. This facile attribution may well be in error. The magnitude of the regression to the mean depends, in part, on the difference between the mean of a deliberately selected subgroup and the mean of the population from which the subgroup was chosen (Cook & Campbell, 1979). Mean scores for speech- or language-delayed children can be expected to regress toward the mean of the population in any subsequent measurement, with or without intervention. For example, Tomblin, Zhang, and Buckwalter (1997) in a preliminary report of a well-controlled, stratified epidemiological sample of children (N = 203) screened primarily for language delay found that all of the improvement in language scores they measured from kindergarten to grade two could be accounted for by regression to the mean. Standard
scores on a wide range of language tests for the children with specific language impairment (SLI) \( n = 71 \), when adjusted for regression to the mean, did not change from kindergarten to grade two.

Regression to the mean refers to a real change in behavior. It is not just a statistical artifact. Both clinical observation and objective measures may reflect regression to the mean and, unless adjusted for, may lead to grossly misleading attribution of the cause of an observed change.

**Guidelines for Controls**

A purpose for including elaborate controls is so the researcher/clinician is not led to believe the independent variable was the factor associated with the change in the dependent variable when, in fact, the change was associated with one of the nontherapeutic effects. As Dembar and Jenkins (1970) phrased it, “the independent variable is not always what the experimenter thinks it is” (p. 54). The clinician is no less at risk in the attribution of cause. For this reason, the federal Food and Drug Administration in regulation 21 CFR 312.126 cites five controls that should be applied in adequate and well-controlled clinical studies to eliminate the extraneous effects discussed (cf. http://www.fda.gov/oe/oha/IRB/drugstud.html).

Standards used in other professions may be applicable to speech-language pathology and our interventions. The Therapeutics and Technology Assessment Subcommittee of the American Academy of Neurology (AAN, 1994) defines the quality of evidence they use to assess safety and efficacy as follows:

**Class I:** Evidence provided by one or more well-designed randomized controlled clinical trials

**Class II:** Evidence provided by one or more well-designed randomized clinical studies such as case-control, cohort studies, and so forth

**Class III:** Evidence provided by expert opinion, non-randomized historical controls, or one or more case reports

Holland, Fromm, DeRuyter, and Stein (1996) suggested that these standards are primarily for the study of pharmacological intervention and may be too stringent for behavior research. However, they are accepted as evidence by a segment of the medical community whose stake in post-stroke rehabilitation is as important as that of speech-language pathologists” (p. S28). Holland et al. (1996) also reported that Melodic Intonation Therapy is the first aphasia treatment to be designated by the AAN criteria as being efficacious. Also the Wertz et al. (1986) large aphasia groups study conducted by the Veteran's Administration meets the AAN standard.

In the ASHA Special Interest Division II Newsletter, Robey (2001a) discussed the interaction between clinical practice and clinical research in terms of clinical outcomes and noted that speech-language pathologists are simultaneously confronted with (a) demands for evidence on the impact of clinical decisions, (b) a professional and ethical obligation to appropriately revise those clinical decisions to reflect advances brought about through clinical research, and (c) an emerging need to incorporate practice guidelines in clinical decisions (p. 1).

Relevant to the current discussion is the need to define the difference between treatment effectiveness and treatment efficacy. Treatment effectiveness means that a given therapy procedure will result in a beneficial outcome; treatment efficacy is the test of a procedure in highly controlled conditions. Robey (2001b) notes that speech-language pathology and other behavioral clinical sciences infrequently have effectiveness studies with prior studies supporting efficacy.

The examination of clinical outcomes in both efficacy and effectiveness of practice continues to be primarily in neurogenic speech and language disorders (Afonemos, Appelbaum, & Steele 1999; Elman & Bernstein-Ellis, 1999; Frattali, 1998; Holland et al., 1996; Katz & Wertz, 1997). However, there have been outcome studies using augmentative and alternative communication systems in language intervention. Harris, Doyle, and Haaf (1996) demonstrated positive outcome of a 5-year-old boy with developmental verbal apraxia in a multiple baseline single-subject study. In another study three preschool children with impaired communication were taught Bliss symbols for communication, and positive treatment outcomes were reported (Hetzroni & Belfiore, 2000). In speech-language pathology, clinical outcomes may represent the link between research and practice.

**CONCLUSIONS**

Speech therapists and communicative disorders researchers may be convinced that a therapy is efficacious because it works based on their experiences. However, they should not be surprised if this allegation is met with derision among the broader scientific community. Medical practitioners, audiologists, speech-language pathologists, and some educators share the fact that they are professional artists, scientists, and business people. That there
is sometimes a conflict in the wearing of many hats is apparent in efficacy claims. There should be no surprise when personal, artistic, and business interests in a profession are vested differently than the scientific interests. In this article, we have donned a critical, scientific hat to clarify some issues in efficacy controversies. Without scientific reality testing, speech pathology risks being driven by an esthetic or an economic fiat disguised as preferred practice policies that ignore or stifle truth.

To establish and enhance the credibility of a therapeutic approach, it is necessary to show beyond reasonable criticism that any observed improvement is unambiguously and uniquely related to the intervention or to a component of the intervention and not to anything else. All plausible rival hypotheses must be objectively and demonstrably eliminated to accomplish this end. The researcher will need the support and assistance of practicing clinicians to achieve this goal.

Certainly well-trained, licensed speech-language pathologist should be trusted in their clinical judgments regarding which therapy approaches are best for a specific disorder. The challenge is to discover which approaches are best and why they are successful, so that we can improve service delivery and better establish our credentials for those who doubt.

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Address correspondence to Scott Lowery, Ph.D., The Transitional Learning Center, 1528 Postoffice Street, Galveston, TX 77550 USA.
e-mail: slowery@tlc-galveston.org

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