ABSTRACT: Clinicians are often faced with decisions concerning whether to use a new or different treatment approach. What factors influence these decisions? Are clinicians more influenced by treatment efficacy studies or their own theoretical biases? What role do parents and families play in influencing these decisions? Why are scientists so skeptical of new treatment approaches? When should a clinician try out a new or different treatment approach? Should clinicians be trusted to use the best treatment approaches? These are the central questions addressed in this article.

KEY WORDS: language, intervention, efficacy, clinical
lived so long without ever jogging, riding a stationary bike, or doing the stairmaster. Despite these misgivings, I jogged for 20 minutes a few times a week until a new study came out showing that a total of 20 minutes of activity each day led to the same cardiovascular benefits as 20 consecutive minutes of moderate exercise.

So, what does all this have to do with speech-language therapy? Think about what you did in therapy today. Did you use a proven treatment technique or approach? Can you cite studies that support the efficacy of the approach? What made you choose the approach you used? Are you primarily using the same approach(es) to treatment that you were taught to use in your graduate training programs? What would make you use a treatment approach that is different from the one you are using now? Do you have trouble answering these questions because you don’t use just one treatment approach? Final question, what would you do if you discovered an incredibly effective treatment approach for speech or language disorders?

There has been a lot of interest in our field recently regarding some new treatment approaches (e.g., Fast ForWord®, Earobics®, Auditory Integration Training [AIT]), as well as renewed interest in approaches that have been around for years, such as Lovaas’ behavioral approach for autism (Lovaas, 1987) and sensory integration therapy (Ayres, 1979). How does one decide whether to use one of these treatment approaches? Should we use the same criterion that leads us to try zinc lozenges?

Many parents have heard about Fast ForWord™ through the national media. A typical newspaper article might say something like: “A promising new treatment approach has been developed to improve language abilities in children with developmental language disorders. Children are making language gains in 4–6 weeks that in the past would have taken up to 2 years.” Such a statement, appearing in a national newspaper (USA Today) or a weekly magazine (Time/Newsweek), has sufficient credibility for many parents to want their children with language or learning problems to receive the program. But a statement like this in the popular media is obviously not sufficient for us, the professionals providing speech-language services, to embrace a different treatment approach. What kinds of statements or evidence does it take for a clinician to embrace a different treatment approach? In order to answer this question, we need to consider the factors that influence the selection of treatment approaches.

**FACTORS THAT INFLUENCE THE SELECTION OF TREATMENT APPROACHES**

A few years ago, I conducted a study on the development of clinical expertise (Kamhi, 1994). In this study, I found that clinicians, regardless of experience, rated interpersonal/attitudinal factors as significantly more important than the technical and procedural aspects of therapy. Not reported in this article were the responses to questions that asked clinicians to characterize their approach to treatment and discuss why they used their particular approach. Most clinicians described their approach as “eclectic,” and often said that they used their particular approach “because it worked.” The “because it worked” answer troubled me at first, because I wanted clinicians to have a better justification for why they used a particular therapy approach. For example, I thought a good answer to the question would be to say how their approach to therapy was consistent with their view of language learning. I also thought clinicians might cite empirical evidence supporting the efficacy of their approach. As I thought more about the “because it worked” answer, however, I came to see that it was actually a very appropriate way to justify the use of a particular treatment approach.

The “because it worked” answer is consistent with a small body of well-designed experimental and quasi-experimental research that has found that a critical determinant of teacher attitudes toward change was not prior attitudes or beliefs, as was commonly thought, but whether new practices led to demonstrable gains in student achievement (cf. Gersten & Brungelman, 1996). Attitudes were found to change dramatically when teachers saw changes in their students’ learning abilities. In related research, a major reason teachers continued to use an innovative teaching approach was that it enhanced performance for difficult-to-teach students (Berman & McLaughlin, 1976).

Importantly, in judging the effectiveness of a lesson, teachers relied more on observable student behavior than on quantitative assessment data (Morine-Dershimer, 1978-1979, cited in Gersten & Brungelman, 1996). It may be that clinicians have stronger theoretical beliefs than classroom teachers, but I doubt that this is the case. Even if this were the case, I think that most speech-language pathologists would still be influenced more by observable behavioral changes than by theoretical beliefs. Most clinicians are pragmatists; they have little problem with theoretical inconsistencies. For example, most clinicians still use behaviorist terminology when they talk about “reinforcing” language behaviors or choosing the best reinforcers, even though they would characterize their treatment approach as naturalistic and communication-based.

Clinicians probably do rely more on quantitative assessment data in evaluating the effectiveness of a treatment approach than classroom teachers do. Teachers have much more time to observe student behavior throughout the day and week than clinicians who, if they use a traditional pullout model, may only see students twice a week for 30 minutes or less. I think it is fair to speculate that as speech-language pathologists spend more time in the classroom working with teachers and students, there will be an increase in their use of observable student behavior to evaluate treatment efficacy.

If clinicians believe that they are using a particular approach because it works, then ethical issues must play some role in treatment decisions. If a treatment approach is not working, a clinician will have to try a different approach because it is unethical to continue to use a treatment approach that does not work. In the same vein, if a clinician learns of a new treatment approach that is more effective than the one that is currently being used, a case
THE IMPORTANCE OF EFFICACY STUDIES ON TREATMENT DECISIONS

The “because it worked” answer also forced me to accept the reality that treatment efficacy studies may have a limited impact on the particular approach a clinician uses. I am not alone in recognizing this “reality.” A recent issue of *Topics in Language Disorders* contained a series of articles on how to improve the link between science and practice (Ingram & Wilcox, 1998). Wilcox, Hadley, and Bacon (1998, p. 11) began their article by noting that “meaningful integration of empirical data into typical practice settings is a pervasive interdisciplinary problem.” After a series of citations to support this statement (e.g., Powell, 1994), Wilcox et al. echoed my belief that “empirical validation is rarely a prime consideration in practitioners’ selection of an educational or therapeutic technique” (p. 11).

Why does the substantial literature on communication and language intervention not have more of an impact on the selection of treatment approaches? Clinicians obviously feel that the intervention literature does not translate well into clinical practice. Wilcox et al. (1998) suggested that many validated approaches fail to meet real practice needs. This may be due in part to the quantitative experimental designs that make up the vast majority of treatment research. Many practitioner questions do not translate readily into methodologically sound experimental studies (Wilcox et al., p. 13). In the same issue of *Topics in Language Disorders*, Fey and Johnson (1998) showed how the need to preserve and enhance internal validity leads to intervention studies in which the measures used to demonstrate improvement “often fall far short of the highly general, functional outcomes desired by clinicians” (p. 26).

Clinicians who do wish to use empirical data to aid and support their selection of a treatment approach are confronted with an enormous and conflicting body of literature in which a wide variety of treatment approaches have been shown to be effective in improving speech and language abilities (cf. Geirut, 1998; Leonard, 1998). On the one hand, it is comforting to find that many different treatment approaches are successful in improving language and that no single treatment approach can be ideal for all children and all structures of language that might be taught (Leonard, 1998, pp. 200–201). On the other hand, some clinicians may be discouraged by Leonard’s conclusion about the research that has compared different treatment approaches:

It is fair to conclude that we have not reached a point of knowing which approaches are the most effective for teaching particular target forms. Similarly, it is not yet clear which children benefit most from particular treatment approaches. (Leonard, p. 204)

There is no ambiguity, however, in the research that shows that, although treatment improves language learning in many children with specific language impairment, this treatment “does not carry far enough to lead to normal language functioning. For such children, language problems, though mitigated, will remain as obstacles to social and academic success” (Leonard, 1998, p. 209). Given statements such as these, is it any surprise that concerned parents are continually on the lookout for more effective treatment approaches?

The situation is similar for remediating phonological disorders. Gierut (1998) recently reviewed approximately 300 articles that in some way addressed treatment efficacy for phonological disorders. She concluded that a wide variety of effective treatment methods are available for facilitating change in children’s sound systems; however, she did find that some procedures were more efficient than others.

Even when research demonstrates that one approach is more efficient than another, it is not the case that clinicians will choose to use the more efficient approach. Clinicians are not naive research consumers. They know the limitations of research and also know that efficiency is not always the most important factor in treatment. Clinicians recognize that there may be significant trade-offs in efficiency and other factors. For example, child-directed treatment approaches (e.g., play therapy in which the clinician follows the child’s lead) may not be as effective as clinician-directed approaches in the short-term, but clinicians may choose to use child-directed approaches because they are more conducive to establishing meaningful interactions that underlie functional communication.

In light of these points, it is not surprising that treatment efficacy studies have had little impact on the treatment approaches clinicians use. Research either cannot differentiate between the efficiency of different approaches, or in cases when it does, clinicians might not be willing to use the more efficient approach because the more efficient approach may compromise other factors, such as functional outcomes. As I noted earlier, clinicians are pragmatists; most treatment studies do not tell them what they want to know (cf. Fey & Johnson, 1998). Clinicians are thus more likely to embrace new treatment approaches if they lead to greater or quicker functional outcomes. Researchers, on the other hand, are less likely to embrace new treatment approaches because they must conform to the scientific orthodoxy and be consistent with their theoretical biases.

CHALLENGING THE SCIENTIFIC ORTHODOXY

Anyone who dabbles in the history of science knows that scientists are, by their training, a conservative lot. They are generally resistant to change and are usually unwilling to embrace new ideas or paradigms until the empirical evidence is overwhelmingly in support of them. But the history of science is about how new ideas and paradigms come to replace the conventional wisdom. This history is filled with stories of how the ideas of particular individuals (e.g., Galileo, Copernicus, Newton, Darwin, Einstein, Chomsky) replaced the existing scientific orthodoxy and in some cases, such as with Newton, were
eventually replaced themselves by new ideas (e.g., quantum theory). Goldstein (1990, p. 42) cited the following quote from Bannister (1970) as an example of how scientific orthodoxy might have influenced Columbus.

Had Christopher Columbus possessed the mind of many modern psychologists, I am reasonably certain he would never have discovered America. To begin with, he would never have sailed because there was nothing in the literature to indicate that anything awaited him except the edge of the world. Even if he had sailed, he would have set forth bearing with him the hypothesis that he was travelling to India. On having his hypothesis disconfirmed when America loomed on the horizon he would have discovered the whole experiment null and void and gone back home in disgust.

Researchers trained in the behavioral science tradition are no different from scientists in other fields. Like our fellow scientists, we are very slow and cautious in embracing new ideas about language or new approaches to treatment. When we do not like a new idea, we offer up sophisticated arguments that question the logic and coherence of the new idea. When new treatment approaches are proposed and promoted, we question the theoretical and empirical bases of the approach. Yet, when we wish to support an idea or clinical approach, we criticize those who do not appreciate or accept our logic or the wealth of empirical support we have to support our views (e.g., Rice, 1997; Tallal, 1997).

To practitioners, the continued jostling among researchers may seem tedious. When scientists debate clinical practice issues, there is often no middle ground. Scientists need to convince clinicians that their view is the only one that has both theoretical coherence and empirical evidence to support it. This often leads to the use of emotionally charged rhetoric and exaggerated claims of the dangers of using the wrong treatment approach and the benefits of using the right one. As an example, consider Goldstein's (1990) admonishment to clinicians about the considerable danger in encouraging language intervention research that is consistent with current fads. If, for example, we were to jump on the "naturalistic language intervention" bandwagon, as appealing as it seems, we might stifle future progress by ignoring the history of contributions to clients and to science that have accrued from more didactic teaching methods. (p. 47)

Goldstein’s warning concerning the danger of naturalistic language intervention research that is consistent with current fads, if, for example, we were to jump on the "naturalistic language intervention" bandwagon, as appealing as it seems, we might stifle future progress by ignoring the history of contributions to clients and to science that have accrued from more didactic teaching methods. (p. 47)

Goldstein’s (1990) concern that at least three studies of a treatment program are needed to address fundamental questions concerning treatment efficacy have been done yet. Fey (1990) described two major types of treatment studies in language intervention—those that evaluate the effectiveness of a single procedure or a tightly constrained "procedural complex" and those that evaluate treatment packages. Both of these types of studies have serious weaknesses. Studies that focus on a single procedure or set of procedures do not reflect the programs used by most clinicians, and the effects achieved are rarely broad enough to have a clinically significant impact on a child’s communication. Studies that examine treatment packages suffer from methodological weaknesses such as problems in subject selection and the use of appropriate controls, and when the appropriate controls exist, "it is virtually impossible to know precisely which aspects of the approach were essential to obtain the effect" (Fey, p. 36).

Fey is not optimistic about researchers making the commitment to do the kind of research necessary to address these concerns:

"Realistically, it will be decades before researchers and clinicians converge on results that are replicable and conclusive and that clearly show that certain treatment variables can be combined in various ways to the best effect with different types of language-impaired children. (Fey, 1990, p. 39)

How does a clinician respond to comments like this? If researchers are so critical of the existing treatment literature that presumably justifies the use of current clinical practices, how is one supposed to react to their criticisms of new treatment approaches? Unlike physicians, who have to wait for approval by the Food and Drug Administration before they can prescribe new medications, clinicians do not need the scientific community’s "seal of approval" in order to use a particular treatment approach. Clinicians can use whatever treatment approach they want. They may be limited by other factors (e.g., budget and time), but their concern is behavioral and functional outcomes, not research publications. Researchers, in contrast, often have to conform to the scientific orthodoxy in order to publish empirical studies. Those who go against mainstream views may have a lot of trouble getting published. For example, researchers who perform qualitative, ethnographic studies often have a difficult time publishing their work in prestigious journals such as the Journal of Speech, Language, and Hearing Research. Another example is provided by Goldstein (1990), who described how he was forced to "disguise practicality [i.e., operant procedures] behind a robe of [currently accepted] theory" (p. 43) in order to publish his behaviorally oriented research. Goldstein was
justifiably proud that “such deceit” was successful in leading to the publication of his work on the use of sociodramatic play and social scripts to improve conversational interaction.

Parents can also play an important role in influencing clinical practice and challenging the scientific orthodoxy. Parents want the best outcome for their children and usually have no commitment to one approach over another or one theory over another. The approach that promises the best outcome is the one they want. Researcher, clinician, parent...where is the clinician in this sequence? Caught right in the middle between the research community with its inherent skepticism and commitment to the scientific orthodoxy and parents who want the best outcome for their children. What is a clinician to do?

**TO USE OR NOT TO USE: THAT IS THE QUESTION**

Last year, one of my students told me to read Catherine Maurice’s (1993) book, *Let Me Hear Your Voice: A Family’s Triumph over Autism*. It was one of the more powerful books I’ve ever read. When Maurice’s first child was diagnosed with autism at age 2 by some of the best professionals in New York City, she was told that he would never be normal and that no type of treatment could ever make him normal. Maurice listened to this prognosis, and although she knew it might be correct, she sought out treatment approaches and practitioners that promised a more optimistic outcome. She decided on a combination of Lovaas’ behavioral approach, speech-language therapy, and holding therapy. For a long time, she thought that the holding therapy was making the most difference. She hated the behavioral therapy and came close to stopping it many times. With time, however, she came to acknowledge that it was the combination of the rigid behavioral program and the speech-language therapy that was having the most impact on her child. By the time he entered school, Maurice’s son was essentially normal—so normal, in fact, that when she took him back to be reevaluated by the same physicians and psychologists who made the initial diagnosis, they told her that they obviously had misdiagnosed him. The 5-year-old child they saw now could never have been autistic.

This book had a significant impact on my views of treatment. Although I have never considered myself an expert in autism, in the few classes in which I discuss autism, I clearly conformed to the scientific orthodoxy. I reviewed the current functionally oriented, communication-based therapy approaches and did not even mention Lovaas’ approach because it was “old history” as far as I was concerned. I knew that the 2–4-hour-a-week treatment that many children with autism receive from speech-language pathologists would never make these children normal, but I thought that no treatment could ever make these children normal. Maurice’s book forced me to think that maybe I was wrong. Even if she fabricated the entire story, or overstated the early autistic behaviors and minimized the later language and learning problems, a basic point remains: By believing that children with autism will never be normal (i.e., the “deficit” view), we run the risk of using treatment approaches that support this belief. We also may not be willing to try approaches that promise significantly better outcomes than our current approaches provide because we think that such outcomes are unattainable and unrealistic.

The parallel with developmental language disorders should be apparent. Recall Leonard’s statement (Leonard, 1998) that language intervention, though effective, does not result in normal language functioning. The language problems, “though mitigated, will remain as obstacles to social and academic success” (p. 208). Statements such as these should be troubling for clinicians and even more troubling for parents of children with language problems. When parents hear about treatment approaches that promise better outcomes for their children, it is natural for them to inquire about the availability of these approaches. If the clinician does not think the alternative approach is a viable one, some parents may seek out a clinician who is using the different approach.

Why shouldn’t a clinician use a promising new treatment approach? At least three reasons are usually given. Scientists, as I have shown, often warn against jumping on the latest intervention bandwagon because the new approach has not been empirically validated. But, as I pointed out earlier, empirical validation has little influence on the treatment approaches clinicians use, so it would be inconsistent for clinicians to let empirical validation play a decisive role in determining whether to use a new approach.

Another reason not to use a new treatment approach is that we may raise false hopes and expectations in parents. Parents, however, are not naive consumers; a “caveat emptor” (buyer beware) mentality is an integral part of our society. Most parents know that the benefits reported concerning new treatment approaches in the media are sometimes overstated, and even if they are accurate, their child may not respond the same way to the treatment. Clinicians also are not easily brainwashed. The profession’s experience with facilitated communication is a case in point. Although many clinicians may have initially jumped on the facilitated communication bandwagon, most quickly jumped off when they saw that it did not work. In any event, clinicians routinely caution parents concerning expectations and clinical outcome regardless of the approach they use. The stories we see on television or read in the newspaper regarding inappropriate clinical practices are the exception, not the rule.

The third reason not to use a new treatment approach is that we do not understand what it is about the approach that makes it work. For example, one cannot deny that Fast ForWord® improves children’s performance on standardized measures of language. It is unclear, however, what it is about Fast ForWord® that is causing these language improvements and what has actually improved in the child. Is Fast ForWord® simply improving attention rather than improving children’s temporal processing abilities? Is the intensity of the program primarily responsible for the language gains? Is it the systematic language instruction...
Many questions can be asked about why children improve with this treatment approach, but these same questions can be asked about other treatment approaches as well. Should clinicians wait until these questions are answered before using a particular treatment approach? If physicians had to know why aspirin and other medications worked before prescribing them, many illnesses would have gone untreated. Practitioners must weigh the cost and risks of alternative treatments against the possible benefits. As I’ve noted earlier, however, practitioners are pragmatists. Dramatic behavioral changes with low cost/risk factors will almost always override uncertainty about underlying causal mechanisms.

TRUSTING CLINICIANS

In practitioner-oriented fields such as ours, there is a perceived gap between research and practice, which is exacerbated by a communication gap (Butler, 1998). There have been repeated attempts to reduce this gap in our profession through formal and informal processes. For example, the recent issue of *Topics in Language Disorders* (Ingram & Wilcox, 1998) has a number of excellent articles that discuss ways to improve interactions and communication between researchers and clinicians. A common theme throughout these articles is that researchers and clinicians need to work together, not only to improve clinical practice through research, but also to make researchers more responsive to practitioner needs. This is a goal worth striving for.

The problem is that such a goal may be unattainable because of the fundamental differences between researchers and practitioners: Researchers are bound by theory and scientific orthodoxy, whereas practitioners are driven by behavior change that will often reflect a plurality of theories (cf. Kamhi, 1993). Apel (1999), in the commentary that follows, writes about how he finds it perplexing that members of our profession are often categorized as scientists or clinicians because he operates as a scientist whether he is in the clinic or classroom, or conducting research. Apel focuses on commonalities between researchers and clinicians rather than on differences, as I have done. For example, researchers and clinicians both use a systematic logical process to search for solutions to problems. These commonalities are not insignificant, but the science that researchers practice with its emphasis on reliability and validity (cf. Fey & Johnson, 1998) is very different than the science clinicians practice, which focuses on behavior change and functional outcomes. Underscoring this difference is the fundamental difference between the doctoral degree, which is a research degree, and the master’s degree, which is the terminal degree for practitioners.

An understanding of the fundamental differences between scientists and practitioners seems crucial in order to improve interactions and communication between the two groups. I think that interactions between scientists and practitioners also would be improved if scientists could better communicate the respect I assume most of them have for clinicians’ skills and decision-making abilities. Although most clinicians are not scientists, they also are not techni- cians. Clinicians in our field have been trained and educated to make informed decisions concerning clinical practice. This is why the master’s degree is the entry-level degree in our profession. One of my favorite quotes in the literature makes the point that no assessment protocol is a substitute for an informed clinician:

The most useful and dependable “language assessment device” is an informed clinician who feels compelled to keep up with developments in psycholinguistics, speech pathology, and related fields and who is not slavishly attached to a particular model of language assessment. (Siegel & Broen, 1976, p. 75)

The same is true for intervention. There is no substitute for an informed clinician who is willing to try different treatment approaches and is able to critically evaluate the effectiveness of these approaches for improving various aspects of communication.

In other words, clinicians are well-qualified to evaluate the effectiveness of new treatment approaches. If a new approach proves to be no better than the currently used approach, or its cost and risks outweigh the potential benefits, it will become one more blip in a long history of treatment fads. If, on the other hand, the new approach proves to be better, and the benefits outweigh the cost and risks, it will be added to clinicians’ treatment arsenal. In time, the new approach may come to be viewed as the “traditional” approach, and its theoretical orientation may become the scientific orthodoxy. Thus the endless cycle of change: New and innovative becomes old and traditional only to be challenged again by new and innovative. I have no problem entrusting clinicians with the responsibility to make sure that only the best treatment approaches become the traditional ones.

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Checks and Balances: Keeping the Science in Our Profession

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ABSTRACT: Kamhi (1999) suggests that clinicians may choose to use an intervention approach "because it works" rather than relying on their theory of language learning. This suggestion spawned a number of concerns, including who the scientists in our field are, whether our professional definition of language is used in our language theories and intervention approaches, and what we say the "best" intervention is. In this article, these issues are discussed as discrepancies, along with some suggestions for addressing these gaps, in order to integrate science into all of our professional endeavors.

KEY WORDS: language, intervention, theory, science


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Because of this education, I view myself as a scientist when I am in the clinic or the classroom, providing language intervention or conducting research. Thus, it is perplexing to me when I hear members of our profession categorized as scientists or clinicians (e.g., Goldstein, 1990: Kamhi, 1999). Science is the search for cause and effect based on careful observations and measurements (Hegde, 1994). It involves a specific outlook that focuses one on discovering how the world works, using methods that help produce reliable and valid answers. In essence, science is what scientists do (Hegde).

Given this simple definition, then, speech and language professionals conduct science in many different contexts and for many different reasons (Ingram, 1998). Some scientists may be attempting to understand language better or documenting an intervention approach that is highly efficacious. These are our research scientists. Others may be more interested in applying their knowledge and theory of language to remediating language learning impairments in specific children. These are our clinical scientists (Holland, 1998). Although differences exist between these groups of scientists, they have at least two characteristics in common: They are using a systematic, logical approach to discover solutions to problems, and they are acting on those solutions (Hegde, 1994). Both clinicians and researchers employ science. The different roles these scientists play are admirable, needed, and intimately intertwined (Ingram).

Perhaps even more puzzling to me, given this idea that researchers and clinicians alike are scientists, is the question that Kamhi (1999) recently posed: “When should a clinician try out a new or different approach?” As scientists, this question should be easily answered. We try new approaches when our theory-driven intervention approach needs to be altered based on new and compelling evidence we obtain concerning children’s language development and impairments. Our careful observations and measurements of children’s language in meaningful real-world activities influence our theory as much as our theory influences our practices.

However, this does not seem to be how Kamhi’s question is typically answered. Instead, it seems that we hear less than scientific answers to this question. Research scientists say, “Let the adults argue theory, but in the meantime, if the games work, let the children play them” (Barinaga, 1996, p. 28). Or clinical scientists say, “I use it because it worked” (Kamhi, 1999). These are not scientific answers to the question: They do not represent a process by which a scientist has attempted to discover cause and effect. These are answers that tell us, “Wait until all checks clear and get a printout.”

Why are we hearing or reading these types of answers? I believe the answer is twofold. First, it may be due to a lack of agreement concerning the definition of language itself (Snow, 1996). Second, it may be that, at times, clinical and research scientists assume they are following their definition of language in their work, when in fact their practices do not match their definitions. With either answer, the ramifications of this discrepancy are great.
Why Are We Measuring Language As We Do?

More the norm than the exception, clinical and research scientists measure children’s progress in language intervention either by partially or completely relying on standardized language tests or other scientist-designed measurement tools that examine isolated aspects of language (e.g., Connell & Stone, 1992; Hegde, Noll & Pecora, 1979; Roseberry & Connell, 1991; Tallal et al., 1996). These measures cannot stand up to our professional definition, simply because they do not examine the interaction of all areas of language, or view language within the larger sociocultural, cognitive, and affective contexts in which it operates. Standardized tests do not reflect our real-world definition of language.

Why then are these tests used rather than measures that match our language definition? I believe they are used because of a blind trust in what is considered to be science. Standardized tests seem “scientific.” They provide us with numbers. They allow us to duplicate procedures used when we attempt to evaluate a child for treatment, document progress in intervention, or choose a subject for a research project. These tests allow us to tightly control and eliminate confounding variables in our clinical and research procedures. They seem to assure us that the procedures are replicable and reliable. Yet, we have ample evidence and warnings telling us that many standardized tests are not highly reliable (e.g., Ehren, 1993; Hutchinson, 1996; Lahey, 1988; McCauley & Swisher, 1984; Plante, 1996). However, no matter the degree of reliability, these measures are not valid measures of language because they do not align with our professional definition of language. Reliability and replication cease to be important when one is not measuring the phenomenon of interest. In this case, applying a standardized set of procedures and documenting the outcome to measure language development, as we as a profession define it, is questionable at best.

This is not to say that we do not see or hear about intervention practices that follow our professional definition of language (Lahey, 1988; Norris, 1997; Owens & Robinson, 1997; Silliman, Wilkinson, & Hoffman, 1993). We just do not seem to see or hear these discussions as often as we should or would expect. This may be because professionals and colleagues do not always hold the same definition of language (Snow, 1996). As Goldstein (1990) suggested, it also may be due to the pressures, real or perceived, to meet the requirements of editorial review boards for publishable research that precludes the use of our professional definition of language.

Why Is Our Definition Absent From Our Practices?

All too often, we hear criticisms or concerns regarding intervention that follows our professional definition of language. How many times have you been asked why you are “just playing” with a child as you attempt to facilitate language in a natural or supportive context? Why does it seem that drill is structured and yet more naturalistic activities are “informal?” I would venture to say that others are not seeing the systematic structure of language input and guidance as an adult and child interact in play or in the classroom. It may be that these criticisms arise from a lack of understanding for how our professional definition of language can or should influence theory and approach. It also may be due to incomplete knowledge of language development itself (Apel, Hurn, Deem, & Rainey, 1994). Whatever the reason, it suggests that support for intervention that follows our definition of language needs to be stronger.

Some have suggested that there is little support for scientific reports of language interventions that closely match our professional definition of language (Goldstein, 1990; Kamhi, 1999; Muma, 1998). They claim that attempts to publish accounts of intervention in our professional journals that follow our definition of language will be rejected because these reports lack the perceived stability of standardized measures or the rigorous control of possible confounding variables. I’m not convinced that is true.

Recent reports of intervention practices that seem to match our professional definition of language can be found
(e.g., Dunn, Flax, Sliwinski, & Aram, 1996; Norris, 1997; Owens & Robinson, 1997; Silliman et al., 1993; Wilcox, Kouri, & Caswell, 1991). However accurate this perception is, it nevertheless appears to cause research scientists to either disguise their definition behind another (Goldstein, 1990), or simply not attempt to publish reports of intervention programs that match our definition of language. What often gets submitted for publication, then, are studies that promote statistical reliability over ecological validity (Muma, 1998; Silliman & Wilkinson, 1994).

Must It Be Reliability OR Validity?

Language in real-life contexts is not meant to be replicated. It is meant to be creative, spontaneous, and infinite in possibilities. Intervention programs, and the manner in which we evaluate them, must be consistent with our definition of language. When we attempt to overlay rigorous controls in our intervention endeavors so that other clinical and research scientists can completely replicate our procedures, we undoubtedly are violating our own definition of language. Without that, we have no validity to our testing, making reliability a moot point.

Does this mean that we should now discard reliability for the sake of validity? No. It means that we should strive for a high degree of ecological validity in our language intervention studies, and redefine how we establish reliability. Clinical and research scientists can never completely replicate previous communicative events that have occurred in language intervention and remain consistent with their definition of language. To attempt this simulation does not help us further our knowledge of language intervention, which, of course, is the purpose of science in this case. However, if we attempt to document reliability through the basic intervention principles and goals that are aligned with our professional definition of language, we are more likely to create intervention programs that are ecologically valid and reliable (e.g., Dodge & Mallard, 1992; Norris, 1997; Owens & Robinson, 1997; Silliman et al., 1993; Wilcox et al., 1991). This different perspective depends on an active reflection on how we define language as well as changing how we measure language development.

Having said this, I am not foolhardy enough to suggest that tests or tools that fail to meet our definition of language should not or will not be used. Having worked briefly for a school district, and currently dealing with insurance companies, I know firsthand the need to provide numbers that indicate the severity of a language-learning impairment so that a child is eligible for school services or so parents’ insurance will cover their children’s language intervention. I am not sure at the present time that there is a way to “beat the numbers game.” However, as Paul (1995) stated, “the perceptions of (the child’s) disability by adults in the environment provides a measure of (ecological) validity” (p. 4). This perception can provide some balance to the lack of validity that is inherent in standardized tests. At the same time, it is important to remember that qualifying children with standardized tests does not preclude clinical scientists from providing intervention that corresponds to their definition of language.

In sum, what are the consequences of leaving the definition of language in our textbooks and not bringing it out into our practices? The answer is threefold. First, a lack of attention to the definition of language leads to a breakdown in communication and exchange of ideas in the scientific community. Catherine Snow (1996) recently wrote:

I suggest one basic source of the miscommunication that has led to the current state of affairs is the lack of an agreed-upon definition for the term language. One might think that the notion of language, the most basic concept in the field, would have been defined by now, but precisely because the notion is so central, its definition has been implicit, even clandestine, and quite different for groups working in different places and theoretical traditions. (p. 386)

Snow’s point is well taken. Scientists are meant to communicate and share ideas together (Committee on Science, Engineering, and Public Policy, 1995). This is impeded when differences in definitions exist.

Second, we cannot claim to be scientists if we abandon our professional definition of language to conform to a set of requirements or guidelines that have been established by individuals who do not understand or share the same definition of language. No matter how diligent, we are not conducting science when we study cause and effect using procedures that do not represent our understanding of language. We must consider that science may not always be found in the numbers.

Finally, abandoning our definition of language can lead to an abandonment of theory. Our definition of language is crucial because it influences and guides the development of our theory of language learning. Without defining what is being learned, we cannot have a theory of language learning. And without theory, we lessen our effectiveness as clinical and research scientists (Johnston, 1983). The outcome, eventually, is that we severely restrict our pursuits for treatment efficacy (Silliman & Wilkinson, 1994).

DISCREPANCY THREE: WHAT IS THE ROLE OF THEORY IN INTERVENTION?

Theory is a belief that guides us in our actions and helps us to explain, understand, and predict the outcome of our scientific endeavors (Westby, 1995). In the case of language intervention, theory becomes translated into practices, determining how we intervene. A theory of language learning helps clinical and research scientists hypothesize and then measure the outcome of intervention practices. In other words, it helps them conduct science. Although one’s theory of language learning may change over time as new data change our understanding of language and learning, clinical and research scientists use their current theory to guide them in their “best” practices.

For some time, there have been scholarly discussions on the use of theory to guide the goals and implementation of language intervention (Johnston, 1983; Kamhi, 1993; Muma, 1998; Nelson, 1998). These continuing discussions have focused on the need for theory in language intervention, whether one theory of language
learning is enough, and how the use of a theory helps or hinders language intervention.

Over 15 years ago, Johnston argued for the use of theory in intervention (Johnston, 1983). She suggested that only theory-driven intervention allows one to be a scientist; that is, theory helps one develop procedures on-line to accommodate the child language learner in a way that best facilitates the functional development of language. When armed with a theory for language learning, a scientist can develop creative ways to meet the individual needs of the child. Theory, then, allows a clinical or research scientist to be “astute” when providing intervention services (Holland, 1998).

Kamhi (1993), on the other hand, suggested that one theory of language learning is not enough, that clinical scientists should be equipped with multiple theories. In his concluding remarks, he stated:

Clinicians must acquire knowledge in a multitude of areas, such as classroom management techniques, curricula, IEPs, behavioral management techniques, service delivery models, special education and other remedial services, psychological testing, peer relations, family systems, multicultural differences, and so forth. No theory of language learning could possibly encompass all of the areas that impact on the provision of effective clinical services. For this reason, providing clinical services that are theoretically coherent is not only impractical, but also unrealistic. (p. 59)

Kamhi is correct in asserting that school-based professionals need to understand all of the knowledge domains he listed. However, it is difficult to imagine how one theory of language learning could or, more importantly, should encompass all of these crucial knowledge domains. For some of these areas, it would be entirely impractical. Take, for example, the areas of classroom and behavior management. Language learning cannot occur if a child is running around a room uncontrolled. The child is not in a situation that is conducive to language learning. At these particular moments, a theory of language learning is moot because language cannot be learned, regardless of the theory to which one subscribes. Instead, knowledge of behavior management is required to allow the child to enter a communicative context that facilitates language learning. When there is an obstacle to language learning, it must be removed before language can develop. It would seem counterproductive to include into a theory of language learning knowledge needed for when language cannot be learned.

Likewise, we do not need to include knowledge of individualized education plans (IEPs) into a theory of language learning. Because of recent legislative changes and specific state or local guidelines, clinical scientists are told how to write IEPs. Thus, clinical scientists do not need to acquire a conceptual model for IEPs, but rather, the knowledge for how to write them. However, a theory of language learning will guide the clinical scientist as the application of that knowledge is used. For example, when I worked part-time with a school district, I needed to know about the IEP process. With that knowledge, I was able to write communication goals that were true to my theory of how children learn language. The same can be said for service delivery models. In my particular situation, I was limited in my choice. However, I tried to integrate my theory of language learning into the choices I had, so that language intervention was embedded as much as possible into the social context of the classroom. My theory of language learning directed which service delivery model I chose.

Finally, because our professional definition of language serves as a foundation for a theory of language learning, some of the knowledge areas discussed by Kamhi (1993) can be covered by that theory. Peer relations and family systems, for example, provide the social context in which language is learned and used. A theory of language learning based on our professional definition of language would necessarily account for these knowledge areas. Why, then, is there a call for a plurality of theories for language intervention (Kamhi, 1993; Nelson, 1998)? It may be that some theories of language learning do not align with our definition of language. For example, Kamhi (1993) stated:

In the area of language, for example, we might look at a child playing with his or her mother and consider language abilities (syntax, semantics, pragmatics, phonology, morphology), cognitive abilities, social abilities, play behaviors, cognitive style, sociability, mother’s responsiveness, physical attributes, and so forth. No one theory can direct attention to all of the behaviors or attributes. (p. 59)

Given this statement, it seems that a different definition of language may be underlying the theory of language learning being used. Using a theory of language learning that encompassed our professional definition of language, we would necessarily describe the young child’s content, form, and use skills as she interacted and was affected by the mother’s nonverbal and verbal interactions with her, the mother’s skills at directing her language to the level of the child, and the interrelationship of the child’s play skills and communication attempts. However, Kamhi’s statement suggests that content, form, and use are separate from the contexts in which they are learned. Thus, given this more limited definition of language, it makes sense that one theory of language learning would or could not encompass all the skills Kamhi listed. Different definitions lead to different theories. Of course, it may be that our professional definition of language is being used, but there is a breakdown, or discrepancy, between our definition of language and the integration of that definition into a theory of language learning.

Thus, although one must accept that clinical or research scientists may need much more knowledge than the knowledge of language to allow language intervention to occur in particular settings, this does not require multiple theories. A theory of language learning that has as its foundation our professional definition of language, coupled with factual knowledge of areas that are specific to a particular clinical work setting (e.g., knowledge of state or state or local guidelines, clinical scientists are told how to write IEPs. Thus, clinical scientists do not need to acquire a conceptual model for IEPs, but rather, the knowledge for how to write them. However, a theory of language learning will guide the clinical scientist as the application of that knowledge is used. For example, when I worked part-time with a school district, I needed to know about the IEP process. With that knowledge, I was able to write communication goals that were true to my theory of how children learn language. The same can be said for service delivery models. In my particular situation, I was limited in my choice. However, I tried to integrate my theory of language learning into the choices I had, so that language intervention was embedded as much as possible into the social context of the classroom. My theory of language learning directed which service delivery model I chose.

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insurance codes for eligibility of services, understanding of rules for documenting services) should be sufficient to guide the clinical or research scientist’s intervention.

There is another equally important reason for discussing theory-based language intervention, other than to emphasize the link between our language definition and theory of language learning. As Westby (1995) pointed out, treatment based on theory allows clinical and research scientists to infer what will happen when they intervene with children from culturally or linguistically different (CLD) populations. Knowledge of how culture interacts with language and context is consistent with our professional definition of language and is a critical component of any theory of language learning. As stated earlier, a clinical or research scientist cannot be truly creative or flexible in intervention without theory (Holland, 1998; Johnston, 1983). Thus, without flexibility and creativity, one cannot individualize intervention for any child, including children from CLD populations.

If clinical or research scientists operate on an atheoretical level in intervention, they also run the risk of becoming technicians (Aram, 1991). Without theory, clinical and research scientists will lack the decision and problem-solving skills required to address individual differences in learning among children. For example, a student may have difficulties understanding and using figurative language in social and academic settings. This occurs even though he easily produces figurative expressions when presented with pictures from a popular intervention kit and is asked to imitate such expressions. Without a theory of language learning based on our professional definition of language, a clinician might not realize that the student’s lack of progress in acquiring this type of language is due to the lack of contextual support needed for the child to understand the reason for using that language (e.g., understanding how peers might use certain expressions as a means for establishing a bond among themselves) as well as its appropriate use in different social and academic contexts (e.g., testing or observing how certain expressions work with peers but not with adults). In this case, the danger is the continued use of a procedure that does not provide the child with the data needed to learn.

Interestingly, there are arguments that theory may not, or should not, always be considered when choosing or implementing language intervention approaches as well as statements against using any theory in intervention (Goldstein, 1990; Kamhi, 1999). Kamhi suggested that clinical scientists are influenced more by observable behavior changes than by theoretical beliefs when choosing intervention approaches, justifying their use of certain intervention approaches with the statement “because it works.” Goldstein (1990) claimed that the use of a theory of language learning in intervention actually stifles creativity. This latter claim is counter to suggestions made by other theorists in language learning (Johnston, 1983; Lahey, 1988). However, I am not convinced that these particular arguments are meant to be contrary to suggestions for theory-based intervention. Rather, I believe that statements like these may be due to a difference in understanding of what an approach is and what an intervention product or tool is. This discrepancy can lead to confusion.

**DISCREPANCY FOUR: ARE APPROACH AND PRODUCT DIFFERENT?**

There appears to be some discrepancy on what qualifies as an approach and what is a product or tool for language intervention. Kamhi (1999) asked whether clinical scientists should use new approaches, using two recent products on the market, Fast ForWord® and Earobics®, as examples of new approaches. I would argue that these products are potential clinical tools, not approaches. Tools are simply devices used to perform an activity. In contrast, an approach is the process or set of procedures that are the manifestation of a clinical or research scientist’s definition of language and theory of language learning.

For example, one scientist may take a behaviorist approach to intervention; another may take an interactional approach (Fey, 1986). The tools they use can be identical (Johnston, 1983). A case in point: Both types of scientists may use a set of dolls for language intervention. The behaviorist might pick up a doll (the tool), begin making it walk, and say to a child, “Say ‘lady walking’” (the approach). Conversely, the interactionist might play with the child and the dolls, follow the child’s lead in play and conversation, and provide exemplars of the new language rule or concept to be learned as a way to scaffold the child’s spontaneous utterances. Same tools or products, different approaches.

The same can be said for some, but not all, computer programs. A behaviorist might use a computer program that posts pictures of “r-words” and have the child repeat the word after the clinician’s production. The interactionist might use the same program and play a guessing game, during which the child gets to take turns describing or labeling the pictures that appear on a monitor that is obstructed from the clinician’s view. Computer programs. Toys. These are just tools by themselves. What really matters is the clinical or research scientist’s theory of language learning. It is theory that dictates how the tools are used or whether they can be used within a specific theory-based approach.

It is interesting that Kamhi focused part of his discussion on the merits of choosing new intervention approaches, or tools, by selecting Earobics® and Fast ForWord® as examples. I am sure this was no accident. As he reports, it is difficult not to be exposed to media discussions of these programs. We can hardly pick up our professional periodicals or log on to our professional listserv without hearing discussions of these computer programs. Oddly, we rarely read discussions or controversial point/counterpoint essays regarding the latest playdoh color or a new and improved dollhouse. But this might be exactly the point. Intervention tools, in this case toys, that allow clinical or research scientists to be flexible and creative, and provide language intervention that is consistent with their knowledge and theory of language, do not warrant any special attention, other than perhaps to let you know they exist. However, other tools, such as computer programs, may have some cautionary aspects.

First, they may be based on a theory to which one does not subscribe. Second, it may not be possible to make
For me, this is the case with Fast ForWord®. This product requires a clinical scientist to provide treatment that is inextricably tied to a specific theory of language learning, one to which I do not subscribe. Because I am not able to modify the product to meet my definition of language and theory of language learning, and because I have not yet seen compelling evidence for modifying my theory of language learning, I cannot use this tool and be a clinical scientist. I must choose other products in intervention.

So, should clinical scientists go ahead and try new approaches, or tools, as Kamhi (1999) suggested, even if the new tool requires a shift in their definition of language, theory of language learning, and intervention approach? No, especially when they have no evidence that changing their theory is necessary. To do so might be detrimental to them, the populations they serve, and our profession. First, those who choose to use a new intervention product or tool because it is easier, faster, or more glamorous are in danger of losing their theoretical compass, and thus their clinical creativity and scientific mind. It puts them at risk for becoming a technician, not a scientist (Aram, 1991). Second, it may be a disservice to the populations they serve. When intervention is provided without a theoretical foundation, clinical scientists can never be sure why certain aspects may work with a program and others do not. Thus, needless time, money, and energy may be spent. Finally, I believe it damages our profession because it suggests that we no longer care about the pursuit of science.

Once we acknowledge that an approach is a theory-driven process or set of procedures and that products are just tools we can use, then the new tools that appear on the market are scrutinized in a different manner. Clinical and research scientists make judgments to include new products into their intervention practices based on their knowledge and theory of language. This causes a shift from whether new products make intervention better to whether a certain approach is better.

**DISCREPANCY FIVE: DO WE KNOW WHAT “BEST INTERVENTION” MEANS?**

Even though this question is frequently asked, it leads to discrepancies in the answer because of differences in opinions of what “best” means. For example, some may suggest that best means most efficient. Silliman and Wilkinson (1994) defined efficiency as whether an approach works to meet a particular scientist’s goal, with possibly little connection to theory. But the goals of scientists can differ. For example, it may be that one scientist values a particular intervention procedure because it quickly leads to an increase in scores on a standardized test. Another scientist might consider an approach worthy when its implementation leads to the use of new language skills in functional, everyday situations, albeit at a somewhat slower pace than the other intervention. Discrepancies in defining “best intervention” arise when the goals of some scientists do not match the goals of other scientists.

Leonard (1998) reported on numerous intervention studies that were effective in improving language skills, summarizing that many studies achieved the research scientist’s goal of advancing aspects of language development regardless of the approach used. Examining many of the articles Leonard reviewed (e.g., Connell & Stone, 1992; Hegde, et al., 1979; Leonard et al., 1982; McGregor & Leonard, 1989; Roseberry & Connell, 1991; Swisher & Snow, 1994; Weismer, Murray-Branch, & Miller 1993), most of the goals in the intervention studies focused on improving a specific aspect of language, most often the morphosyntactic aspect of language, ignoring the interaction among systems, such as content and use. In addition, the majority of these studies did not use the real-life contexts that typically influence language development. In other words, intervention was not embedded into the natural sociocultural contexts of children’s daily lives. Interestingly, when Leonard (1998) reviewed studies that compared two or more approaches, he found that “the most successful approaches were those that encouraged production and provided multiple yet naturalistic cues for desired responses” (p. 203). Leonard’s finding suggests a reason to use approaches that are closely matched to our professional definition of language.

However, others report that the most efficient treatment approaches are those with a decidedly behaviorist bent (Goldstein, 1990; Kamhi, 1999). Why are most intervention studies behaviorist in nature? Perhaps Johnston (1983) explained it best when she asserted that, once clinical scientists adopt a theory of learning, they will not likely change their theory. I believe this is accurate. Many clinical and research scientists in this field were taught, and in many cases continue to be taught, to use behaviorist methods, such as control over the environment and the child’s responses, reinforcement schedules, and prompts for imitation (Fey, 1986), when intervening with children with language-learning impairments. Even after a major shift away from behaviorism occurred in the 1970s, and the foundation for current definitions of language were being established (Bloom & Lahey, 1978), it seems that many clinical and research scientists cling to a behavioral view of learning for language intervention. But such a view of language and learning seems incompatible with our professional definition of language and the data that have been collected over the last 20 years. We simply cannot acknowledge that children’s language is dually affected by external influences (social and cultural domains) as well as internal influences (cognitive and affective domains) and still maintain a behaviorist’s approach. Thus, another discrepancy in our practices seems unchecked.

So, when we consider efficiency of intervention, we must always evaluate whether cheaper, faster, or easier is
best (Johnston, 1983), and determine whether other scientists’ goals for efficiency match our goal for children’s language development. Similarly, we must determine whether research scientists’ theory of language learning encompasses our professional definition of language. There is a good chance that conflicts will occur. However, best does not need to be defined as most efficient. Best also can be defined as most efficacious.

Silliman and Wilkinson (1994) suggested that intervention efficacy studies enable clinical and research scientists to know how and why an intervention approach works. Efficacy studies attempt to examine the social context in which language learning takes place, what the child learns, and how the child applies that new knowledge. These studies validate an approach by determining that rule learning has occurred as evidenced by the child functionally applying that new language in multiple contexts. In this way, what is measured is not the scientist’s particular goal, but the impact the new language has on the child’s life (Silliman & Wilkinson). There have been some language intervention efficacy studies (Silliman et al., 1993; Wilcox et al., 1991), but they are greatly outnumbered by efficiency studies.

The question of the “best intervention” needs to be answered by both research and clinical scientists alike. As this question is pursued, scientists must match their definition of language and theory of language learning to what they do. Without this match, they cannot begin to determine the efficacy of their intervention. Ultimately, this entails reconciling the discrepancies we seem to have in our efforts to serve children with language-learning impairments.

SUGGESTED RECONCILIATIONS

Clinical and research scientists must take ownership of the discrepancies outlined in this article and be willing to work toward reconciling them. Some discrepancies can be reconciled by either clinical scientists or research scientists; others can be accomplished together.

Suggestions for Clinical Scientists

Clinical scientists need to be astute professionals who are perceptive observers and flexible problem-solvers (Holland, 1998). They need to acknowledge that they are scientists looking for causes and effects, and not simply relying on the idea that “it works.” Part of being a scientist involves using theory to guide intervention because theory is what brings science to practice. Without a theory, clinicians will not be able to explain why a certain cause had a specific effect. If clinicians are accustomed to saying, “I use it because it works,” then it is a good time to start looking at why “it” works. If clinical scientists are choosing new tools, then it is a good time to ask, “How can this product be used with my theory and knowledge of language?”

Clinical scientists also need to be open to new measures of language, including context-based assessment and intervention methods (e.g., assessing students’ communication skills in the classroom). Professionals in the schools already judge these to be the best method for clinical services (Beck & Dennis, 1997; Elksnin & Capilouto, 1994). Implementation is the next logical step. As scientists, clinicians need to ask probing questions of their research colleagues, and be willing to keep actively engaged in continuing their own education, using the myriad ways that are available to them (Holland, 1998). They need to know language and then apply that knowledge to their clinical practice.

Suggestions for Research Scientists

Research scientists must attempt to understand the job of the clinical scientist (Holland, 1998) and recognize that clinicians are, or should be, scientists. If research scientists conduct studies on intervention efficacy, they must not lose sight of the phenomenon of which they speak. Those engaged in efficacy studies need to develop reliable yet valid language measures and intervention approaches for language development and ensure that those measures and approaches can be used by clinical scientists. Theory-driven approaches that are constructed in such a way that they prohibit practical application are clinically useless (Stone, 1996). In addition, if findings from intervention studies cannot be used in daily intervention settings because they fail to meet our definition of language, then studies need to be changed. Researchers cannot sacrifice validity for reliability.

Common Goals

As a larger group, clinical and research scientists can work collaboratively toward reconciling some of these discrepancies as well. First, they must reach consensus on a definition of language. Without a shared definition of language, they will end up on opposite ends of the pole as they attempt to apply and integrate a definition of language into their theory of language learning and clinical and research practices (Snow, 1996). A theory of language learning that does not match an accepted definition of language cannot successfully describe how language is learned. In addition, without a shared definition of language, reports of intervention progress and innovative intervention approaches will be stymied by the documentation of decidedly different phenomena.

Second, both groups should be committed to following a theory of language learning. By using a theory of language learning to plan, implement, and evaluate language intervention, clinical and research scientists can answer the crucial question of why a certain approach does or does not work.

Third, the two groups can agree to separate but equal roles and responsibilities in making these reconciliations work (Fey & Johnson, 1998). Both groups have a common goal: helping children with language-learning impairments. Scientific breakthroughs and advances in understanding occur only when scientists, in this case clinical and research scientists, work together (Committee on Science, Engineering, and Public Policy, 1995). To do this, both
group of scientists must become more accessible to each other and let go of preconceived notions of ivory towers and workers in the trenches (Fey & Johnson, 1998). Together, these two groups should promote the publication of intervention efficacy studies that differ from past attempts at documenting the efficiency of intervention approaches.

Finally, because clinical and research scientists often take on the roles of professionals who educate new clinical and research scientists, they will need to encourage science in all of the roles that professionals-in-preparation are learning. They need to do this, not only by teaching this philosophy, but also by modeling it to the best of their abilities (Hodson, 1998). With their students’ first entry into this profession, clinical and research scientists alike need to acknowledge the importance of clinical work and the symbiotic relationship of clinical and research scientists. That philosophy should set our newest scientists on solid ground for checking that these discrepancies are reconciled, and that an equal balance is obtained between clinical practice and research.

CODA

As I wrote this article, I was well aware of the optimistic, idealistic frame of mind in which I was writing. This gave me a momentary pause for concern. Yet, I decided that I still have a good amount of time left in this profession. I’m in it for the long haul. So I might as well encourage changes in the way we practice. I am well aware of the time that may be involved in moving this along. Goldstein (1990) mentioned 8 years ago about the temptation to jump on the “naturalistic language intervention bandwagon.” However, that “bandwagon” has been around for over 45 years (Backus & Beasley, 1951). I’ve been on it for at least 15 years, and feel like it is not moving very fast. Maybe the problem is that we are jumping on it and sitting there instead of pushing it along. Whatever the reason, I’m ready for it to move along. I am hopeful that, in the coming years, it will pick up some momentum, at least faster than it will take me to learn to balance my checkbook.

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