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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

I am not good at balancing my checkbook. When I try, I am faced with one discrepancy after another. No matter how hard I strive, I never quite make the process work out the way it should. Now, I could try to take a methodical, one might even say scientific, approach to solving these discrepancies. I could follow the accepted procedural definition of check management and actually record checks as I write them. I might take the time to notice when I believe that I have $12.95 left in my account and the bank says that I have $10.95. I could even painstakingly cross-reference each check with my monthly statement, being true to what most typical

check-users identify as an almost theoretically grounded approach to balancing a checkbook. In short, I could try to reconcile my checkbook with my monthly statements. But that's not easy. And that's not the way I'm used to doing it. So, instead, I rely on my old way of dealing with the issue, the approach that is familiar to me. I stop writing checks for a while, then go to the bank, get a printout, make sure my checks have all come in, and then take the new balance off the printout and start fresh again. It's not the most scientific or efficacious way (I sometimes bounce checks), but it works!

I wonder whether it is not just as easy for us as professionals to overlook discrepancies we encounter in our clinical and research endeavors. For some, these discrepancies may seem small or unimportant. They may be viewed as easily managed in a way that still gets us to our desired endpoint. But it may be that the discrepancies add up to greater issues that truly need to be reconciled. Kamhi's (1999) question, and ultimate answer, concerning when clinicians should use new approaches made me think about some discrepancies I see within the science of our practices, and how these might add up to some larger issues. For example, I am perplexed as to why we continue to refer to members in our profession as scientists or clinicians, why a shared definition of language does not seem to infiltrate our theories and approaches to language intervention, how confusion sometimes occurs on what is an approach and what is a product, and what we choose to call "best intervention." This article reflects on what I see as five specific discrepancies that remain unchecked in our clinical and research practices, and offers some suggestions for reconciling them. My goal here is to encourage incorporating a definition of language and a theory of language learning into our professional practices, with the payoff being a healthy return on all of our scientific pursuits.

DISCREPANCY ONE: WHO ARE THE SCIENTISTS?

I was taught to be a child language scientist. With my first steps into this field, I was challenged by my mentors to approach my clinical and research experiences with a scientific mind. A scientific mind meant evaluating the issue at hand and searching for an explanation. It also meant applying a method, based on a specific definition of language and a theory of language learning, to address the issue and then examining the effects of that method. In essence, it meant following the scientific method in all of my professional efforts. In this way, science was applied to my clinical work: I identified a child with a language-learning impairment, provided intervention grounded in language knowledge and theory, and examined the outcome, making theory-based alterations when needed. I learned to conduct research in a similar fashion: identify a question, apply a certain procedure to investigate the question, and evaluate the outcome, using my definition of language and theory of language learning within each step of the process. I learned to "do" science.

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.

DISCREPANCY TWO: DO WE USE A SHARED DEFINITION OF LANGUAGE?

Although I believe that the answer to this question should be affirmative, I'm not convinced that is the case. At first glance, we as a profession seem to agree on our definition of language. Thumbing through our child language textbooks (e.g., Bernstein & Tiegerman-Farber, 1997; Nelson, 1998; Owens, 1996; Paul, 1995), similar definitions of language appear, most of which are strikingly similar to the definition proposed by the American Speech-Language-Hearing Association (ASHA). ASHA defines language as a dynamic system embedded in and influenced by the biological, social, cultural, cognitive, and affective domains and contexts we face daily (ASHA Committee on Language, 1983). Language is rule-governed, and involves the integrated use of all areas of language, including phonology, morphology, syntax, semantics, and pragmatics. Proficient use of language requires a comprehensive knowledge of human interaction and the communicative demands of the environment. Language is expressed in a number of modes, including oral, text, and signed modes. Taken as a whole, this definition represents our professional definition of language, and one that will be used throughout this article. Given our professional definition, then, we might assume that the tools and procedures used by clinical and research scientists for language intervention match our professional definition, that we integrate our definition into our work.

Typically, this has not been the case. When we examine the tools and materials used by clinical and research scientists to identify, assess, and treat children with language-learning impairments, a different story, or in this case, a different definition, emerges. Our professional definition of language seems to stay in our textbooks, and does not move into our scientific practice of intervention.

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.[1]

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

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[1] A discussion of different theories of language learning is beyond the scope of this paper. However, the point here is that a theory of language learning should naturally follow or include one's definition of language, which does necessarily restrict the list of theories to which one could subscribe. Not all theories are equivalent in their ability to explain the phenomenon of language development and the process of language intervention, simply because they are not loyal to our professional definition of language. See Nelson (1998) and Fey (1986) for discussions of different theories of language learning.

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

modifications to the computer software. The tool, rather than the clinician, prescribes the intervention approach. In this case, clinical scientists may reject the use of that tool because it constrains their ability to be creative and flexible vis-á-vis their theory. 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).[2] 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

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[2] This finding seems to be an artifact of an overabundance of behaviorist approaches used in intervention studies and a lack of other theoretically driven approaches that have been examined (Goldstein, 1990).

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

groups 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 ways 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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