Advice to Students of Stuttering Treatment

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ABSTRACT: This article proffers advice to students of stuttering treatment, with appropriate caveats attached. Students are advised to stay away from continua in clinical practice. It may well be the case that there is a continuum from normally developing speech in preschool children to the disorder of stuttering in preschool children. It may also well be the case that it can be difficult to decide whether a certain speech event is a normal disfluency or a stutter. However, students are advised not to be concerned about those issues because they impact on researchers only and do not affect day-to-day clinical practice. Students are also advised to be careful about their choice of treatments, and not to do any therapy that is based on a theory of stuttering, because that theory is likely to be wrong, and therefore the treatment is likely to be wrong. Instead of theories, students are advised to look to clinical trials evidence to establish “best practice” for their clients. Finally, students of stuttering treatment are advised to take the challenge of being accountable. This means being specific about treatment goals, using speech measures to determine outcomes, and benchmarking clinical performance. The benefits of accountability in clinical practice are argued. After closing discussion, some questions are posed for reflection.

KEY WORDS: stuttering, treatment, child, adult, diagnosis, accountability

The title of this article fairly well states its purpose—to provide some advice on the treatment of stuttering to students of speech-language pathology. In the strictest sense, I refer to a student as someone who is in a course of formal study at a university to qualify as a speech-language pathologist. Such students are the primary focus for my comments. However, they are intended for a wider audience also—anyone, in fact, who seeks to learn about the treatment of stuttering. That could be an experienced clinician who has never treated stuttering and has found a new professional situation where such treatments need to be delivered. Such a person may have presented stuttering treatment decades earlier, and now wishes to return to the pursuit at a time when the scene has changed considerably. However, I am assuming that much of my readership will be quite junior in years in this field, in the midst of professional preparation, or having just completed it. Consequently, I have assumed only a modest knowledge about stuttering.

My advice, naturally, is biased toward my own views. So, please be aware that what you will read here is not a balanced view, but an extremely personal one. As such, it does not take account of all viewpoints that exist. Indeed, there are many that exist that are far discrepant from mine. If you are not already aware of the historically controversial nature of stuttering treatment, then you need to be now. The passions that have been engendered by the controversies about stuttering therapy have been extreme at times, and I will touch on that matter later. So, the rule is, caveat emptor—let the consumer beware. Hopefully, as you prepare to treat your first stuttering clients, you will seek advice from many, and you will consider all these sources of advice before forming any views. So, welcome to stuttering treatment circa 2004 and my advice on your arrival.

STAY AWAY FROM CONTINUA

What Are Continua?

My friend and colleague, Gerald Siegel, who is now retired after a distinguished career in stuttering research—the
majority of it at the University of Minnesota—once invited me to do the following: Invite 20 people outside as dusk approaches and ask them all to note independently the exact time that night arrives. If you do it, you will almost certainly get 20 different answers. And at any time during dusk, if you ask them whether it is night or day, you will get a mixture of “night” and “day” answers. And that illustrates perfectly what a continuum is. Night is obviously night, and day is obviously day, and for the most part, we are under no misapprehension concerning which is which. But, there is a fuzzy region where one becomes the other, when we may be unsure of our judgment.

And so it is with stuttering. There are recurring mentions of continua that you will find in the literature. Probably, every academic in the course of your training will have mentioned them, and quite rightly so. Yet, of all the issues that confuse student clinicians about the treatment of stuttering, it is issues that arise from continua that bend them out of shape more than any other I know of. And, here is the rub: This is all unnecessary. So, my first advice to a student clinician is to stay away from continua. In what follows in this section, I will outline the issues and then explain why they need not trouble you.

When Does Night Begin? The Continuum From Normal to Abnormal Speech

Some researchers have argued that normal speech and stuttered speech are at opposite ends of a continuum. In other words, stuttering is obviously stuttering, as is night, and normal speech is obviously normal speech, as is day. But, there is a region where the two overlap, as do night and day. This is sometimes referred to as the continuity hypothesis, and you can read about it if you wish in Bloodstein (1995). Bloodstein explained it by saying that there are two types of disorder. The first is a categorical disorder, such as having a fractured bone or conjunctivitis, and people cannot be considered to be healthy if they have such things. But the other is a matter of degree, such as anxiety or hearing loss. In Bloodstein’s words, these conditions “merge by fine degrees with the normal” (p. 404). In other words, everyone can expect to have some anxiety or hearing loss, and it is only a disorder if you have a certain amount that crosses a certain threshold into the region of a disorder.

If we apply this concept to stuttering, and believe that everyone stutters to some extent, the clinical implications of this are quite dramatic for diagnosing early stuttering. This is because of the quite unusual circumstances in which stuttering begins in children. I think that there are no other speech or language disorders in children that occur unexpectedly, with the exception of those that are the consequence of neurological trauma. If a child is to have phonological or language problems, in a sense, they develop—or rather fail to develop—with the child. The presenting problem is the continued absence of normal development, and that is what parents complain about when they come to the speech clinic. With stuttering, however, the reverse happens. Stuttering appears without warning, after a period of perfectly normal language development. 1

So this is where the notion of a continuum comes into clinical practice with children who stutter. If it is held that stuttering and normal childhood speech are on opposite ends of a continuum, and if a child begins to stutter some time in early childhood, then it must also be held that the child may pass through a period when an observer will be uncertain about whether the child is stuttering or not. This notion has led to many statements in the literature that it is difficult to distinguish the early signs of stuttering from normal speech development in the first years of life.

Another way this is said in the literature is that it can be difficult to distinguish between stuttering and the “normal disfluency” of early childhood.

Wendell Johnson, who we will meet later in a different context, made an important contribution to the field by beginning the development of a set of terms to describe the so-called “normal disfluencies” of early childhood and the nonnormal “dysfluencies” of early stuttering (the prefix “dys” is different from the prefix “dis” because it means “abnormal”). As everyone knows, speech in preschoolers, during its normal developmental course, contains hesitations, starts and restarts, and repetitions. Johnson’s terms described these “normal disfluencies,” along with the speech events that are definitely nonnormal. This is not the place to go into detail about these so-called data languages of early stuttering and normal disfluency. For the interested student, their history, strengths, and limitations have been described in a number of places (Onslow, Gardner, Stuckings, Bryant, & Knight, 1992; Packman & Onslow, 1998; Wingate, 1988). However, the point of the present discussion is that they have found their way into clinical practice in the diagnosis of early stuttering, to aid in the clinical task of distinguishing between stuttering and normal disfluency. In other words, to assist clinicians in the task of distinguishing stuttering from normal disfluency when a child’s speech is located in the “dusk” region of speech where it is not clear whether it is day or night—whether the child is stuttering or not.

There have been many diagnostic aids developed to assist clinicians in this clinical task. Gordon and Luper (1992a, 1992b) overviewed them comprehensively. Probably the most significant development in the area since that time is the system that was developed by Yairi and colleagues over several years (see Ambrose & Yairi, 1999). This system

1This unexpected appearance of stuttering is commonly distressing for parents, particularly in cases when the disorder appears suddenly, as it can. Alternatively, according to the reports of parents, the onset of stuttering can be quite a slow process, occurring over weeks or months. Some useful reports by Yairi and colleagues (Yairi, 1983; Yairi & Ambrose, 1992) provide breakdowns of the proportions of stuttering children who have sudden, not-so-sudden, and quite slow onset of stuttering. It is surprising, incidentally, how often parents will report that the onset of stuttering occurs suddenly, in the face of excellent speech and language development. Some insights into how distressing this can be for parents can be found in the case report by Wyatt (1969). In her words, “because of Nana’s excellent language development, her stuttering came as a complete surprise. Her speech disturbance aroused in me strong feelings of anxiety and helplessness” (p. 42). Wyatt’s case history is also of interest, incidentally, because it details the developmental course of stuttering in a child.
counts the number of “stuttering-like disfluencies” (SLDs) in a child’s speech. These SLDs are speech behaviors that are described with terms such as “syllable repetitions,” “monosyllabic word repetitions,” “dysrhythmic phonations,” and “tense pauses.” This system has appeared in many publications by Yairi and colleagues; however, the most direct clinical application of SLDs to the task of diagnosing early stuttering is seen in a report by Ambrose and Yairi (1999), where they developed an arithmetic algorithm for use by clinicians in making a diagnosis. In essence, the clinician records and transcribes a speech sample and counts the number of SLDs. Inevitably when you do something like this, there is some overlap. So, Ambrose and Yairi recommend a simple formula to weight certain disfluencies highly. For example, “dysrhythmic phonations” are given quite a strong weight. This is probably a good idea, because “dysrhythmic phonations” are generally thought to be rather “classic” stuttering behaviors. To give you some idea of what they are, another way that they can be described is as a fixed posture of the articulatory mechanism with audible airflow (Packman & Onslow, 1998). Ambrose and Yairi reported that when their algorithm was applied, the SLD system was able to distinguish between stuttering children and nonstuttering children. Nonstuttering children attained a “weighted SLD” of less than 3.0, and stuttering children attained a “weighted SLD” of more than 3.0. Another way of saying this is that all children have “stuttering moments,” and the SLD system helps the clinician decide what number of them in a child’s speech should be considered abnormal.

Is It Day or Night? The Continuum From a Moment of Stuttering to a Normal Disfluency

The notion of a continuum has another significant application to clinical practice. This is the notion of a continuum of individual speech events. Many researchers hold the view that individual speech events lie on a continuum, from those that are perfectly normal to those that are obviously abnormal. An example of a perfectly normal disfluency might be “um,” provided it is said in a certain way that all normally speaking people do to help them gather their thoughts. The fixed posture of the articulatory mechanism with audible airflow referred to above, provided it is said in a certain way, might be a description of a speech event that is obviously stuttering. If you understand speech acoustics at this stage of your career, then you might be interested in a spectrogram of a speech perturbation that not only was a perfectly obvious stutter in perceptual terms, but also showed a great many aberrations in acoustic terms (Onslow, 1995).

There is a concept of a moment of stuttering that is integral to this discussion. We need to mention Wendell Johnson again. You may know that the person who is attributed with founding our profession, in the first half of the last century, is Lee Edward Travis (for an overview, see Johnson, 1955). Wendell Johnson was one of his students, and arguably, his ideas have been more influential than anyone else’s in this field. As well as the idea of a data language for stuttering and normal disfluency, to which I referred earlier, Johnson also had the idea of a moment of stuttering. In a clinical sense, this was a wonderful idea because it provided a way to measure the severity of the disorder in terms of the rate at which moments of stuttering occur. Without question, judging by its popularity in the current literature, the measure of “percentage of syllables stuttered” is an excellent one. For example, if someone speaks 1,000 syllables, and 100 of them are moments of stuttering, then the person is said to be stuttering at a rate of 10% of syllables stuttered (written as 10 %.SS).

Clinically convenient as this notion of a moment of stuttering is, it comes, nonetheless, at a cost. For as I stated earlier, there are those who believe that speech events lie on a continuum. So, assuming that they are correct—as they probably are—how can an observer be sure that a moment of stuttering really is a moment of stuttering and not a normal speech event? Normal speakers, as well as those who stutter, repeat words. So if you observe a repeated word from someone who has the disorder of stuttering, how do you know where it lies on the continuum from a moment of normal speech to a moment of stuttering? I can assure you that, in your future career, those who stutter will utter many speech perturbations that will confuse you. They will be “dusk” disfluencies if you like—neither obviously night nor day, but in that region where you just do not know. And if we have 20 observers listen to such a speech event, just like we had 20 people judge whether night had arrived, we will get a mix of answers about whether it is a stutter or a normal disfluency.

The importance of the issue can become clear to you by putting it in a clinical context. As I shall point out later, removing or reducing moments of stuttering is an important part of clinical practice. Another way of saying this is that stutter-free speech is a clinical target. That being the case, much of clinical practice will involve people who stutter (PWS) being given tasks where they attempt to not stutter or to reduce their stuttering. In other words, their speech should have no moments of stuttering, or a greatly reduced number of moments of stuttering. That being the case, part of being a clinician who treats stuttering is making a judgement about whether or not the client has succeeded in meeting the therapy target. Say for argument’s sake that, under your direction, a client is attempting to speak 1,000 syllables with fewer than three moments of stuttering. As the client speaks, you are counting stutters online, as we say. The client is up to 737 syllables and has done two moments of stuttering. Then the client says “I, I went to the shop.” You are confused. Was that a moment of stuttering or was it not? You are tape recording the client’s speech, and you replay the utterance, and you are still not sure.

*The most convenient way of measuring %SS in all likely clinical contexts, which include talking with a child on the clinic floor, is with one of the many commercially available, small button-press counting devices. With this type of device, one button is pushed for every syllable spoken without a stutter, and another button is pushed for every syllable that was stuttered. The machine automatically calculates %SS.*
Difficulty because of this continuum issue, in separating children who stutter. This is where researchers have children, and that the control group does not contain any mean something. Simply, the researcher has to be sure that stutter. It might be stating the obvious, but this first step is children who stutter and a group of children who do not. It might seem to be a bit difficult to understand how you could stay away from continua in clinical practice, when they seem to be all pervasive in the core business of diagnosing and treating stuttering. But no, this is not the case. They are certainly pervasive to the core business of being a researcher, but they are not pervasive to the core business of being a clinician. These continuum issues emerged as troubling ones for researchers, not clinicians.

To take them on as problems as a clinician is to participate in a problem needlessly.

**You Do Not Need To Participate in This Problem**

So, to recap, there are concerns, that may be justifiable, about how to know whether a child’s speech is developing normally or whether the child is stuttering. Further, there are concerns, that may be justifiable, that on occasions it may be problematic to distinguish between a moment of stuttering and a moment of normal disfluency. I know that these issues trouble students of this disorder, at least with reference to stuttering in preschoolers, because I am questioned about them by students probably more than about any other issue. So, what is my answer? My answer is that they are real problems indeed, but they are not clinical problems. To restate my opening assertion for this section: If you are a clinician, stay away from continua.

It might seem to be a bit difficult to understand how you could stay away from continua in clinical practice, when they seem to be all pervasive in the core business of diagnosing and treating stuttering. But no, this is not the case. They are certainly pervasive to the core business of being a researcher, but they are not pervasive to the core business of being a clinician. These continuum issues emerged as troubling ones for researchers, not clinicians.

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**Distinguishing Stuttering Children From Normally Developing Children in Clinical Practice**

Let me explain a little. In conducting research into stuttering, it is critical to the success of that research to select subjects. As an example, consider the currently topical issue of the language skills of children shortly after the onset of stuttering. If a researcher wants to demonstrate that, say, the language skills of a group of preschool children who stutter are different from the language skills of a control group of children, then, naturally enough, the first step for the researcher is to establish a group of children who stutter and a group of children who do not stutter. It might be stating the obvious, but this first step is critical to do properly so that the results of the research mean something. Simply, the researcher has to be sure that the group who stutters does not contain any nonstuttering children, and that the control group does not contain any children who stutter. This is where researchers have difficulty because of this continuum issue, in separating night from day. There is simply no generally agreed-on way to be absolutely certain that the groups are discrete. Another way of saying this is that researchers are confronted with the problem of needing to define stuttering in small children, when there is in fact no agreed-on way of doing that. (In fact, there is no agreed-on way of defining stuttering in any age group). The efforts of Ambrose and Yairi (1999), to which I referred earlier, although useful, did not entirely solve this problem (see Onslow & Packman, 2001).

By and large, researchers get around their problem quite well in a number of ways—some of them better than others. However, the point here is that if you are a clinician, you do not have these inherent problems in distinguishing between stuttering and normal disfluency. Why not? My colleagues and myself have discussed this matter in detail elsewhere (Onslow, Packman, & Gordon, 2003), but to cut a long story short, parents generally do not bring normally speaking children into the clinic thinking that they are stuttering! They normally bring them into the clinic some time after onset when it is as plain as night or day that they are stuttering. Curlee (1999) expressed this most succinctly as follows: “In more than 25 years of clinical practice, I have evaluated only a handful of preschoolers suspected of stuttering whose fluency was unequivocally normal” (p. 14). I would certainly endorse that comment based on my own clinical experiences, and also on common sense. Parents just seem to know when their children’s speech is not right, and they all seem to know what stuttering is. After all, it is not a difficult disorder to recognize, especially in light of the point made earlier that it is not insidious, as failure to develop normal speech and language are. It happens quite unexpectedly, often after a period of perfectly normal speech development. It is also possible to substantiate this assertion empirically. Onslow, Packman, and Gordon reported that in one clinic, the Stuttering Unit in Bankstown Health Service in Sydney, over the course of more than 1,000 assessments, only 0.9% children were identified to be not stuttering. And on those few occasions when a child comes to the clinic really in the “dusk” of stuttering when you are not sure, then it is no problem. You can just wait until it becomes obvious that the child is either stuttering or not. I certainly cannot recall during my own clinical career, or in my long career associations with professional clinicians at the Stuttering Unit and other specialist stuttering clinics, any episode of prolonged indecision about whether or not a child was stuttering. It just does not happen. Think about it. It just cannot be the case that speech-language pathologists generally have a problem recognizing normal speech. If that were the case, speech therapy could not possibly work.

I realize that all this is easy for me to say with more than 20 years of clinical experience and research in stuttering behind me. It may not be possible for a student to be particularly confident about this matter, particularly if the student has not encountered many cases of early stuttering in preschool children. However, should this be the case, then the antidote is obvious: Spend some time in a preschool kindergarten observing and talking to normally developing preschool children. Then you will rapidly
develop a feel for normal speech in that age group, and will be able to join parents in readily recognizing any departure from that norm.

Distinguishing a Stutter From a Normal Disfluency in Clinical Practice

Now, to the issue of determining whether a certain speech event was a stutter or a normal disfluency. Again, problematic as this may be, the problem is nonetheless the domain of the researcher. For, as far as I can tell, they are the only personnel involved with this disorder who need to be troubled by it. For example, consider another topic of research interest—whether the speech of those who stutter is acoustically any different from the speech of those who do not stutter. Now, if you consult the Onslow (1995) reference I cited earlier, you will see that this matter could be confounded, as we say when we are doing research. A confound in research is something that makes it difficult to interpret its results. And the presence of stuttering would be a confound in the acoustical research under question. You can see in Onslow that the presence of stuttering introduces various acoustic anomalies—unusual durations of acoustic segments, nonnormal articulatory noise, and abnormal looking waveforms, to name a few. So, with research into the acoustic differences between those who stutter and those who do not, the researcher needs to be sure that speech from the stuttering group that is acoustically analyzed contains no stuttering moments. But, exactly how this should be done poses a problem because there is no known way of objectively determining whether a given speech event is a stutter or a normal disfluency. Again, by and large, researchers handle this problem quite well (again, in some ways that are better than others).

But a clinician has no such problems. Why? Because the realm of the clinician is unambiguous moments of stuttering. In other words, stuttering that to the trained observer—you—is obviously stuttering and not a normal disfluency. Trust me on this point. If someone warrants a diagnosis of stuttering, he or she will generally have many moments of such unambiguous stuttering. There will be some exceptions in the case of adults who stutter and who are rarely heard doing so. But for the greater part of clinical practice, it is just not a problem. Think about it. The problems of stuttering arise because the affected person’s speech contains observable aberrations. That is what causes all of the problems. If stuttering moments were not readily perceptible in those who stutter, the disorder would not have been known to affect humankind, as it has, since the ancients. I am confident in assuring you that if you stay with the notion of unambiguous moments of stuttering, you will have no problem with this issue in clinical practice with adults.

Perhaps the question I am asked most of all by student clinicians and experienced clinicians who are learning about treating early stuttering is how you know with preschoolers what is a moment of stuttering and what is a normal disfluency. Indeed, I can understand the concern if you choose to attempt with preschoolers—as you should, in my view—to do a treatment that focuses on identifying stuttering and getting rid of it. In the context of treatment of early stuttering, you can read a more detailed account of this issue in Onslow, Packman, and Harrison (2003). However, the short version of it is that, in clinical practices with preschool children who stutter, you should focus on unambiguous stuttering. Any child who warrants a diagnosis of stuttering will have many such speech events each day. That is why parents bring them to speech clinics complaining about stuttering. If a child does what you might think is a suspicious-looking normal disfluency that might well have been a stutter, do not worry about it. Let it go. It does not matter whether it was a stutter or not.

Wait instead for a moment of unambiguous stuttering to occur. With small children, there will never be any treatment that will not work if you fail to detect some stutters.

BE CAREFUL HOW YOU CHOOSE YOUR TREATMENTS

Where Do Your Treatments Come From?

During your career, you will always be faced with the challenge of treatment choice for your stuttering clients because the field is changing all the time. That is why you will benefit from the insistence by the American Speech-Language-Hearing Association (ASHA) that its clinicians meet certain continuing education requirements after graduation. Variations on existing treatments will arrive, examples of which are the Craig et al. (1996) report of a parent-based version of a popular treatment for children, and the O’Brian, Onslow, Cream, and Packman (2003) treatment development that was published just before the writing of this article. And there certainly will be new treatments appearing during your career. Examples of new treatments to have appeared recently in clinical trials are the electromyography procedure reported by Craig, Hancock, Tran, and Craig (2003) and the self-modelling procedure reported by Bray and Kehle (1996, 1998, 2001). To that list, we may add the currently popular in-the-ear or behind-the-ear devices (e.g., http://www.speecheasy.com/home.shtml).

So, where do your treatments come from? There are a great many of them. Glancing up from my desk, I can easily count 20 commercially packaged treatment programs for stuttering on my bookshelf. They continually arrive from commercial publishers for me to review in the hope that I will recommend them for my students. Most of them do not even make it to my bookshelf these days. A scan of the Internet adds a range of mechanical devices to that plethora. If the answer to my question above is that the treatments that you give to your clients are simply treatments that exist, then you have a problem. Which of the many will you choose? And having chosen one of the many for one client, will you choose the same treatment with the

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1If you do not know already, it is folklore with this disorder that the first clinician to have attempted to cure stuttering may have been Demosthenes, who treated the orator Satyrus around 400 BC (Van Riper, 1973).
next client, or will you choose another? And how will you choose? Will you randomly cycle through the many treatments available for your clients until you find one that you like? And how will you determine that you “like” a treatment? In the remainder of this section, I will provide some concrete advice about these issues. I begin with some advice about where not to get treatments.

Do Not Do any Therapy That Is Based on a Theory of Stuttering (at Least Not Just Now)

I foreshadowed the possibly controversial nature of my topic, and at first blush, this might be the first manifestation of it in my advice to a student. Indeed, theories of what causes and perpetuates the condition are not only a complex topic, but a controversial one. However, my fundamental contention here is not at all controversial, and I doubt that many researchers and theoreticians in the area would challenge it. A bit later in this section, we will wade into controversial waters. But for now, simply, any theory of stuttering that is held at present is likely to be wrong. And as such, any therapy based on such a theory is also going to be wrong. That seems to be a logical enough sequence to present to inexperienced clinicians. I will also argue that, because theories are generally around for the long term, drawing on them for treatment practices fosters a static clinical community rather than a dynamic one that changes for the better in a timely manner.

Before continuing, perhaps I should back up a bit and overview the theories of stuttering that are currently prominent, and provide a reference source for interested students. There are many such reference sources. However, in my view, the finest and most up-to-date text on this topic is by my colleagues Packman and Attanasio (in press). I wish to cut very short here what is a really long story. Packman and Attanasio categorize theories into five kinds. Do not be confused in the following by the use of the various terms “theory,” “model,” and “hypothesis.” In a strict sense, they all have different meanings, but in the context of theories about what causes stuttering, they are used interchangeably by writers.

Theories of speech motor control. The first type of theory categorized by Packman and Attanasio (in press) deals with “speech motor control.” The term “motor” refers to movement, and hence these theories deal with how speech movements are controlled. As you know, such control is neural, arising in the brain. Interestingly, Travis, whom I mentioned above, made popular an “interhemispheric interference” theory that has lasted in various guises through the decades to the present time (for an overview, see Packman & Attanasio, in press). The idea behind this theory is simply that one hemisphere interacts with the other maladaptively during speech production to cause stuttering.

Theories of systems control modelling. Packman and Attanasio (in press) also refer to “systems control modelling” theories, which are slightly different from theories of speech motor control because they suggest that stuttering is a problem of some kind in the self-regulation of speech motor activity. A useful way of presenting this concept is with reference to a thermostat that keeps a room at a certain temperature. The machine regulates itself; if the room becomes hotter, it senses this and adjusts the room temperature to become colder. Speech, although infinitely more complicated than that, is a self-controlling mechanism. We need feedback about our speech in order to speak properly. One type of feedback, for example, is auditory, and the effects of having this feedback disturbed are obvious when you listen to the speech of someone who has a hearing impairment.

Theories of cognitive and linguistic processing. A third type of theory referred to by Packman and Attanasio (in press) is that involving “cognitive and linguistic processing.” The descriptive term really says it all about what these theories say about the nature of stuttering. Probably the most popular of these theories is the covert repair hypothesis, about which you may have heard. This somewhat ingenious theory states that stuttering is caused when the speaker has a faulty phonetic plan. To summarize in simple terms what is, of course, an extremely complex system, when the speaker knows what is to be said, the speaker organizes that utterance phonemically just before executing the motor command to say it. The covert repair hypothesis says that those who stutter make errors in assembling this phonemic plan for an utterance, and what we can observe as stuttering is the attempt to correct the problem. To many, this is an intuitive prospect, that stuttering is an attempt to repair, online, some fundamental error in speech production.

Theories of anticipatory struggle. Along with Wendell Johnson, one of the most influential figures in thinking about stuttering in the past few decades is Oliver Bloodstein, who I cited earlier. His landmark text, A Handbook on Stuttering (1995), is in its fifth edition. He has vigorously championed an “anticipatory struggle” theory about stuttering. This theory states that PWS do so because they have come to believe that speech is a problematic and troublesome process for them, and that belief causes their speech to disintegrate. As the name says, PWS anticipate difficulty with speech and consequently struggle with it through life. They get this problem during early childhood, when the environment in which they find themselves is a troublesome and problematic one for speech and communication.

Theories of stuttering as a multifactorial disorder. The final class of theories referred to by Packman and Attanasio (in press) is “multifactorial” theories. These theories say that stuttering is caused and maintained by numerous things, in numerous combinations, and not always in the same combinations in different cases of stuttering. In a sense, because they specify the possibility of multiple causes, they can be said to be dynamic. In fact, one is specifically called the dynamic, multifactorial model (Smith & Kelly, 1997). However, dynamic as they may be, they are certainly not specific. In a clinical sense, the most influential of the multifactorial theories would be the so-called demands and capacities model. For that reason, I would guess that it is likely that you know more about this model than any other that I have mentioned so far. The
demands and capacities model states that children begin to stutter, and continue to do so, because the demands for fluent speech—that is, stutter-free speech—exceed the capacity of the child to produce fluent speech. The model states that the “demands” for fluency arise mostly from the environment in which the child lives, including time pressures, parents’ demands for language and cognitive performance, excitement, and the like. In the model, a child’s capacities for fluency include motor, language, and cognitive development.

The basic textbook for the procedure is Starkweather, Gottwald, and Halfond (1990). The demands and capacities model appears to have been clinically influential for some time, with reports of it being published from the late 1980s to recently. I say it “appears” to have been clinically influential for some time because, although it is written about a lot by academics in universities, I am aware of no evidence that it is in widespread clinical practice in everyday speech clinics. If you have not already, you might wish to look at an entire edition of the Journal of Fluency Disorders that was devoted to this model (Volume 25, Issue 4). The fact that it was given such attention is a sign of its popularity—at least among those in universities. The demands and capacities model is the subject of a video by The Stuttering Foundation of America (2001), which may also interest you if you have not seen it. My colleagues and myself, and others, have discussed this model at length (Ingham & Cordes 1998; Packman & Attanasio, in press; Packman & Onslow, in press), and this is not the place for a detailed critique. However, because of the model’s considerable contemporary clinical implications, some further explication of it is warranted in the present context.

The fundamental purpose of the demands and capacities model appears to be to guide a treatment where the environmental demands for fluency responsible for the stuttering are identified by the clinician and rectified. A publication by the Stuttering Foundation of America (2003) summarized the kind of family changes that would normally occur with this style of treatment:

Speak with your child in an unhurried way, pausing frequently ...reduce the number of questions you ask your child...use your facial expressions and other body language to convey to your child that you are listening to the content of her message and not to how she’s talking...set aside a few minutes at a regular time each day when you can give your undivided attention to your child...help all members of the family learn to take turns talking and listening...observe the way you interact with your child. Try to increase those times that give your child the message that you are listening to her and she has plenty of time to talk. Try to decrease criticisms, rapid speech patterns, interruptions, and questions. Above all, convey that you accept your child as he is. (pp. 3–5)

These treatment concepts have been reiterated in various clinical texts over the years (e.g., Conture, 1990; Guitar, 1998).

The Limitations of Theories as Sources of Treatments

So, to the point. These theories, and others that have come and gone in the history of thought about this disorder, are just that—theories. They are not facts. They are notional attempts to understand this disorder, which is really quite a puzzling one. They are attempts to understand such puzzling things as why children begin to stutter unexpectedly, often after a period of perfectly normal speech development, and why stuttering disappears under the influence of rhythm. As such, they are all vulnerable to being wrong, and most of them probably will eventually be shown to be wrong. Their state of acute vulnerability can be demonstrated with a cursory look at what Packman and Attansio (in press) described as their explanatory power. All theories must explain what is known about stuttering—the explicanda, as Packman and Attansio put it. That is the raison d’etre of theories of stuttering. If they cannot account for the explicanda, they are probably wrong.

As an illustration of the typical frailty of theories, consider some of the unusual explicanda of stuttering. For example, it is now virtually certain that PWS do something quite similar while playing wind instruments (Meltzer, 1992; Packman & Onslow, 1999; Silverman & Bohlman, 1988). For my money, this makes it difficult to believe that stuttering is a problem with cognitive and linguistic processing, or a cognitive problem of belief in the difficulty of speech, as suggested by the anticipatory struggle hypothesis (Bloodstein, 1995). How on earth could such problems cause “stuttering” while playing musical instruments? Another example of the frailty of contemporary theories is that in recent years, it has become apparent that stuttering can be treated, in effect, by telling children that you and their parents want them not to stutter. According to the demands and capacities model (Starkweather, Gottwald, & Halfond, 1990), this should make stuttering worse because it increases demands for fluency, but in fact, it is starting to look like a really effective treatment for the disorder (see Onslow, 2003). So, this information seems to seriously weaken the demands and capacities model (Packman & Onslow, in press).

I should also add that theories are not only liable to be wrong, or likely to be wrong, but that they are vulnerable to the emergence of a simple, replicated research finding that brutally renders them all useless. If this happens, then there is just no need for a theory of the cause of stuttering, because the cause of stuttering would be known. Elsewhere (Onslow, 2003), I have related a narrative about how we might be close to just such a theory-meltdown scenario. For, recently, there have been two independent replications of neuroanatomical—that is, brain structure—problems in the areas that are important to speech and language (Foundas, Bollich, Corey, Hurley, & Heilman, 2001; Sommer, Koch, Paulus, Weiller, & Buchel, 2002). You can read a summary of the potential importance of these findings in Packman and Onslow (2002). I am not saying that this is a fact yet, only that two independent groups of researchers have reported these findings. If a few more do, then we will have to accept that it is the case, and that we have a simple and parsimonious explanation for the onset of stuttering in children: They have a structural brain problem in the areas that are important for the development of speech and language. That would account for all the explicanda.
So, to summarize, we are considering where to get your treatments when you start to have stuttering clients, and there are many theories of stuttering that may serve as a basis for the formulation of treatments. There certainly have been many such examples in the history of this field, apart from the demands and capacities treatment model. Some examples of theory-based treatments have been quite dramatic. Ingham (1984), for example, provided a fascinating account of the therapy based on Bryngle’s hemispheric interference theory, which involved the strapping up of arms and legs of clients. There have been even more dramatic examples, such as the 19th-century surgeon Dieffenbach, who had a theory that stuttering was caused by problems with muscles in the tongue, and who consequently attempted to fix the problem surgically by cutting a wedge from the tongue (Van Riper, 1973).

At first blush, it might seem logical that theories are a suitable means to go about developing treatments. In fact, there is literature brimming with vigorous debates on this topic, and I do not see the need to cover that material here. But all that I think needs to be said is that if the theory is questionable, then so is the treatment. And considering that I have argued that all current theories are questionable—if only because they are theories—then any treatment derived from them must also be questionable.

Of course, the notion of a “questionable” treatment to arise from a theory primarily is concerned with whether the treatment might be expected to work. However, the history of stuttering treatment contains an episode that shows other problems associated with the building of treatments from theories. You may have heard of an outdated theory called the “diagnosogenic theory” (Johnson & Associates, 1942). This was put forward by Wendell Johnson and, in my view, was one of his least successful contributions to the field. This theory suggested that the cause of stuttering was when parents thought that their children’s normal disfluencies were stuttering and pressured them to stop doing them. In other words, the cause of stuttering was its misdiagnosis. For much of the last century, Johnson had the entire Western World convinced that his theory was right. We know today that it was wrong. However, for much of the last century, clinicians and parents believed that if a child was thought to start stuttering, then the last thing to do was to directly draw attention to the problem. In effect, parents and clinicians, because of Johnson’s theory, failed to act directly at the time of stuttering onset in early childhood. I hate to think how many people are stuttering today because of that wrong treatment. And I am sure that the arrival of serious research into direct, up-front treatment might be expected to work. However, the history of influence of the demands and capacities treatment model, clinicians who rely on it are in a static period of their professional career where change is unlikely.

So, the hub of my advice: Don’t do any therapy that is based on a theory of stuttering. Notice that earlier I presented the caveat to that advice “at least not just now,” meaning that there might be some circumstances in which you would consider doing it. One circumstance, actually. Considering that all current theories are likely to be wrong, I think it unlikely that the circumstance I have in mind will eventuate. But it may prove to be the case that there will be published a credible clinical trial of a treatment that is based on a theory. That is a different story, for then there is evidence for the value of the treatment, and that brings me to my next item of advice.

### ONLY USE EVIDENCE-BASED TREATMENT PRACTICES

So now we are at the bottom line. If you cannot get treatments from theories, then where do you get them? My answer is in this section. Fortunately, neither the concept nor the practice of evidence-based treatment is rocket science. Anyone can understand its fundamentals quickly and easily, as you will shortly. So, what is it? There are many ways of saying what it is, and Ingham (2003) explained them in an article that you might care to read. But what it boils down to is the simple idea that the source of treatments should be scientific research, and that it is the responsibility of the clinician to access that research to determine what treatments have the best evidence, and to select from those which ones to use to treat clients. This gives “best practice” to the client.

The opposite of evidence-based clinical practice is assertion-based clinical practice. The latter is a treatment that is thought to be of value because someone or a group of people, normally with “expert” status in the field, says that it is of value. In Australia, the National Health and Medical Research Council is somewhat equivalent to the United States National Institutes of Health, which has, among other functions, the responsibility to determine health care policies. That Australian organization recently disavowed the opinion of experts as a way to justify treatments, and opined that the only choice in the matter can be an empirical one (National Health and Medical Research Council, 1999). Most students to whom I mention the concept of evidence-based practice just accept it as a matter of course. Indeed, why would you want to choose anything but the best proven treatments for your clients? After all, treatment of stuttering is about them. It is not about what you or anyone else believes is the best treatment for them, nor is treatment about the latest fashion or fad. Treatment of your clients is about what really is the best treatment for them.
Levels of Evidence

So, how does scientific research come up with evidence about what is the best treatment for stuttering? Again, it is not rocket science, and again, there are various ways of saying it. A detailed exposition of the matter with reference to speech-language pathology can be found in Reilly, Douglas, and Oates (2003), but below is my version, simplified for your absorption.

Evidence for treatments does not just appear overnight. It is in fact quite a long process, and the development of new evidence-based practices is thought to go through three stages: Phase I, Phase II, and Phase III trials. You can read about this general process in great detail in Piantadosi (1997) and Pocock (1996). Closer to home, you may wish to read an account by Jones, Gebski, Onslow, and Packman (2001) about the process of Phase I to Phase III clinical trials development with a treatment for early stuttering. In summary, though, Phase I trials are designed to demonstrate the safety, viability, and initial clinical promise of a treatment. Phase II trials carry the process a bit further. They confirm the clinical benefits of the new procedure and allow some estimates of so-called “effect sizes”—how well clients respond to the new treatment—so that a Phase III trial can be properly designed. Finally, a Phase III clinical trial is often referred to as the “gold standard” of scientific evidence.

Now, the above Phase I-to-Phase III sequence is, as you would have gathered, a process where escalating quality of scientific evidence is attained. The least compelling level of evidence is the case study, where a few subjects are simply studied before and after treatment. Used mainly as Phase I evidence, case studies offer little more than optimism that a treatment might be of value. An example of such a trial of a stuttering treatment would be Wilson, Onslow, and Lincoln (in press). The next levels of evidence are nonrandomized and noncontrolled trials. Nonrandomized means that the subjects in the experiments—or the participants, as is said often in the current politically correct age—are put in different treatment groups in a systematic manner. For example, all participants in one clinic have one treatment, and all those in another clinic have another treatment. A classic example of this nonrandomized design in stuttering treatment research is Craig et al. (1996). Noncontrolled means that there is no group of participants in the experiment who receive no treatment. A classic example of such a trial would be O’Brien, Onslow, et al. (2003). Nonrandomized and noncontrolled clinical trials constitute Phase II evidence. Finally, Phase III evidence is a randomized controlled trial where all participants who agree to be in the trial are randomized to one of the experimental groups and the experimental groups who receive the treatment are compared to a group of participants who receive no treatment at all. An example of such a trial of a stuttering treatment, in its final stages at the time of writing, is Jones et al. (2001).

The elegant design of the randomized controlled trial provides the ultimate evidence base. This is evidence that “proves” that it was the treatment that made the participants get better, in which case we can say that we have evidence of “efficacy.” By contrast, Phase II trials allow us only to state something about the “outcomes” of the treatment—that the participants improved after the treatment, but we do not have conclusive proof that it was the treatment that did the job. And little more can be said of the results of Phase I trials than that there may be some promise in the treatment.

Evaluating the Evidence

Standards of clinical trials vary considerably, and assessing their value is quite a complicated matter. But again, in a simplified version, here are some issues that might be taken account of in judging the value of a clinical trial. It is important to use this information, or some other information that you might access, to get some kind of idea of how you evaluate the merits of any clinical trial that you read. The reason is simply that there is a wide range of standards of clinical trials research in stuttering. Many have written about these standards (e.g., Bloodstein, 1995; Ingham & Riley, 1998), but here is my version of what to look out for.

First, the ultimate test is of whether the trial has been replicated by an independent group or groups to the first group that published the trial. You can be a lot more confident in an independent replication of a research finding than you can in a finding by a single group of researchers. Second, a trial will be more believable if the primary outcome measures—the one or two measures used to convince you of the merits of the treatment in question—are objective ones, such as percentage of syllables stuttered that I described previously. This is not to say that subjective measures should not be included in scientific research; in fact, there are many reasons to argue that they should be (O’Brien, Packman, Onslow, & O’Brien, in press). However, in my view, any subjective measure used to quantify an objective behavior, at least in clinical trials research, is not quite as good as an objective measure. The argument behind that statement is that one can be more confident that results have been truly replicated if objective measures were included in the research.

Nonetheless, for all the benefits of objective speech data such as percentage of syllables stuttered, they mean nothing if you cannot somehow determine what is called their “social validity.” This notion incorporates ideas such as whether the reduced stuttering improves the person’s quality of life and whether it is accompanied by a subjective feeling that speech has changed for the better. In terms of treatments for adults, many of which can result in bizarre-sounding speech, it is absolutely critical to measure how natural speech sounds. Reducing stuttering is of no value unless the person sounds fairly much like everyone else. It is amazing to me that the majority of adult treatments reported in trials have sought to induce an unusual speech pattern in order to control stuttering, but have not incorporated any measure of how natural speech sounds (Onslow, Costa, Andrews, Harrison, & Packman, 1996). It is pointless to reduce a client’s stuttering rate to zero if the client sounds completely strange. Further, evidence in a clinical trial must show that speech improvements occur in
everyday life, and in several of the speaking situations of everyday life. And finally, and perhaps most importantly, because stuttering is notoriously relapse prone, a clinical trial must convince you that the benefits of treatment last for a clinically significant period.

**How Good Is the Clinical Trials Evidence for Stuttering?**

So, how do we stand with the evidence base for stuttering from clinical trials? After all, if there is no evidence, then it is difficult to argue with those who favor assertion-based treatment. Fortunately, we are in decent shape, but not as good as some in the health sciences. For example, should you happen to be a herbalist and wish to treat someone for depression with St. John’s wort, according to Linde et al. (1996), around the time of their review, there were 23 published randomized trials of this treatment, with 15 of them controlled with a placebo drug. Eight of them were noncontrolled, involving a comparison with another substance. More than 17,000 patients participated in all this research. Ideally, if you were really interested, you would spend the time reading all those trials. Certainly, though, you would be well advised to read an expert interpretation of the literature, such as provided by Linde et al. On the basis of this, you would have good information about what the evidence says about the effects of St. John’s wort on depression.

Admittedly, it is much more difficult to administer stuttering treatments than it is to administer drug treatments, and so it is much more difficult to do a randomized controlled trial. There are also far fewer PWS seeking treatment than there are people who are depressed. So, it is not surprising that we are in quite a modest position in relation to St. John’s wort. Apart from a few randomized controlled trials of drugs for stuttering, which offered little benefits and many side effects (and were never justified in the first place, in my view), there has never been a published Phase III trial of a standard speech pathology treatment for stuttering. The closest we have come so far to that “gold standard” has been the Craig et al. (1996) study, which was a Phase II, controlled nonrandomized trial of treatment for children and adolescents. Lincoln, Onslow, Wilson, and Lewis (1996) was a nonrandomized noncontrolled trial of a treatment for school-age children. We are up to speed with a reasonable number of Phase II nonrandomized noncontrolled trials for adults—around 10. A couple of Phase I and Phase II nonrandomized noncontrolled trials have been completed for early stuttering, and, as noted earlier, a Phase III trial for this age group (children younger than 6 years) is nearing completion at the time of writing (Jones et al., 2001). Added up, a few hundred subjects have been involved in fairly decent trials for adults, and approximately 40 preschool children and approximately 80 school-age children have been participants in trials.

What Are the Benefits to You?

The point here is not to tell you what the evidence is for various treatments and advise you which to use. That decision is yours alone. In making your decision, you can either read all the literature (it is not that imposing) or read some of the summaries of it that are published at regular intervals (e.g., Bloodstein, 1995; Ingham, 1984; Onslow, 1996). However, the point here is that I commend evidence-based treatment of stuttering to you, vigorously. Why? Well, if you want to read a detailed answer to my views on that question, you can find it in Onslow (2003). In that article, I address a range of professional issues in relation to the topic. That edition of the *Journal of Fluency Disorders* (Volume 28) also includes other articles devoted to this topic, and you can find out much detailed information there.

But here I want to finish the section by emphasizing one particularly important advantage that evidence-based practice with stuttering has for you as an inexperienced clinician. In short, it makes for a dynamic, changing clinical life, where you move progressively with a scientifically oriented community of clinical practitioners. In such an environment, best practice for your clients is always in a process of change. That is the point of clinical trials: to find out the value of new treatments or modifications of previous ones. I believe that the record shows that evidence-based treatment practices for adults and children who stutter have undergone considerable evolution during the past 20 years, and that the same cannot be said for assertion-based treatment practices. A case in point would be the demands and capacities treatment model, overviewed previously. As I have said (Onslow, 2003), it appears that the historical reliance on this assertion-based treatment method has led to an unexciting clinical environment where treatment practices have been handed down, unchanged, from generation to generation of clinical practitioners. Again, caveat emptor! Be careful that your treatment selection is in the interests of your most fulsome professional development as a clinician.

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**FEEL THE FEAR AND BE ACCOUNTABLE ANYWAY**

**What Is Accountability?**

In a professional sense, you can see from my adaptation of Jeffers’ (1997) well-known phrase that I think it is something that could be pretty scary when you first think about it. But on reflection, it is not that bad. The notion of accountability in clinical practice has many dimensions, but what I focus on here is the idea that you have to pay some attention to how well you are doing therapy. If there is room for you to improve, then you should do so. And, in my view, if you are doing an excellent job in helping your clients, you have to find ways to be even better. (Why stop at being excellent?) But you cannot do anything about your clinical performance if you never know what it is. This section is about knowing how well you are doing and making a professional response to that knowledge.
Be Specific About What You Want to Achieve in Treatment

Why do you need to be specific about what you want to achieve in treatment? Well, if for no other reason, it just makes no sense to do things without a clear purpose. And treating clients without any idea of what you are attempting to achieve is not a particularly helpful thing to do for your clients. Indeed, if you do not specify what it is you intend to achieve, then you cannot ever be said to have not achieved it. Absence of accountability is certainly comfortable. By definition, in a climate of unaccountability, you can never be wrong in any treatment decision that you make. You can just go on and on with a treatment that is not effective, for a completely unreasonable period.

Probably the most extreme version of nonaccountability through nonspecific treatment goals would be the psychoanalysis that Travis (1971, 1986) used for hundreds of hours with stuttering clients. Fortunately, those kinds of excesses are behind us. But I suspect that the potential for it lingers always in this field. A few years ago (Onslow, 1999), I complained that recommended treatments for children could last for up to 3 years, or even longer. In my view, that is not sustainable in any sense, neither financially, as I remarked in the 1999 article, nor in a functional sense. I just think that a treatment for a child that can continue for 3 years or more is worrying, and I suspect that the cause of that problem is the absence of a clear goal. Again, my complaint here is not that the treatment in question was not effective; my complaint is that the ineffectiveness was not identified earlier, so that another could be selected. No child deserves to be in speech therapy for 3 years for stuttering! In the case of children, this issue is particularly worrisome considering that available data now suggest that there is a “window of opportunity” to fix stuttering in preschool children and that tractability begins to be lost at some time during the school years. If you are a child who stutters, a delay of several years in finding the correct treatment could have serious, lifelong ramifications. But more of that in a while.

Where Do Your Treatment Goals Come From?

Again, the answer to that question is simple: You use clinical measures to specify treatment goals. In essence, the treatment goal is operationalized in terms of some number or set of numbers. That is about as specific as you can get. The short answer is that your professional literature will be a big help in establishing outcome measures. With regard to the measurement of clinical outcomes of treatment, you can find two sources of information: recommendations about how to do it, and demonstrations about how to do it. One of the intriguing discrepancies in our stuttering treatment literature is the differences in measuring outcomes when people talk and write about it in comparison with when people actually do it in clinical trials. When people do it, they do much less than those who talk about it. My guess at explaining the discrepancy is that measuring treatment outcomes is something that is a lot easier to talk about than to actually do. When you are a clinician, whether conducting a clinical trial or treating a client in your clinic, less complicated outcome measures are better than more complicated ones. The measurement of clinical outcomes is not effortless.4

My second answer to the question of where you get the specifics about your treatment outcomes is the long one. The answer to the question, and again I stress caveat emptor, is to get the specifics of the treatment goals from the client. How? Simply, just ask. Don Baer (1988, 1990) introduced this concept into stuttering treatment some time ago: Treat what the client is complaining about. Stuttering can be a nasty disorder, but it is not life threatening, nor has it yet been demonstrated to cost communities vast sums of money. There are treatment options here.

Treatment goals for early stuttering. In the case of children who have recently experienced the onset of stuttering during the first years of life, you will have to ask their parent or parents. And inevitably, what they will complain about is stuttered speech that had its onset, sometimes rapidly and sometimes slowly, after a period of normal speech development, as I stated earlier. They will complain about repeated movements and/or fixed postures of the speech mechanism and/or behaviors that are extraneous to speech. Sometimes, in effect, the children will complain too, by putting their hand over their mouth in surprise, or they will say to their parents something to the effect that “I can’t say it.” There is no reason to believe that there is any other problem with preschoolers who stutter other than that they stutter. As children leave the preschool years, that is another story, which I will come to shortly. But for preschoolers, my advice is to check that what I say is true with the first preschool stuttering clients that you encounter in clinical practice, and ask the parents what their complaint is. I would guess that, like myself and my clinical colleagues, you will stop asking and assume that the presenting problem with early stuttering is stuttered speech. In any event, the proof of the pudding here is in the eating. If, when preschool children have stopped stuttering, you have reason to believe that there is any

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4 At this point, it may be worth clarifying a distinction that is often not understood. This is the distinction between clinical measurement as part of the treatment process and clinical measurement to document treatment outcome. Treatment process and treatment outcome are two completely different things and can require two completely different clinical measurement methods. By way of example, if you should read the O’Brien, Onslow, Cream, and Packman (2003) clinical trial—a nonrandomized noncontrolled one—you will see that measures used during the treatment process were different from those used during treatment outcome. For this case in point, the measure of stuttering severity that was used during the treatment process was a 9-point scale, where 1 = no stuttering and 9 = extremely severe stuttering. This happens to be a particularly convenient measure for use in clinical practice, and it correlates well with the objective measure of %SS. (You can read a review of the literature in support of this contention, and a further study of the matter, in O’Brien, Packman, Onslow, & O’Brien, in press). However, the measure of %SS was used to demonstrate the outcomes after their treatment. The point I am getting at here is that you do not need to feel constrained to use the same clinical measures during the treatment process as are used to demonstrate the outcomes of that process. You can if you wish, but there is no reason to think that you need to.
residual problem some time after, then you may change your mind on that matter. But I doubt that you will.

If all that I say is indeed true, then the outcome measures that you use in preschool stuttering should be straightforward, and a reflection of the ones that have appeared in clinical trials to date: measures of %SS combined with parent self-report inventories about their children’s speech. You may wish to include a severity rating scale, or even a measure of speech rate in syllables spoken per minute.

Treatment goals during school age and adolescence. As we move the discussion past the preschool years to the school years and early adolescence, my advice may become a little controversial. There is no doubt that other factors besides stuttered speech might assume importance at some time during this period. Nonetheless, my advice is to focus exclusively, and also vigorously, on getting rid of stuttered speech in this age group. The reason I say this is that all available data indicate that stuttering becomes less responsive to treatment when the child leaves the preschool years and is on a path to becoming intractable some time during early adolescence (Onslow & Packman, 1997). Available clinical trials data on school-age children show that, although these children do respond well to treatment, they do not respond as well as do preschool children (Craig et al., 1996; Onslow et al., 1996). This means that, most likely, the school years are the last chance a child has for obtaining long-lasting protection from the debilitating effects of the disorder.

The other issue here is that one of the biggest problems in the field of stuttering treatment at present is the fact that, despite the important clinical responsibilities faced by clinicians when treating this age group, it is by no means clear yet how this should be done. The two published clinical trials are problematic. The Lincoln et al. (1996) trial adapted a treatment for preschool children for this age group, but it is not quite right to do so. The reason is that treatment models for preschool children are normally parent delivered, and for various reasons, this does not work as well as it might with school-age children. The Craig et al. (1996) trial was of an adult treatment adapted for school-age children, and again this is not quite right because it requires children to speak with unusual sounding speech. What is needed at present is the development of a new treatment model that is absolutely ideal for this age group. For what it is worth, I think that such a new model might incorporate a combination of the self-modelling technique reported in a series of trials by Bray and Kehle (1996, 1998, 2001), and the self-imposed time-out treatment reported in several trials (Hewat, O’Brien, Onslow, & Packman, 2001; James, 1981). At the time of writing, my colleagues and myself are planning such treatment developments; however, the results are some years away.

For now, you are left in a situation where best practice for school-age children is not as good as it might be, yet you need to find the best treatment for every such child as a matter of urgency. Hence my view is to focus exclusively, and urgently, on getting rid of their stuttering. Indeed, during the school years, they will more than likely experience problems with their stuttering. For example, Langevin, Bortnick, Hammer, and Wiebe (1998) reported that 81% of primary school children with stuttering problems are bullied during their time at primary school, with 56% reporting being bullied about their stuttering once a week or more often. Further, they are perceived negatively by peers (Langevin & Hagler, in press) and are rejected by peers more often than children who do not stutter (Davis, Howell, & Cooke, 2002). The quickest way to stop those problems is to stop the child from stuttering, so I offer the identical advice to that given for what should be the specific treatment targets for this age group: Just get rid of the stuttering. You may be aware that there is a growing literature discussing when exactly you should choose to intervene with a child who has begun to stutter in the preschool years (e.g., Curlee & Yairi, 1998; Ingham & Cordes, 1998; Zebrowski, 1997). However, years later, at school age, there is no place for such a debate—just get on with it quickly and urgently.

Treatment goals for adults. In the case of adults who seek treatment, it is not as straightforward to establish what the therapy targets should be. One issue is that, unlike for preschool children, existing data do not provide any confidence that getting rid of stuttering will always be an acceptable treatment goal. In the first instance, it is known that with attempts to stop adults from stuttering, or greatly reduce their stuttering, relapse to pretreatment stuttering levels is common. This is an extremely well documented aspect of the treatment of adults. Depending on how you define relapse, the estimates of its rate will vary. Craig and Hancock (1995) used a self-report index and reported that 72% of their 152 respondents reported relapse lasting more than a week. A much earlier review of the matter (Martin, 1981) reported that only one third of those who attained control of their stuttered speech experienced long-lasting effects free of relapse. The other complicating factor affecting the specification of treatment goals for adults is that, not surprisingly, many years of stuttering can have adverse effects on people. Apart from the social anxiety mentioned previously, stuttering is capable of causing social maladjustment and failure to attain occupational potential (Craig & Calver, 1991; Hayhow, Cray, & Enderby, 2002). You need to be a bit careful if you believe that those effects will go away if stuttered speech goes away. It is also the case that PWS have reported that the harmful effects of the condition, and feelings of being “different” as a communicator, do not go away when stuttered speech goes away (Cream, Onslow, Packman, & Llewellyn, 2003). There is just no way that you can consider that taking away or reducing stuttered speech will “cure” the problem of stuttering.

So, that brief overview of the effects of stuttering gives an interesting context for what you may find when you ask adult clients who come to you what they want you to do for them. I guarantee that you will not get the same straightforward answer in every case as you will with parents of preschoolers who stutter. In particular, you should note that you may encounter clients who simply do not want zero or reduced stuttering to be part of their treatment goals. Indeed, it would be a big mistake to assume that an adult client wanted stuttering to go away.
For one thing, you would give a completely wrong treatment—as wrong as if you based a treatment on a wrong theory, as discussed in a previous section. Consider someone, for example, who complains of stuttered speech but who is highly successful in business, with excellent leadership and communication skills. On face value, it is difficult to believe that stopping stuttering would make any positive change to such a person’s quality of life. Make no mistake, stuttering does not automatically cause problems in life. For example, I have just finished reading a long biography of Charles Darwin (Desmond & Moore, 1991), which contained no mention of the fact that he stuttered.

I should note that it is a simplification to state that you should “ask” adult clients what they want from treatment. The effects of stuttering on an adult may be straightforward, but they may be complicated. You may need to allow yourself to be a sounding board to clarify the client’s thoughts about the exact problem or problems. The successful business person above may, for example, be looking for ways to deal with specific instances in life when stuttering becomes awkward. This process of working with the client in reflecting on treatment goals is particularly important when stuttering has caused considerable distress, as it can do. That distress may cloud the judgment of the person seeking a consultation. At first blush, what the person tells you he or she wants from treatment may not in fact be the correct answer to the question. I venture that, if you follow this advice and really explore with adult clients what they want from treatment, you will have a really interesting clinical time.

Actually, it is an interesting question to contemplate exactly how many adults who stutter want to get rid of their stuttered speech. What do you think? Do you think that the number would be in the majority or the minority? The only way you are going to find out is to ask them. Interestingly, there are some reasons to believe that the answer to the question will depend on the clinical community in which the person who stutters has lived with the disorder (Attanasio, Onslow, & Menzies, 1996). It appears, for example, that more Australians who stutter want to get rid of stuttered speech than is the case with Americans, because of the historical prominence of treatments designed to do that.

**CONTROVERSIES ABOUT TREATMENT GOALS**

Earlier in the article I mentioned that there may be some controversy associated with my advice. The idea of getting rid of stuttering as a treatment goal is likely to not sit well with many in our profession. The reasons for this are historical. In the first instance, there is a long history of division on this issue. You can read about this division in Ingham (1984) and Onslow (in press) if you wish. However, the short version of this story is that those who began the systematic study of this disorder at the University of Iowa did not place much emphasis on stuttered speech as a treatment goal. Those pioneers were Travis and his students, including Wendell Johnson, who I mentioned earlier. These early figures in the field were extremely influential in their thinking, and until the middle of the last century, were virtually unchallenged in their disregard for the idea of getting rid of stuttered speech. Around that time, a group of researchers, known as behaviorists, began to change that view, causing quite a stir with their idea that the prime focus of therapy should be stuttered speech. An example of the disquiet caused by their work is the description by Joseph Sheehan (who trained at Iowa) of their efforts as an “indecent scramble” (Sheehan & Sheehan, 1984, p. 250). In the case of treatment at the early stages of the condition, Cook and Rustin (1997) are on the record much more recently with serious reservations about stuttered speech as a treatment target. I am fairly sure that these reservations still linger in our profession, to some extent, at the time of this writing. So, again, caveat emptor.

The other reason that the idea of getting rid of stuttering as a treatment goal may be controversial arises from the historical popularity of theories as a basis of treatment. A few have been mentioned here, but there are many others. The most popular currently, that I mentioned, is the demands and capacities treatment model. Previously, I stated that the most obvious problem with reliance on such theories as sources of treatment is the fact that they will probably be wrong. However, there is a more subtle problem with reliance on theories for treatments, and the demands and capacities treatment model illustrates it perfectly: They require the practitioner to use treatment goals that reflect not the presenting complaint, but a theoretical notion about the disorder. The treatment goals of the demands and capacities model include changes to parental behavior in addition to reduced stuttering (Starkweather et al., 1990). These parental changes are illustrated in the Stuttering Foundation of America (2003) publication to which I referred previously, and which have been reiterated continuously through the period during which this theory has been popular. Such practices are quite different from the methods I suggested for finding treatment goals from the scientific literature and from the complaints of clients or their parents. Earlier, I said that the essence of evidence-based treatment practice is that treatment is not about you and what you believe to be an effective treatment, and here I add that it is not about theories of the nature of stuttering either.

**BENCHMARK YOUR CLINICAL PERFORMANCE**

**What Is Benchmarking?**

If you took my advice to feel the fear and be accountable anyway, perhaps in your fearless state you are ready to take the ultimate step toward accountability by benchmarking your clinical performance. What does “benchmarking clinical performance” mean? It is an interesting twist on what I was discussing previously, under the heading dealing with being specific about treatment.
outcome by measuring it. Benchmarking also involves measuring clinical performance, but it is yours that you measure this time! This is the best way I know to become an outstanding clinician.

How Successful Are You?

There are two parts to the concept of benchmarking your clinical performance, the first being how successful you are with your clients. Regarding how successful you are, again, it is not rocket science to find out. If you are using clinical trials to establish best practice for your clients, then those clinical trials will tell you how good the treatment is. For example, the recent trial reported by O’Brien, Onslow, et al. (2003) showed that at 6 months and 12 months post-treatment, the average %SS scores of a group of 16 clients were below 1.0. The treatment in question, which you may have heard of, is called prolonged speech, and involves quite a risk that stuttering will be reduced at the expense of speech that does not sound quite natural. When you do a treatment that involves a novel speech pattern, the real index of how well you do is how natural your clients sound—it is no challenge to get rid of stuttering by having your clients use a bizarre-sounding speech pattern. There is a well-known way to measure speech naturalness, with a 9-point scale (Martin, Haroldson, & Triden, 1984). We have a pretty good idea of how nonstuttering speakers will score on the scale, and so speech naturalness scores can be calibrated against that. In the O’Brien, Onslow, et al. report, the mean speech naturalness score for the clients, given by a group of observers after treatment, was 4.5 (range 1.3–7.3). The scores for a group of matched controls were in the expected range of 2.0–4.7, with an average of 3.6. In general terms, this means that approximately half of O’Brien, Onslow, et al.’s clients achieved speech naturalness within or near the range of normal speakers, and approximately half did not. That, then, might be what you would consider an excellent result for a caseload of clients to whom you gave this treatment.

In the case of another well-known style of treatment, operant methods, the version that is popular for treating preschool children is the Lidcombe Program. In this case, there are available data to show that the mean stuttering rate of a caseload of stuttering preschoolers, 6 months and 12 months after receiving this treatment, measured in everyday speaking situations, was less than 1 %SS (Lincoln & Onslow, 1997; Onslow, Andrews, & Lincoln, 1994).

This means that, should you choose to use the treatments recommended in the trials mentioned above, then you have benchmark data available so that you can compare your results against those of the experts. As is the case with all popular treatments, manuals are readily available (in the present examples, manuals for the Camperdown Program and the Lidcombe Program can be downloaded from the Web site of the Australian Stuttering Research Centre at www.fhs.usyd.edu.au/asrc), so you can be sure that you are doing the treatment as intended. Basically, to benchmark, all you need to do is have your clients send you tape recordings of their speech in a few everyday speaking situations, 6 months and 12 months after treatment, and to listen to those tape recordings and measure %SS. You may also wish to consider, in addition to or instead of the foregoing, the “surprise telephone calls” technique that has appeared in clinical trials (Block, Onslow, Packman, Gray, & Dacakis, 2003; Boberg & Kully, 1994). It is a convenient method, which probably will produce more valid speech samples than having clients record their speech. Just remember to use one of those devices that enable you to record telephone conversations so you can keep audiotape records. Whatever method you use, it might be a good idea to do something similar at some time before treatment so that you can see how severe your cases are as compared to those in the trials literature. There are many training packages to assist you to learn to measure %SS, should you require them.

A note here is warranted about the use of the speech naturalness scale, which, as I have said, is particularly important in treatment outcomes for adults. Because the scale is a subjective judgment, a score by a single clinician, although useful as part of the treatment to give the client feedback, is not so useful as a treatment outcome measure. O’Brien, O’Brien, Packman, and Onslow (2003a, 2003b) developed and used a mathematical procedure to determine that three observers measuring speech naturalness, from 15-s speech samples, are adequate. If you want, you can fill a large pot of coffee and read the details of the mathematical procedures involved in those two reports, or you may prefer to just skim to the end of the second of the articles cited above to see the recommendation. In any event, it appears that it can be done well enough with three observers on 15-s samples, and for my money, that is a procedure that a clinician can replicate for benchmarking purposes.

The clinical trials of the Camperdown Program and Lidcombe Program treatments both included self-report inventories that were completed by the clients and, in the case of the Lidcombe Program, the parents of the children after the treatment. Such inventories provide a particularly useful means of benchmarking against reported clinical trials data because they provide a range of information, such as perceptions of speech improvement, quality of life, and the effort involved in maintaining treatment benefits. These days, it is considered mandatory to publish such data in clinical trials; hence, these inventories provide further invaluable means by which clinicians may benchmark their clinical performance against that which appears in the trials literature.

How Quick Are You?

The other part of benchmarking is how quick you are. In other words, given that you can achieve what is achieved in the clinical trials literature, can you do it in the time taken in that literature? As I mentioned earlier, it is not good if you can get a child to stop stuttering but it takes you 3 or 4 years to do it! So, to return again to the examples of the trials above, how long does it take to get people who are stuttering to below the benchmark of 1.0 %SS? Again, the answer is simple and can be found in the trials in question. In the case of the O’Brien, Onslow, et al. (2003) trial, an average of 20 hr of face-to-face treatment,
In the case of the Lidcombe Program, an average of 11 visits to the clinic by the parent and child. In the case of the Lidcombe Program, there are data, derived from 141 cases, available to indicate that the average child reduced stuttering by approximately 30% during the first few weeks of treatment (Onslow, Harrison, Jones, & Packman, 2002). In a previous discussion of this matter (Onslow, 2003), I drew attention to a technique my colleagues and I have found to be particularly useful. This is the recovery plot, which is simply a graph that shows how long it has taken for your caseload of clients to “recover,” or to meet the specific treatment targets that I have mentioned in this section. You can do this plot with as few as 20 clients, but reports by Kingston, Huber, Onslow, Jones, and Packman (2003) and Jones, Onslow, Harrison, and Packman (2000) involved a total of 328 children who were treated with the Lidcombe Program and achieved the program criterion of stuttering less than 1.0 %SS. You can consult these sources for details of the procedure, but there are some excellent advantages to it that are worth explaining now. For the sake of completeness, though, before proceeding, I will mention that this type of research is referred to as “treatment process research.” This is research into the treatment process rather than into its outcomes and efficacy. As such, it is an ideal complement to the clinical trials research that I outlined earlier. Taken together, clinical trials research and clinical process research provide the clinician with a rich source of scientific evidence to use to find best practice for clients.

The first advantage of the recovery plot procedure is that you can keep a running check over the years of how quick you are in relation to your chosen benchmarks. In the case of the Lidcombe Program, the Kingston et al. (2003) and Jones et al. (2000) reports, which are independent replications, provide strong evidence that the median time to achieve below 1.0 %SS during the Lidcombe Program treatment process is 11 clinic visits. The other useful feature of the plotting procedure is the shape of the graph. In the case of the recovery plot for the Lidcombe Program, it is steep on the left and flat on the right, meaning that there are many cases who are treated quite quickly, but that there is a “tail” of children who take longer than the median of 11 clinic visits. The benchmark value of 11 clinic sessions is of itself indispensable. The clinicians with whom I associate use it every day. If a child has reached 11 treatment sessions and is a long way from meeting treatment criteria, then something is not quite right. In such a scenario, the clinicians I work with know that it is something about the child that is causing the delay, rather than them not being quick enough, because their caseloads all meet the benchmark of 11 clinic visits. The final advantage to a clinician of the recovery plot technique is that it can be used for any treatment outcome. The plot procedure contains no mention of any specific measure related to stuttering; it only refers to “time to recovery.” This means, incidentally, that the procedure is applicable to benchmarking clinical performance during the management of any speech or language disorder.

Benchmarking with results obtained from treatment process and clinical trials research is of course an important part of your accountability as a clinician. However, it has one limitation. This is related to your inexperience as a clinician. Simply, it is difficult to know how your own performance should relate to published benchmarks in this research, which invariably are produced by experienced and specialized clinicians. Are you to be expected to match those benchmarks at some time during your career, or are you expected to match them now? Is it possible for you to match published benchmarks when you are an inexperienced clinician? Unfortunately, the answer to the latter question is currently unknown, and I can offer no advice there. I have been involved in a recent clinical trial, however, that did show that student clinicians under supervision of experienced clinicians could conduct a prolonged speech treatment that met published benchmarks for that style of treatment (Block et al., 2003). In the case of the Lidcombe Program, there are some data to suggest that junior clinicians might take some time to find their way (Rousseau, Packman, Onslow, Robinson, & Harrison, 2002). Beyond that, at least with the treatments I have mentioned, I cannot offer any advice in the matter.

Who Can Help You With Benchmarking?

Do not misunderstand the point of this section. No one would have any right to complain if you failed to help a client. It happens all the time that some adult clients cannot be helped, or that they take a long time to achieve their treatment goals. It is common that a handful of preschool children take a whole lot longer than others to get rid of their stuttering (Jones et al., 2000; Kingston et al., 2003). The point here is not that you need to be effective with your treatments, but that you need to know when you have room for improvement. And if you discover that you have room for improvement, then you can remedy the situation. How?

One obvious answer to that question is to obtain further clinical training in addition to that you received during your professional preparation. It certainly would be no surprise if you felt the need to do that, considering the 1993 decision by ASHA to remove compulsory professional preparation in stuttering in U.S. universities. You can easily find practical training programs in most treatment methods. Where I work, for example, at the Australian Stuttering Research Centre, clinicians and university researchers collaborate to present a comprehensive set of continuing education activities for clinicians. You can find information about this at the Web site of the Centre at www.fhs.usyd.edu.au/asrc. There is also a network of trainers, located in many countries, including the United States and Canada, who provide continuing education in the Lidcombe Program of Early Stuttering Intervention. This is

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5 The median is a better measure than the mean to use in this context because it is not affected by extreme scores, as is the mean. So, if a particularly difficult case takes an inordinate amount of time, it will not have an undue effect on the median treatment time of your caseload.

6 There are children who simply do take longer to treat with the Lidcombe Program, for reasons that we do not fully understand at present. The Kingston et al. (2003) and Jones et al. (2000) reports show that 10% of children require more than 22 clinic visits to meet program criteria.
Another approach to benchmarking your clinical performance is to find a clinical mentor to assist you with it. Professional speech-language associations in many countries have formal mentorship programs for junior clinicians. Ideally, as an inexperienced clinician, it would be useful to use the recovery plot procedure described above to benchmark against your more senior colleagues. In principle, it is possible for a group of mentor clinicians to benchmark their clinical performances, using data from treatment outcome and process research in the literature, and using the recovery plot technique. That would be a fine leadership role. If it is the case, as I suggested, that a generalist clinician cannot be expected to match what is achieved in the literature by specialist and experienced clinicians, then such a benchmarking process would make that clear. A junior clinician would have performance goals set by mentors. No student treating this disorder could ask for more than that.

FINAL COMMENTS

Whatever you may think of the advice I have proffered, I am certain that you will not think that it is complicated. Much of it focuses on working around some potential complications in your future clinical life. Stay away from continua, and do not worry about diagnosing early stuttering or whether what a client just did was a stutter or a normal disfluency. Disregard theories as a source of treatment. That will simplify your life a great deal. You will not have to worry, for example, about the virtually endless changes to the living and communicating styles of families that you will have to make if you use the currently popular demands and capacities model to find your treatments. And only use a treatment on a client if clinical trials evidence supports its use as best practice. Simply, any other treatment is not good enough for your clients, and it is not ethical to subject them to any such treatment. Just ask those people what they want from treatment and use evidence-based practice to give it to them. Specify with clinical measurement, derived from clinical trials and the input of your clients, what your treatment goals are. Then, you can look at clinical trials data and treatment process research and know how effective and quick your treatments are.

In a sense, my advice to look to clinical trials for finding the best treatments, and to look to the clients for the best ways to evaluate them, is atheoretical. In other words, it disregards theory unless theory leads to a successful clinical trial, which, as I noted, has not happened yet. I need to conclude by stating that I am not urging you to be opposed to theories, only not to use them to find therapies. Stuttering is a much misunderstood disorder, and the range of theories that exist are our attempt to provide some understanding of it, and theory development is an essential underpinning to a field that seeks to know about and rectify a human disorder. As I stated, theory development may lead to a way to effectively treat PWS. A theory may, a long time from now, be shown to be conclusively correct. Such a theory may lead eventually to the development of effective treatment, but I fear that will not happen in our lifetimes. For now, we need to look elsewhere to know what to do in the clinic.

SOME QUESTIONS FOR YOU

In the present context, I have offered many answers to many questions. So now it is your turn to do the answering. Perhaps you might like to discuss the following questions with your colleagues and instructors, and form a view about what the answer should be. Beware, though, they are a little curly! I think you will need to discuss them with many people to explore them thoroughly.

Perhaps you can let me know your thoughts. It is easy these days to find and communicate with academics over the Internet.

- In this article, I have discussed where treatments come from and have mentioned the scientific evidence base as being an important source. So, is it acceptable for you to decide one day to use a treatment that has been shown scientifically to be effective for another disorder and apply it to stuttering? For example, aspirin is known to have many health benefits, such as relieving pain and reducing the risk of stroke and heart disease. Would it be the right thing to do to see if it had any beneficial effects on stuttering? After all, if it does not work on your clients, nothing is lost, because aspirin is cheap and harmless. Surely, there is much to gain if it does work, and little to be lost if it does not. So, is it acceptable for you to recommend it to your clients? Again, if so, why, and if not, why not?

- I referred to the troublesome problem that there really are no convincing clinical trials of treatments for school-age children; neither treatments for adults nor treatments for preschoolers have been shown to be completely satisfactory for this age group. If you accept that the clinical trials evidence for adults and preschoolers is satisfactory, then you have an “off-the-shelf” treatment that you can give to clients in those age groups. But this is by no means the case with children of school age. If you want to present “best practice” treatment to school-age children, how will you handle this situation? How will you establish what is “best practice” treatment for such a child when there really is no ready made off-the-shelf one that will probably work for all of them?

- The article suggested clinical trials as a source of treatment methods, and that a good source of measurement procedures to assess treatment outcome would be the clinical trials literature. However, consider a scenario where a clinician decides on treatment goals that include some dimension of stuttering that had not been measured in the clinical trials literature. An example of how this might happen is social anxiety in those who stutter. Not surprisingly,
research evidence is accumulating to suggest that social anxiety is common in those who stutter, even to the extent that approximately half of those who go to speech clinics warrant a comorbid diagnosis of social phobia, which is a psychiatric condition involving extreme fear of social situations (for an overview, see Menzies, Onslow, & Packman, 1999, and a subsequent study by Kraaimaat, Vanryckeghem, & Van Dam-Bagen, 2002). In fact, during the writing of this article, a large population study was published, showing that PWS were more anxious than a control group (Craig et al., 2003). So, if you take my advice and listen to what clients complain about when they come to seek treatment for stuttering, you would likely make a decision at some stage to target social anxiety as a treatment goal. Yet, although there is preliminary work underway at present (McColl, Onslow, Packman, & Menzies, 2001), there are no clinical trials published to show the benefits of treatments for social anxiety on stuttering. What do you do? Can you be an evidence-based practitioner and do a treatment that has not been reported in the clinical trials literature? And where do you get your measures of social anxiety if they have not appeared in the clinical trials literature?

- I mentioned the use of measures to specify therapy goals. Suppose that you specify that a preschool child, for a year after therapy ends, should stutter less than 1.0 %SS in the clinic in a series of visits to you during the maintenance phase of treatment, after you have essentially eliminated stuttering. (If you are interested, you can read the details about maintenance procedures that we recommend for preschoolers in Onslow & Webber, 2003.) Right after treatment ended, the child was scoring 0.2 %SS, and this is an excellent treatment gain, considering that pretreatment stuttering was around 20 %SS. The child comes in one day during the maintenance phase and you think that child has stuttered 10 times in a 1,000-syllable conversation with you. This is 1.0 %SS, and the child has failed to meet the criterion. But wait! Four of those 10 “stutters” may not have been “stutters” at all. They may have been normal disfluencies, and you may have overreacted by counting them. On reflection, perhaps they were not “unambiguous stutters” at all. What do you do? Do you let it go by? Or do you deem that the child has not met the speech criterion for successful maintenance of treatment gains, and do you then implement a return-to-treatment procedure as outlined by Onslow and Webber? In answering the “what do you do” question, keep in mind that there are two dimensions to it. The first is what you do in that specific instance, and the second dimension of the question is what you do in the future—if anything—to prevent the problem.

- In discussing benchmarking, you may have been struck by how little effort beyond everyday clinical practices is required to attain outcome measures similar to those reported in the clinical trials literature. Now, one of the issues that I raised is whether practicing, professional clinicians can be expected to meet those benchmarks. Take my word for it, it is of enormous scientific interest to know the answer to that question. It is a question that goes one step beyond the issues of treatment outcome and treatment efficacy that I touched on in this article. It is a question that involves treatment effectiveness, which deals with how well treatment works at a population level. It is one thing to have university researchers doing randomized controlled trials to establish treatment efficacy, but the only way to establish treatment effectiveness at the population level is by having your clinical outcome data, from your everyday clinical practices, published in the scientific literature. Would you want to do that? If so, why, and if not, why not? And if so, how would you go about doing it?

Enjoy.

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REFERENCES


Onslow: Advice to Students of Stuttering Treatment 21


Stuttering Foundation of America. (2001). Stuttering and the...


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