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## A Retrospective Look at Stuttering Therapy

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Retrospection is a sobering business. At first glance one is tempted to view many historical treatments of stuttering with a mixture of amusement, astonishment, and condescension. How could anyone possibly have believed and acted that way?

The amusement is quickly tempered when we realize that real people, suffering stutterers, were receiving those treatments. Astonishment gives way when we observe that most of us are caught in a set of assumptions that we rarely question. The intellectual climate of our age tends to restrict our ability or willingness to examine reality without biases. Our condescending view of past therapies also will change if we speculate about what speech pathologists of the year 2089 will think when looking back at popular therapies of 1989.

Perhaps a healthier perspective is to view past therapies as instructive stages we have passed through in seeking to understand and deal with a disorder. Each generation builds on the knowledge base and accomplishments of the preceding. Each generation hopes and believes that it will expand the knowledge base and devise a more successful therapy.

In this paper we will not attempt to describe all of the diverse therapies of the 20th century. Such reviews are available elsewhere (Van Riper, 1973; Ingham, 1984; Bloodstein, 1987). Instead, we will try to sketch what we perceive to be the major trends in therapy with emphasis upon the ideas, individuals, and conflicts that have had the greatest impact upon our own thinking and clinical practice. We all peer out at reality through windows covered with the dust of personal biases and experiences, as Johnson argued so persuasively in *People in Quandaries* (1946). This paper is written in a somewhat informal style and is a personal, perhaps even idiosyncratic, retrospective look at how we have arrived at our present level of knowledge and practice.

To gain a perspective on current therapies let us start at or near the beginning. In a delightful and amusing essay first published in the *New Yorker*, Gerald Jonas (1976) retells stories about early attempts at treating stutterers. One of the first recorded "cures" concerned a young Greek prince named Battus who beseeched the oracle of Delphi to heal him of his affliction. The oracle observed that Battus stuttered most when

speaking to his friends but rarely with strangers. Therefore, she advised him to gather an army, sail to Africa, where he had no friends, and never return. The tale concludes that Battus obeyed, triumphed in battle, and happily became the golden-tongued ruler of the Greek colony of Cyrene. Perhaps this drastic change in environment was the forerunner of the more modest environmental manipulations in much current practice.

Seeking a defect in the individual rather than his environment can be traced to such practitioners as Hippocrates, who linked stuttering to an excessively dry tongue and prescribed blistering substances to drain away the "black bile" causing the trouble. Galen, a second century physician, decided that the tongue was too wet and cold, so he used red hot coals to cure the condition. Many centuries later, Bacon, agreeing with Hippocrates that the tongue was too dry and stiff, recommended a much more palatable treatment: a draught of mulled wine. This long tradition of locating both the cause and cure of stuttering in the tongue came to a horrifying climax in the early 19th century with the Prussian surgeon Johann Dieffenbach. Without obtaining normative data on tongue length he decided that stutterers' tongues were too long. Therefore he cut a triangular wedge from the base of the tongue and sewed it back together, all before the advent of modern anesthetics. According to Dieffenbach's own reports, the operations were highly successful, and stutterers throughout Europe were treated surgically for much of the last century. It is hard to imagine a more vivid example of how a theoretical belief can shape a person's view of reality.

Jonas (1976) also recounts an intriguing explanation of stuttering by George Catlin, the frontier portrait painter. During his long travels with the Plains Indians, Catlin made several shrewd observations:

Unlike the slack-jawed white settlers, the Indians kept their lips firmly sealed most of the time and when they did open their mouths to speak they never stuttered. (p. 20)

Stuttering, according to him, was caused by "a nervous hesitation and vibration of the underjaw when brought up from its habitual hanging state to perform its part in articulation" (Jonas, 1976, p. 20).

Interest in elocution peaked in the 19th century, and it is not surprising that stutterers received enthusiastic attention from some elocutionists. A basic premise of this group was that people acquired bad speech habits during childhood but that these habits could be corrected by teaching "proper" use of the speech organs. Many elocution teachers treated stuttering without worrying whether there were any underlying causes. Alexander Melville Bell, a teacher of the deaf and father of the famous inventor, strongly rejected the "popular delusion" that stuttering was caused by some kind of organic defect. It clearly was nothing but a bad habit, maintained Bell, that could not be excised by scalpel or cured with tonic. The stutterer should spend his time reeducating his "unruly speech organs":

Let the stammerer particularly attend to the disposition of his lips [...] to prevent their officious meddling with sounds over which they have no legitimate influence. (p. 19)

The tongue, which Bell characterized as "the most alert and obedient organ in the body," should not give too much trouble: "This member is fairly easily disciplined to good habits and broken off from bad ones" (p.19).

Although the elocutionary movement gave rise to legions of quacks and charlatans, Jonas maintains that it also provided the seed for the science of speech pathology. Bell compared the task of a speech teacher to that of a music teacher whose first qualification was a thorough knowledge of the instrument. If the elocutionists' efforts to teach stutterers proved anything, it was that no one knew much about the speech instrument. From this awareness eventually arose attempts to understand the speech mechanism and the realization that stuttering was no simple defect due to a misshapen tongue.

Stuttering did not escape the attention of the psychoanalysts. Freud's disciples agreed that stuttering was a neurotic symptom, but could not agree on what it was a symptom of. Among the more colorful explanations of stuttering is one by Coriat (1928) who suggested that it is the persistence into adult life of infantile nursing activities. He insisted that careful observation would reveal that the stutterer's speech attempts represent motor patterns of nursing at an illusory nipple. He did not suggest specific treatments related to the alleged cause but recommended working toward a resolution of the psycho-neurosis through psychoanalysis.

Another psychoanalyst, Brill (1923), addressed the puzzle of why there were far more male than female stutterers. In summarizing Brill's explanation of the lower incidence in females, Glauber (1958) writes:

The female has more control of her speech but a smaller vocabulary. The male tends to be more quiet and silent but more aggressive in his thoughts. The female talks about simpler things, encounters less criticism. Present-day competitive civilization leaves little time for speech to the male, while at the same time he must use all his mental efforts. The female, on the other hand, according to his (Brill's) view, has no need for involved thinking, as she is in constant close relationship with the simple human being, the child. She can cook, bake, crochet, and talk at the same time (p. 89).

One would expect Brill's hypothesis to evoke some excited comments from modern speech-language pathologists and others. It is hard to fathom how a highly educated physician could adopt such a position, but we see it as an illuminating example of the power of the *Zeitgeist* and perceptual expectations: We seek and find evidence to support what we already believe. If an interviewer operates with a particular theoretical expectation, he may unknowingly reinforce subjects' statements revolving around one theme, while ignoring statements concerning other experiences and thus systematically shape responses to support his own theoretical position. Of course, this possibility of experimental bias applies to all of us, but to some more than others, we suspect.

From our present vantage point we suggest that the transition from speculation to serious investigation of stuttering occurred with the fortunate gathering in Iowa City in the late 1920s of Lee Edward Travis, Samuel Orton, and Wendell Johnson. Orton and Travis (1929) had advanced their cerebral dominance theory and needed a human guinea pig. Wendell Johnson was the ideal candidate.

Orton and Travis (1929) pointed out that the left hemisphere was normally dominant for speech. According to their theory, the trouble with stutterers was that neither hemisphere was dominant, which resulted in the two sides competing for control of the centrally located speech musculature. To explain how this curious cerebral anarchy had come about, Orton and Travis speculated that many stutterers had been born left-handed but forced to use their right hand as children. This forced use of the right hand in the naturally left-handed child was assumed to strengthen the left hemisphere at the expense of the right, not enough to reverse the dominance but enough to prevent either side from being dominant. Without a dominant hemisphere to synchronize the messages to the centrally located speech organs, the result was confusion for the child trying to speak.

The therapeutic implications were obvious: Switch right-handed stutterers back to their natural left-handed state. Wendell Johnson arrived in Iowa City in 1926 as a young man desperately seeking treatment for his severe stuttering. Al-

though he had always used his right hand, he accepted the suggestion that he had done so only in unconscious imitation of family members. He set out to become left-handed in the hope that he could thus reestablish the natural dominance of his right hemisphere. The severity of Johnson's stuttering can be gauged from his description of an episode shortly after he arrived in Iowa City to meet Travis (Johnson, 1961):

A few days after I arrived, Dr. Travis asked me to come with him to one of his classes. He explained that he wanted the students to observe my speech. I sat in a chair beside his desk at the front of the room. There were thirty or forty students looking at me. Dr. Travis told them who I was and that I was from a small town in Kansas and then he handed me a book and asked me to read aloud to the students. I read for five minutes—and got out four words.

After class I went with him to his car and we got in and started for the other end of the campus. When we had gone about a block he stopped to give one of the staff members a ride and as she got in the car he introduced us. Then he started the car again and we drove down a long curving hill and crossed a river [...] Altogether we had gone about a mile when all of a sudden I finally managed to blurt out, "Puhleased to meet you!" They were startled, and so was I, and I wished I hadn't said it, only I would have been even more embarrassed if I hadn't said anything at all. (p. 25)

With grim determination, Johnson set out to reeducate his neuromuscular control system by abandoning all right-handed and bilateral activities in order to concentrate solely on development of the left hand. He reported that the severity of his stuttering declined markedly and that his attitude toward stuttering improved. His initial optimism was premature. In 1961 he wrote:

Ten years and countless bruises later, having become a threat to my own thumbs, I placed in storage my many ingenious braces and mittens [...] put away my left-handed scissors, and with my right hand wrote 'Finis' to the experiment, still stuttering splendidly. (p. 27)

The lack of therapeutic success was less important to our professional development than the fact that an hypothesis was proposed, tested, and rejected. This represents a significant advance over the non-testable fantasies of Coriat and Brill. Not one to give up easily, Johnson next set about determining how stuttering children differed from non-stuttering children when they first began to stutter. He wondered why all the theorists before him had looked only at adult stutterers and then grandly announced how stuttering began. It was curious that no one had thought it necessary to study the onset of stuttering.

Through the media he invited parents to bring for a thorough examination their children whom they considered to be stuttering. A control group child was found to match each experimental group child. Thus began an investigation of the onset of stuttering that was to continue for almost 30 years (Johnson, 1959).

From these investigations emerged the diagnosogenic theory, one that would dominate stuttering research and therapy for many decades. Johnson asserted that the child's troubles began not with a faulty nervous system but rather with a faulty diagnosis, usually by the parents. His interviews with parents convinced him that some parents have unusually high expectations for their children, particularly with regard to speech development. When the child fails to live up to their expectations, they communicate their concern to him in various ways, all of them harmful. The child soon senses their concern and begins to react in ways that are incompatible with normal speech production. He tries to avoid dysfluencies but ends up with increased tension, begins to doubt his ability to talk well enough to please his critical parents, and learns to anticipate and fear stuttering. He tries everything possible to keep from stuttering, such as pressing his lips together or holding his breath. Unfortunately, the techniques he adopts to reduce or avoid stuttering are ones that most interfere with talking and that others see as stuttering. Johnson maintained that stuttering was what you did to avoid stuttering. Once the child is convinced that he cannot talk normally and that there is something wrong with him, he quickly becomes enmeshed in the vicious circle of stuttering.

Johnson's theoretical formulations led to specific clinical strategies. If children were still in the early stages of stuttering, the therapist's role was to restrain and reeducate the over-enthusiastic parents. They were urged to ignore the children's dysfluencies, reduce their demands on them, respond to what they say rather than how they say it, and let them know that talking is fun.

For advanced stutterers with their panoply of struggle behaviors, such nostrums would be of little value. Johnson recognized this and set about developing therapy techniques consistent with his theoretical model. It was most fortunate for the profession that Charles Van Riper came to Iowa City seeking a doctorate a few years after Johnson. Although he left Iowa City in 1932 to establish a clinic at Western Michigan University, the two men collaborated a great deal in the development of many innovative techniques of therapy that together came to be known as the Iowa Method and are still in use today.

Perhaps the most interesting of Van Riper's voluminous contributions to the professional literature is his report on 20 years of experiments in stuttering therapy (Van Riper, 1958). He varied his therapeutic methods from year to year, kept

careful records, and used each fifth year to evaluate his program. His reports are fascinating to read and form the basis for what he would eventually present as a fully developed clinical program (Van Riper, 1973). Building on Johnson's work, Van Riper devised a method he called "fluent stuttering." He told his clients that if they were going to stutter they may as well learn to do it properly with as little effort as possible. To achieve this goal, his stutterers first learned to identify and confront their stuttering. As they gradually became desensitized to the emotional impact of stuttering, clients were directed to explore how they might vary their stuttering pattern gradually to approximate normal speech. Attitudes toward themselves and their stuttering were coaxed into the open, examined, and modified. Avoidance of feared situations was forbidden, and the stutterers learned to live with and control their unruly speech.

The first author experienced such a therapeutic regime at the University of Minnesota in the 1950s and can attest to its effectiveness, at least for some of the clients who attended the five-week intensive clinic with him. This author remembers with mixed emotions the first terrifying days in the clinic when, under the able direction of Dorvan Breitenfeldt, then a graduate student, he was required to make dozens of phone calls, engage total strangers in conversation, and arrange a date with the waitress in a local restaurant. The threatening nature of those assignments can be appreciated when it is understood that before the clinic this author had avoided phone calls, could not manage even the simplest verbal exchange with a stranger, and had never had a date. As the clients discovered that they could indeed complete such onerous tasks and survive, their confidence began to grow. Increasing confidence allowed them to experiment with and vary their stuttering patterns. Some clients made remarkable improvements in fluency and learned to accept and live with their stuttering. Unfortunately, there was also a group of clients who, although they made gains in self-confidence, experienced little improvement in the frequency and severity of their stuttering blocks. Furthermore, little attention was given to the possibility of relapse or to what clients might do to maintain their gains. This author experienced a total relapse eight months after completing the clinic but attended a second intensive clinic the next summer and eventually gained control over his stuttering.

In the late 1950s the leading edge of a tsunami hit the therapeutic community. Flanagan, Goldiamond, and Azrin (1958) published a paper in which they reported a successful attempt to modify stuttering behavior with operant conditioning techniques. This paper and the experimental therapies it spawned (Martin & Siegel, 1966a, 1966b) stunned the clinicians who had been nurtured by Johnson and Van Riper. These crazy behaviorists were actually punishing stutterers with electric shock, time-out, or saying "No" to the clients whenever they stuttered. How could therapists, who allegedly cared

about their clients, engage in such outrageous behavior when Johnson had clearly demonstrated that it was the punishment of dysfluencies by parents that had started the whole sorry mess in the first place? In one particularly heated exchange at a conference, Sheehan (1970) exclaimed that:

Life is too full of punishment to make it necessary or advisable to administer more of it in the clinic. Therapists who use punishment are probably incompetent to use anything else, or they have a neurotic need to assume the role of the punisher as a reassurance against their own fear of being in the role of the one punished [...] if punishment were in any way effective, every stutterer would have been cured in childhood. (p. 132)

The iconoclastic psychologist, Goldiamond, had not absorbed the conventional wisdom in speech-language pathology, nor had he accepted Johnson's premises and conclusions. When he encountered a stutterer, he simply wondered whether he could apply the principles of operant conditioning to this disorder and set out to investigate (Goldiamond, 1965). By the late 1960s the full force of the tsunami hit our beaches. Dozens of eager investigators rushed to their laboratories to count responses, devise complex schedules, and measure the effectiveness of reinforcing stimuli. At last, real science had come to the clinic! No longer did we need to worry about attitudes and messy self-concepts that we could not see or measure. We now had an objective system to quantify clinical change and were confident that we would shortly play in the big leagues with the physicists. It was a heady time. Zealous behaviorists, well fuelled with the brew of therapeutic success, launched a tirade at the Van Riperians. They charged these "traditionalists" with failing to measure clinical change and with using methods that did not reduce the frequency of stuttering. Mowrer (1971), an enthusiastic behaviorist, published a program in which the clients started by reading a list of monosyllables. If they read a word fluently, they were reinforced and went on to the next word. If they stuttered, the word was repeated until they could produce it fluently. When they reached the pre-determined criterion on monosyllables, the clients would progress to multisyllabic words, two word phrases, sentences, paragraphs, and finally pages, with the reinforcement schedule adjusted accordingly. The clients then would be recycled through monologue and conversational modes.

In this and similar programs, data were gathered carefully and progress could be graphically displayed as the client worked through the program steps from Establishment through Transfer to Maintenance. And it worked! Results were often dramatic and enthusiastically reported at conferences and in journals. It was wonderfully gratifying to report clinical results in terms such as the following for a hypothetical client, J.S.:

On pre-treatment measures client J.S. stuttered on 64% of total syllables with a speaking rate of 42 syllables per minute. On posttreatment measures the percentage of stuttering had dropped to 1.8% and rate had increased to 185 syllables per minute.

Many clinicians preferred this type of report to the more subjective statements of clinical progress that were typical of the avoidance reduction therapies.

Charges and countercharges swirled throughout the 1970s and tended to separate people into two camps. The behaviorists, waving graphs and tables with neat rows of numbers, emphasized the importance of changing behavior and focussed their energy on what could be observed and measured. Although most behaviorists acknowledged that stutterers had feelings and fears about stuttering and about themselves, it was generally agreed that since these internal states could not be observed or measured, they should not be addressed in therapy. Moreover, it was argued that if the therapist succeeded in altering overt stuttering behavior, the internal conditions would probably correct themselves. Correcting the speech pattern was the most effective way of diminishing anxiety about stuttering. It was a persuasive argument in view of the research findings indicating that stutterers were psychologically normal and that if abnormalities existed they were likely to be a result and not a cause of stuttering (Goodstein, 1958; Bloodstein, 1987). The behaviorists argued forcefully that the task of the clinician was to design a systematic program based on operant procedures, one that could countercondition the stutterer's inappropriate and unadaptive stuttering behaviors by instating or increasing fluent speech behaviors.

For a few years during the 1970s, it appeared that the behaviorists had triumphed, and the Van Riperians had conceded defeat. It was not so: They were merely regrouping, awaiting long-term results from the behavioral clinics. The results were not long in coming, as the behaviorists were committed to collecting and publishing data. In a wonderfully frank update of his S-1 program, Mowrer (1975) described the long-term results as far from satisfactory and admitted that he was not "overjoyed" with them. Many of the clients had relapsed. The enthusiastic claims for lasting success with easily administered programs such as Mowrer's S-1 appeared to have been premature. Such reports were greeted by the Van Riperians as validating their criticism of what they saw as simplistic behavioral approaches. Van Riper (1973) declared that rate control therapy was "another hoary old method for producing temporary fluency" that had been "exhumed from the grave where it belongs" (p.81). Cooper (1987) talked and wrote extensively about the fallacy that "the single most reliable and valid measure of stuttering severity is dysfluency counts" (p.126). Cooper, Sheehan, and many others in the Van Riper camp insisted that the assessment and treatment of

stuttering must recognize and address the cognitive and affective aspects of the disorder.

Gregory (1979) recognized the futility of these unfocussed charges and countercharges and invited prominent clinical scientists from both camps to state their case in writing. He divided the contributors into those who advocated helping the stutterer to "stutter more fluently" and those whose goal for the stutterer was to "speak more fluently." In this excellent book the issues were clarified, rationales were presented, and arguments were advanced in a form that helped readers understand the different perspectives more clearly and did much to defuse the hostility.

Another positive development in the late 1970s was the gradual recognition that we did not know much about what happens to the client after he leaves therapy. Boberg, Howie, and Woods (1979) reviewed the literature and reported that while most behavioral treatments produce dramatic increases in fluency, they also encounter serious relapse problems. The Van Riperians chuckled and said, "We told you so!" to which the behaviorists replied that if the Van Riperians collected post-treatment data systematically, they too would discover that many of their clients were experiencing similar difficulties.

There was indeed a dearth of published clinical reports that included useful information about post-treatment performance. In early editions of Bloodstein's handbook (1975, 1981), only a small percentage of the studies reviewed by him included any post-treatment results. Furthermore, those that did so often used vague terms such as "improvement maintained" without any description of how and when the post-treatment measures were obtained.

The issue of maintenance was addressed seriously at the first Banff International Conference on Stuttering held in 1979. A group of clinical scientists, including R. Martin, W. Perkins, R. Ingham, B. Ryan, N. Owen, and G. Shames, debated why stutterers tended to relapse after successful therapy, what could be done about it, and how the results might be measured reliably (Boberg, 1981). The conference focussed attention on the need for careful measurement of post-treatment performance and for meticulous planning and monitoring of maintenance procedures.

Another fascinating development during this period emerged during the Banff Conference. This was the reference by several key speakers, those generally associated with the behaviorist approach, to unobservable and unmeasurable events such as feelings, motivation, attitudes, and interpersonal relationships. We were entering a period in which individuals from both camps seemed to realize that the issues that separate the two approaches are few, while the concerns that

draw them together are many. The tsunami had passed and the schism between the two groups was narrowing. We had discovered that learning from each other might be more fruitful than insulting each other.

Throughout the 1970s several clinical scientists were beavering away in their laboratories looking for physiological differences in stutterers, while others were elaborating and evaluating behavioral therapies. There was a growing recognition that for some stutterers the early operant programs did not produce lasting changes and that the programs would need to be further developed and expanded.

Australians played a key role in these developments. Working at first with Gavin Andrews, Roger Ingham developed and evaluated a large number of treatment components and combined them in an innovative and effective therapy program. This pioneering work included such significant elements as on-line monitoring of clients' speech rate by means of electronic counting equipment, application of systematic techniques to transfer and maintenance, and the rigorous evaluation of treatment effectiveness (Ingham & Andrews, 1973).

Many others contributed to behavioral treatment methodologies, notably Martin, Kuhl, and Haroldson (1972), Perkins (1973a, 1973b), Perkins, Rudas, Johnson, and Bell (1976), Ryan (1974), Shames and Florance (1980), Shine (1980), and Costello (1983). Webster's *Precision Fluency Shaping Program* gained widespread use throughout the U.S.A. and Canada (Webster, 1974).

While the clinical scientists were developing behavioral technologies, other scientists were re-examining the age-old question whether stutterers differ physically from normal speakers. Investigators such as Conture, Freeman, Adams, Zimmermann, and Kidd studied physiological and genetic factors and reported significant differences in stutterers. Summaries of their work appear in the excellent volume edited by Curlee and Perkins (1984).

The research findings contributed to the gradual shifting of outcome expectations. Where the early behavioral programs had confidently set 100% fluency as an attainable goal (with the right program), many clinicians in the behavioral camp began to suspect that goal to be unattainable for some clients. Two avenues of investigation contributed to the shift in outcome expectation. The first was the mounting evidence from neuropsychological studies (Moore, 1984; Moore & Boberg, 1987), a line of research that hearkened back to Orton and Travis (1929) but with the benefit of new technology and procedures.

Moore's (1984) pioneering work demonstrated unusual hemispheric processing of speech in stutterers and was supple-

mented by subsequent investigations such as Boberg, Yeudall, Shopflocher, and Bo-Lassen (1983) and Webster (in press). Although the nature and location of these differences are not yet clear, there is a strong suggestion that the stutterer's brain mechanisms for speech processing are less efficient and/or more vulnerable than those of normal speakers. The third Banff Conference on Stuttering held in 1989 was devoted to exploring the neuropsychology of stuttering (Boberg, in press). The conference was the occasion for a lively discussion on the implications of the research for assessment and therapy. We expect this debate to continue for some years.

A second contributing factor was the accumulating evidence from the maintenance studies noted above. Many clients, blithely ignoring the expectations of 100% fluency that had been set for them, were trying to deal with residual stuttering during the maintenance period. As clinicians became more careful about collecting valid post-treatment data, it became increasingly evident that many clients simply could not achieve full fluency. This was not for lack of trying. Clinicians wondered if the assumed underlying neurophysiological conditions continued to operate in such a way as to interfere with efficient speech production, particularly when the underlying variables acted in concert with the accumulated effect of the negative experiences and attitudes characteristic of advanced stuttering.

By the 1980s many of us were persuaded by the evidence from the genetic, physiological, and neuropsychological studies, as well as the post-treatment data, that the stutterer was different in some important ways. As Alexander Melville Bell had advocated in the 19th century, we were indeed learning more about the instrument and slowly recognizing that the stutterer has a different instrument; a slightly altered flute. Although the flute might play flawlessly at times, at other times the keys might stick.

Along with many other clinicians, we recognized that our therapies must accommodate the flawed flute on which many of our clients were playing. We can best illustrate the influence of the research findings by referring to the development of our own clinical program (Boberg & Kully, 1985). In devising the program we tentatively assumed a decreased capacity to program and execute the motor components of speech at normal rates. Drawing on the research by Perkins and Ingham, cited above, we arranged training of fluency skills within the framework of prolongation. Recognizing the importance of building self-confidence, improving social skills, changing attitudes, and reducing avoidances (Van Riper, 1973), we included clinical components that would address these needs. Finally, assuming that many clients would need to deal with residual stuttering, we included training in self-management along with suggestions and procedures for use after therapy.

We believe that other programs also are reflecting this move toward a more comprehensive treatment of stuttering. For example Webster and Poulos (1989) apply principles of cognitive psychology in their manual to enhance the long term outcome of behavioral programs such as the PFSP (Webster, 1974) and the CSP (Boberg & Kully, 1985).

As we struggle to incorporate new findings into expanded clinical procedures, we sometimes liken ourselves to a salad chef. We have added many ingredients to our salad because we suspect that they are nutritious. Unfortunately, we do not yet have hard evidence that any one ingredient is essential or even helpful, and we suspect that this applies to most other clinical programs. Although the clinical outcome of many current programs is very encouraging (Howie & Andrews, 1984; Boberg & Kully, 1988), we are urgently in need of rigorous testing to determine how and why any particular technique works.

We will conclude this paper by noting current trends that we believe are significant. The first of these is the move toward early intervention in children. The tenacious grip of the diagnosogenic theory on stuttering therapy began to loosen in the 1970's as it became apparent that the evidence did not support its basic premise (Andrews, Craig, Feyer, Hoddinott, Howie, & Neilson, 1983). Curlee (1984) reviewed the literature on early childhood disfluency and concluded that "...the data indicate that there is little overlap between most nonstutterers and most stutterers" (p.237). Guidelines to assist in differentiating between normal nonfluency and incipient stuttering were published by Adams (1977) and Gregory and Hill (1980).

The new evidence and some early experimental and case studies (Ryan, 1971; Martin et al. 1972; Reed & Godden, 1977; Johnson, Coleman, & Rasmussen, 1978) led to the development of a wide variety of behavioral and non-behavioral treatment programs, varying in their degree of directness, but still advocating some type of early intervention. Examples of such programs would include Gregory and Hill (1980), Johnson (1980), Shine (1980), Costello (1983), Riley and Riley (1983), Cooper (1984), Heinze and Johnson (1985), Rustin (1987), and Fraser and Avery (1987). This trend toward early intervention was perhaps most clearly manifested in the 1981 conference on Treatment of Stuttering in Early Childhood (Prins & Ingham, 1983). Prins and Ingham, in their summary chapter, point out that the practice of ignoring early stuttering has persisted in spite of no clear evidence for this position and considerable evidence against it. They forcefully state that "when a young child shows persistent and unusual signs of disfluent speech ...intervene—directly with the child and with the parents" (p 145).

A second trend is the emergence of stuttering specialists working in specialized treatment centres. As the treatment for

stuttering becomes more complex and comprehensive and as therapy is more often provided within an intensive format, it seems inevitable that clients will gravitate to centres that offer flexible scheduling and highly experienced clinicians. These centres frequently employ several staff members who may combine clinical and research activities. This arrangement provides a context in which clinical issues such as treatment efficacy can be addressed. Moreover, the large client population provides a subject pool for research into the nature of stuttering. In Canada such major centres have developed in Ottawa, Toronto, Montreal, and Edmonton, and it is likely that other centres will emerge in future.

The last trend we will note is the formation of self-help groups. Perhaps the most notable example is the almost explosive growth of the National Stuttering Project in the U.S.A. under the dynamic leadership of John Ahlback. Similar groups have been formed and are steadily expanding in Australia, Britain, Canada, Germany, Japan, and Scandinavia. We expect that these groups will play an increasingly important role in the rehabilitation process and provide an excellent forum through which stutterers can express their needs. We hope that speech-language pathologists and self help groups will work together effectively to improve treatment and foster public understanding of stuttering.

Finally, we sense that the *Zeitgeist* is shifting ground again. For several decades we looked principally to the environment for clues to understanding and treating the stutterer. With the exciting research findings now flowing from neuropsychological and physiological laboratories, many of us have shifted our focus and are once again looking within the stutterer. The findings so far are enormously exciting and lead us to hope that continued investigations will reveal still more about the nature of stuttering. We might also expect that as we gain more precise information about the neuropsychological events associated with changes in speech performance, we may develop a sound basis for choosing one treatment technique over another. The possibilities are intriguing. However, in order to avoid another tidal wave—biological explanations for all aspects of stuttering—we will need to keep a broad perspective and remember that stuttering is indeed a multifaceted and complex disorder.

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