The "Future of Science and Services" Seminar

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THE "FUTURE OF SCIENCE AND SERVICES" SEMINAR

Edited by
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PREFACE

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The importance of research and the scientific aspects of a discipline cannot be overemphasized. Communication sciences and disorders is no exception. Without applying research knowledge and scientific advances we cannot improve our professional practice. Without research, our discipline will suffer from "a failure to thrive."

The importance of science is recognized by the American Speech-Language-Hearing Association (ASHA), both historically in its mission statement and currently in its Long Range Plan, and by its current president, Roy A. Koenigsknecht, whose theme is one of rededication and renewal. A seminar focused on the future of science and services is especially timely to ensure ASHA's continued interest in research in the future.

A National Office committee comprised of representatives of the Professional Affairs, Professional Practices, and Public Information Departments planned such a seminar over the course of a year. To represent researchers at different stages in their research careers as well as the three major areas of scientific endeavor—hearing, language, and speech—were the foundation features upon which the program was built.

The "Future of Science and Services Seminar," held on October 23, 1990, was part of the dedication events of the expansion of the National Headquarters of ASHA in Rockville, MD. The seminar was attended by 100 invited guests, who represented a variety of affiliations with research and service, including past and current members of ASHA's governance structure (Legislative Council members, Presidents, Executive Board members, ASHA Committees), National Office staff members, State Association Presidents, ASHF representatives, federal agency dignitaries, professional association representatives, university faculty, and service providers, among others.

Two researchers each in hearing, language, and speech, whose doctoral degrees had been earned within the last 10 years, made presentations in their respective areas addressing the future and current research trends, with an emphasis on the future. As part of their charge, they were asked to include the impact of research on clinical practice. Each pair of these researchers was followed by a career scientist, whose presentation discussed the two previous papers. It is these 9 papers that comprise this *ASHA Report.*

A publication of this scope cannot be produced without the dedication and cooperation of many individuals. From the President of the Association to the employees in shipping, this product is truly the outcome of a well-synchronized team. The ASHA President, Executive Board members, Executive Director, and numerous National Office staff members from the Professional Affairs, Professional Practices, Public Information, and Administrative Departments combined their efforts to produce a timely and quality product.

Just as the seminar generated interest, discussion, and enthusiasm for the participants and audience, it is hoped that this volume will do the same for the reader. It is further hoped that this positive energy can be transformed into scientific activity. Research ahoy!

Acknowledgments

This ASHA Report represents the combined efforts of many people. Of course, we wish to express our gratitude to all speakers at the seminar. We also gratefully acknowledge the individuals who assisted in selecting speakers for the seminar and in reviewing papers for this report.

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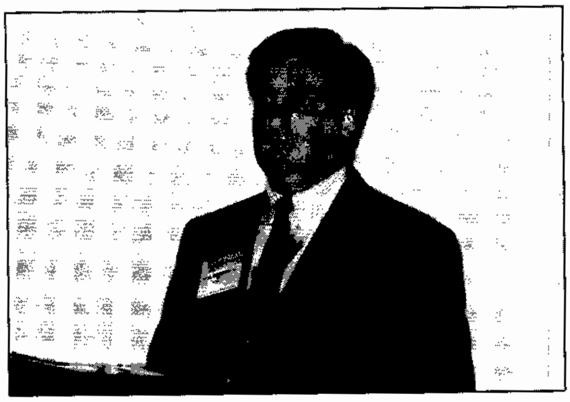
CONTENTS

SCIENCE AND SERVICES—EMBARKING ON THE FUTURE



ROY A. KOENIGSKNECHT
American Speech-Language-Hearing Association
Rockville, MD

Thank you for joining us for the "Future of Science and Services Seminar". Today's examination of the possibilities that tomorrow offers to our professions holds particular meaning for me. Research and service are important cornerstones for our discipline. Research provides new knowledge about communication and its disorders, knowledge that we can apply to improve our service to individuals with communication disorders.

As we prepare for tomorrow's dedication of the expansion of our headquarters building, we cannot help but think of those dedicated individuals who paved the way for us. Who in 1925 would have predicted that the tiny organization for "speech correctionists" would, by 1990, have blossomed into our 62,000 member-strong, internationally known, universally respected American Speech-Language-Hearing Association (ASHA)?

To those men and women of foresight and vision, we owe a great debt of gratitude. Some of the pioneers of speech, language, and hearing disorders are with us today; others will join us tomorrow. Many are my own long-time personal heroes.

We are also indebted to the present-day pioneers of our professions who are helping us prepare for the future by making us stop and consider where we are today and where we are headed tomorrow—to predict the unknown—the future. These pioneers, our speakers today, will help us to make that evaluation. We know that a chief characteristic of prediction is error. All of our predictions will not come to pass. Others, not anticipated, will. However, the journey will be an exciting one.

In addition, we thank Dr. Ted Glattke, Vice President for Education and Scientific Affairs, for guiding the development of today's seminar, and Dr. Cynthia Shewan, director of ASHA's Research Division, Dr. Stan Dublinske, director of our Professional Practices Department, and Dr. Russ Malone, director of our Public Information Department, for taking the concept of today's seminar and making it a reality.

"There is nothing more difficult," said Machiavelli, "to take in hand, more perilous to conduct, or more uncertain in its success than to take the lead in the introduction of a new order of things." Those words come from the past, but we know them to be timeless and true.

So why does ASHA dare to undertake such an unpredictable exercise? Why peer into the future and prognosticate? Why speculate what innovations we will devise to cope with the changes that will inevitably arise?

The answer is, simply, because we must.

Our beautiful building expansion did not just appear miraculously complete. Hours and hours of professional time went into planning, refining, and adjusting. Plans were drawn, blueprints drafted, and a foundation poured. Yes, unanticipated events shaped the final construction as well, but, the point is, the planning and preparation were imperative to the successful completion of the project.

Similarly, we must look at our environment, our society, the realities of communication disorders and their treatment, and plan for their evolution. Today, we will hear from some of the finest minds in our field, their scientific thinking on this evolution. We will listen, as they describe new areas of research that portend to change both the scope and nature of our professional practices. We will hear about advances in technology that hold unprecedented promise for our research and our clients. We will consider options for the practice of speech-language pathology and audiology that heretofore seemed distant or even impossible. We will listen as the visionaries describe our country's rapidly changing demographics and how they will affect service delivery. We will explore possibilities and potentials of what will be a very exciting journey into the new frontiers of communication sciences.

You and I will witness with our own eyes some of these developments. As thrilling as research is, we must also be mindful of our fundamental constituency—those individuals with communication disorders. Current and new disorders represent the challenges whose solutions will ensure for us a future as glorious as our history.

Whatever transpires and however we respond to future changes, we will remain professions committed absolutely and without compromise to the highest standards of service and ethics. To serve our clients well we must apply research knowledge to service delivery issues. Many of our professionals are on the front lines as clinical service providers. We have a reputation for exemplary service. We truly make a difference in the lives of the infants, children, and adults with communication disorders. We are the ones who keep children learning in school, who return the disabled to independence, who uphold the basic human dignity of communication.

Welcome to the "Future of Science and Services" seminar and to what portends to be a very exciting day.

RESEARCH PAPERS: A. HEARING

RESEARCH DEVELOPMENTS IN ACOUSTICAL AND PHYSIOLOGICAL MEASUREMENTS AFFECTING THE PRACTICE OF CLINICAL AUDIOLOGY

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I have been asked to describe areas in which research efforts in audiology and hearing science might potentially have an impact on clinical service. In the space available, it is not possible to describe all areas in audiological research that might alter the practice of audiology. As a consequence, I have chosen to focus on just a few areas of research which are concerned primarily with physiological and acoustical measurements of peripheral auditory function. Dr. Jerger will discuss research endeavors that are more directed toward behavioral measurements, with greater emphasis on central auditory function than will be evident in my paper.

In an effort to provide a feel for the breadth of work that currently is being conducted, I will describe some areas in a relatively superficial manner. In some cases, I will try to highlight how technological breakthroughs have altered clinical service. The routine application of auditory brainstem response measurements, the use of cochlear implants, and the application of real-ear probe-microphone measurement systems would fall into this category. For example, much, if not all, of the research that was needed in order to apply ABR techniques to clinical questions is now complete. None of these data were available, however, as recently as 20 years ago. Although the research and clinical database is less developed for cochlear implants, it is clear that these devices represent an important rehabilitative achievement for some hearing-impaired patients. Some important questions remain unanswered, however, including candidate selection, especially for prelingually deafened infants and young children. Real-ear probe-microphone measurement systems provide the opportunity to measure the actual sound pressures developed by hearing aids near the eardrum of individual patients. However, information is still needed regarding the perceptual consequences of peripheral hearing loss if appropriate amplification is to be selected.

I will also describe some advances that have impacted on the scope of practice in audiology. Although identification, diagnosis, and rehabilitation of hearing loss remain primary responsibilities of audiologists, expansion into areas such as intraoperative monitoring and vestibular assessment and rehabilitation has radically changed the scope of practice within the profession.

Finally, I will describe a number of areas in which additional research is needed in order to realize the clinical potential of new developments. Otoacoustic emissions and hearing-aid applications of digital signal processing represent two areas falling into this category. This list of areas is not meant to be exhaustive, but was selected to represent some of the ways research and technology have, are, and will continue to influence the practice of clinical audiology.

Given the broad scope of the charge for this discussion, I felt compelled to ask some of my colleagues for input in areas with which I am less familiar. While I accept full responsibility for statements made in this paper, I also would like to acknowledge some of my colleagues who had important input. They include Kathryn Beauchaine, Arlene Carney, David Cyr, Walt Jesteadt, Mary Joe Osberger, Donel Sinex, and Patricia Stelmachowicz.

AUDITORY BRAINSTEM RESPONSES

The first topic I would like to discuss is auditory brainstem response (ABR) measurements. This section will be the longest because it is an area with which I am most familiar and about which the most is known. The development of digital technology in the early 60s provided auditory physiologists with the opportunity to examine quantitatively the responses of isolated primary auditory neurons. A voluminous literature has developed, describing response latencies, thresholds as a function of frequency, adaptation and recovery, discharge synchronization or phase locking, spontaneous and saturation rates, speech encoding, and dynamic range (e.g., Delgutte, 1984; Delgutte & Kiang, 1984a,b,c; Evans, 1974; Harris & Dallos, 1979; Javel, 1986; Johnson, 1980; Kiang & Moxon, 1974; Kiang, Watanabe, Thomas, & Clark, 1965; Liberman, 1978, 1984; Liberman & Dodds, 1984a,b; Liberman & Kiang, 1984; Rose et al., 1967; Sachs & Abbas, 1974; Sachs & Kiang, 1968; Sachs & Young, 1979; Smith, 1977, 1979; Smith & Zwislocki, 1975; Sinex & McDonald, 1988, 1989; Young & Sachs, 1980). Descriptions of these properties are now available for both normal and impaired peripheral auditory systems. For example, detailed descriptions of the neural correlates of hearing loss due to ototoxicity and noise exposure are now available (e.g., Dallos & Harris, 1978; Kiang, Liberman, & Levine, 1976; Liberman, 1984; Liberman & Dodds, 1984a,b; Liberman & Kiang, 1984).

Even before single-unit recordings in animals were routinely possible, efforts were underway to provide better understanding of gross electrical potentials recorded from around the ear (Davis, Fernandez, & McAuliffe, 1950; Peake & Kiang, 1962; Teas, Eldredge, & Davis, 1962). Indeed, the earliest physiological measurements of auditory function in animals relied on gross potential recordings because the technical advancements needed for single cell recordings had not as yet occurred (Derbyshire & Davis, 1935; Wever & Bray, 1930). This process was significantly accelerated by efforts to understand the relations between these far-field recordings and what was fast becoming known about peripheral response properties through single-unit recordings (e.g., Antoli-Candela & Kiang, 1978). Although single-unit recordings are not feasible in humans, it was recognized that gross potential recordings could be performed with humans, and that such recordings might have considerable clinical value in identification and diagnosis of hearing loss.

Initial efforts to measure gross electrical activity from the auditory system were devoted primarily towards measurements of responses from the cochlea and the eighth nerve, including the summating potential (SP), cochlear microphonic (CM), and whole nerve action potential (AP). The reader is referred to any of a number of reviews for a comprehensive description of these potentials (Dallos, 1973; Moller, 1973; Pickles, 1982). SP and CM are the responses from sensory cells within the cochlea, with the CM following the stimulus and the SP following a rectified version of the stimulus waveform. For this reason, CM and SP, respectively are referred to as the AC and DC responses of the cochlea. The AP represents the complex addition of discharges from a group of neurons within the auditory nerve (Antoli-Candela & Kiang, 1978). This entire class of measurements is often referred to as *electrocochleography* (ECochG). Fairly well developed responses were observed when recording electrodes were placed close to the cochlea, such as on the promontory or near the round window. However, placement of these invasive, transtympanic electrodes in humans required a physician and was not without risk, which limited the clinical utility of the technique. External ear-canal electrodes were tried without satisfaction due to problems with comfort, electrode impedance, and response reliability (e.g., Coats, 1974). It should be recognized that while problems associated with electrode placement and design have resulted in clinical ECochG being almost entirely replaced by ABR measurements for estimates of peripheral hearing sensitivity, newer electrode designs and other applications have resulted in renewed interest in the technique.

In 1967, it was noted that gross neural responses could be recorded from surface electrodes placed around the ear (Sohmer & Feinmesser, 1967), and in 1971, Jewett and Williston provided the now classic description of what is currently known as the ABR. Noninvasive, neural response measurements were possible in humans if one exploited the signal (response)-to-noise improvements that were possible through the combined use of low-impedance surface electrodes, differential amplification, and signal averaging. In the nearly 20 years following that report, it has been demonstrated that the clinical applications of the ABR include early identification of hearing loss and assessment of the integrity of neural pathways in lower portions of the auditory nervous system. In an effort to complete the circle from laboratory experimentation to clinical application, let us now briefly review some of the ways ABRs are used clinically.

The ABR test represents the single most reliable procedure for the early identification of hearing loss in neonates, infants, and other difficult-to-test patients. There are many reports describing the application of this test with high-risk infants, such as those graduating from intensive care nurseries (ICN) (e.g., Galambos & Despland, 1980; Galambos, Hicks, & Wilson, 1984; Dennis et al., 1984; Durieux-Smith et al., 1985; Gorga et al., 1987). Indeed, ASHA now recommends that this test be used to identify hearing loss in children under 6 months of age (ASHA, 1989). Although there are some reports of unexpectedly high failure rates among these patients (Roberts et al., 1982; Shimizu et al., 1985), these failure rates are not being reproduced in other facilities. Most clinical facilities using the ABR in the ICN report failure rates of about 10–15% (Galambos et al., 1982; Hyde et al., 1984; Jacobson & Morehouse, 1984), and some as low as 4–5% (Gorga, Kaminski, & Beauchaine, 1988; Schulman-Galambos, & Galambos, 1979). Obviously as our skill at performing the test improves, so that we avoid or at least account for such problems as ear canal collapse, testing in noisy environments, or testing very sick babies not ready for discharge, false-positive rates should be reduced.

Reports also exist describing the use of the ABR with other difficult-to-test patients (e.g., Stein & Kraus, 1985). The difficulty in assessing multiply handicapped and/or developmentally delayed patients using behavioral test techniques are recognized by any clinical audiologist who has been asked to evaluate hearing sensitivity in these individuals. Although it is typically more difficult to assess hearing sensitivity in developmentally delayed patients using behavioral methods, the incidence of hearing loss is greater among these patients compared with developmentally normal children and adults (Goudreau et al., 1989; Karchmer, 1985). The ABR has been used to test the hearing sensitivity of these patients because it does not require active participation and therefore can be used while the patient is either in natural or sedation-induced sleep.

The relation between click-evoked ABR thresholds and behavioral thresholds is well understood. Clearly, the click-evoked ABR threshold correlates best with behavioral thresholds in the 2,000 to 4,000 Hz region (Gorga et al., 1985; Jerger & Mauldin, 1978; van der Drift, Brocaar, & van Zanten, 1987). As our ability improves to generate stimuli having temporal characteristics that will elicit an ABR and yet still have narrowly defined spectra, it is likely that we will be able to use this technique to provide evoked potential equivalents of the pure-tone audiogram (e.g., Gorga & Thornton, 1989). It is important to recognize, however, that the vast majority of hearing losses include those frequencies to which the click-evoked ABR is most sensitive, and, in most instances, information regarding hearing in only these frequencies will provide important information in the habilitation of the patient.

Although there are isolated reports that the ABR may overestimate hearing loss in some patients, in particular multiply handicapped patients (e.g., Worthington & Peters, 1980), the occurrence is very rare. In our facility, we experience a very low false-positive rate, estimated to be no more than about 0.1%. More importantly, when the ABR errs, it is always in the direction of estimating more hearing loss than actually exists (i.e., false positives). We know of no cases when the ABR indicated that hearing was normal and yet significant peripheral hearing loss existed (false negatives). Indeed, it is impossible to contrive a scenario in which the ABR would indicate hearing is normal and a peripheral hearing loss is present. Thus, the ABR can be used accurately to assess hearing sensitivity, and when it errs (which is very infrequently), the direction typically is to over-estimate hearing loss. The long-term consequences of missing an existing hearing loss are far greater than the consequences of intervening with a child who is later found to have less hearing loss than originally suspected.

A second major application of ABR techniques is as part of an overall otoneurologic assessment. The ABR consists of a number of waveform components that are generated at progressively higher levels of the auditory brainstem. Although determining the exact generator sites may be difficult, it is clear that the first component of the response (wave I) comes from primary afferent fibers and that the most robust component in the response (wave V) comes from brainstem structures, probably the pathway of the later lemniscus (Moller & Janetta, 1985). Neurologic diseases affecting portions of the pathway leading from the cochlea into the brainstem typically alter the latency of the later components of the response. Indeed, ABR measurements of wave V latency and interpeak latency differences probably represent the most sensitive tests that can be performed by audiologists as part of an otoneurologic assessment. It clearly is more sensitive than other tests, such as acoustic reflex threshold and decay, tone decay, and high-level speech discrimination, for identifying neuropathy affecting auditory brainstem pathways (Turner, Frazer, & Shepard, 1984).

The use of evoked potential techniques has expanded the scope of practice in audiology. Audiologists now are routinely going into the operating room to measure both auditory and facial nerve function during such procedures as removal of acoustic neuromae and vestibular nerve sections. If the tumor is sufficiently small, the audiologist may be asked to provide feedback to the surgeon so that hearing may be preserved during the removal of the tumor (Kemink et al., 1990). At the very least, monitoring facial nerve function (even when hearing must be sacrificed) has resulted in a significant reduction in the incidence of facial nerve paralysis following surgery (Kartush, 1989; Kileny & Niparko, 1988). As audiologists, we must decide whether we want to be involved in these applications, or whether we wish to leave these applications of evoked potential measurements to other professions. These are questions related to the scope of practice and must be addressed so that training programs can provide educational experiences that are appropriate for the profession.

One application of ABRs that has been less successful is their incorporation into the hearing-aid selection procedure. Some reports suggest that a comparison of the ABR with and without amplification can provide information useful in the selection of hearing aids (e.g., Hecox, 1983; Mahoney, 1985). However, our own data suggest that ABR measurements with and without amplification do not provide accurate information regarding hearing-aid frequency response and, at best, provide limited information about high-frequency gain (Beauchaine et al., 1986). Furthermore, the effects of nonlinear signal processing, which is incorporated into many hearing aids, often cannot be assessed accurately by measuring hearing-aid responses with the rapid-onset, short-duration stimuli that are needed to elicit ABRs (Gorga, Beauchaine, & Kaminski, 1987). The fact that the ABR is not being used routinely in this fashion suggests that clinicians have not found it valuable in the process of choosing among different hearing aids. We have suggested that a more effective approach would be one in which the ABR is used to provide a comprehensive description of the magnitude and configuration of hearing loss, following by hearing aid prescriptive decisions based on real-ear measurements of hearing aid response (Beauchaine & Gorga, 1988).

One fact should be obvious to anyone charged with the early identification and habilitation of hearing loss: the routine availability of ABR measurements means that the peripheral auditory sensitivity of any patient, regardless of age, developmental level, or intellectual skill, can now be assessed very accurately. Although these technological advancements provide us with a means to assess any patient, economic and logistical problems remain which prevent us from systemically evaluating even those infants at risk for hearing loss.

REAL-EAR MEASUREMENTS OF HEARING AID CHARACTERISTICS

The development of efficient, objective, and relatively inexpensive probe-tube microphone systems has altered dramatically the way hearing-aid evaluations are performed. These systems have many advantages. They enable us to measure the actual sound pressure developed in the ear canal of individual patients. They typically provide information at 50 or more frequencies in a very short period of time. We are now able to evaluate efficiently and compare real-ear responses of multiple hearing-aid configurations with excellent resolution in the frequency domain. These measurements also take into account individual differences in ear canal size, input impedance, and earmold configuration (Libby & Westermann, 1988). Although the first generation of real-ear measurement systems used only pure-tone or narrow-band stimuli, newer versions employ a variety of complex signals, and the analysis is accomplished in real time. This expanded capability has not only improved speed, but allows hearing aid performance to be evaluated with signals that are similar to those encountered in daily life. In this fashion, the audibility of speech with a particular hearing aid can be quantified more easily, allowing for efficient adjustments to the hearing aid.

Prior to the development of this technology, decisions regarding hearing-aid selection were based on either measures of speech perception, functional gain, electroacoustic characteristics in standard couplers, or subjective preferences. Measures of electroacoustic characteristics in standard couplers have the advantage that they do not require patient cooperation; however, the range of differences between these measures and real-ear responses in both children and adults can be quite large (Feigin et al., 1989; Nelson Barlow et al., 1988; Sachs & Burkhard, 1972). Although behavioral test measurements in some combination may provide useful data for cooperative hearing-impaired patients, they generally are not applicable with difficult-to-test patients. With the development of these probe-microphone systems, we at least can measure the sound pressure developed in individual ear canals with the same degree of precision in difficult-to-test patients as would be possible in cooperative patients because we are no longer forced to rely solely on behavioral responses. However, for some uncooperative patients, it may be necessary to perform these procedures during sedation-induced sleep.

Even though the hearing-aid selection and evaluation process has been improved by the development of these systems, other questions remain unanswered. For example, it is important to know the extent to which performance can be predicted from these acoustic measures. Thus, work is required that describes the perceptual consequences of various acoustic manipulations for hearing-impaired patients. Obviously, this work will be facilitated by our ability to specify precisely the acoustic input to the ear. We now have the capability to examine a number of hearing-aid response characteristics in the real ear, including harmonic or higher order distortion products and input-output functions. It remains undetermined whether any or all of these measures will increase our ability to remediate hearing loss through amplification. Finally, we are moving to a point when real-ear probe-microphone systems will be used in conjunction with digital or quasi-digital amplification systems. The audiologist will select "optimum" hearing-aid characteristics, and, through a series of iterations, real-ear responses will be measured until this ideal response is achieved. In order to be successful, however, it is essential to know what hearing-aid characteristics result in optimum performance for patients having hearing losses which differ in terms of magnitude and configuration, and which may differentially alter percepts of loudness and speech. The importance of knowing optimum conditions for individual patients will also be discussed in the next section.

DIGITAL SIGNAL PROCESSING

The continued development of digital signal processing and its application to hearing-aid development is another example of how technological advancements have affected the clinical practice of audiology. Currently, there are a number of wearable systems available that provide advanced processing such as noise suppression, programmability, and multiple memories which allow the user to vary the frequency response depending on listening conditions (see Sammeth, 1990, for a review). In short, it is now possible to provide hearing-impaired listeners with a hearing aid that provides essentially any response we desire. The problem, however, is determining which response properties will result in the best performance for an individual listener. For example, some patients appear to benefit greatly from the use of compression amplification whereas others do not (Braida et al., 1979; DeGennaro et al., 1986; Laurence et al., 1983; Moore et al., 1985; Nabelek, 1983; Villchur, 1973; Yannick et al., 1979). Noise suppression seems to assist some patients, but results in no improvement (and sometimes reduced performance) for other patients (Brey et al., 1987; Stein et al., 1989; Van Tassell et al., 1988). One goal of audiological research might be to develop techniques that enable us to identify specific characteristics in individual patients that will enable us to select the appropriate response properties. It appears that the extent to which optimum performance is achieved is no longer determined by instrument limitations but by our inability to describe completely the consequences of peripheral hearing loss.

Assuming that it becomes possible to describe optimum response characteristics for individual patients and to achieve these characteristics through digital signal processing, a paradoxical situation will likely exist. Post-auricular hearing aids, which are of sufficient size to accommodate digital signal processing, are being replaced with in-the-ear or in-the-canal hearing aids. The application of digital technology will not be possible with these smaller devices for many years, due to limitations in size and power consumption needs. If digital hearing aids really do provide better performance, then the public will need to be convinced that cosmetic considerations are less important and that they will need to wear larger instruments in order to minimize any handicapping effects of peripheral hearing loss.

OTOACOUSTIC EMISSIONS

Ever since Kemp (1978) demonstrated that the ear, in its normal state, re-emits sound, there has been a considerable effort to understand basic properties of otoacoustic emissions (OAE) and to determine the extent to which these phenomena can be used clinically. OAEs occur as a byproduct of underlying nonlinear response properties within the cochlea, and the source of these nonlinear behaviors is presumed to be the outer hair cell (OHC) system.

It is not generally accepted that the auditory system is inherently nonlinear in its normal state (Goldstein & Kiang, 1968; Kim, Siegel, & Molnar, 1979; Neely et al., 1988; Sachs & Kiang, 1968). That is, active, nonlinear processes are necessary in order to achieve the low thresholds and sharp frequency selectivity evident in both psychophysical and physiological responses. One consequence of cochlear hearing loss (involving damage to outer hair cells) is the loss of this normal nonlinear behavior (Dallos et al., 1980; Kim, Siegel, & Molnar, 1979). Nonlinear phenomena, present in normal ears, such as two-tone suppression and combination tone generation, are absent when there is outer hair cell damage. Although it is sometimes stated that hearing loss results in "distortion," it would be more accurate to characterize hearing loss as altering the normal nonlinear processing of acoustic signals. In fact, hearing loss tends to make the peripheral auditory system more linear and, by definition, less distorting. Because OAEs are a byproduct of normal nonlinear behavior, changes in the auditory system that cause damage to the nonlinear mechanisms should result in loss or alteration of OAEs. Because the outer hair cell system is believed to be the source of nonlinear behavior, in a strict sense, we can think of OAE measurements as tests of outer hair cell function. However, because of the high correlation between OHC damage and hearing loss (e.g., Dallos & Harris, 1978; Kiang, Liberman, & Levine, 1976; Liberman, 1984), it is easy to see why an absence of OAEs may be taken as an indication of hearing loss.

OAEs fall into two broad categories: spontaneous and evoked. Spontaneous otoacoustic emissions (SOAE) are acoustic events measured in the ear canal in the absence of any stimulation. Their source is not well understood and they appear to be present in only about 40–50% of normal hearing subjects (Lonsbury-Martin, Harris, Stagner, Hawkins, & Martin, 1990; Probst et al., 1986; Strickland, Burns, & Tubis, 1985; Wier, Norton, & Kincaid, 1984; Zurek, 1981). Although there was some speculation that SOAEs might be objective manifestations of tinnitus, experiments designed to test the relationship between SOAEs and tinnitus have suggested that, in the majority of cases, these phenomena are unrelated for the most part (e.g., Penner & Burns, 1987). Given the large percentage of normal-hearing subjects who do not have SOAEs and the lack of a strong relation between SOAEs and tinnitus, it is unlikely that measurements of SOAEs will have clinical utility.

Evoked otoacoustic emissions (EOAE) are ear canal acoustic events that are the direct result of acoustic stimulation. Currently, there are a number of laboratories developing and evaluating the clinical utility of EOEAs, most notably groups in London, Kansas City, and Houston. We are also interested in assessing the clinical utility of these techniques. The two types of EOAEs holding the greatest clinical promise are transient (TEOAE) and distortion product (DPOAE) emissions.

Kemp and his colleagues have demonstrated that transient otoacoustic emissions (TEOAE) are routinely present in normal-hearing subjects and are typically absent in patients with hearing loss exceeding 30 dB HL (e.g., Kemp, Bray, Alexander, & Brown, 1986; Kemp, Ryan, & Bray, 1990). TEOAEs are elicited by either clicks or tone bursts, and occur several milliseconds after the stimulus. For this reason, they are sometimes referred to as *delayed TEOAEs*. Because TEOAEs occur in the presence of normal nonlinear function, one would predict the TEOAEs would be present in frequency regions of normal hearing and absent from regions of hearing loss. Thus, one could present a broad spectrum stimulus, such as a click, and spectrally analyze the TEOAE to determine the frequency regions in which reflected energy is observed. Hearing loss configuration then would be predicted from these spectra. However, there are many reports indicating that TEOAE energy for click stimulation is not always present in all regions of normal hearing, at least for adult subjects. In contrast, there is some evidence to suggest that more broadband TEOAEs are observed in the ears of neonates and young children (Kemp, Ryan, & Bray, 1990; Norton & Widen, 1990). It is also the case that TEOAEs can be measured easily in infants, resulting in the application of the technique as a primary screening measure for hearing loss in this group (Bonfils & Pujol, 1988; Bonfils, Uziel, & Pujol, 1988; Johnson, Bagi, & Elberling, 1983; Stevens, Webb, Hutchinson, Connell, Smith, & Buffin, 1990). The extent to which configuration of hearing loss can be predicted from a spectral analysis of the TEOAEs elicited by broadband stimuli such as clicks remains undetermined. However, because broadband TEOAEs are observed in young ears, it may be the case that spectral analysis provides more meaningful information related to configuration of hearing loss in young children than it does for adults. Although more work is needed, it appears that the sensitivity of TEOAEs to hearing loss, their ease of measurement in infants, and the speed with which they are measured may make the test an ideal screening method for hearing loss.

Investigators at Baylor College of Medicine described the basic properties of distortion product OAEs (DPOAE), including patterns of response for normal-hearing subjects as well as patients with sensorineural hearing loss (Lonsbury-Martin & Martin, 1990; Martin, Ohlms, Franklin, Harris, & Lonsbury-Martin, 1990; Martin, Whitehead, & Lonsbury-Martin, 1990; Ohlms, Lonsbury-Martin, & Martin, 1990). In these measurements, two different sinusoidal stimuli are presented simultaneously. When such a stimulus set is used, the normal auditory system will respond to these two frequencies, but also will respond as if energy was presented at other frequences not included in the stimulus. These components, generated by the nonlinear cochlear mechanism, are called distortion products. The most robust of these is the cubic difference tone, defined as 2f1 minus f2, where f1 and f2 are the frequencies of the two tones that originally were presented to the ear. Evidence for the generation of these products may be observed by analyzing the spectrum of the sound pressure in the ear canal. DPOAEs should be present in regions of normal hearing and absent from regions of hearing loss. Although instrumentation for clinical measurement of DPOAEs is not yet routinely available, it appears that these techniques have significant potential in identifying hearing loss, describing its configuration, and in helping to determine the etiology and/or site of lesion. DPOAEs typically take a few more minutes to measure than TEOAEs, but still can be performed in very short periods of time and potentially provide greater precision in estimating configuration of hearing loss than TEOAEs. As was the case for TEOAEs, more data are needed before DPOAEs can be incorporated into routine clinical assessments. Descriptions of false-positive and false-negative rates, and any interaction of these error rates with magnitude and configuration of hearing loss, require relatively large sets of clinical data which are not yet available. This is not unlike the situation that existed for ABR measurements 10 or 15 years ago or for acoustic immittance measurements 15 or 20 years ago. Many studies were needed before we fully understood the potential and the limitations of ABR and immittance measurements; yet, it is now difficult to imagine providing complete audiological services without the availability of these techniques. I would anticipate that within the next 2 to 5 years the necessary research will be performed that will lead to a more comprehensive understanding of both TEOAEs and DPOAEs in relation to the practice of clinical audiology, and may result in improvements in our ability to identify and characterize hearing loss.

COCHLEAR IMPLANTS

Multichannel cochlear implants have now received Federal Drug Administration approval for use both in children and in adults with profound hearing loss. It is widely recognized that, for some patients with profound hearing losses, performance with these devices far exceeds what is possible with conventional assistive devices, such as hearing aids. Advanced implant designs and more sophisticated speech processing schemes have resulted in favorable performance for many adult patients receiving implants (Dowell, Mecklenburg, & Clark, 1986; Dowell et al., 1986; Gantz et al., 1988; Tyler et al., 1989). However, all patients with profound hearing loss do not perform similarly. Some patients derive considerable benefit from the use of cochlear implants, while others derive much less benefit from their use. Efforts are underway to understand those factors that might be predictive of cochlear implant use (Quittner & Steck, 1990). Additionally, some patients perform very well with conventional amplification, even though the magnitude of their hearing losses might meet implant selection criteria (Boothroyd, 1989).

Determining the appropriate assistive device for individual patients is one area of clinical practice needing more research. Clinically, we are interested in knowing whether conventional amplification, a tactile aid, or a cochlear implant will result in the best performance. In adventitiously, post-lingually deafened adults, this question can usually be addressed through measures of open-set speech recognition ability. This option, however, typically is not available in very young children whose hearing loss occurred either before or during the acquisition of speech and language skills. Although electrophysiological measurements are extremely useful for identifying the presence of hearing loss, it would appear that lack of response during ABR measurements is not a sufficient cochlear implant selection criterion. Many children having no ABR response for clicks have residual, aidable hearing (Brookhouser, Gorga, & Kelly, 1990).

It is important to address the problem of selecting the appropriate rehabilitative plan because children with profound hearing losses do not all perform in exactly the same manner. Osberger and her colleagues have shown that some children with profound hearing loss achieve excellent performance with conventional amplification, compared with other children using two-channel tactile aids or single-channel or multi-channel cochlear implants (Osberger et al., 1990). Children receiving multi-channel implants performed almost as well as these conventionally aided patients, at least on some tasks. In contrast, subjects with single-channel implants or tactile aids had the lowest performance. Direct comparisons between those children using hearing aids and the other groups are complicated by the fact that the characteristics of the children using conventional amplification differed significantly from those of the children receiving either implants or tactile aids. Specifically, they derived significant benefit from amplification and were more experienced users of their devices. The cochlear implant selection criteria, by definition, eliminated these children as potential implant candidates. Still, it is important to distinguish among these different patients if we are to provide optimum conditions for individual patients. The patients with tactile aids performed the least successfully. It should be remembered that much less effort has been devoted to the development of these aids and (typically) to rehabilitation of patients using these devices. Furthermore, there are inherent limitations in the skin as a sensor for complex patterns such as speech. Still, there may be a distinct subclass of patients for whom these devices provide the best (or, at least, no worse) performance than any other device, and, given other constraints, these may represent the best assistive listening devices in those situations.

VESTIBULAR ASSESSMENT AND REHABILITATION

The evaluation and management of the dizzy patient has not been considered within the routine practice of audiology. However, many audiologists work in medical settings, which include the disciplines of otolaryngology and neurology. In these settings, audiologists routinely are asked to provide information that will help distinguish between peripheral and central vestibular disorders. Increasingly, audiologists are playing proactive roles, in collaboration with physical therapists, in the planning of rehabilitative programs for the balance-disordered patient once it is determined that surgical or pharmacological intervention is not indicated.

Indeed, audiologists historically have been involved in the assessment of patients with dizziness and imbalance for a number of reasons, including their expertise related to the function of the inner ear and their professional relationships with neurologists and otolaryngologists. Recent surveys have indicated that a significant number of audiologists are involved in the assessment of dizzy and balance-disordered patients (Martin & Morris, 1989).

Standard electronystagmography is the most commonly used procedure in the assessment of dizzy patients. It has proved to be a very sensitive test for distinguishing between peripheral and central vestibular dysfunction. These tests rely on the fact that the density of vestibular fluids (specifically those in the horizontal semicircular canal) can be altered by introducing into the ear canal water or air at temperatures either above or below body temperature. These changes in density are acted upon by gravitational forces resulting in stimulation of vestibular sensory epithelia, thus eliciting a vestibular ocular reflex. The development of computer-based ENG systems has made it possible to measure subtle eye-movement abnormalities that previously were not feasible. Still, the technique is limited by the fact that it evaluates only the horizontal semicircular canal and superior branch of the vestibular nerve, without assessing the other semicircular canals, otolith organs (utricle and saccule), or the inferior branch of the vestibular nerve.

A number of more recent technological advancements have altered the evaluation of these patients. For example, tests are now being performed in which eye movements are monitored while the subject sits in a chair that rotates. As yet unresolved is whether tests using low- or high-frequency rotations and/or sinusoidal or pseudorandom rotations are more sensitive. Major advantages of these techniques are that they can be applied with younger children and other special populations, and they are sensitive to subtle changes in the vestibular-ocular reflex (particularly in patients receiving ototoxic treatments), although they do not consistently identify either the side or site of lesion (Cyr, Moller, & Moore, 1989). Part of the reason for these limitations is the fact that current rotational tests assess function only for the horizontal semicircular canals, which are in the axis of rotation. It may be possible to assess other portions of the vestibular system with off-axis rotations. Although work in this area has not progressed rapidly, it may potentially provide a means to assess portions of the vestibular system that previously were inaccessible.

Over the past 3 to 5 years, more sophisticated tests of balance function have become available. In a normal standing position, we use combined information from the vestibular, visual, and somatosensory systems to maintain balance and posture. A breakdown in any of these systems could cause a balance disorder. However, the central nervous system is very adaptable and can quickly learn to de-emphasize impaired information from one of these systems. On the other hand, if input to the central nervous system is restricted to only one of these systems, then it must rely on that information alone as it attempts to maintain body position and equilibrium. For example, patients with bilateral vestibular weaknesses are able to maintain adequate balance in light, relying on visual and somatosensory input, but often experience difficulties in darkness when limited to somatosensory information. A new procedure, referred to as *dynamic posturography,* is designed to evaluate the patient's ability to maintain balance function, using information restricted as much as possible to one of the three systems involved in maintaining balance without relying on information from the other two. However, it is not possible to completely eliminate information from other systems. As a consequence, the test should be viewed more as an indication of the kinds of situations in which individual patients will experience difficulties. That is, it remains a test of functional balance and is not specific as to the site of lesion within the vestibular system. On the other hand, the results from this test highlight the fact that balance disorders are not only the result of problems involving the inner ear, but may be due to problems in other systems as well. Such information is essential to the planning of appropriate treatment, including medical intervention and rehabilitation programs.

Rotational testing and dynamic posturography are relatively recent developments, but highlight how the evaluation of the dizzy patient has evolved in recent years (Cyr, 1990; Kileny, 1985). As these measurement systems become more a part of typical balance assessments, large clinical databases will develop which may help to identify characteristic patterns of response for specific disorders. In addition, it is likely that developmental data will become available so that some of these measures may also be applied with appropriate consideration of system changes with age.

The development of these tests represents another case in which technological advancements have altered the manner in which clinical services are provided and affected the scope of practice in audiology. For example, audiologists in collaboration with physical therapists, may be asked to help design appropriate rehabilitative programs for patients suffering from vestibular dysfunction (Shepard, Telian, & Smith-Wheelock, 1990).

SUMMARY

There have been a number of technological advancements that have altered the manner in which clinical audiological services are being provided. The development of ABR techniques, cochlear implants, real-ear probe microphone systems, computerized ENGs, rotational testing, and posturography have all resulted in changes that have affected clinical service but also increased the scope of practice in audiology. It is difficult to conceive of clinical audiology not involving at least some of these techniques, even though additional research may be needed to understand fully the clinical utility of these techniques. Additionally, technological advances have outpaced our knowledge of the perceptual consequences of hearing loss. In order to exploit some of the opportunities provided by devices that allow for wearable digital signal processing, work is required that provides better descriptions of the consequences of hearing loss in individual patients. Finally, new developments, such as EOAEs, potentially provide a cost-effective way to screen all infants for hearing loss, and may provide additional information related to site of lesion in cases of sensorineural hearing loss. As clinicians, we not only need to be aware of these research and technological developments, but also must participate in clinical studies that lead to the eventual application of these new techniques to the diagnosis and treatment of hearing-impaired patients.

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THE BEHAVIORAL SCIENCES: CURRENT RESEARCH FINDINGS AND IMPLICATIONS FOR CLINICAL AUDIOLOGY

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Thank you for inviting me to participate in the "Future of Science and Services Seminar," dedicating the new wing of the national office. I've been coming to the Washington, DC, area for many years and have witnessed many changes during that time, as summarized in Table 1. In the 60s, I met hippies; in the 70s, preppies; in the 80s, yuppies; and now, in the 90s, Fed-uppies. Fed-uppies are people who are tired of VCRs with 50-page manuals, who prefer MacIntoshes to IBMs, and who are amazed at the number of forms it takes to get anything done. For Fed-uppies, it's easy to admit that the best thing about the *New Yorker* is the cartoons.

In the 60s, we talked of peace and in the 70s, of love. In the 80s, the change in our cautiousness resonated in the phrase "Let's do lunch." In the 90s, it's "I'll have my machine call your machine."

Audiology has also changed during these years. In the 60s, we concentrated on pure tones; in the 70s, on immittance audiometry and acoustic reflexes; and in the 80s, on evoked potentials. As we enter the 90s, our focus is shifting to speech processing. But unlike the rest of this changing world, the luminary of the hearing and speech sciences has not changed. Dr. Hirsh's recent publications on temporal aspects of auditory perception reflect his continuing influence on our field for more than 40 years (Hirsh, Monahan, Grant, & Singh, 1990; Monahan & Hirsh, 1990).

As Table 1 illustrates, the area of evoked potentials yielded some of the most exciting research and clinical findings in the 1980s, as Dr. Gorga just highlighted. Although this important work will undoubtedly continue, the trend in audiology in the coming decade is predicted to center more around issues involving speech processing. Our success in understanding speech processing hinges, at least in part, on applying our existing knowledge to formulate more meaningful questions and more sensitive experimental approaches to the problem.

Historical orientation. The measurement of speech understanding has been a basic component of the audiologic evaluation almost since its inception. As early as the 1940s, for example, clinicians were applying the monosyllabic (PB) word materials developed at the Psycho-Acoustic Laboratory of Harvard University (Egan, 1948) to the evaluation of patients (Carhart, 1946; Davis, Hudgins, Marquis, Nichols, Peterson, Ross, & Stevens, 1946; Thurlow, Davis, Silverman, & Walsh, 1949). This is perhaps one of the earliest interactions between "science and service." The researchers were interested in assessing the intelligibility of speech through electrical systems; the clinicians were interested in assessing the intelligibility of speech through impaired auditory systems. Dr. Hirsh's influence on clinical audiology also began around this time when he and his colleagues at the Central Institute for the Deaf modified the Harvard PAL word lists into clinical speech audiometric materials (Hirsh, Davis, Silverman, Reynolds, Eldert, & Benson, 1952).

Results of this early interaction between clinicians and speech and hearing scientists produced the striking finding that speech recognition was better in patients with conductive losses than in patients with sensorineural losses. The value of this arresting observation was diminished, however, when it became apparent that speech recognition could not be predicted accurately from the audiogram. As early as the 1960s, several investigators were cautioning about the difficulty of predicting speech

TABLE 1. Shifts with time.

1960	*1970*	*1980*	*1990*
Hippies	Preppies	Yuppies	Fed-uppies
	Love	Let's do lunch	I'll have my machine call your machine
Peace			
Pure tones	Immittance	Evoked potentials	Speech processing
Ira Hirsh	Ira Hirsh	Ira Hirsh	Ira Hirsh

understanding from the degree and type of hearing loss (Elliott, 1963; Young & Gibbon, 1962). Clinical experience over the decades has strongly supported this viewpoint (e.g., Boothroyd, 1984; Marshall & Bacon, 1981; Yoshioka & Thornton, 1980).

Interpatient variability. Figure 1 illustrates the characteristic variability in maximum PBS word performance in individuals with the same degree of hearing loss (from Marshall & Bacon, 1981). These data are from 774 patients with a wide range of hearing losses. The degree of hearing loss is represented by the threshold hearing level (HL) at 2,000 Hz. Results at this isolated frequency, out of a large number of possible audiometric indices, yielded the best prediction of the speech scores. On average, speech scores significantly decrease as hearing loss increases. The clinical value of this general trend is limited, however, by the large spread of scores. Only 37% of the variability in the speech scores is predicted by knowledge of hearing sensitivity. In the range from 65 to 80 dB HL, for example, speech scores vary from the floor to the ceiling in subjects with the same degree of hearing loss.

Marshall and Bacon questioned whether age might account for some of the extreme variability because their subjects ranged in age from about 15 to 95 years. The addition of age to the predictive equation, however, improved the variance accounted for to only about 45%, from 37%. As we enter the 1990s, several investigators are conducting studies that are beginning to yield some valuable insights into the contribution of age to variability in speech understanding (CHABA, 1988; Dorman, Marton, Hannley, & Lindholm, 1985; Gordon-Salant, 1987a,b; Jerger, Jerger, Oliver, & Pirozzolo, 1989; van Rooij, Plomp, & Orlebeke, 1989). As of today, however, the most impressive result continues to be the large interpatient variability that remains unexplained.

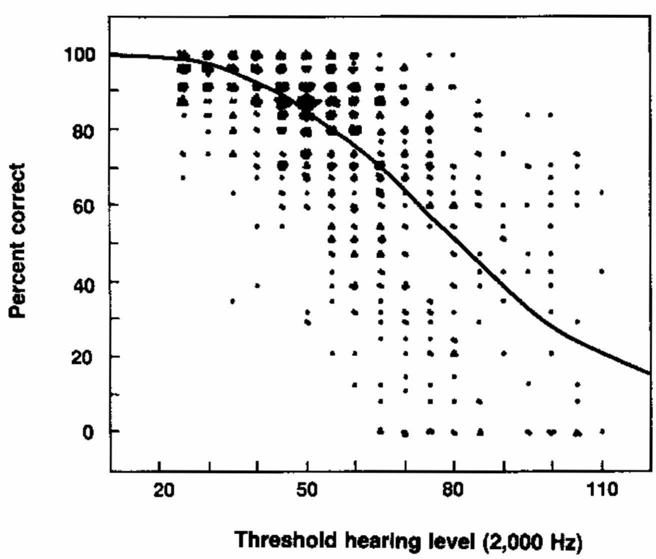

FIGURE 1. Maximum PB word scores in 774 patients with a wide range of hearing losses. The degree of hearing loss is represented by the threshold HL at 2,000 Hz. (Redrawn from Figure 2 of Marshall & Bacon, 1981).

The widespread clinical observation that patients with minimal hearing losses may show pronounced variability in their ability to understand speech is also illustrated in Figure 1. Speech scores in the Marshall and Bacon patients with minimal hearing loss (20 dB HL) vary over a range of at least 35%, from about 65 to 100%. Another interesting facet of the "variability problem" is that individuals with audiometrically normal hearing sensitivity may also show pronounced speech understanding deficits, particularly when the listening situation is made difficult. Figure 2 illustrates this finding in 20 individuals with normal hearing sensitivity (Jerger & Jordan, 1980). The average hearing sensitivity in these subjects was 9 dB HL; the average age was 39 years. Results show the percentage correct performance for synthetic sentence identification (SSI) materials in the presence of a competing message. The SSI scores in these patients range from 0 to 100% correct.

The distinguishing attribute of the subjects in Figure 2 is that each person had a documented central nervous system (CNS) lesion affecting the pathways and nuclei of the central auditory system. The lesion affected speech understanding in a difficult listening situation but did not affect pure-tone sensitivity or PB word scores. The range of speech deficits in these patients is thought to reflect the different degrees of involvement of the central auditory system. A prominent finding of the 1970s and 1980s, however, was the observation of abnormal speech understanding in normal-hearing individuals without evidence of CNS lesions, particularly young and elderly listeners (e.g., Jerger, 1984a,b; Pinheiro & Musiek, 1985). The reason(s) for the speech deficits in these latter patients is not well understood. Comprehensive reviews of speech audiometry in both adults and children are available for interested readers (Elkins, 1984; S. Jerger, 1984; Olsen & Matkin, 1979).

Understanding the factors underlying the ability to understand speech, in both normal and hearing-impaired individuals, will be a prominent concern in the next decade. This problem has important theoretical implications for understanding hearing impairment. Additionally, it has important clinical implications for predicting success with amplification and for developing more effective speech processing strategies for amplification systems. Why do persons with the same audiograms differ so markedly in their speech perception abilities? The problem is being studied from at least two different perspectives.

Behavioral and physiologic perspectives. Table 2 summarizes two current orientations to the study of some possible factors underlying different speech perception abilities: a behavioral information processing approach and a physiologic approach. Both approaches are being carried out from "bottom-up/peripheral" and "top-down/central" orientations. In general, the bottom-up/peripheral school of investigators has concentrated on studying subjects with significant hearing losses whereas the top-down/central school has focused more on studying subjects with normal hearing or minimal hearing losses.

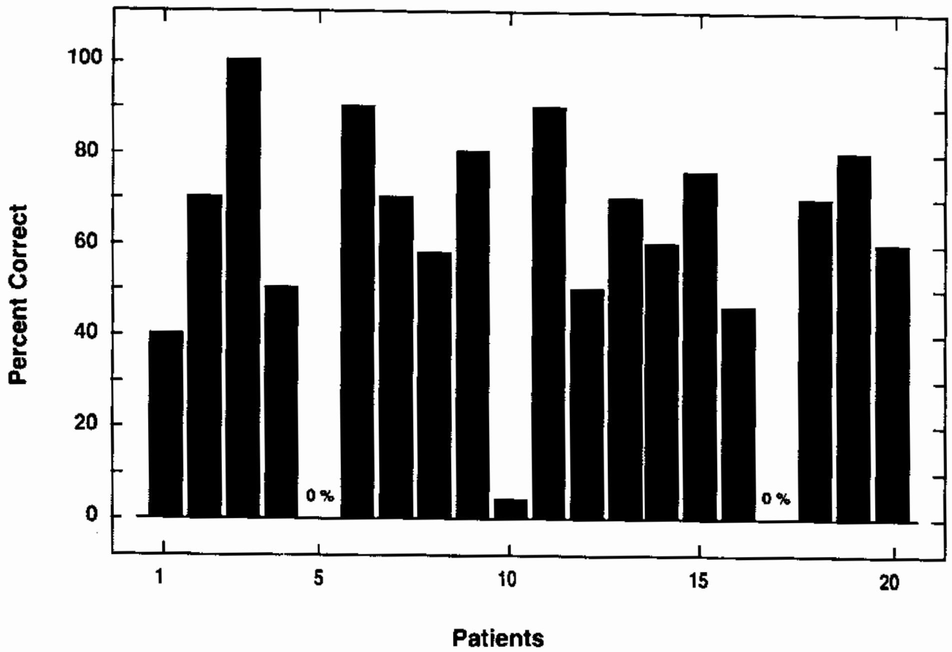

FIGURE 2. Percent correct performance for SSI materials in the presence of a competing message in 20 individuals with normal hearing sensitivity and normal maximum PB scores. Each person had a documented CNS lesion affecting the pathways and nuclei of the central auditory system. (Drawn from Table 1 of Jerger & Jordan, 1980).

TABLE 2. Current orientations to the study of the factors underlying differences in speech perception and in success with amplification.

Approach	*Differences in:*
I. Behavioral Information Processing Approach	
Bottom-Up	Frequency, time, and intensity resolution
Top-Down	Relevant cognitive-linguistic factors
II. Physiologic Approach	
Peripheral	Integrity of cochlear mechanics and neural tuning
Central	Integrity of central auditory system

The bottom-up/peripheral school hypothesizes that patients with similar audiograms may have dissimilar speech perception abilities because of differences in the sensory-neural dynamics of the peripheral auditory system. The bottom-up/peripheral school emphasizes the importance of the acoustic signal and its peripheral representation for the perception of speech. The top-down/central school, on the other hand, proposes that patients with similar audiograms may have dissimilar speech perception abilities because of differences in the central auditory nervous system or in auditory perceptual skills. The top-down/central school also acknowledges the importance of other factors for the perception of speech (e.g., linguistic knowledge, such as what a person expects to hear; processing mechanisms, such as short-term memory; and neurophysiologic structures, such as primary auditory cortex). None of the approaches listed in Table 2 seems sufficient, in and of itself, to account for real-life speech perception. During the 1990s, it is probable that both a "middle-out" behavioral approach and a "combined peripheral-central" physiologic approach will become more prevalent as the knowledge of each school is integrated into more expansive models of perceptual processing. Two areas of research, illustrating some of the findings and questions that are leading the search for the factors underlying variable speech perception in listeners with similar audiograms, are now briefly reviewed.

Frequency resolution. Impaired frequency resolution in ears with cochlear hearing loss has been noted by investigators representing both the bottom-up/behavioral and the peripheral/physiologic schools (e.g., Salvi, Henderson, Hamernik, & Ahroon, 1983; Stelmachowicz, Jesteadt, Gorga, & Mott, 1985). This finding raises the question of whether the variable speech perception deficits in hearing-impaired listeners may be associated with variable additional deficits in spectral resolution that are not revealed by the audiogram. A primary thrust of current research is to determine whether cochlear pathology impairs frequency resolution or whether all ears, both normal and hearing impaired, are characterized by poor frequency resolution at the sound pressure levels (SPLs) necessary for testing subjects with impaired hearing (Humes, Espinoza-Varas, & Watson, 1988).

To address this issue, some investigators are comparing frequency resolution ability in hearing-impaired listeners and normal-hearing listeners who are tested in broadband noise in order to simulate impaired hearing. The rationale for this approach is that poor frequency resolution may not be attributed to cochlear pathology if results in hearing-impaired listeners and masked normal-hearing listeners are comparable at equal SPLs. Figure 3 shows frequency resolution ability, as assessed by psychophysical tuning curves, in normal-hearing listeners who were tested in different levels of broadband noise (Dubno & Schaefer, in press). Data are the SPLs at which a 1,200 Hz tone can just be detected in the presence of different pure-tone maskers. The shape of the normal curve reflects the principle that masker tones near the probe (1,200 Hz) mask it more effectively than masker tones distant from the probe. Panel A shows results for the normal subjects listening in a broadband noise of 30 dB SPL. Threshold SPLs were shifted to approximately 10 dB SPL, but the shape of the psychophysical tuning curve remained normal. As the level of the broadband noise is increased (Panels B, C, & D), the threshold SPLs progressively increase and the shapes of the masking functions become progressively more abnormal. The psychophysical tuning curves show a blunting of the sharp tip and a flattening of the slope of the function, particularly toward the low-frequency end.

To look at the relation between results in masked normal listeners and hearing-impaired listeners with a comparable degree of threshold elevation, Figure 4 reproduces some of the data of Figure 3, with results from six hearing-impaired subjects, two in each panel, superimposed on the masked normal results (Dubno & Schaefer, in press). The degree of hearing loss in the impaired subjects was selected to be comparable to the masked threshold SPLs in the normal subjects.

Results clearly suggest that the poor frequency resolution in hearing-impaired listeners is representative of the capabilities of the normal auditory system, when it is measured at comparable signal intensity levels. Dubno and Schaefer, however, issue an important cautionary note: "This finding does not . . . diminish the communi-

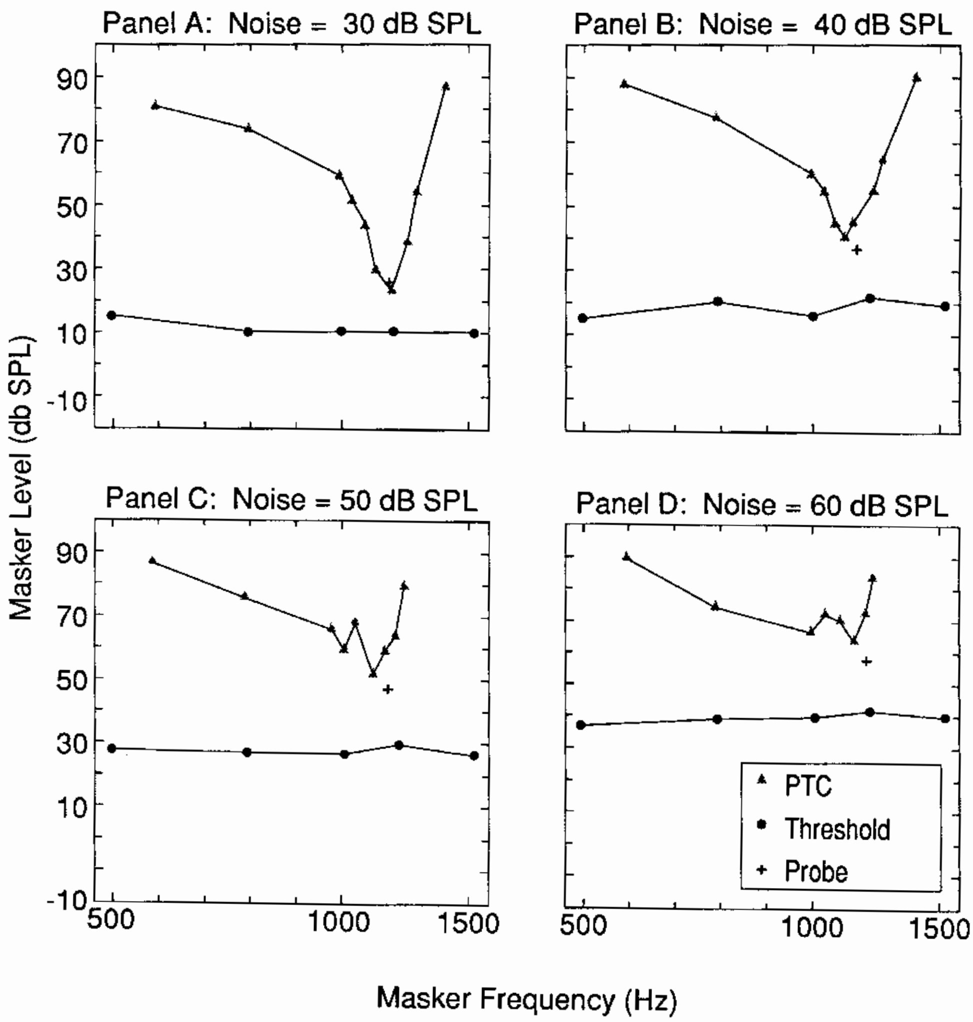

FIGURE 3. Psychophysical tuning curves (PTCs) in 7 normal-hearing listeners who were tested in different levels of broadband-noise, ranging from 30 dB SPL (Panel A) to 60 dB SPL (Panel D). Data in each panel are the masking function for detecting a 1,200 Hz probe-tone with 10 different pure-tone maskers, ranging from 600 to 1,400 Hz. The probe tone was of 20 ms duration; the pure-tone maskers were of 200 ms duration. The masker-probe separation was 2 ms (forward-masked). The masked threshold SPLs are also shown. (Redrawn from Figure 5 of Dubno & Schaefer, in press).

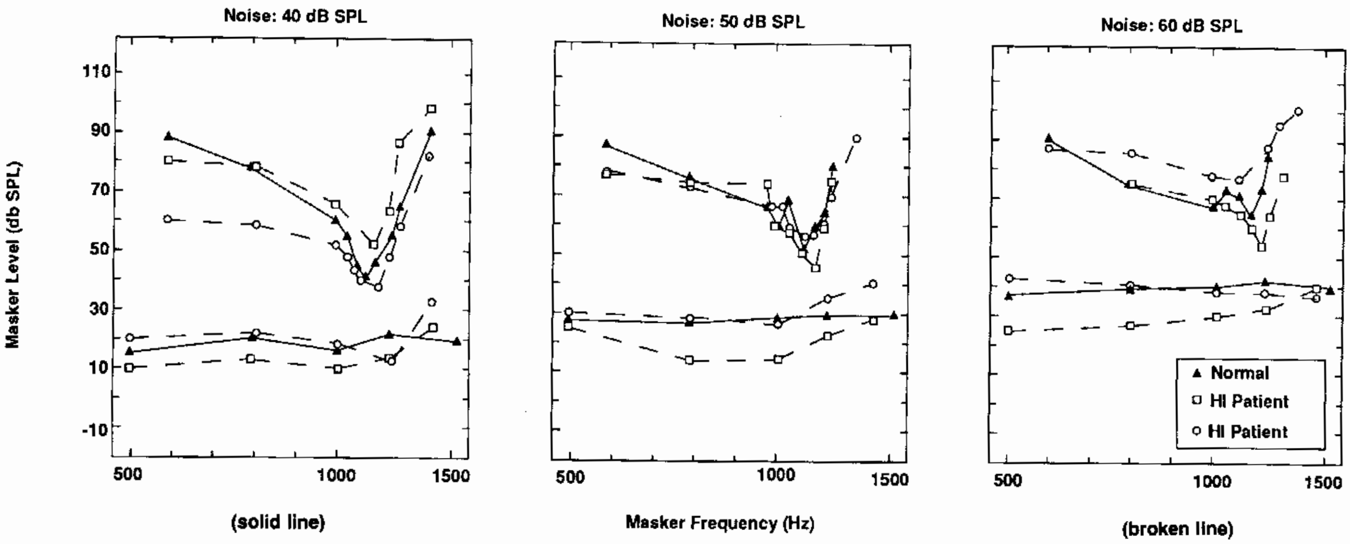

FIGURE 4. The data of Figure 3 are reproduced with results from 6 hearing-impaired (HI) subjects, 2 in each panel, superimposed on the masked normal results. The degree of hearing loss in the impaired subjects was selected to be comparable to the masked threshold SPLs in the normal subjects. (Redrawn from Figure 6 of Dubno & Schaefer, in press).

cation difficulties faced by hearing-impaired listeners in most listening environments. . . . (Even) if the speech signal is amplified to assure maximal audibility, speech recognition may not improve as expected if the auditory system does not function optimally at the signal levels provided by the amplification system" (p. 16).

At present, several investigators are carrying out exciting programs of research to determine whether (a) cochlear pathology produces poorer frequency resolution than might be expected on the basis of normal results at that SPL, and (b) whether and how abnormal frequency resolution may be related to speech perception (Arlinger & Dryselius, 1990; Dreschler & Plomp, 1985; Dubno & Dorman, 1987; Glasberg & Moore, 1989; Hannley & Dorman, 1983; Preminger & Wiley, 1985; Stelmachowicz et al., 1985; Tyler, 1986). During the 1990s, it will be an interesting challenge to untangle the latter question, in view of the complication that frequency resolution and speech recognition may appear to be related because both are associated with the degree of threshold elevation (Dubno & Dirks, 1989, 1990; van Rooij et al., 1989). The important interaction between "science and service" that is characterizing this area of research is illustrated by the concurrent attempts to develop clinically feasible methods for determining frequency resolution (Davidson & Melnick, 1988; Lutman & Wood, 1985). Turning now to an example of a more top-down/central orientation, readers will notice that manipulating the behavior of normal listeners in order to simulate the disordered auditory system is a less tenable experimental approach.

Auditory processing. A primary concern of investigators in the top-down/behavioral and central/physiologic orientation is whether disproportionately poor speech perception in both normal-hearing and hearing-impaired listeners is reflecting auditory-specific processing abnormalities or more generalized linguistic-cognitive abnormalities (CHABA, 1988; Cohen, 1987; Gordon-Salant, 1986; Jerger, Johnson, & Jerger, 1988; Jerger, Stach, Pruitt, Harper, & Kirby, 1989; Stach, Jerger, & Fleming, 1985; van Rooij et al., 1989). Currently, this field of work is flourishing and progressing, as it begins to develop more precise definitions of its terms and more viable approaches to its questions. Unfortunately, however, as of today, the definition of "auditory processing disorder" continues to be equivocal. One school of investigators seems to define auditory processing disorder in a broad sense (i.e., as an impaired ability to discriminate, identify, or otherwise process auditory information that cannot be attributed to impaired hearing sensitivity or to impaired cognitive-linguistic functions) (e.g., Jerger, Oliver, & Pirozzolo, 1990; Keith, 1986). Another school of investigators, on the other hand, seems to view an auditory processing disorder more specifically; that is, as a disorder in the coding and transformation of sensory input for perception (or, in other words, the processing that occurs prior to the conscious experience of sound) (e.g., Sloan, 1986). Both of these viewpoints endorse the reality of an auditory processing disorder; the latter views the disorder as belonging to the precursor events leading to perception whereas the former does not differentiate precursor and perceptual processes. In contrast to these two orientations, another viewpoint questions the reality of auditory processing disorder and attributes disproportionately poor speech understanding to conceptual-linguistic, rather than precursor-perceptual, deficits (see Levinson & Sloan, 1980, and Lubert, 1981, for discussions). To address some of the above issues, several investigators are carrying out multidisciplinary studies of subjects with abnormal speech perception. Below, one area of research that illustrates some of the current findings and questions is reviewed.

Multidisciplinary approach. A team of audiologists and psycholinguists recently evaluated a child with normal hearing, but complaints of difficulty in understanding speech, on an extensive battery of auditory and cognitive-linguistic measures (Breedin, Martin, & Jerger, 1989; Jerger, Martin, & Jerger, 1987; Martin, Jerger, & Breedin, 1987). Figure 5 summarizes some of the results in terms of an information processing model of speech comprehension. The model proposes four hypothetical levels of processing. First is an auditory stage that transforms the speech waveform into a neurally encoded signal. Next are the phonetic and phonologic stages that analyze the transformed acoustic pattern into phonetic features and bundle the features into a particular phoneme. The final two stages involve linguistic functions that organize the phonologic segments into a grammatical structure (syntactic level) and specify the semantic content in order to form a conceptual representation.

Level of Processing	Test	Outcome
AUDITORY	Acoustic Reflex	■
	ABR	◧
	Middle Latency	■
	Late V Potential	□
PHONETIC-PHONOLOGIC	Phoneme	
	Discrimination	■
	Identification	■
	Word	
	Discrimination	■
	Identification	■
	Phonologic Processing	■
SYNTACTIC	Sentence Identification	
	(auditory)	■
	(visual)	□
SEMANTIC	Vocabulary	□
	Word Identification	□
	Naming to Definition	□

■ Abnormal □ Normal

FIGURE 5. Hypothetical levels of processing involved in speech comprehension in an information processing theoretical model and the functional adequacy of each level as predicted from physiologic or behavioral measures that attempted to isolate a particular processing component. (With permission from Jerger, Martin, & Jerger, *Ear & Hearing*, 8, 78–86, 1987).

The functional adequacy of each of the levels of processing was predicted from data on physiologic or behavioral measures that attempted to isolate a particular processing component. In terms of the auditory stage of processing, results suggested that the acoustic waveform was not being transformed into an appropriate neurally encoded signal. Evidence for abnormal auditory transformation was provided by abnormal acoustic-reflex thresholds and morphology, abnormal middle latency responses, and degraded morphology of the auditory brain stem response (ABR). In terms of the phonetic-phonologic stages, results suggested that the transformed acoustic patterns were not being mapped onto appropriate phonologic representations, or, alternatively, the transformed acoustic patterns were degraded to the extent that appropriate phonologic representations could not be consistently determined. Evidence for abnormal phonetic-phonologic processing was provided by abnormal discrimination and identification abilities for both phoneme and word measures. An example of abnormal phoneme discrimination was a performance deficit on a task requiring the subject to decide whether consonant-vowel pairs differing in one distinctive feature ("ba-pa," "ba-ba") were the same or different (two-alternative forced-choice). An example of abnormal word discrimination was a performance deficit on a task requiring the subject to decide whether an orally presented word ("crown") matched a picture ("clown") in a two-alternative (yes-no) forced-choice paradigm. Further evidence for abnormal phonologic processing was a performance abnormality for identifying uncommon words (e.g., Say the word, "corpse") and for performing phonologic operations on strings of sounds (What is the first sound in "chid?" What word is formed by "k-a-t?").

Finally, results suggested that the syntactic and semantic levels were functioning normally. In contrast to the abnormalities observed on measures of auditory-phonologic abilities, syntactic and semantic abilities were normal, except when syntactic measures were presented via the auditory modality. Examples of normal semantic abilities were vocabulary skills at or above age level on the Peabody Picture Vocabulary Test (Dunn & Dunn, 1981) and the Wechsler Intelligence Scale for Children-Revised (WISC-R) (Wechsler, 1974), normal performance on a picture-word test requiring correct discrimination between semantically related concepts (chicken-duck), and normal performance on a naming-to-definition task ("Name: clothing worn on the hands to provide warmth").

Assessment of syntactic abilities involved the information conveyed by word order and grammatical markers. The test materials were sentences ranging in difficulty from simple active sentences to complex embedded relative-clause sentences containing a passive form. With visual presentation, performance for all sentence types was normal; with auditory presentation, however, results were depressed. Normal performance for the visual presentation indicated that the subject's knowledge of syntactic structures is normal. Abnormal results on the auditory condition therefore have to be attributed to some processing factor specific to the auditory modality. This pattern of results supports the reality of an auditory disorder in the presence of normal linguistic-cognitive skills.

During the 1990s, an important interaction between "science and service" will be to determine more conclusively the nature of the relation between auditory-perceptual and language-learning abilities and to develop and apply more appropriate clinical assessment measures. The importance of the interaction in this area cannot be overemphasized. The auditory disorder in the 11 1/2-year-old child whose data are summarized in Figure 5 was not recognized on three audiologic evaluations between ages 6 and 11 years. Results of all three evaluations were reported as "normal," with the comment that there was no auditory basis for the child's consistent complaints of difficulty in understanding verbal instructions in the classroom. An important goal for the 1990s is to assess auditory processing disorders in a sensitive manner clinically, which will necessitate expanding both pediatric and adult audiologic evaluations beyond the narrow confines of pure-tone thresholds and simple PB word scores (Hannley, 1986; Jerger & Jerger, 1981; Jerger, Johnson, & Loiselle, 1988). One of the spin-offs of the current emphasis on multidisciplinary collaborations has been the application of several novel paradigms to the evaluation of subjects with auditory disorders and/or language/learning disorders.

Innovative approaches. Underlying some of the variation in speech understanding or in success with amplification that cannot be predicted from the audiogram may be forthcoming from several novel approaches to evaluating speech perception and auditory-cognitive functions. Historically, clinical speech audiometric materials have been designed only to identify speech understanding deficits and have not attempted to illuminate the factor(s) underlying the deficits. Several novel experimental paradigms, however, might be applied clinically to gain insights into the nature of the complex interactive processes associated with deficits in spoken word recognition. Table 3 summarizes some of these novel approaches.

The *word frequency task* is based on the well-recognized phenomenon that words that are common in the language (e.g., *dog*) are more readily perceived than uncommon words (e.g., *corpse*) (Savin, 1963). According to the logogen theory of word recognition (Morton, 1979), uncommon or low-frequency words require more extensive auditory and phonologic processing in order to be identified than high-frequency words. The necessity for the more extensive perceptual processing of low-frequency words is attributed to experiential factors and to differences in the structural, phonotactic characteristics of the words per se (Landauer & Streeter, 1973; Pisoni, Nusbaum, Luce, & Slowiaczek, 1985).

This approach has been used to study some of the factors underlying speech understanding differences between adults and children and between normal children and learning-disabled children (Elliott, Clifton, & Servi, 1983; Jerger et al., 1987). A value of the approach is that it places some degree of stress, other than a competing signal, on the speech perception mechanism. It is possible that speech understanding could be evaluated in a more sensitive manner clinically if the concept of the word-frequency effect were applied to clinical speech audiometric materials. As an example, would PB word subscores based on different word-frequency categories provide more meaningful clinical information for predicting real-life speech understanding or success with amplification in hearing-impaired patients? Would PB word subscores provide some insights into the interaction between auditory perception and receptive language skills in hearing-impaired patients? Would PB word subscores vary as a function of the degree of hearing loss? In short, would the word-frequency effect provide an interesting possible basis for a "science and service" interaction in the 1990s?

The second task in Table 3 is a *gating paradigm* for studying on-line speech processing (Grojean, 1980; Pickett & Pollack, 1963). The gating task determines how word recognition varies as a function of the portion of a word that is presented (e.g., the first 50 msec, the first 100 msec, etc.). Results define how well a listener can recognize words from partial auditory information. This approach has been used to study some of the factors associated with speech understanding differences between young and old listeners and between normal and learning-disabled listeners (Craig, 1989; Elliott, Hammer, & Evan, 1987; Elliott, Scholl, Grant, & Hammer, 1990). Again, a value of the approach is that it places some

TABLE 3. Novel approaches to the study of some possible factors associated with differences in speech perception and in success with amplification.

Approach	*Definition*
Word Frequency Task	How word recognition varies as a function of the frequency of occurrence of the word in the language
Gating Task	How word recognition varies as a function of the portion of the word that is presented
Serial Recall Task	How recall varies as a function of modality of presentation
Auditory-Visual Task	How visual and auditory information interact during speech processing
Auditory-Semantic Task	How the voice and semantic dimensions of speech interact during processing

degree of stress, other than a competing signal, on the speech perception mechanism.

A question is whether speech understanding could be evaluated in a more sensitive manner clinically with a gating paradigm. As an example, do hearing-impaired listeners require more auditory information (longer temporal slices) to recognize words than normal-hearing listeners? Does the degree of hearing loss and/or the patient's age interact with the length of the word that is required for identification? How does sentence context interact with a hearing-impaired listener's ability to identify gated words? And, again, would data from a gating paradigm provide more meaningful clinical information for predicting real-life speech understanding or success with amplification?

The third task in Table 3 is a *serial recall task*, which requires listeners to reproduce short lists of items in the correct order. A general characteristic of serial recall tasks is that the probability of recall is higher for items presented auditorily than for items presented visually (Penney, 1975; Watkins & Watkins, 1977). This auditory advantage, termed the *modality effect*, is thought to reflect echoic memory, or the retention of the most recent auditory stimuli with a fidelity that resembles an echo of the stimuli (Morton, Crowder, & Prussin, 1971; Watkins, Watkins, & Crowder, 1974). Echoic memory is thought to play an important role in speech perception since speech is necessarily spread out in time (Darwin & Baddeley, 1974; Luce, Feustel, & Pisoni, 1983).

In hearing-impaired subjects, recall tasks for visual or redundant visual plus articulatory/auditory (read aloud) stimuli have received prominent, although sporadic, attention for almost 75 years (Conrad, 1979; Engle & Cantor, 1989; Pintner & Paterson, 1917). Yet, very few investigators have studied recall for purely auditory stimuli in hearing-impaired listeners (Furth & Pufall, 1966; Jerger & Watkins, 1988; Jerger, Watkins, Stout, Jorgensen, Blondeau, Albritton, Chmiel, & Lew, submitted; Ling, 1975, 1976). Data of the latter investigators suggest that echoic memory seems normal in some hearing-impaired listeners. Further research is now being conducted to determine whether an impoverished echoic memory may be a limiting factor in the speech perception abilities of some hearing-impaired individuals.

An advantage of the serial recall task is that the status of some central auditory/cognitive functions can be assessed without the typical necessity of complicating the perceptual task. Thus, patients with severe hearing impairments, who cannot be tested on traditional sensitized speech audiometric tasks, can be tested successfully on serial recall tasks, which can be limited to a small set of highly discriminable targets. To the extent that echoic memory is important for speech perception and to the extent that the serial recall task provides a valid index of echoic memory, results of a serial recall task could play a useful role in predicting differences in speech understanding or in success with amplification for hearing-impaired patients with similar audiograms.

The fourth task category in Table 3 explores *visual influences on speech perception*. The benefit of seeing as well as hearing a speaker has been recognized for many years (Dorman, Hannley, Dankowski, Smith, & McCandless, 1989; MacLeod & Summerfield, 1990; Osberger, Robbins, Miyamoto, Berry, Myres, Kessler, & Pope, in press). An interesting twist on studying bisensory perception, however, is to present mismatched cues in the two modalities (e.g., hear the word *box* and see the word *fox*). Results in normal listeners have shown that discrepant visual cues can exert a powerful influence on auditory speech perception (Green & Kuhl, 1989; Massaro, 1984; Massaro & Cohen, 1983; McGurk & MacDonald, 1976; Roberts & Summerfield, 1981; Welsh & Warren, 1980). This is known as the "McGurk effect." A clearly heard, unambiguous, auditory word can be modified perceptually, without a change in the acoustic structure, by the synchronized visual-only presentation of a talker uttering a different word.

Recently, Walden and his colleagues (Walden, Montgomery, Prosek, & Hawkins, 1990) studied the McGurk effect, or the visual biasing of auditory speech perception, in hearing-impaired listeners. The hearing-impaired listeners showed an even greater susceptibility to biasing from visual cues than the normal-hearing listeners. To the extent that the integration of auditory and visual information is an important fundamental aspect of speech perception for hearing-impaired listeners, clinical assessment of the interaction between auditory and visual cues could play a useful role in predicting differences in real-life speech understanding in hearing-impaired patients. Also, to the extent that the degree of visual biasing may be reflecting the relative "goodness" of the auditory and visual sources of information, the McGurk effect may be a powerful measure of the "degradedness" of auditory input in hearing-impaired listeners.

In concert with an auditory-visual, intersensory, discrepancy task, the fifth task in Table 3 is an *auditory-semantic*, or *interdimensional, task* that utilizes both conflict and congruence between the auditory and semantic dimensions of speech (Jerger, 1986; Jerger, Martin, & Pirozzolo, 1988). This task is a pediatric auditory analog of the conventional visual Stroop test (Stroop, 1935). In the conventional Stroop procedure, subjects name the ink-colors of printed words that spell the names of conflicting colors (e.g., the word *red* printed in *blue* ink). In the newly developed pediatric auditory task, children listen to words spoken by a male and a female voice. Subjects are instructed to ignore what is said and to respond, "Mommy" to the female voice and "Daddy" to the male voice, irrespective of the actual utterance. The voice-gender of the target corresponds to the ink color of the conventional visual Stroop stimuli; the semantic content of the target corresponds to the written word.

The visual Stroop task typically represents color-word "neutral" and "conflict" stimuli only. The pediatric auditory analog, on the other hand, defines performance for a battery of speech targets representing "neutral," "congruent," and "conflict" conditions: *neutral* semantic content (i.e., male voice saying "Ice Cream"), *congruent* semantic content (i.e., male voice saying "Daddy"), and *conflicting* semantic content (i.e., male voice saying "Mommy"). Data are the reaction times of correct responses.

The effect of hearing impairment on the processing of Stroop stimuli has not been well studied. One previous study found significantly less Stroop (conflict) interference on a visual (color-word) task in deaf subjects than in normal-hearing subjects (Allen, 1971). Preliminary work in our laboratory with the pediatric auditory (voice-word) task also has suggested significantly less Stroop (conflict) interference in deaf children than in normal-hearing children (Jerger, 1986). Although these data are tentative at best, the implication of less Stroop interference in severely hearing-impaired subjects for both a visual and an auditory paradigm is that results may be reflecting a generalized perceptual-cognitive deficit, rather than an auditory-specific abnormality. Presently, we are actively testing hearing-impaired subjects on both visual and auditory physical-semantic interaction tasks (i.e., color-picture and voice-word). Our hope is that results will yield insights into the nature of the interaction between auditory and semantic information in spoken word recognition by hearing-impaired subjects. Such findings could have relevant clinical implications toward understanding why hearing-impaired listeners with the same degree of hearing loss may exhibit significant differences in speech recognition and in the degree of success with amplification.

In short, interesting novel paradigms are actively being applied by speech and hearing scientists who are attempting to elucidate some of the factors that may be associated with differences in speech understanding. All of the important prior to contemporary work has not, of course, been mentioned. Readers are encouraged to seek information about at least the following:

1. phoneme feature perception, spectral cues, and speech understanding (Bilger & Wang, 1976; Dubno & Levitt, 1981; Gordon-Salant, 1985; Hannley & Jerger, 1985; Hood, Svirsky, & Cullen, 1987; Leek, Dorman, & Summerfield, 1987; Lindholm, Dorman, Taylor, & Hannley, 1988; Van Tasell, Fabry, & Thibodeau, 1987; Wang & Bilger, 1973),
2. contextual influences on word recognition (Bilger, Nuetzel, Rabinowitz, & Rzeczkowski, 1984),
3. the change/no change task for assessing suprasegmental or segmental aspects of speech processing (Carney, Osberger, Robbins, Renshaw, & Miyamoto, 1989; Gravel, 1989; Kuhl, 1985; Osberger, Todd, Berry, Robbins, & Miyamoto, in press; Sussman & Carney, 1989),
4. the objective versus subjective measures of speech understanding (Cox, Alexander, & Rivera, in press),
5. the processing of complex nonspeech stimuli and the implications for speech processing (Leek & Watson, 1988; Watson, in press; Yost & Watson, 1987), and
6. the development of psychoacoustic abilities (Allen, Wightman, Kistler, & Dolan, 1989; Olsho, Koch, & Halpin, 1987; Schneider, Trehub, Morrongiello, & Thorpe, 1989; Wightman, Allen, Dolan, & Jamieson, 1989).

Now let's explore briefly the contemporary scene characterizing current clinical practices in speech audiometry.

Clinical speech audiometry. Advances in clinical speech audiometry do not seem to have kept pace with advances in the clinical research on speech understanding. Apparently, routine speech audiometry has not changed in the United States for more than 40 years (Martin & Morris, 1989). Speech understanding continues to be evaluated by a speech reception threshold (SRT) and a PB word score at 40 dB Sensation Level (SL). About 90% of audiologists responding to a questionnaire use monitored live voice to obtain SRT results; about 70% of responding audiologists use monitored live voice to obtain PB word results.

Certainly, many clinical audiologists administer very sophisticated test batteries for diagnostic audiologic evaluations. The Martin-Morris data, however, suggest that an important "science and service" interaction for the 1990s is to foster a commitment not only to excellence, but also to action. More fruitful exchanges between clinicians and behavioral researchers should be encouraged, to maximize the application of the achievements and promises of behavioral research into clinical audiology.

It certainly seems the case that advances in electrophysiology and electroacoustics have been more readily incorporated into clinical audiology than advances in the behavioral sciences. This raises the question of why behavioral researchers have been less effective in impacting clinical audiology. Perhaps it is because behavioral evidence is less categorical, less normal versus abnormal, than electrophysiologic/electroacoustic measures. Perhaps, also, it is because clinical audiology began as a behavioral science, making it relatively easier to introduce "new" electrophysiologic/electroacoustic concepts or techniques than to modify traditional behavioral concepts or techniques to which clinicians are intellectually committed.

Let me suggest two long-term goals that may strengthen the relation between the behavioral sciences and clinical audiology. First, in our educational programs, more time should be spent teaching the principles of science. The facts about an individual science will change, but the principles by which a "trained mind" marshals the evidence, addresses an issue, and forms an opinion remain steadfast. Second, a "questioning" attitude should be promoted. Encourage students to ask the impertinent questions. Perhaps this can be accomplished by mixing the clinical training with a much heavier dose of theoretical issues that stimulate thought.

Excellent role models for us are the clinicians and researchers working in the area of cochlear implants. These clinicians and researchers have worked together to channel research findings into the solution of clinical problems. The behavioral research findings in cochlear-implant subjects have been applied imaginatively and have impacted significantly the structure of clinical practice. Each of us understands only the tiniest fraction of the knowledge in the speech and hearing sciences. Working together, however, we can frame appropriate questions and obtain cogent answers. Let's go forward into the

1990s, the dawn of the 21st century, with shared experiences and mutual hope.

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PROLOGUE

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By hearing research, I would mean our understanding of the basic physiological mechanisms and psychological dimensions of hearing, as well as those areas that enhance our understanding of clinical problems—whether of pathology, functional testing, psychosocial impairment, or rehabilitative procedures.

Under basic auditory sciences, I would expect important consequences of the increased acceptance of nonlinear processes in the ear—especially the cochlea. Among these consequences may be a clearer delineation of the functions of inner and outer hair cells and perhaps also separable pathologies. At the other biological extreme we can anticipate that the burgeoning knowledge about the brain will help our understanding of the more complex functions of the auditory system, beyond those associated with simple tonal sounds.

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This is a much appreciated opportunity for me to learn about recent research findings in the general field of hearing that appear to have relevance to clinical service, principally in audiology. Drs. Gorga and S. Jerger have provided us with excellent surveys of what has been done, what we now know, and how this new knowledge has or may affect service to the hearing impaired. I will address their contributions with much admiration and, of course, with some argument. They have our thanks. I thank also the Seminar organizers who saw fit to invite me to come along.

ACOUSTICS AND PHYSIOLOGY

Gorga is certainly correct—the last two or three decades of technological fruit have changed physiological testing and acoustical measurement and manipulation in the ear canal. He foresees great promise in the extension of these techniques along with further development of signal-processing schemes for both hearing aids and cochlear implants, and further knowledge of otoacoustic emissions.

For the future, I would add the promise of new knowledge from psychology and neurobiology, concerning that processing of complex sounds in general and of speech in particular. We have far to go, and one cannot now predict how that new knowledge will affect procedures of clinical audiology.

My quarrels with his presentation concern, first, a different view of history and, second, some implications for the future profession of audiology.

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Professional Concerns

The changing scope of activities for the future audiologist will, of course, influence the teaching and curricula in our academic programs. Gorga's emphasis on technological developments suggest that audiologists will become technicians. Their expertise will lie in physical acoustics, signal processors, and bioelectric recording. That may be a way to go, but we must look at the consequences.

When "audiology" was born, or at least named, its clinical activities included testing, hearing-aid selection, and rehabilitative procedures—lipreading or speechreading, auditory training, and speech conservation or speech improvement. Now somewhere along the way, the rehabilitative part seems to have got lost. Did it disappear because studies showed that it was ineffective? I do not recall reading such. Or maybe people with technical interests do not like to engage in such activities.

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Gorga suggests that the scope of audiology is widening. I agree, but only in certain dimensions, whereas for other dimensions it appears to me to be narrowing. These questions should be addressed by the professional groups concerned.

SPEECH UNDERSTANDING

You will understand my hesitation in criticizing anything that Dr. Susan Jerger has said—in view of her too effusive praise. I thank her for it, and for emphasizing one of the most vexing problems in clinical audiology—the low predictability of understanding speech by the hearing impaired from their audiograms. I agree with that importance; in fact, it was the topic of my "Carhart Lecture" to the American Auditory Society in 1989, entitled "Speech perception and the audiogram." The observation is not new. It caused Davis (1948), in formulating his "social adequacy index," to postulate two separate indices of hearing impairment—one, indicating loss in decibels, and the other "loss" in word discrimination. In the same year, Gaeth (1948) and his teacher, Carhart, started to speak of "phonemic regression" as almost a new dimension of hearing impairment.

Jerger's theoretical interpretation of recent and current research is right on target. Many people have held that if audibility did not tell the whole story about speech understanding, then surely frequency resolution would. What frequency?—a tone, vowel formants? The results have been suggestive but equivocal. Even if one would maintain that processes at the top of the top-down system are at fault, there must be some hesitation since simpler psychoacoustic abilities are not exhausted by measures of frequency resolution. Other kinds of resolution may be fair game—especially one in which I have had a long-standing interest, namely *temporal resolution*. Between sensory factors and cognitive abilities, we must search for something more purely perceptual.

I agree also with Jerger's emphasis on the nature of the task that is put before the listener (or the perceiver in the case of multisensory studies). We could learn about the upper end of the auditory system if we were to combine some audiological concepts of testing with those of students of memory and psycholinguistics. Such studies are going forward with respect to normal listeners; their adaptation to the hearing impaired is proceeding more slowly. Again, our professional categories may get in the way. Such difficulties look very much like *receptive aphasia*, an old term that would appear to belong to speech-language pathology.

There is one other problem in speech understanding by the hearing impaired that may be even more vexing than the low correlation between audiogram and speech perception. It is the imperfect correspondence between speech perception in quiet and in noise. Difficulty in hearing speech in noise is one of the most frequent complaints, particularly by the elderly and listeners with noise-induced hearing loss. The difficulty is not well predicted by their speech-perception performance in quiet. We need something like attention and related processes. It is the same brain attached to the auditory system which, in one case, may yield normal speech understanding in quiet, and, in another, show special difficulties in noise. At least part of this problem will involve assessment and correction of the whole binaural system.

EPILOGUE

There is still plenty of work to do. We and the hearing-impaired population will continue to benefit from advances on the technological side in acoustics, electronics, and bioelectric recording. We must keep our progress in those areas in touch with comparable developments in technical areas that are not normally in the audiological domain. Perhaps the less predictable progress is on the behavioral side, where we still wrestle with old problems and new hypotheses about the special difficulties of hearing impairments and the strategies for clinical help. Here we must broaden our assessment and rehabilitative goals to include both the top and bottom parts of the auditory-perception system.

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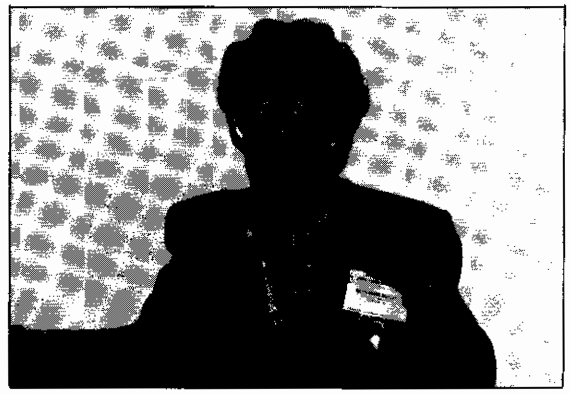
THE FUTURE OF LANGUAGE SCIENCE: IMPACT ON CLINICAL PRACTICE



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The President of the American Speech-Language-Hearing Association (ASHA), Dr. Roy Koenigsknecht (1990), recently reflected on the role played by the ASHA Task Force on Research in identifying the research needs of speech, language, and hearing scientists, the role of the publications of the association, the efforts of the Scientific and Professional Programs Board, the information provided by ASHA's Research Division, and, finally, this seminar on the "Future of Science and Services." He emphasized that ASHA "takes research seriously indeed" and that it is his perception that we must shape our own destiny, rather than leaving it to those outside our professions to conduct the research that will ultimately determine both our theoretical base and our clinical practice. He concluded by indicating that speech-language pathologists and audiologists should "spearhead" this effort, since "we know best what we need to know" (p. 14).

My first task today is to comment on the two preceding language science papers: Dr. Marc Fey's *Understanding and Narrowing the Gap between Language Treatment Research and Clinical Practice,* and Dr. Howard Goldstein's *The Future of Language Science: A Plea for Language Intervention Research.* Both papers addressed an important topic—intervention. Although not the whole of language science, intervention is studied for several reasons: (a) to test the theories of language acquisition and disorders, (b) to test the efficacy of certain interventions, and (c) to test the effectiveness of interventions across contexts (settings, clinicians, etc.).

My second task is to comment on current and future trends in language science. From my perspective, if speech-language pathologists and audiologists are to succeed in their quest for substantive outcomes from such research, they must have, and will continue to require, partners from other disciplines to develop the theoretical base upon which the delivery of services to individuals with language impairment must rest.

TRENDS IN LANGUAGE

Over the past few decades, speech-language pathologists have vastly expanded their services to those with childhood language disorders. As Shewan (1988) reports, 89.3% of speech-language pathologists regularly serve this population (see Figure 1). With the advent of P.L. 99-457, there is now specific interest in the "language" of infants and toddlers, and a resurgence of interest in child language acquisition and language disorders. In addition, more than 40% of all speech-language pathologists serve other disorders that require expertise in language (e.g., stroke, traumatic brain injury, and degenerative neurological disorders). Attendance at any state or national convention provides further evidence of clinicians' increasing interest in language disorders.

There has been growing interest among researchers and clinicians to view language within a number of naturalistic (real life) contexts, stemming from converging theoretical findings that emerge from a number of disciplines. "Language in context" is expoused by those who conduct descriptive research in language, but, in addition, is also supported by sociopolitical movements in education and healthcare settings.

External factors are impinging on our profession in general and on the language area in particular. Federal and state legislation increasingly has an impact on the autonomy of our profession. Not only has the specter of external control of our clinical practice by legislative or third-party payor fiat arisen, but depleted federal budgets for field-based research have drastically reduced support. Indeed, Congress's ever-skeptical view of anything but the most applied research is cause for considerable concern, since "pay-off" for more basic language acquisition research is not likely to have a high priority. Even with the increase in speech, language, and hearing disorders occurring throughout our nation, support for language intervention is also low priority.

Another trend is to view language in a broader context of language learning and its relationship to literacy and academic success. ASHA's recent symposium, *Partnerships in Education: Toward a Literate America* (Stewart, 1989), described these relationships. The popular press and a review of the literature supports the importance of language and literacy in a post-industrial society.

As we shall see, there are a number of other perspectives on science, particularly language science, that will

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SPEECH UNDERSTANDING

You will understand my hesitation in criticizing anything that Dr. Susan Jerger has said—in view of her too effusive praise. I thank her for it, and for emphasizing one of the most vexing problems in clinical audiology—the low predictability of understanding speech by the hearing impaired from their audiograms. I agree with that importance; in fact, it was the topic of my "Carhart Lecture" to the American Auditory Society in 1989, entitled "Speech perception and the audiogram." The observation is not new. It caused Davis (1948), in formulating his "social adequacy index," to postulate two separate indices of hearing impairment—one, indicating loss in decibels, and the other "loss" in word discrimination. In the same year, Gaeth (1948) and his teacher, Carhart, started to speak of "phonemic regression" as almost a new dimension of hearing impairment.

Jerger's theoretical interpretation of recent and current research is right on target. Many people have held that if audibility did not tell the whole story about speech understanding, then surely frequency resolution would. What frequency?—a tone, vowel formants? The results have been suggestive but equivocal. Even if one would maintain that processes at the top of the top-down system are at fault, there must be some hesitation since simpler psychoacoustic abilities are not exhausted by measures of frequency resolution. Other kinds of resolution may be fair game—especially one in which I have had a long-standing interest, namely *temporal resolution*. Between sensory factors and cognitive abilities, we must search for something more purely perceptual.

I agree also with Jerger's emphasis on the nature of the task that is put before the listener (or the perceiver in the case of multisensory studies). We could learn about the upper end of the auditory system if we were to combine some audiological concepts of testing with those of students of memory and psycholinguistics. Such studies are going forward with respect to normal listeners; their adaptation to the hearing impaired is proceeding more slowly. Again, our professional categories may get in the way. Such difficulties look very much like *receptive aphasia*, an old term that would appear to belong to speech-language pathology.

There is one other problem in speech understanding by the hearing impaired that may be even more vexing than the low correlation between audiogram and speech perception. It is the imperfect correspondence between speech perception in quiet and in noise. Difficulty in hearing speech in noise is one of the most frequent complaints, particularly by the elderly and listeners with noise-induced hearing loss. The difficulty is not well predicted by their speech-perception performance in quiet. We need something like attention and related processes. It is the same brain attached to the auditory system which, in one case, may yield normal speech understanding in quiet, and, in another, show special difficulties in noise. At least part of this problem will involve assessment and correction of the whole binaural system.

EPILOGUE

There is still plenty of work to do. We and the hearing-impaired population will continue to benefit from advances on the technological side in acoustics, electronics, and bioelectric recording. We must keep our progress in those areas in touch with comparable developments in technical areas that are not normally in the audiological domain. Perhaps the less predictable progress is on the behavioral side, where we still wrestle with old problems and new hypotheses about the special difficulties of hearing impairments and the strategies for clinical help. Here we must broaden our assessment and rehabilitative goals to include both the top and bottom parts of the auditory-perception system.

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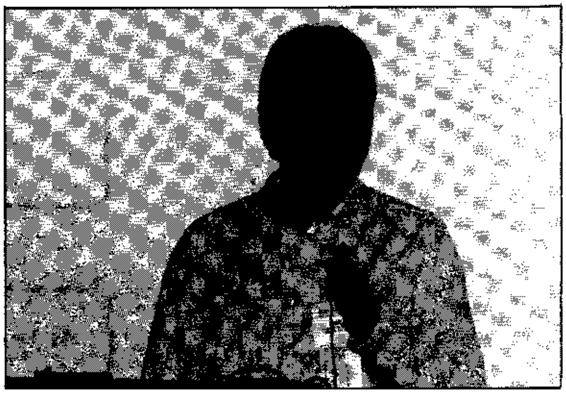
RESEARCH PAPERS: B. LANGUAGE

UNDERSTANDING AND NARROWING THE GAP BETWEEN TREATMENT RESEARCH AND CLINICAL PRACTICE WITH LANGUAGE-IMPAIRED CHILDREN



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One of the principle responsibilities of our profession is to provide an empirical foundation upon which important clinical decisions can be reliably based. To form this sort of empirical basis for decisions related to treatment for language-impaired children, theoretically well-motivated, thoughtfully designed, and carefully implemented research that is dedicated to the development and evaluation of effective and efficient language intervention approaches is necessary (Laney, 1982; McReynolds, 1983; Minifie, 1990; Schery & Lipsey, 1983). Unfortunately, the number of factors that pose threats to internal validity in treatment studies involving language and communication is staggering. In most employment contexts it is extremely difficult, if not impossible, for clinicians to control and manipulate these variables systematically. For these reasons, it is unreasonable to expect practicing clinicians to fill the gaping holes that currently exist in our understanding of important intervention variables. I will argue later that clinicians must play a vital role in any concerted effort to find new and better ways to help language-impaired children learn language. Notwithstanding this view, the lion's share of the experimental research responsibility must rest on those of us in the research community (see Laney, 1982).

Given this responsibility, it is both reasonable and important to ask whether efficacy research in our field is providing answers to or even addressing the most crucial questions facing the practicing language interventionist (Attanasio, 1986). As I assess the literature on child language intervention over the past 20 years, my answer to this question is, "in large part, no." Although intervention research has helped us to learn a great deal about certain specific procedural variables (e.g., Courtright & Courtright, 1976, 1979; Halle, Baer, & Spradlin, 1981; Hegde, 1980; Leonard, 1975) and about some of the learning characteristics of language-impaired children (e.g., Connell, 1987a; Hegde, Noll, & Pecora, 1979; Wilcox & Leonard, 1978), many of the most crucial intervention variables from the standpoint of practicing clinicians have been almost entirely neglected. This causes me considerable concern and frustration. My greatest, concern, however, is not that child language intervention research has in many respects failed to address the needs of practicing professionals. Rather, it is that I see so little movement toward the types of cooperative, programmatic efforts that I believe are necessary to help us to obtain our ultimate objective of improving clinical practice through research.

In this paper, I hope to clarify my view that language intervention research is not addressing the needs of clinicians or consumers. I will point out what I think are some very important and reasonable clinical issues and hypotheses that warrant experimental attention in the near future. I will also proffer some suggestions on how we can reach the long-term objective of examining these types of issues in detail.

TREATMENT STRATEGIES IN LANGUAGE INTERVENTION

To illustrate what I see as problems with the state of the art in child language intervention research, it will be useful to examine carefully just what I believe is or should be involved in a language intervention plan for a language-impaired child. This will give me the opportunity to raise a number of general issues that I feel researchers must address if their objective is truly to provide clinicians with useful information that can make clinical intervention programs more effective and efficient.

Figure 1 is a flow chart that presents what I regard as the most essential components of such a program. In my discussion of these components, I will point out key general issues that arise from each.

Intervention Goals

There are at least four general types of goals that a speech-language pathologist can consider when planning a comprehensive treatment program for a language-impaired child. These include basic goals, intermediate goals, specific goals, and subgoals (Fey, 1986; Fey & Cleave, 1990).

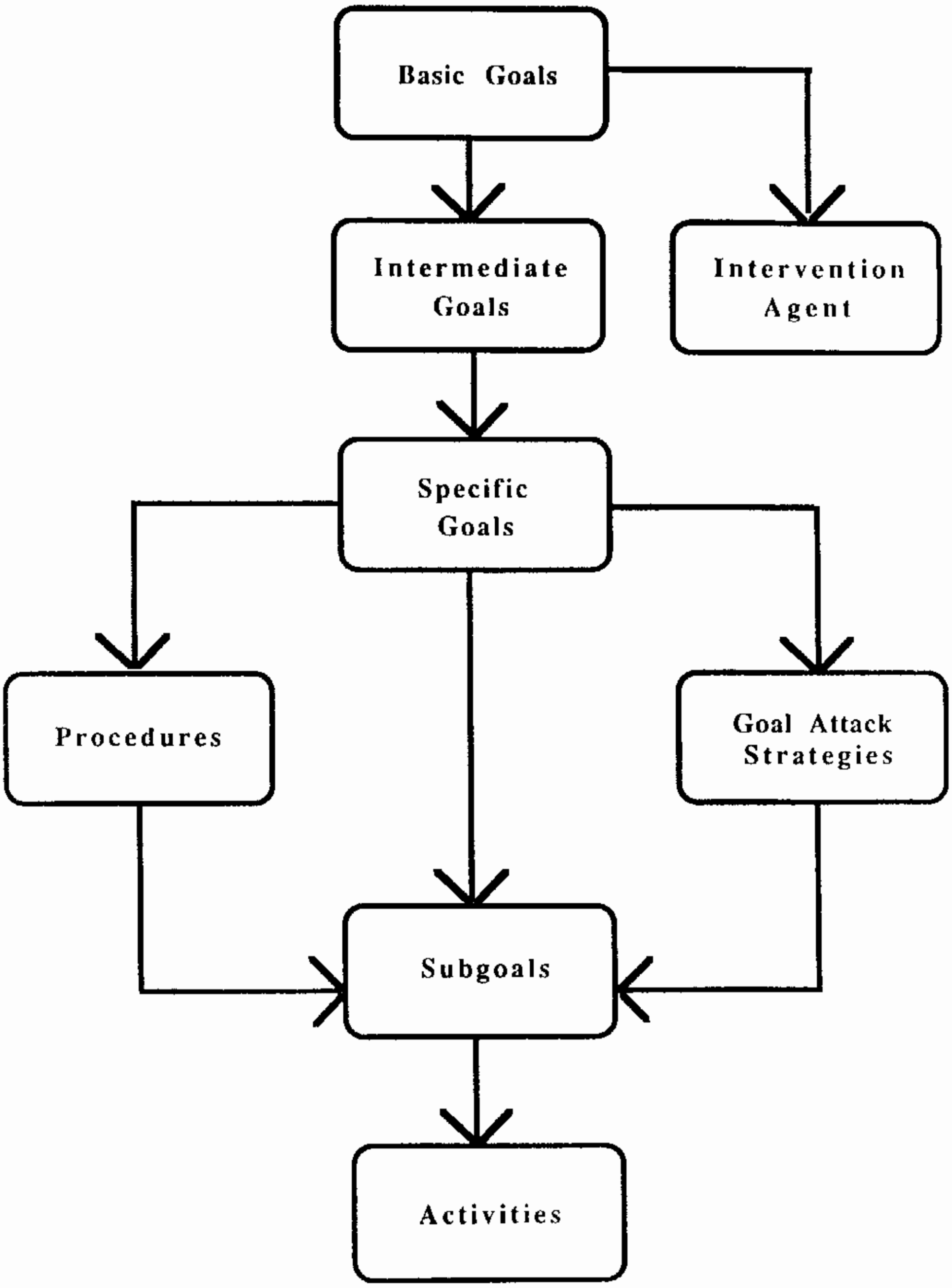

FIGURE 1. The components of a language intervention plan arranged in hierarchical relationships to one another. *Note*, From *Language intervention with young children* (Figure 4-1; p. 78). By M. Fey, 1986, San Diego: College-Hill Press. Adapted by permission.

Basic goals are the most general type of goal in a treatment plan. These are broadly stated objectives that help the clinician to provide focus on those aspects of communication that pose the greatest obstacles to the child's communicative effectiveness. As we have described them (Fey, 1986; Fey & Cleave, 1990), basic goals are derived from a careful assessment of the child's communicative performance in naturalistic contexts. For example, some language-impaired children seem to make efficient use of limited linguistic resources. They participate actively and effectively in communicative interactions as both listener and speaker, even though their use of conventional language forms is restricted. The basic goals for such children should probably be to facilitate the acquisition and use of new lexical, grammatical, and phonologic forms and to stabilize and automatize those forms that are in the process of development. Other language-impaired children exhibit different patterns of social-conversational performance, however. For example, some language-impaired children are extremely reluctant to talk. When they do talk, they do not utilize the cognitive and linguistic resources that they seem to have at their disposal. Even though these children may also exhibit significant delays in the development of words and grammar, form-based goals may be entirely inappropriate for them, at least at the outset of their intervention programs. For these children, the primary basic goal might be to encourage more active conversational participation in social interactions. Little or no emphasis may be placed on the linguistic form of the children's social bids, at least during the early stages of intervention (see Fey, 1986, for more specific examples).

I cannot imagine planning an intervention program for a language-impaired child without consideration of general objectives at this basic level. For me, this is the first and most important step in the hypothesis development and testing process that is or should be the cornerstone for intervention programs. For example, when planning at this level, a clinician might hypothesize that by facilitating the child's social and communicative interactiveness over a period of several months, the child will also show gains in the use of lexical and grammatical forms. This strategy is exemplified in child-oriented intervention approaches (Fey, 1986). In this type of approach, the intervention agent follows the child's lead and is highly responsive to the child's communicative bids regardless of the form of those bids (Norris & Hoffman, 1990; Tannock, Girolametto, & Siegel, 1990; Weiss, 1981). But the opposite strategy might also be taken in some cases. That is, the clinician might assume that the child's deficits in social-conversational interactiveness are due to problems with language expression. In such cases, the primary basic goal would focus on enhancing the child's use of more conventional language forms. The clinical hypothesis would be that once the child learns to make fluent use of more mature language forms (including phonological forms), indirect gains would be observed in the child's social use of language. We have very little experimental and quasi-experimental evidence pertaining to these issues (Hart & Risley, 1980; Hoffman, Norris, & Monjure, 1990; Matheny & Panagos, 1978; Tannock et al., 1990; Warren et al., 1984). Given the importance of basic goal decisions in the treatment process, concentrated efforts to examine them in detail seem warranted.

Perhaps the broadest and most basic goal would be to activate the child's language learning resources (i.e., to help the child to learn language more efficiently from sources outside the clinical context). Language clinicians know at least implicitly that they cannot teach everything about language that the child needs to learn to become a competent language user. They also know that most language-impaired children have the capacity to learn at least some language on their own from sources outside the intervention context. Consequently, the most efficient intervention program would be one aimed at positively influencing the child's language learning and utilization processes rather than to teach the child to make use of certain specific language behaviors. I am not at all certain that we can have this type of effect on children's language learning systems. But a hypothesis that such effects are obtainable can be motivated from several different theoretical perspectives (Bernstein & Stark, 1985; Connell, 1988; Fey, 1988; Leonard, 1981; Warren, 1988). Therefore, clinicians can and, I believe, should be designing programs with this broad objective in mind. If these broad effects are indeed possible, the potential benefit of intervention on children's language development would be much greater than is often imagined, at least for some children. It seems essential, then, that experimental attention be drawn to this issue.

Whatever basic goals are selected, the clinical hypotheses that are generated at this level can exert a rippling effect on the rest of the intervention planning process. For example, once the general areas of intervention concentration have been identified, it is possible to select one or probably several areas of general weakness that fall within the boundaries set by the basic goals. Some clinicians may target these general areas of weakness rather than specific language forms or rules. We have called this type of treatment target an intermediate goal (Fey & Cleave, 1990). The clinical hypothesis underlying an intermediate goal is that if intervention can help a child to induce a general principle, category, or relation, the child may exhibit gains over time on specific properties of language governed by that principle or category without additional direct intervention. For example, Connell (1986) selected as an intermediate goal all of the grammatical properties associated with sentence subjects. This included the use of nominative case pronouns in subject position, subject-verb agreement, and transposition of subject and auxiliary to form yes-no questions. His clinical hypothesis was that by helping his subjects to recognize the grammatical role of sentence subjects in English, the children would learn the individual properties associated with sentence subjects without further training. He presented preliminary evidence in support of this hypothesis.

At present, there is limited empirical support for the notion of intermediate goals. But such goals are plausible from different theoretical perspectives. For example, Connell's selection of the sentence subject as an intermediate goal can be motivated from the perspective of functionalism (Connell, 1986) or of parameter setting (Connell, 1988; Leonard & Loeb, 1988). If a clinician's general treatment objective is to move toward basic goals as quickly and efficiently as possible, experimental evaluation of hypotheses associated with the concept of intermediate goals seems important if not necessary.

Specific goals reflect those aspects of language knowledge and use that the clinician is trying to affect directly. Intervention procedures and activities are developed to facilitate the acquisition and/or use of these specific aspects of language in discourse contexts. In most language intervention programs that include specific goals, several goals are selected. This may be done simply to avoid tedium in the intervention context or to provide a more comprehensive treatment program. But clinicians may also choose a particular set of goals under the assumption that the interaction between these goals will be maximally beneficial to the client. For example, Connell (1982; 1987b) has presented cogent arguments that specific goals of language form, such as the use of auxiliary *is*, may best be taught by illustrating to the child how the target form contrasts with other related forms, such as the past tense or the future modality. To my knowledge, there have been no attempts to evaluate this or any other hypothesis regarding the potentially interactive influences of simultaneously treated specific goals. Given that so few clinicians focus their attention on one specific goal at a time, the absence of such studies is striking and disconcerting.

The final type of goal shown in Figure 1, subgoals, is developed primarily to assess a child's progress toward attainment of a specific goal. Subgoals represent intermediate steps along the way toward the accomplishment of specific goals. One of the most common subgoals used by clinicians and, especially, researchers is a probe that is designed to test the child's use of a new language form using nonlinguistic stimuli and vocabulary items that have never been presented in treatment. For example, if the treatment target for a child were the use of the regular past tense morpheme, the clinician might set aside a group of pictures representing completed actions. These particular pictures would never be presented in treatment, and the sentences used to describe the pictures would never be practiced in treatment. If the child makes accurate use of the past tense morpheme on the probe following treatment, it can be claimed that the child has acquired some abstract rule or principle. Importantly, however, high-level performance on a probe does not imply that the child has learned the language rule that was targeted. Similarly, it cannot be concluded that the child will be able to use the new form for social or problem-solving purposes at the same time that he or she is performing well on clinical probes (Connell, 1982; Fey & Cleave, 1990). In fact, the evidence is rather strong that without further intervention or maturation, children are unable to do so (Costello, 1983; Culatta & Horn, 1982; Fey, 1986; Hughes, 1985; Leonard, 1981).

In this section, I have shown that a number of crucial issues arise when a multilevel system of goals is developed as a part of a broadly based, comprehensive language intervention program for a language-impaired child. Because speech-language pathologists must develop comprehensive treatment programs for their clients, these are issues of vital importance to their clinical practice. Therefore, they warrant careful experimental scrutiny. In the next section, I will demonstrate the impact that the goal selection process can have on the development of treatment methods.

Treatment Methods

There are four general components shown in Figure 1 that are highly likely to have some important influence on treatment outcomes. These include the intervention agent, the procedures, the goal attack strategy, and the activity (Fey, 1986).

The intervention agent is the person or persons providing the treatment. This is not limited to the clinician or parent. Teachers, siblings, babysitters, and other competent communicators who interact frequently with the child can be employed as intervention agents.

The procedures represent the techniques that are presumed to have a facilitating effect on the child's language learning and communication. This includes things like provision of imitative models and reinforcement, modeling of target forms, expansions and expatiations of child utterances, and so on.

Goal attack strategies reflect the clinician's decision about how specific goals will be managed. Whenever more than one specific goal is chosen, the clinician must have some way of addressing each. In a vertical goal attack strategy (Steckol, 1983; Stremel & Waryas, 1974), the clinician focuses treatment on one goal until some criterion is reached. A horizontal goal attack strategy (Blank & Milewski, 1981; Steckol, 1983) requires the clinician to attend to all specific objectives simultaneously. The intervention agent employs techniques whenever the opportunity arises in a session. Alternatively, in a cyclical goal attack strategy (Hodson & Paden, 1983), the clinician focuses on one goal for a specified number of sessions. Then, the next goal is addressed regardless of whether any observable progress was made on the preceding objective. When all goals have been treated in this manner, a new cycle is begun by returning to the first goal in the sequence.

The final treatment component in Figure 1 is the activity. Activities reflect the tasks that are performed during intervention. They include the physical and social context in which intervention is provided. For example, intervention may involve the child and a clinician describing pictures in ways that require the use of some target linguistic act (e.g., Hegde, 1980; Wilcox & Leonard, 1978). In this activity, the clinician places heavy constraints on what the child attends to and says. In contrast, intervention might involve children at work and at play in the classroom amongst teacher and peers (e.g., Weiss, 1981) or telling stories in collaboration with a clinician (Lee, Koenigsknecht, & Mulhern, 1985). In these cases, the language that is the focus of the intervention program is also used purposefully to accomplish some greater social or personal objective (e.g., building a tower, performing an experiment, baking muffins, creating or retelling a story). Activities range in naturalness from being highly constrained and adult-oriented to being relatively unconstrained and essentially child-oriented (Fey, 1986).

Each of the treatment components may figure importantly in a comprehensive treatment plan. The benefits of at least some of these variables, however, may not be discernible except in long-term intervention in which more than a single subgoal or specific goal is targeted. This is most clear in the case of goal attack strategies. Unless at least two specific goals are targeted, only a vertical goal attack strategy is relevant. In contrast, to evaluate the influence of one treatment goal on another using a horizontal or cyclical strategy, at least two and probably more goals would need to be targeted.

A valid test of the effectiveness of different agents and activities may also require multiple targets and a relatively long period of intervention. For example, the quickest route to attainment of a subgoal would probably be a highly regimented drill involving the clinician. This activity would be similar in many respects to the targeted subgoal task. If the objective is to reach several specific goals, an intermediate goal, or a basic goal, however, it seems at least reasonable to hypothesize that application of procedures in naturalistic contexts from the outset of intervention would be more successful (cf. Johnston, 1985).

It is frequently assumed that the best or only way to reach a meaningful language intervention objective is to establish the use of the targeted language feature in some highly constrained activity (i.e., to reach a low-level subgoal). This establishment phase is then followed by a generalization phase in which the target is used in increasingly more naturalistic activities and with new intervention agents (Costello, 1983; Gray & Ryan, 1973; Mulac & Tomlinson, 1977). Independent of evidence demonstrating that this method of sequential modification is best, there is no reason to believe that a treatment approach that leads to the rapid attainment of subgoals will be similarly efficient in helping a child to attain related specific and intermediate goals. Other approaches that encourage a slower but broader pattern of learning in more naturalistic contexts may, in fact, be more efficient if the objective is a specific or intermediate goal (Fey, 1988).

The hypothetical learning curves shown in Figure 2 illustrate this possibility. Gains on the subgoal treated using Approach A were extremely rapid, but the corresponding curve for use of the same language target in the home and classroom shows very slow progress. As I have already pointed out in the discussion of subgoals, this is a common and highly predictable pattern with approaches that focus on the rapid acquisition of a single subgoal in some limited context before extending treatment to more natural social and physical contexts. In contrast, Approach B yielded relatively slow, gradual gains on subgoals, but the performance under conversational conditions was essentially the same as that for subgoals.

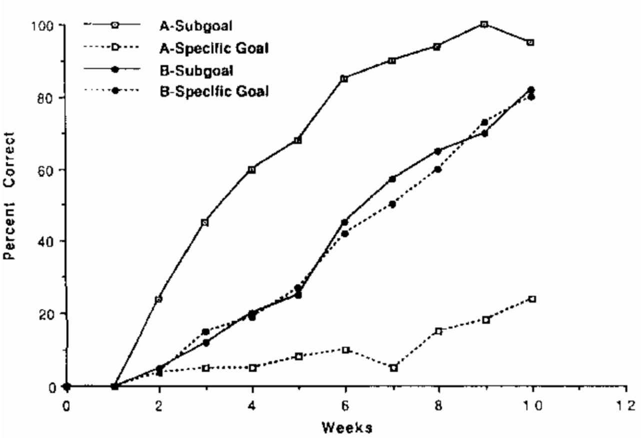

FIGURE 2. A comparison of the effectiveness of two hypothetical intervention approaches, A and B.

Whether this type of pattern for Approach B is typical of a particular approach is unclear, but we have noted it often in our clinical efforts to make intervention activities more naturalistic (Fey, 1988).

Which of the two approaches in Figure 2 should be judged the most efficient? The answer depends on which measure is selected as the dependent variable. Researchers often find that their experimental questions can be answered by using a measure characteristic of a subgoal as the dependent variable. Clinicians, who are seeking more efficient means of attaining multiple specific, intermediate, and basic goals, can never be fully satisfied with the conclusions of such studies because the situation shown for Approach B in Figure 2 is a logical possibility that is not without theoretical support (Johnston, 1985).

Research Strategies in Language Intervention

Many of what I consider to be the most challenging and important language intervention issues involve the attainment of intermediate and basic goals and the procedures, activities, and goal attack strategies that are most effective and efficient in helping language-impaired children to reach these goals. Unfortunately, these issues are rarely addressed in clinical research.

For the most part, treatment studies involving language facilitation with language-impaired children have evaluated the effectiveness of a single procedure or a tightly constrained procedural complex (see Costello, 1983; Fey, 1986; Hughes, 1985; Leonard, 1981; Schery & Lipsey, 1983). Only a single specific goal or, far more commonly, subgoal is selected as the outcome variable used to measure the treatment effect (e.g., Courtright & Courtright, 1976; Ellis-Weismer & Murray-Branch, 1989).

Studies of this general type are always interesting, and they are potentially very important to clinicians. When properly carried out, they have the great virtue of indicating not only that a treatment effect occurred but which specific intervention variable was responsible for that effect. But the applicability of these studies must be questioned because the interventions tested are always so limited with respect to the programs employed by most clinicians. Furthermore, the effects achieved are rarely sufficient to satisfy any clinician aspiring to have broad effects on a child's communicative development. Finally, application of the results to clinical contexts requires acceptance of an untested assumption. This assumption is that intervention variables that are effective in helping a child to attain isolated subgoals and specific goals in a highly efficient manner are similarly effective and efficient in helping a child reach multiple specific goals, intermediate goals, and basic goals. Although this assumption is plausible, it is not logically necessary and could be entirely wrong. Acceptance of the assumption without empirical verification of its adequacy could restrict clinicians in the types of comprehensive intervention programs that they create.

Another type of study that is far less well-represented in the literature involves the evaluation of treatment packages. In these studies, the effects of many variables are measured as they interact with one another and with nontreatment variables over time. Some interesting and important initiatives have appeared in which investigators have addressed issues related to the attainment of basic (e.g., Cole & Dale, 1986; Friedman & Friedman, 1980; Hart & Risley, 1980; Hoffman, Norris, & Monjure, 1990; Lee, Koenigsknecht, & Mulhern, 1975; Warren, McQuarter, & Rogers-Warren, 1984) and intermediate goals (Connell, 1986; Olswang, Kriegsmann, & Mastergeorge, 1982). The intervention approaches in these studies all reflect an underlying appreciation that language intervention is a long-term process involving the interaction of many clinical and extra-clinical variables. The clinically significant effects of these approaches can only be measured over extended periods of time in meaningful social contexts. The conclusions of these studies are generally much more clinically applicable than those that focus on a single subgoal or specific goal.

But there are important reasons to be cautious in the interpretation of these studies and the clinical application of their results. Some (e.g., Cole & Dale, 1986; Lee et al., 1975) employ procedures that are difficult to implement in many clinical contexts and/or are administered in contexts such as model university programs in which treatment is applied much more intensively than could ever be hoped for in most clinical settings. Many suffer from methodological weaknesses, such as the failure to select subjects randomly and/or to use appropriate controls. This makes their results difficult to interpret. Even when controls are adequate to ensure the intervention was effective, it is virtually impossible to know precisely which aspects of the approach were essential to obtain the effect. Finally, although these studies are more applicable than most studies that evaluate a single intervention component, without the necessary replications, it is difficult to know how generally the results can be extended. Technically, effects similar to those of the original would be likely only if clients sampled from the same population were used and the approach were administered exactly as it was applied in the original study. When the approach studied comprises numerous variables, it is often difficult to know exactly how the intervention was carried out.

DEVELOPING CLINICALLY RELEVANT TREATMENT RESEARCH PROGRAMS

It should be clear that neither of these approaches to studying intervention is satisfactory. In fact, it is naive to think that either of these approaches could be entirely satisfactory. As is the case in all areas of science, answers to difficult questions are elusive. They rarely are apparent after a single experiment but, rather, are the outcome of carefully plotted, programmatic research courses involving multiple experiments, all designed to lead researchers closer to the answers to their most fundamental questions (Hegde, 1987). Such research programs are exceedingly difficult to find in child language disorders treatment research. The first step toward improving the relevance of our research to the clinic is to recognize that no single investigation is capable of giving us the answers we need to clinically relevant questions. Consequently, comprehensive programs of research are necessary and must be initiated.

The plan illustrated in Figure 3 is typically viewed as the model of choice (Hegde, 1987; McReynolds, 1983). By design, the researcher (or group of researchers) first discerns which individual components are effective. Then, as represented by Point 2 in Figure 3, effective components are systematically placed together into more

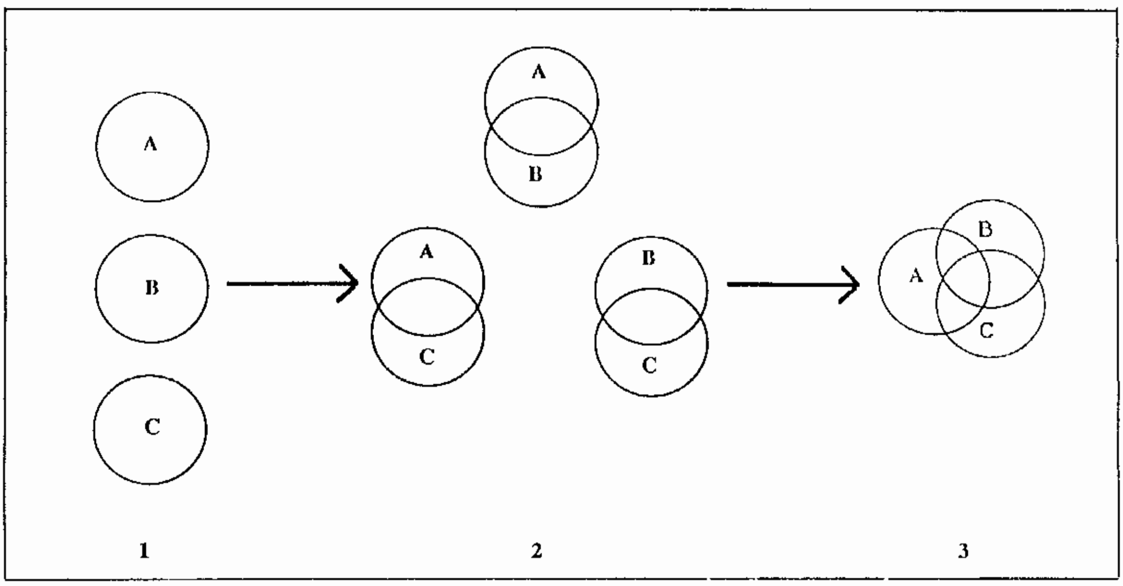

FIGURE 3. The "one variable at a time" treatment research strategy.

complex treatments to assess their combined effects. Eventually, the researcher reaches Point 3 where a host of treatment variables are combined into a package, and the package is evaluated.

Despite its many advantages, this research strategy has several drawbacks. For example, it is extremely slow (McReynolds, 1983). To carry out all of the necessary primary experiments and replications would take many years or careers. Teams of researchers can and should work cooperatively (and/or competitively) toward similar long-term objectives. This can lessen the burden and increase the efficiency of the strategy considerably. Still, the overall process will take a strong commitment and a very long time. Few language intervention researchers have been willing to make such a commitment.

A more important problem with this programmatic approach is that it is based on the strategy of combining variables that are shown to be effective in isolation into progessively more comprehensive programs. As I have already asserted, it is conceivable that some intervention variables that cannot be shown to have short-term effects nevertheless are effective over the long term, especially when they are combined with other key variables. Consequently, some variables that would be effective in a comprehensive treatment package might be discarded erroneously after early experiments testing them in isolated formats showed them to have little or no short-term effects. The research strategy illustrated in Figure 3 is an important one, but in my view, it must be supplemented with other research approaches.

Figure 4 differs from Figure 3 in only two respects. First, note the black arrows pointing in a right to left direction, from Point 3 to Point 1. These arrows represent what I regard as a viable and important treatment research strategy. In this strategy, the researcher develops broad but clinically relevant questions that can be addressed by evaluating the effects of treatment packages, as indicated at Point 3. Once the entire treatment approach is shown to be effective, the investigator can begin to break the program down, determining which components are essential and which are wasteful. This approach will also take a long time and will be very expensive. It has the virtue, however, of making it possible to assess relatively quickly the efficacy of intervention programs that are currently in use but that have not been empirically evaluated in any adequate manner (e.g., Tannock et al., 1990). Follow-up investigations would lead the investigator to more accurate statements of which combinations of variables were most responsible for the observed effects.

The second aspect of Figure 4 that distinguished it from Figure 3 is the presence of dotted arrows. The dotted arrows indicate the direction of influence of research outcomes. In other words, investigators using a left-to-right strategy should be cognizant of the results and conclusions of experiments performed at Point 3 when they refine their experimental questions and design their interventions. Researchers employing the right-to-left approach must be aware of developments occurring at Point 1 and Point 2 in developing their treatment packages.

Unfortunately, even if many researchers are actively involved in comprehensive research programs such as those illustrated in Figure 4, it will be impossible to evaluate experimentally all of the clinical variables known to operate in intervention contexts. Furthermore, the generality of the results of experiments must always be questioned. Therefore, as part of any concerted effort to evaluate the efficacy of language intervention programs, clinicians must play a critical role. This is shown

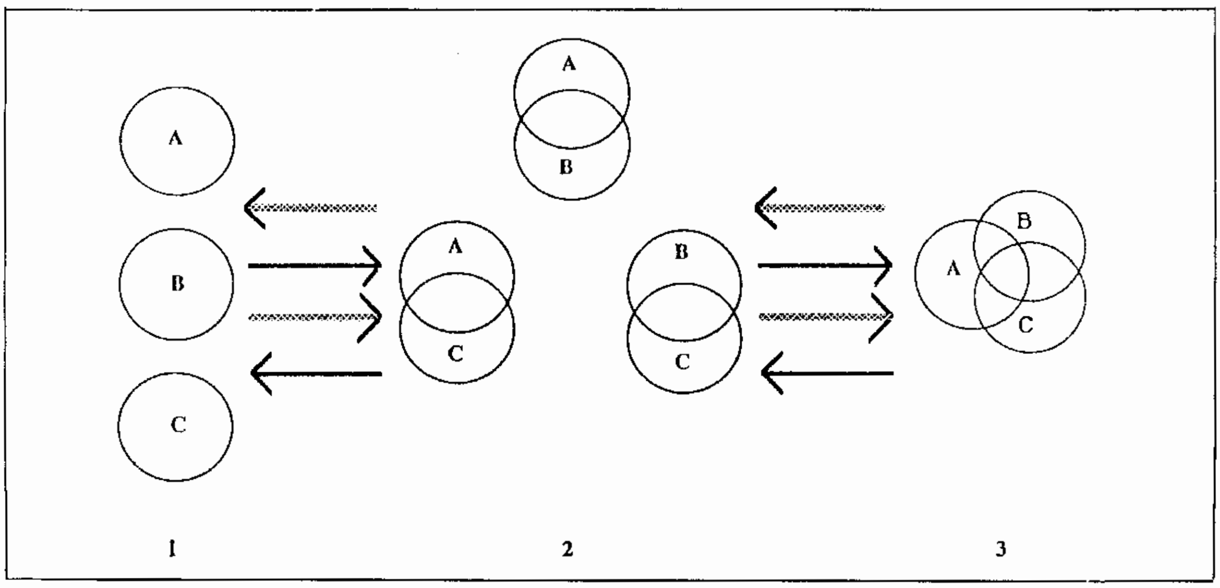

FIGURE 4. A treatment research strategy involving both "one variable at a time" treatment research programs and "right-to-left" program. *Note:* Solid lines refer to the direction of each research program component. Dotted lines refer to the direction of influence of one stage of the plan to other stages.

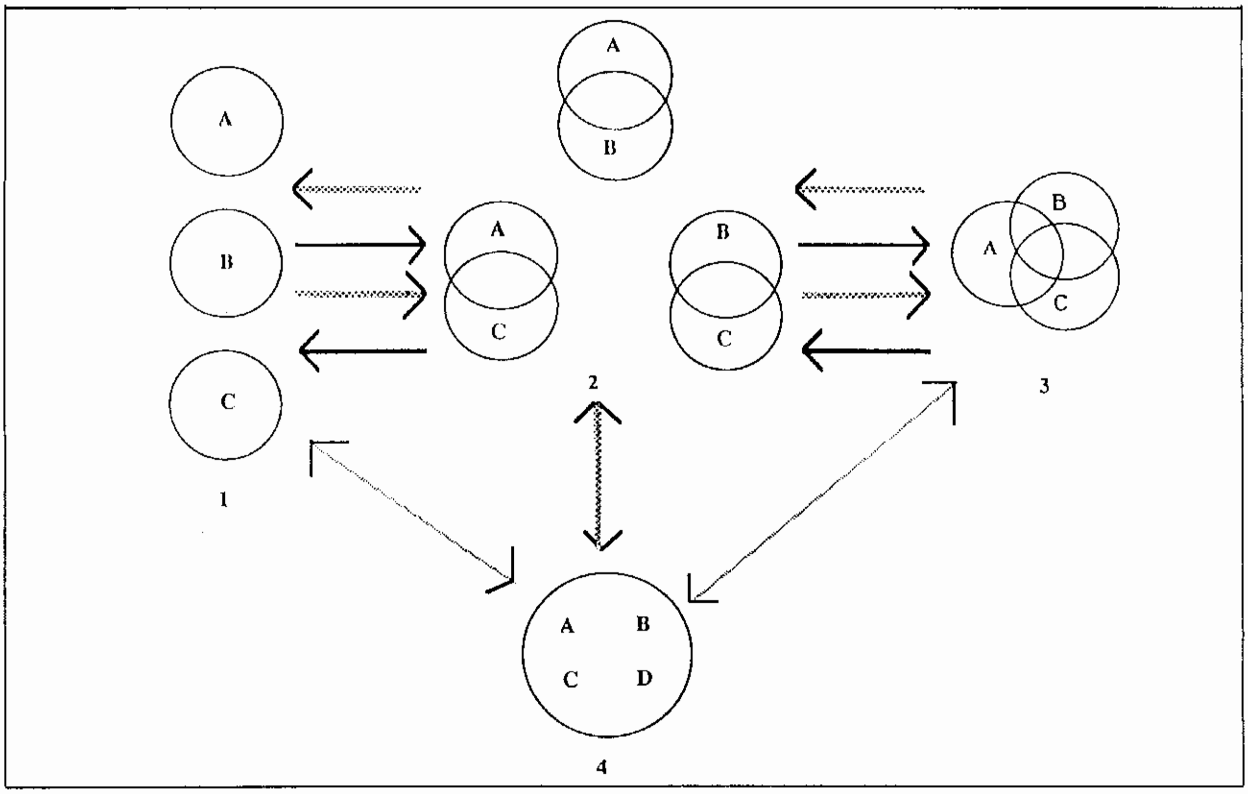

FIGURE 5. A treatment research strategy involving both research program strategies and the influence of practicing clinicians. *Note:* Points 1 to 3 refer to activities of researchers. Point 4 refers to activities of practicing clinicians. Solid lines refer to the direction of each research program component. Dotted lines refer to the direction of influence of one stage of the plan to other stages.

in Figure 5. Note that the arrows headed away from Point 4 in the figure are dotted. This indicates my belief that, although clinicians must make use of some treatment research methods to evaluate objectively their own practices, their basic role in the research enterprise is not to carry out primary research (see also, Laney, 1982). Rather, they can and must exert their influence in two ways. First, clinicians can have an influence on research at Points 1, 2, and 3 by working with and consistently informing researchers about their most basic clinical concerns. Intervention research programs that are designed to focus on clinically relevant problems will probably be most relevant to clinicians at all points in the research program. Clinicians can also inform researchers about the types of intervention strategies that they have developed and with which they are having some success (Laney, 1982). It is essential, of course, that researchers be highly responsive to the requests and expressed needs of clinicians as they develop research questions and plan research strategies.

There is a second important way in which clinicians can assist in the research and development of better intervention approaches. This involves applying knowledge gained from treatment research and from basic research to their own clients, where it is theoretically reasonable to do so, and reporting the results of those efforts in professional journals and at professional meetings. Case studies that may have no experimental foundation can be highly useful in informing clinicians about the possible generality of treatment experiment results. For example, MacDonald, Blott, Gordon, Spiegel, and Hartmann (1974) reported an experiment demonstrating the effectiveness of the Environmental Language Intervention Strategy (ELIS) on the language development of six children with Down syndrome. In a letter to the editor of *Journal of Speech and Hearing Disorders*, Simon (1976) reported on numerous successful applications of the ELIS procedures including "autisticlike" children, mentally retarded children, and learning-disabled children. Simon's report no doubt had an impact on the clinical community. More letters and case studies would have helped us all to get a better picture of the types of clients for whom this approach is and is not useful. This type of response by clinicians should also have sparked an interest in researchers in validating the reported experiences experimentally.

CONCLUSION

I am not so naive as to believe that researchers and clinicians will suddenly join in the spirit of cooperation and collegiality in long-term efforts to develop and carry out complex and sometimes terrifyingly labor intensive treatment research programs. Nor do I believe that even the type of cooperative research programs shown in Figure 5 will enable us to examine in detail all of those variables that might conceivably influence treatment results with all types of language-impaired children. Realistically, it will be decades before researchers and clinicians converge on results that are replicable and conclusive and that clearly show that certain treatment variables can be combined in various ways to the best effect with different types of language-impaired children. But I am convinced that clinician need and consumer protection are factors that should motivate long-term, intensive, and programmatic research efforts (see also, Connell, 1990). Indeed, until we take the initiative to develop coherent programs of research designed to address clinically relevant questions, the term *clinical science* will be a misnomer as applied to this area of our profession.

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THE FUTURE OF LANGUAGE SCIENCE: A PLEA FOR LANGUAGE INTERVENTION RESEARCH

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Implicit in the desire to solicit papers on the future of science and services is a concern that science has not impacted sufficiently on service delivery in the field of communication disorders. I will argue that language scientists in our field have not taken seriously their responsibility for producing research that will dictate the future of service delivery for children and adults with language impairments. I will attempt to analyze some of the reasons for the scarcity of our contributions to language intervention research literature and offer some suggestions for reversing this trend as we get ready to enter a new century.

The 1970s marked a time of growing interest and advocacy for the study of language development and language intervention. Within the American Speech-Language-Hearing Association, for example, there was a push for more courses and clinical practice in the language area. I am sure that many of those advocates expected that falling on the heels of research on language development would be increasing numbers of articles on language intervention. That has not been the case. Goldstein and Hochenberger (in press) reviewed child language intervention studies published between 1978 and 1988 in 17 journals. Language intervention research referred to data-based studies evaluating a treatment for a language disorder using single-subject or group experimental designs. They found an average of 13.7 articles published per year (without a striking upward trend). Much of this research has been conducted by scientists working in related disciplines, namely psychology and special education. Testifying to the relative lack of this type of scientific contribution from speech-language pathologists was the fact that only 17.2% of these articles (an average of less than 2½ articles per year) were published in the American Speech-Language-Hearing Association journals. There is reason for concern, when the discipline expected to offer leadership in ensuring that high-quality service is provided for individuals with language impairments is producing little of the experimental research on language intervention.

Judging from Thompson's (1988) review of adult language intervention literature, an even more serious dearth of intervention studies of adults with language impairments is apparent. Over a 17-year time span, Thompson found only 35 adult language intervention studies (at least 10 of which would not have met inclusion criteria specified by Goldstein & Hochenberger, in press) that included some measure of generalized effects of language treatment. The lack of adult language intervention research would appear to be more acute than in the child language area, perhaps because fewer investigators from other disciplines are contributing to this knowledge base.

SHOULD MORE LANGUAGE RESEARCH APPEAR IN FUTURE LITERATURE?

Clearly, the bulk of the research of language scientists deals with more basic issues and not with language intervention. Let me propose a continuum rather than a dichotomy so as not to get bogged down in a discussion of distinctions between basic and applied research. On one hand, one can argue convincingly that discoveries of profound applied significance have frequently arisen from basic research. But is this true of the work of language scientists, as well?

In the language literature, basic research might involve research on normal paths of development of language skills, factors affecting the production and comprehension of language behavior, processing abilities associated with differential performance on language tasks, comparisons in each of these domains among populations of individuals who differ in their language or cognitive abilities. Indeed, we continually tell our students that "basic" research on language development promises to provide knowledge that will prove applicable to the assessment and management of individuals with language impairments. But frankly, that promise is not kept very often. One need only look at the relatively small body of research literature on language intervention to see that few intervention studies stem from theoretical or empirical foundations growing out of more basic research literatures. I would argue that complaints we often hear about the lack of clinical relevance of articles in the

Journal of Speech and Hearing Disorders and the *Journal of Speech and Hearing Research* are warranted. People with a fascination with language may find most of the language literature interesting, but this literature provides little direction for either clinicians or scientists wrestling with language intervention.

Even if we heed the call for more applied research, it is fair to ask whether clinicians would be better served by the publication of more research on language intervention. A couple of notes of caution should be considered. First, treatment efficacy research is a difficult and expensive endeavor. Little useful knowledge will be derived from reports of incompletely developed treatments, even when seemingly robust effects are demonstrated. A fellow scientist has passed on a maxim from the applied behavior analysis folklore, arguing that it takes three studies on one intervention before really meaningful results are revealed. Scientists who demonstrate such persistence find it difficult to argue with this rule of thumb. This maxim is consistent with the obligation of scientists to guard against premature dissemination of research lacking adequate empirical support.

A different set of reinforcement contingencies exists in the professional community than in the scientific community, however. The continued hue and cry for more clinically relevant information from fellow professionals and professional organizations results in the continued acceptance of the dissemination (mainly at workshops and conferences) of treatment programs with no empirical support. Scientists who remain dependent on their scientific community for their livelihood may resist temptation. "Deserters" from the scientific ranks find a demand for their services, however. Perhaps scientists should be concerned, especially when their professional organizations sanction a willingness to accept shortcuts to knowledge. Surely, there is a danger to the profession and to clients in continuing to train speech-language pathologists to implement language intervention programs that have not been evaluated experimentally.

Second, one should realize that even if more language intervention research was conducted, few studies would be directly transferable to service delivery systems. That is, scientists conducting treatment efficacy research seek to control the intervention setting to investigate the role of selected environmental conditions on language learning. Generalizability of findings is always compromised to varying extents by the need to ensure internal validity. Nevertheless, it would seem that the job of translating more applied research to service delivery should be easier than translating more basic research. In either case, the job of rendering research findings applicable to service providers has been taken for granted. One might posit a continuum of empirical findings that vary along the dimension of applicability to service delivery (from no applicability to direct applicability). I believe that all scientists in the field of communication sciences and disorders could take greater responsibility for pursuing their lines of research further towards the goal of disseminating knowledge more directly applicable to service providers. Such a pursuit implies that language scientists need to be willing and able to delve into a variety of types of research, including language intervention research. I would submit that the bulk of child and adult language research is far from the direct application end of the continuum. Carefully crafted language intervention research could represent a much sought after bridge. In sum, although language intervention research is difficult to do well and usually is still not directly applicable to service delivery, it represents movement in the right direction.

HOW TO PROMOTE MORE LANGUAGE INTERVENTION RESEARCH

Although this seminar provides a forum for discussing relationships between science and service, it most likely will prove ineffective in changing the behavior of scientists. A cursory examination of research on the history and philosophy of science and on the behavior of scientists reveals that the problems are deep-seated and are not peculiar to our discipline. I will attempt to analyze why the future for research with more direct application to service delivery is not overly promising and to suggest some means of altering this trend.

Bannister (1970) offered an apt observation that seems pertinent to the training of behavioral scientists today:

> Had Christopher Columbus . . . possessed the mind of many modern psychologists, I am reasonably certain he would never have discovered America. To begin with, he would never have sailed because there was nothing in the literature to indicate that anything awaited him except the edge of the world. Even if he had sailed, he would have set forth bearing with him the hypothesis that he was travelling to India. On having his hypothesis disconfirmed when America loomed on the horizon he would have discovered the whole experiment null and void and gone back home in disgust (p. 6).

As Bannister points out, our training of behavioral scientists instills a sense of primacy in theory. In general, biases toward retention of one's theoretical perspective, even if held unconsciously, make it difficult to alter in any substantial way one's conceptual framework. Thus, scientific training may have a tendency to stifle exploration. The training of language scientists is no exception. Two impediments to the pursuit of language intervention research may be highlighted. First, language scientists typically receive little training in treatment efficacy research (a topic discussed further below). Second, prevailing theoretical accounts of language focus on the underlying organization of the language system. This never-ending search for more complete and accurate descriptions of language and related structural or cognitive systems continues to divert attention from exploration of the effects of environmental factors on language behavior (and those underlying systems).

What is proposed is not a dismissal of theory-driven research, merely a greater acceptance of research that is not designed to test theory. Even staunch critics of hypothetical-deductive approaches to scientific endeavor have acknowledged the contributions of theory. For example, Skinner (1950) contended that there is a need for a formal representation of the data reduced to a minimal number of terms and that a theory may yield greater generality than any accumulation of facts. Nevertheless, he proposed that developing an understanding of learning may progress most rapidly through research that is not designed to test theories. Language scientists, on the other hand, have not been too tolerant of research that addresses language learning among individuals with language impairments. Such endeavors are too often dismissed as trivial or misguided if not based on linguistic theories.

We need to explore ways to alter current trends. When it comes to an interest in language intervention, persons with the "Columbus mindset" still exist surely. One might argue that a good place to look is at people presently in our clinical ranks. On the other hand, speech-language pathologists who are trained by scientists are not immune from developing equally zealous belief systems about language, as well. Nevertheless, I would suggest that increased collaboration among scientists and clinicians is a strategy worth pursuing.

Collaboration with clinicians. Scientists must be aware of at least three guidelines if they are to implement this strategy of collaborating with clinicians effectively. First, the scientific community is not likely to be accepting of treatment efficacy studies justified in terms of a "good clinical idea." Hence, we must teach scientists to disguise practicality behind a robe of theory. Examples of such deceit are difficult to uncover, so I will draw from my own experience. Two studies on the effects of training sociodramatic scripts (Goldstein & Cisar, 1990; Goldstein, Wickstrom, Odom, Hoyson, & Jamieson, 1988) were couched in discussions of the desirability of encouraging sociodramatic play and the role of social scripts on conversational interaction in children. In reality, this study evaluated the idea of two teachers who thought that teaching children to play in sociodramatic activities would result in more independent play and social interaction during subsequent play times. The resultant studies did not simply evaluate the efficacy of an educational procedure, however. The intervention was altered and developed in ways that made it more effective clinically and more acceptable conceptually. Although these studies involved a bit of tedium and hard work for the scientists and educators involved, the collaboration was fun, stimulating, and fulfilling. Furthermore, by finding a theoretical hook on which to hang this research, the experiments also were published.

Second, one should be aware that treatment efficacy research in the language area is often a high-risk venture. Promising language intervention ideas do not always avail themselves to clear demonstrations of treatment effects. One's first suspicion might be that the intervention program is ineffective or perhaps inefficient, especially if logistical constraints prove a hindrance. In the language area, one must be suspicious of measurement problems, as our standardized measures in particular prove to be insensitive to behavior change. The need for innovative research into the development of sensitive and comprehensive measures of language is discussed below. The quality of scientists' and clinicians' achievements might best be measured by their ability to triumph over these setbacks and to develop intervention programs that show robust, clinically significant effects. But rarely will it be easy.

Third, in light of our second guideline, one must plan for three evaluations of a treatment program if you really desire to "get it right." Assuming that one has identified a clinically important problem, adaptations of the treatment protocol, measurement schemes, experimental designs, subject selection criteria are possible. What often is reflected, however, are refinements in the formulation of questions. In science, progress can be conceptualized in terms of more refined answers to long-standing questions or in terms of more refined questions. The latter is more likely to require that one stretch one's imagination and look at old problems from new angles. Scientists and clinicians working together on a problem may help cultivate creativity that alone they are not likely to explore adequately.

Dealing with constraints on scientific endeavors. Philosophers of science often have been critical of modern psychologists. Grover (1981) noted that theoretical biases sometimes have restricted the range of problems considered and the perspectives held; she states, "Psychologists often tend to place artificial limits on the research enterprise by prematurely designating particular topics and research strategies illegitimate" (p. 41). An example that hits close to home is the claim by a prominent language scientist who concluded in a discussion of theoretical accounts of language that "the reasons for the shift from behaviorism to mentalism are compelling" (Muma, 1978, p. 241). Assigning a death sentence to operant learning approaches is an obvious contradiction to current trends. The bulk of language intervention research continues to emanate from behavioral perspectives. However, progress emerging from an applied behavior analysis perspective, much of which diverges from Skinner's analysis of verbal behavior (1957), has been disregarded. There remains a tendency to characterize behavioral perspectives based upon old stereotypes that often bear little resemblance to state-of-the-art views and practices.

Difficulties in gaining acceptance of scientific work cut across disciplines, as this quote from a letter written by Thomas Huxley illustrates:

> I have just finished a Memoir for the Royal Society, which has taken me a world of time, thought, and reading, and is, perhaps, the best thing I have done yet. It will not be read till May, and I do not know whether they will print it or not afterwards; that will require care and a little manoeuvering on my part. You have no notion of the intrigues that go on in this blessed world of science. Science is, I fear, no purer than any other region of human activity; though it should be. Merit alone is very little good; it must be backed by tact and knowledge of the world to do very much.
>
> For instance, I know that the paper I have just sent in is very original and of some importance, and I am equally sure that if it is referred to the judgment of my "particular friend"——

> that it will not be published. He won't be able to say a word against it, but he will pooh-pooh it to a dead certainty . . . . So I must manoeuvre a little to get my poor memoir kept out of his hands (L. Huxley, 1902, p. 109f.).

A number of strategies might be used to remedy such hindrances to language research applicable to service delivery, such as behavior analytic research. First, one might champion the cause for a "kinder and gentler" scientific community. Publication boards and editors might be called upon to assist with the education of reviewers. The ability of scientists to review, like the ability of professors to teach, is taken for granted in institutions of higher learning. The ability of scientists to find flaws may be carefully honed, but little attention may be given to refining one's ability to frame criticisms constructively and to maintaining a positive tone. Moreover, it is difficult for the theoretical biases not to be influenced by one's evaluations of research, position papers, and review papers submitted for publication. But when research not consistent with current trends (be it behavioral, ethnographic, etc.) is submitted, great care is needed to ensure that rejection is not a euphemism for censorship.

A second strategy that must be pursued is to teach young scientists how to "maneuver," as Huxley called it. Perhaps persistence should be taught as a foremost response to rejection. My fear is that young scientists too quickly become disillusioned, especially if they do not know what to expect. Young scientists are not handled with kid gloves and perhaps they should not be; it is the senior scientists who are given the benefit of the doubt. On one hand, it would be deleterious to the scientific community to publish articles of questionable quality. On the other hand, the desire to accept top quality treatment efficacy studies has resulted in increasingly stringent criteria. One must recognize the danger of imposing criteria that transcend the constraints of typical economic resources and logistics of working in applied settings.

Liberalizing scientific training. Similar to effects of prejudices stemming from language scientists' theoretical frameworks, are the biases emanating from orthodox perspectives on *the* scientific method. Most research training programs are built upon formalized constructions of statistics and the scientific method. Statisticians and methodologists purport to tell us "how problems arise, how hypotheses are formed, deductions made, and crucial experiments designed" (Skinner, 1956, p. 222). The crucial aspects of the discovery process, the behavior of scientists, have not been formalized. We "cannot refer the young psychologist to a book which will tell [one] how to find out all there is to know about a subject matter, how to have the good hunch which will lead [one] to devise a suitable [method], how to develop an efficient experimental routine, how to abandon an unprofitable line of attack, how to move on most rapidly to later stages of [one's] research" (Skinner, 1956, p. 222).

Teaching young language scientists how to evaluate the effects of language intervention programs experimentally is not difficult, but it is insufficient. Incorporating courses on single-subject experimental design into graduate curricula is now common, but it is not enough. The necessary and sufficient conditions that might produce good scientists cannot be explicated. Nevertheless, a more liberal, if not more humble and more humane, outlook needs to result. There needs to be greater understanding of the distinctions between the methods of science and the pursuit of science. There needs to be an appreciation of the numerous reasons scientists perform experiments besides "to evaluate hypotheses" (cf. Sidman, 1960). Language intervention procedures need to be developed, not just evaluated; the sources of variation in experimental results need to be explored rather than lamented; psychological principles need to be discovered, not just applied to language intervention. There has been a long history of resistance to experimental analyses of human behavior (Grover, 1981; Kantor, 1953), and nowhere is that more apparent than in the study of language behavior. It is the professional obligation of scientists in our field to not only study language behavior, but to discover and apply behavioral interventions that will enhance the communicative functioning of children and adults with language impairments.

Recruiting language scientists. Much of the discussion above relates to how to train future language scientists. The assumption that we will have sufficient numbers of prospective language scientists entering the field needs to be called into question. The most common approach to recruiting students for our doctoral programs is to seek out the best and brightest prospects from a larger population of students interested in pursuing advanced graduate training. There is probably a strong relationship between our assessment of prospects and the extent of exposure to or participation in relevant academic, clinical, and especially research experiences. Rarely is this exposure to research accomplished without a great deal of self-selection by students. Of course, there is a fear among faculty members that involving the wrong students could taint our research enterprise; so selection by faculty also must be considered. Limits on selection and limits on opportunities have serious ramifications, however.

What we have failed to do in any systematic fashion is to enlarge the population of potential students who might flourish if trained as language scientists. We need to attempt to build the interest and instill the knowledge necessary to be a language scientist among undergraduate and master's level students. The alternative strategy of simply seeking those with the interest and knowledge represents a condemnation of our educational institutions. Concerted efforts that include but are not limited to research practicum experiences need to be employed. Practitioners, supervisors, and administrators in our field need to shoulder some of the responsibility for this objective, in addition to professors.

Most people who enter the field of communication disorders are interested in helping others. Skinner (1972) has pointed out that helping others as a clinician is necessarily a short-term measure and that there are other ways to reach the same goal:

> A classical example from another field is Albert Schweitzer. Here is a brilliant man who, for reasons we need not examine, dedicated his life to helping his fellow men—one by one. He has earned the gratitude of thousands, but we must not forget what he might have done instead. If he had worked as energetically for as many years in a laboratory of tropical medicine, he would almost certainly have made discoveries which in the long run would help—not thousands—but literally *billions* of people. We do not know enough about Schweitzer to say why he took the short-term course. ... Whatever his reasons, his story warns us of the danger of a cultural design which does not harness some personal reinforcement in the interests of pure science. The young psychologist who wants above all to help his fellow men should be made to see the tremendous potential consequences of even a small contribution to the scientific understanding of behavior (p. 322).

The paucity of applicants to our doctoral programs these days means that many people who are capable of making a long-term contribution to our field are being enticed by prevailing contingencies of reinforcement into short-term contributions (and not necessarily into other fields). A profession (and a society at large) that does not show adequate regard for the long-term contributions of their scientists cannot expect anything different.

I would assume that students are most often motivated to enter the field of communicative disorders because they would like to learn how to ameliorate clinical problems. Although this motivation is seemingly consistent with language intervention research, opportunities for students to participate in such endeavors are limited because of the lack of language scientists pursuing such research. An increase in both status and amount of language intervention research is needed to make doctoral training more attractive to prospective language scientists.

One might look to clinical settings to find models of language intervention research programs. Unfortunately, this is unlikely to be the case given the preeminence of billable contact hours. One might hope that the call for "outcome indicators" and greater accountability from the insurance industry and government regulators would be translated into resource allocations for clinical research. It would seem to fall to professionals and language scientists, not administrators, regulators, or reimbursement agents, to lead such a fight on the behalf of their clients.

In short, recruitment of language scientists represents a pervasive problem. We need to develop a larger pool of good prospects, our profession and society need to attach more value to scientific contributions, our programs need to be able to attract and meet the needs of students interested in clinical research, and professionals in clinical settings must advocate research. The success of our recruitment efforts will bear directly on the survival of the field itself or at least the position of the field as a contributor to language science.

FUTURE CLINICAL RESEARCH

The final portion of this paper seeks to illustrate how there is no shortage of ways in which research pursuits might be expected to advance service delivery in the language area. Four general directions that are ripe for further research activity pertinent to service delivery are outlined.

Using descriptive research to develop interventions. Much of the language literature is comprised of descriptive studies. Although one can argue that most of these studies should be valuable in developing intervention programs, the likelihood seems small unless the development of more effective language intervention strategies is a primary goal of the researcher. Four research strategies exemplifying the coupling of descriptive and applied research are offered below. The key to much of the success of the applied researcher is the selection of appropriate language goals. One strategy has been to identify seemingly facilitative behaviors and subsequently to evaluate their function by measuring the effects of increasing the frequency of those behaviors. A second strategy has been to identify behaviors that are disruptive to interaction and subsequently to teach alternative behaviors. A third strategy has to do with identifying what variations of behavior should be taught and in what variety of situations to maximize generalized effects for individual clients. A final strategy involves the use of descriptive data as a yardstick against which the effects of intervention can be measured.

First, based on observational studies of normal ecologies it is possible to identify behaviors that are most effective in maximizing communicative functioning. Then one can capitalize on behaviors likely existing or emerging in participants' repertoires to improve one's interventions. For example, much of the literature on the systematic use of peers as intervention agents was based on ecobehavioral analyses of social and educational interactions in naturalistic settings. Based on determinations of effective strategies for promoting interaction (Goldstein & Kaczmarek, in press; Tremblay, Strain, Hendrickson, & Shores, 1981) interventions were developed. When peers have been prompted and reinforced for using behaviors thought to facilitate interaction (e.g., suggesting joint play ideas) substantial improvements in social and communication skills in classmates with disabilities resulted (e.g., Goldstein & Ferrell, 1987; Hendrickson, Strain, Tremblay, & Shores, 1982; Odom, Hoyson, Jamieson, & Strain, 1985; Shafer, Egel, & Neef 1984).

In a similar vein, Greenwood and his colleagues (1984) determined from descriptive research that achievement in language-related academic skills (i.e., spelling, reading, and math) was correlated with opportunities to respond (or active academic engagement). They developed peer tutoring programs resulting in tremendous increases in opportunities to respond. Significant improvements in achievement accrued for children who were normal, at-risk, and learning disabled (cf. Greenwood, Delquadri, & Hall, 1989). Other, more adult-mediated, intervention procedures also might have been developed based on these descriptive data. Nevertheless, these examples of the development of two types of peer intervention programs targeting communicative interaction skills and academic skills hint at the increasing number of individuals (peers, siblings, teachers, parents, spouses, and caregivers) who might take part in language intervention programs. Thus, speech-language pathologists' traditional roles are likely to change as researchers demonstrate how other "intervention agents" can be taught to optimize environments for the learning and use of language skills by individuals with language impairments.

Second, Carr and Durrand (1985) used descriptive research to generate hypotheses about environmental factors that evoked disruptive behavior in children with autism. They were able to demonstrate in subsequent experimental analyses that inappropriate behavior could be controlled either by altering the environment or, more importantly, by teaching alternative communicative behaviors. Gallagher (1990) has proposed a similar tactic for identifying and remediating interaction deficits among children with specific language impairments. She proposes identifying behaviors that are socially penalizing in peer interactions and then teaching alternative communicative skills.

The third strategy emphasizes that, even when one has decided what to teach, a careful descriptive analysis of the natural environment may be required to devise an effective intervention program. Horner, Bellamy, and Colvin (1984) have argued that descriptive analysis of natural contexts in which language skills are used is necessary to maximize the functionality and generalization of skills selected to be taught. The general case programming approach has shown the value of cataloging the variations in situations, circumstances, responses, and consequences to identify an *instructional universe.* The selection of a few goals (or general cases) is done to sample broadly across the instructional universe so as to maximize the likelihood of appropriate generalization of skills within the instructional universe. This approach diverges from the idea of facilitating generalization by selecting goals likely to be functional or receive natural reinforcement in the client's environment. A broader view of the circumstances under which those goals and their variations are likely to be most relevant requires more exact teaching, but promises much broader generalization.

Although the general case programming approach has proven valuable in treating a variety of complex task sequences (e.g., grocery shopping, street crossing, vending machine operation, telephone use), its application has yet to make its mark in language intervention research (cf. Horner & Billingsley, 1988). Goldstein and Kaczmarek (in press) noted that a general recognition of the complexities involved in adapting interaction to different types of situations and different types of children is required to enhance training efforts in peer-mediated intervention studies. Instead of setting up "generic" role play situations to illustrate the behavior of a child who is socially withdrawn, one might teach peers to recognize different interaction scenarios (cooperative play, isolated play, and no play). Then peers can be taught how to vary their use of interaction strategies depending on different antecedent conditions.

The fourth strategy is an extension of the use of normative data, but collected specifically for the purpose of developing beforehand a criterion for measuring the successfulness of an intervention (Goldstein, 1990). Language scientists and clinicians have relied heavily on developmental information using a relatively small database of normative information. Social comparison data have been valuable in substituting for the limitations in our normative database. Collecting social comparison data is akin to developing a specialized normative sample. Data are collected using the same measurement system, procedures, and settings as in one's experimental protocol, but with an appropriate peer group to which experimental subjects ultimately should be compared. This data set can be valuable in establishing an appropriate criterion for terminating intervention. This criterion could be based on quantitative data, such as the rate of occurrence of certain behaviors, or qualitative data, such as certain language content (e.g., variety of topic initiations).

Note that the research highlighted illustrating ties between descriptive and applied research has stressed the need to study the natural ecology of individuals with language impairments. I believe this to be a particularly promising approach, but we should not overlook how similar advances have been generated from laboratory research. A classic example is the work on short-term memory done with individuals with mental retardation. Belmont and Butterfield (1977) studied how memory performance varied given a myriad of task variations before devising a program for teaching individuals with mental retardation a strategy that could be used to enhance their short-term memory abilities. I am not aware of research that sought to evaluate or enhance the impact of this improved cognitive ability on everyday life, however.

Evaluating treatments. There have been a number of advances in techniques for teaching language skills. Goldstein and Hochenberger (in press) discussed five themes that characterized much of the child language intervention research in recent years: (a) the development of augmentative and alternative communication systems, (b) the provision of language stimulation to take advantage of observational learning, (c) the teaching of various language functions, (d) the teaching of language as a means of environmental- and self-control, and (e) the study of different types of generalization processes. It is likely that contributions to our knowledge base in each of these areas can be found in the adult language intervention literature as well.

Classifying advances in treatment procedures is difficult. Examples will be drawn first from didactic teaching situations and then from more naturalistic teaching situations.

There have been hundreds of investigations of a myriad of prompting procedures used to teach a variety of skills in didactic teaching situations. Wolery and Gast (1984) were able to extract a set of six basic procedures (i.e., most-to-least prompts, graduated guidance, system of least prompts, time delay, stimulus shaping, and stimulus fading) that distinguish among the variety of methods that have appeared in the literature. Many of these methods for manipulating stimuli and responses to prompt correct responses and for shifting control to more relevant, naturally occurring stimuli have been developed in tightly controlled clinical environments. Nevertheless, their development was in response to very real problems of teaching difficult-to-teach skills primarily to children with mental retardation or autism. Given the abundance of "basic" research in this area, the potential for using improved instructional procedures with a variety of language skills has not been tapped. Moreover, there is a need to take these intervention strategies developed in highly controlled contexts and to design and evaluate practical ways to implement them in typical clinical contexts.

Other advances in more naturalistic teaching techniques have been refined from observations of parent-child interaction and systematized into potent procedures for teaching language skills in more naturalistic settings, such as classrooms and homes (Hart, 1985). Milieu teaching procedures have been used to promote the use of progressively more linguistically sophisticated request forms first with at-risk children and more recently with children with more severe language impairments (Warren & Kaiser, 1986). Analogous procedures exist for teaching language forms in the context of other communicative functions such as commenting (Cole & Dale, 1986; Friedman & Friedman, 1980; Leonard, 1981), but they have yet to be systematized to the same extent as milieu teaching procedures. Similar procedures have been applied to adults with Broca's aphasia, but with a different result (Doyle, Goldstein, Bourgeois, & Nakles, 1989). Instead of promoting more elaborated language forms, more successful communication resulted when the clients were encouraged to produce forms that were simpler rather than more advanced grammatically.

Contributions to our knowledge base continue to come from a variety of perspectives. Consequently, there is considerable danger in encouraging language intervention research that is consistent with current fads. Education is full of examples of new policies being promoted with little if any empirical basis. Even with some empirical support, language scientists need to be cautious about efforts to constrain evaluations of alternative intervention approaches. If, for example, we were to jump on the "naturalistic language intervention" bandwagon, as appealing as it seems, we might stifle future progress by ignoring the history of contributions to clients and to science that have accrued from more didactic teaching methods.

Much of the research that needs to be done by language scientists involves adapting previously applied treatments to new behaviors and to new client populations and evaluating effectiveness. This type of research is not viewed as terribly glamorous, but these types of systematic replications are crucial to determining the boundaries of effectiveness that too often can be blurred either by group designs or inadequate attention to subject selection or goal selection in any experimental design.

An emphasis should be placed on developing and refining intervention programs rather than comparing treatment approaches. Well-developed interventions should be refined to the point that they can be implemented reliably and their effectiveness and efficiency have been optimized. That is, refining treatments implies not only the development of effective treatment packages, but also the clear specification of treatment components. Investigations in the adult language intervention literature have investigated whether language therapy in general helps aphasics in general (Howard & Hatfield, 1987). Such studies suffer from the need to specify what constitutes language therapy and the corresponding need to ensure treatment fidelity. Furthermore, the demand to know what kinds of aphasia need what kinds of treatment limits the usefulness of such general research questions (Byng, 1988).

Too often our experimental design courses focus on comparing treatments. One should question the wisdom of conducting comparison studies to compare language curricula or treatment packages. The problem is that many treatment comparisons in our field have examined treatment packages that have not been well-developed. Treatment comparison studies are expensive endeavors, and resources for fair tests should be conserved for comparisons of two or more treatments that have been optimized. It is likely that overall differences in treatment effects will be small when intervention programs have been well-developed. But at that point it might be interesting to investigate treatment by aptitude interactions. That is, differential effects for subpopulations of subjects or behaviors may be detectable for the treatments.

Tailoring interventions to individuals. Clinical wisdom tells us to consider carefully the match between the abilities of individual subjects and what we are trying to teach. Unfortunately, too little research has sought to investigate how intervention procedures should vary depending on individuals' behavioral repertoires that are relevant to particular objectives or particular teaching techniques. This type of research should provide information clinicians can use to predict the likelihood of success of intervention efforts and help guide the modification of intervention efforts when clinical outcomes fall short of predictions. For example, Goldstein (1985) summarized how matrix-training techniques should differ depending on the relationship between an individual's lexical repertoire and the semantic relation being taught. A similar set of relationships needs to be uncovered for other phenomena. For example, research on observational learning might elucidate relationships between individuals' repertoires and skills being taught to be able to predict what skills are likely to be learned and what skills are unlikely to be learned through observation in similar situations. The interesting question is not *who* will learn modeled language skills observationally, but *when* individuals will learn new modeled responses and how one can expand that ability.

Unsuccessful intervention efforts will continue to be a rich source of research ideas. It may be convenient to point to a mismatch between child abilities and language objectives when intervention procedures are not particularly successful. But one needs to pursue that line of reasoning further. For example, a careful task analysis of one's language objective might uncover potential prerequisite or basic skills that need to be addressed. Continued attention needs to be directed to developing methods for teaching basic skills. For example, the ability to learn conditional discriminations is crucial to much of linguistic and cognitive functioning. Recent efforts to teach individuals with profound mental retardation have developed teaching techniques for skills that this population was thought incapable of learning (e.g., McIlvane, Bass, O'Brian, Gerovac, & Stoddard, 1984).

Alternatively, it is sometimes necessary to question one's choice of language objectives altogether. A conventional language system need not be thought of as necessary or even desirable for many clients. Growing attention is being paid to identifying alternative systems and compensatory skills that can be taught to enhance communicative functioning. Augmentative or alternative communication systems, compensatory skills, and self-prompting strategies have enormous potential for substituting for attempts to retrain lost skills due to neurogenic insults or for attempts to train difficult-to-teach skills. Similar procedures could be used for a variety of purposes. For example, one must distinguish between the use of sign language or an augmentative communication system as a prompting procedure (e.g., Carr & Dores, 1981; Hoodin & Thompson, 1983; Kearns, Simmons, & Sisterhen, 1982; Yoder & Layton, 1988), as a limited means of controlling one's environment (Reichle & Brown, 1986; Romski, Sevcik, & Pate, 1988; Sommer, Whitman, & Keogh, 1988; Steele, Weinrich, Kleczewska, Carlson, & Wertz, 1987; Tonkovich & Loverso, 1982), or as a complete communication system enabling the variety of functions associated with conventional language systems.

Improving systems for measuring effects. Most language scientists seem to be accepting the perspective that language can only be understood in a larger developmental context. Given the pervasive reciprocal effects among language, social, cognitive, socioemotional, academic, and vocational domains, effective intervention programs might have wide reaching effects. The development of comprehensive and sensitive measures of linguistic skills alone continues to present significant challenges. Nevertheless, the complexity of such endeavors is compounded by the need to consider cognitive and social influences on language development. This view is consistent with the clinical view that focuses on a person's overall functioning, often employing interdisciplinary teams. Unfortunately, our ability to document secondary changes that result from modifying one's linguistic repertoire has not kept up with theoretical perspectives. Thus, another need for researchers is to develop more enlightening measurement systems to identify secondary effects of intervention efforts in linguistic, cognitive, social, socioemotional, academic, and vocational domains.

I am not sure that many language intervention programs have such far reaching effects at this point. Nevertheless, some language researchers have sought to identify heuristic strategies that help children learn independently, allowing them to exploit whatever environmental circumstances they confront. For example, Shatz (1987) identified "bootstrapping operations" that she proposes can be used to acquire all sorts of knowledge about language. Warren (in press) suggested that some of the following strategies may be fundamental to language learning: spontaneous imitation, social referencing, initiating frequently, and asking questions. It is difficult, however, to document experimentally the ultimate effects that can be attributable to specific strategies. So one challenge is to develop interventions to facilitate the generalized use of such learning strategies, and another challenge is to assess effects.

Finally, language intervention efforts need to incorporate social validity measures to ensure that treatments are acceptable and outcomes are perceptible to clients and others in the client's social milieu. One must select from among a variety of strategies for assessing social validity. Social validity measures could assess satisfaction with outcomes and whether the behavioral changes evidenced are likely to impact in any significant way on communicative functioning. Also, one might assess satisfaction with the treatment program itself to ensure that the treatment outcome is sufficiently large to outweigh the effort and resources allocated to the intervention program. Or if the intervention program is viewed as noxious, it may be that alternatives must be found regardless of the treatment outcome. At this point, the more typical measures of social validity have assessed the perceptibility of outcomes. Judgments of pre- and post-intervention communicative functioning have been based on responses to questionnaires by people familiar to the clients (Bourgeois, 1990). In other studies, blind raters have made judgments based on tape recordings of communicative interaction (Angelo & Goldstein, 1990; Doyle et al., 1989).

CONCLUSION

The field of communication sciences and disorders faces a significant challenge for the 1990s in the language area. One would assume that the academic training and clinical experiences of speech-language pathologists have put us in a unique position to direct the future of service delivery for individuals with language impairments. But recent history shows an underrepresentation of our contributions to language intervention research literature. Fortunately, other disciplines have helped us build an empirical foundation related to our service delivery. But that foundation is a meager one. And clearly speech-language pathologists focus on an extraordinarily important aspect of human development and functioning. Hence, it behooves us to shore up that foundation and to take more responsibility for directing our own destiny when it comes to improving service delivery.

My goal is to have us explore ways to encourage applied research sorely needed by our clinicians and our clients. It is not my purpose to denigrate basic research or any other type of research endeavors for that matter. Stating the need for more language intervention research is a simplistic strategy for increasing productivity in this area. A more concerted effort needs to be directed to developing more nurturing professional and scientific communities within the discipline.

We need to examine whether the discipline has constrained scientific pursuits in the area unnecessarily and unwisely. We need to be more accepting of diversity in methodological and theoretical approaches. We need to realize that research inspired by clinical problems can yield scientific discoveries, some of which will be important enough to direct theory development; theory need not always direct the development of interventions. We need to be more encouraging of creativity. We need to realize that some of the most promising ideas for improving language intervention have come not from the language literature, but other literatures (e.g., applied behavior analysis, mental retardation, early intervention, stimulus control, and instructional technology). We need to be more encouraging of persistence. We need to realize that producing findings worth disseminating is not going to be easy in the language intervention area.

The future is rich with possibility if we take steps to foster growth in ways that intertwine science and service. Language encompasses such a large repertoire of skills that much work will be needed to outline the best paths for optimizing individuals' communicative capabilities. Nevertheless, the short-term gains of treating individuals and the long-term gains of producing scientific discoveries should be exciting and fulfilling endeavors.

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THE FUTURE OF LANGUAGE SCIENCE: IMPACT ON CLINICAL PRACTICE

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The President of the American Speech-Language-Hearing Association (ASHA), Dr. Roy Koenigsknecht (1990), recently reflected on the role played by the ASHA Task Force on Research in identifying the research needs of speech, language, and hearing scientists, the role of the publications of the association, the efforts of the Scientific and Professional Programs Board, the information provided by ASHA's Research Division, and, finally, this seminar on the "Future of Science and Services." He emphasized that ASHA "takes research seriously indeed" and that it is his perception that we must shape our own destiny, rather than leaving it to those outside our professions to conduct the research that will ultimately determine both our theoretical base and our clinical practice. He concluded by indicating that speech-language pathologists and audiologists should "spearhead" this effort, since "we know best what we need to know" (p. 14).

My first task today is to comment on the two preceding language science papers: Dr. Marc Fey's *Understanding and Narrowing the Gap between Language Treatment Research and Clinical Practice,* and Dr. Howard Goldstein's *The Future of Language Science: A Plea for Language Intervention Research.* Both papers addressed an important topic—intervention. Although not the whole of language science, intervention is studied for several reasons: (a) to test the theories of language acquisition and disorders, (b) to test the efficacy of certain interventions, and (c) to test the effectiveness of interventions across contexts (settings, clinicians, etc.).

My second task is to comment on current and future trends in language science. From my perspective, if speech-language pathologists and audiologists are to succeed in their quest for substantive outcomes from such research, they must have, and will continue to require, partners from other disciplines to develop the theoretical base upon which the delivery of services to individuals with language impairment must rest.

TRENDS IN LANGUAGE

Over the past few decades, speech-language pathologists have vastly expanded their services to those with childhood language disorders. As Shewan (1988) reports, 89.3% of speech-language pathologists regularly serve this population (see Figure 1). With the advent of P.L. 99-457, there is now specific interest in the "language" of infants and toddlers, and a resurgence of interest in child language acquisition and language disorders. In addition, more than 40% of all speech-language pathologists serve other disorders that require expertise in language (e.g., stroke, traumatic brain injury, and degenerative neurological disorders). Attendance at any state or national convention provides further evidence of clinicians' increasing interest in language disorders.

There has been growing interest among researchers and clinicians to view language within a number of naturalistic (real life) contexts, stemming from converging theoretical findings that emerge from a number of disciplines. "Language in context" is expoused by those who conduct descriptive research in language, but, in addition, is also supported by sociopolitical movements in education and healthcare settings.

External factors are impinging on our profession in general and on the language area in particular. Federal and state legislation increasingly has an impact on the autonomy of our profession. Not only has the specter of external control of our clinical practice by legislative or third-party payor fiat arisen, but depleted federal budgets for field-based research have drastically reduced support. Indeed, Congress's ever-skeptical view of anything but the most applied research is cause for considerable concern, since "pay-off" for more basic language acquisition research is not likely to have a high priority. Even with the increase in speech, language, and hearing disorders occurring throughout our nation, support for language intervention is also low priority.

Another trend is to view language in a broader context of language learning and its relationship to literacy and academic success. ASHA's recent symposium, *Partnerships in Education: Toward a Literate America* (Stewart, 1989), described these relationships. The popular press and a review of the literature supports the importance of language and literacy in a post-industrial society.

As we shall see, there are a number of other perspectives on science, particularly language science, that will

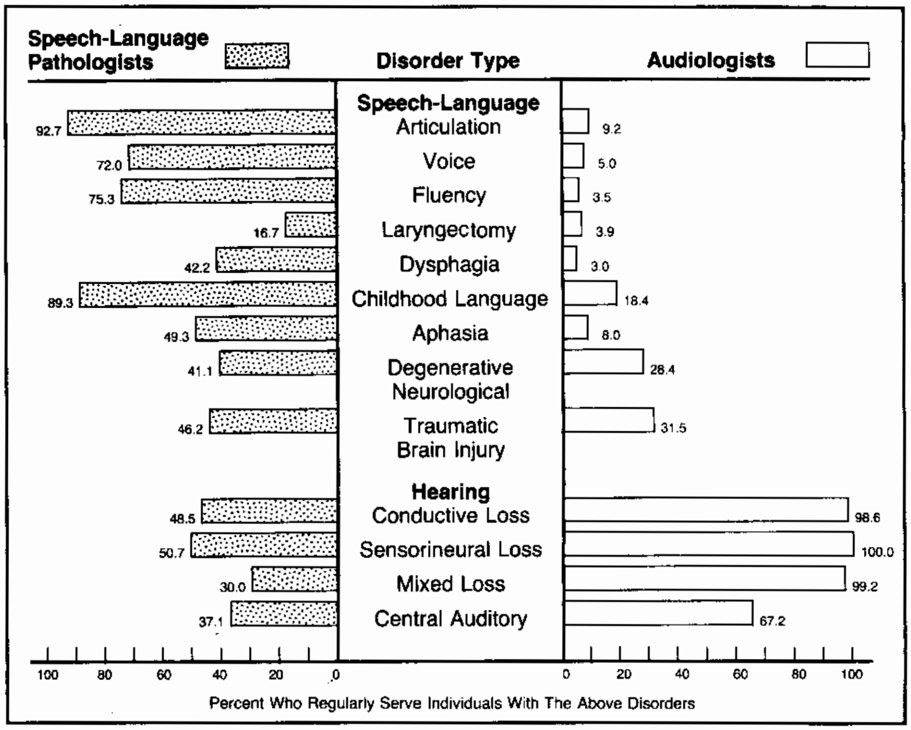

FIGURE 1. Percent of speech-language pathologists and audiologists who serve individuals with communication disorders. Reprinted from *Asha, 30*(8) 1988.

be addressed in the sections to follow. Certainly, language science reflects behavioral, cognitive, and linguistic theoretical perspectives—all three inherent in the presentations of Dr. Fey and Dr. Goldstein. Dr. Fey, for example, provides a view of how the myriad of variables interacting in complex ways affect the intervention process. We also have an opportunity to view an elaborated, broad-based clinical intervention plan that might assist us in further defining desirable treatment programs.

FEY'S VIEW OF NARROWING THE GAP BETWEEN RESEARCH AND PRACTICE

In this report, Fey recounts the difficulties encountered in implementing well-designed research that attempts to evaluate "effective and efficient language intervention approaches." Absolving clinicians from the assumption of research tasks, due to the external constraints on almost all clinical settings, Fey nevertheless argues that clinicians can and must play a vital role in establishing new treatment program paradigms. Fey believes that cooperative, programmatic efforts constitute an answer to improving clinical practice through research.

Fey provides commentary related to his earlier work (Fey, 1986), which stressed the importance of goal statements, be they basic, intermediate, or specific goals and subgoals. All basic goals are seen as derived from a careful assessment of communicative performance in naturalistic contexts and should be responsive to the needs of the language-impaired child as derived from that assessment. As Fey notes, the first and most important step for the clinician is the development of a hypothesis regarding the child's language status, which can then serve as a cornerstone for an intervention program. It is apparent that Fey favors broader goal orientation over the teaching of specific language procedures, but that, within this paradigm, he also views generalization of language learning and utilization processes and criteria by which language intervention must be evaluated.

Within his careful description of the hypothesis-generating process by clinicians, Fey advocates for a contrasting model in which specific goals are viewed as being best taught within a context of multiple goal orientation. Treatment outcomes are dependent upon the intervention agent, the procedures, the "goal attack strategy," and the activity. Fey describes the dramatic differences that may stem from how those components are related to the clinician's selection of goals.

Fey's concern for ecological validity reflects his premise that natural contexts are the appropriate scenario for language intervention, particularly over the long term. Fey notes that short-term and rapid gains in lower order language skills may actually sacrifice slower but more broadly based language changes as they occur over time in the real world. Fey stresses that some of the most important research questions cannot be answered if only a single procedure or a series of constrained procedures is used to measure effectiveness of short-term language intervention.

Fey discusses a group of studies that attempt to measure the effects of many variables over long time periods but also notes that random selection of subjects is frequently not observed nor are appropriate controls necessarily in place. Such weaknesses necessarily suggest that clinicians may not find great success in applying the intervention procedures within their local setting and within their own heterogeneous groups of language-impaired children.

Fey concludes that a more successful method for establishing favorable intervention outcomes would likely stem from programmatic research. Multiple experiments, designed to provide multiple answers, will bring researchers and their colleagues, clinicians, closer to the answers they seek. However, Fey also notes that such an undertaking requires teams of researchers and clinicians and inordinate amounts of time. He proposes an additional strategy that would require the researcher first to develop broad, clinically relevant treatment questions and then to evaluate the effects of treatment packages, examining and verifying the effectiveness (or ineffectiveness) of each component. This, too, he states, is long term, but would at least deal with extant intervention programs that have not been evaluated empirically.

Importantly, Fey assigns two roles to clinicians: (a) one is to express their greatest clinical concerns to researchers, and (b) the other is to inform researchers of intervention strategies they deem relevant and successful. He also advocates a return to the case-study approach and urges clinicians to provide such information to other clinicians and to language researchers.

Finally, Fey concludes that treatment research should address more than the researcher's needs to evidence scholarly activity within the framework of a research institution of higher education. Rather, it should address the needs of children with language impairment through clinically relevant means.

A Pause for Reflection

Fey's presentation brings to the fore his approach to language intervention within specific, yet naturalistic, contexts. He builds upon his previous work, favoring goals that achieve long-term improvement in language development over lower-level target behaviors, even when such lower-level behaviors appear to be successful in a clinical setting. Fey also recommends the intertwining of science and service on behalf of language-disordered children as well as clinical inquiry. He provides a series of possible avenues by which intervention packages might be sorted out. Fey concludes by realistically evaluating the opportunities and constraints of carrying out his recommendations.

In so doing, Fey goes beyond the notion of "observational research" (Good, 1988) and those who hold that naturalistic contexts can be used to identify complex practical issues but represent a poor setting for solving such issues. Fey strongly supports sound research in real world settings. In taking that position, he concurs with Odom (1988) who concluded that applied research, such as that conducted in the area of language acquisition and disorders, is more relevant than earlier research, conducted exclusively in laboratory settings. Odom concludes that applied research is not necessarily theoretical; rather, it attempts to answer certain *pragmatic* questions, such as those posed by language clinicians.

GOLDSTEIN'S VIEW OF THE FUTURE OF LANGUAGE INTERVENTION

Dr. Goldstein, representing a more behavioral view, argues that it is the language scientist who must bear the brunt of the responsibility to produce more applied research on behalf of the *practitioner*. He also supports the notion that research on *language intervention* is the most important, because that research will have the most impact on direct service delivery by speech-language pathologists.

Goldstein (1990) conducted a search for child language intervention studies over the period between 1978 and 1988. In that decade he found an average of only 13.7 articles per year; these studies were conducted, not by speech-language pathologists, but primarily by psychologists and special educators. ASHA journals, he reports, published only 17.2% of the intervention manuscripts, defined as data-based studies evaluating treatment of language disorders using either single-subject or group experimental designs. He concluded that the language research reported in the *Journal of Speech and Hearing Research (JSHR)* and the *Journal of Speech and Hearing Disorders (JSHD)* is most likely to deal with more basic issues, and not intervention research. In his view, *JSHR* and *JSHD* tend to lack clinical relevance for the practitioner, and the literature contained therein provides little or no direction to clinicians or scientists interested in language intervention. Although this conclusion is supported by commentary from the field, ASHA is currently revising its journals to enhance both clinical and scientific informational needs.

Goldstein provides a caveat, which should be heeded. He notes that scientists are under an obligation to guard against premature dissemination of language intervention research that lacks adequate empirical support. He cites a number of examples, and this reviewer can provide an additional one. The current acceptance of the collaborative consultation model of service delivery, and the tidal wave of programs in both regular and special education that hail "whole language" as a positive intervention scenario for language-disordered children, are based not upon empirical evidence but on a philosophy, which may indeed be useful under certain circumstances but is not yet verified. Dissemination has gleefully preceded documentation and verification.

Despite its difficulties, Goldstein urges us to consider both further language intervention research that is directly applicable to service delivery and increased interactions with practitioners. However, as also noted by

Huberman (1990), who discusses linkages between researchers and practitioners, it is a well-known fact that the university scientific community provides little in the way of reward structure for carrying on intensive networking with practitioners.

Nevertheless, researcher/practitioner involvement may be crucial. Miles and Huberman (1984) completed a multiple-case field study of 12 research projects utilizing interorganizational ties to determine whether conceptual use (i.e., research data) and instrumental use (i.e., practice application) occurred in the information transfer between researchers and practitioners. They found that whether basic or applied research was involved mattered little. What mattered was the involvement of the *researcher(s)* with the practitioners before, during, and after the research project itself. The stronger the linkages between the researcher and the practitioner, the stronger the acceptance of research outcomes and the application of its outcomes by the practitioner. Based on their study, Miles and Huberman recommended that researchers should ask for funding to support researcher/practitioner interactions prior to, during, and following clinical research efforts. They also noted that personal involvement with practitioners far outweighs the effects of single articles, vulgarized monographs, and one-shot workshops, which tend to be problematic (p. 387).

Goldstein argues that scientific training may have a tendency to stifle exploration and that we should consider acceptance of research, as advocated by Skinner (1950), that is not designed to test theory. Later, Goldstein notes that operant learning approaches constitute the bulk of language intervention research, and reports that progress emerging from an applied behavioral analysis perspective has been disregarded. He suggests that much of this research diverges from Skinner's early analysis of verbal behavior. Goldstein points out that ethnographic research, as well as behavioral research, has been subjected to unfair criticism. He comments that the increasingly stringent criteria that transcend the constraints of economic resources and logistics of working in applied settings can be harmful. As we all know, there are few "perfect" standardized assessment instruments, and at least a part of this lack of perfection may have to do with the astronomical costs, in terms of fiscal and temporal resources, for both assessment and intervention research. That is not to say, of course, that shoddy work should be rewarded.

In strongly supporting behavioral control techniques, Goldstein notes the need for greater understanding and a more liberal outlook. Somewhat later in his paper he proposes four research strategies, which couple descriptive and applied research, and discusses various aspects of generalization. It would appear that he supports the need to study the natural ecology of individuals with language impairments but also wishes to stress the importance of laboratory research as well. Milieu teaching is noted to be a more systematized way of approaching certain language intervention procedures.

Speaking out against treatment comparison studies, Goldstein notes the sad state of treatment packages, and thus the difficulty in comparative studies of treatment effects. Many would agree with his position. On the other hand, he favorably comments on the intervention potential provided by augmentative and alternative communication systems, compensatory skills, and self-prompting strategies, noting their widely recognized potential.

Goldstein observes the pervasive reciprocal effects among language, social, cognitive, socioemotional, and academic domains. He appears to view a number of these domains as secondary to linguistic skills, per se, a perspective that differs to some degree from the views of other scientists who see, for example, social aspects of language as most critical.

A Pause for Reflection

In surveying the broad scope of language science and research, there are, of course, additional literatures to which language researchers, particularly those interested in intervention and instruction, may be drawn. The learning literature reviewed by Glaser (1990) provides several examples. Glaser notes the success of Brown and Palinscar's (1989) research in reading, undergirded by the Vygotskian tradition. Their Reciprocal Teaching approach, which reflects Vygotsky's contention of teaching to the Zone of Proximal Development (ZPD), uses adult modeling and scaffolding to provide the essential ingredients to a higher level of comprehension and verbal performance. Researchers interested in children's language performance in classroom settings as well as "naturalistic language intervention" may be encouraged by their demonstrated success.

Glaser also comments on Scardamalia, Bereiter, and Steinback's (1984) research on the process and products of written language.

Glaser (1990) maintains that cognitive science has ignored the study of learning. Rather it has focused on the structures and processes of human competence and on the nature of the performance system. Nevertheless, this focus on competence has led to "significant advances in the analysis of the organization of memory, the knowledge and information-processing requirements for solving problems, the characteristics of understanding, and the nature of domain-specific performances resulting from long-term learning and extended experience" (Glaser, 1990, p. 29). Indeed, he notes that we may now view competence (in cognitive terms) as grounded in the "compiled, automatized, functional and proceduralized knowledge characteristics of a well-developed cognitive skill, the effective use of internalized self-regulatory control strategies for fostering comprehension, and the structuring of knowledge for explanation and problem solving" (p. 30).

Piaget (1926) and Vygotsky (1978) note that internalizing knowledge stems from theories that emphasize the impetus of social forces as the key to learning. Glaser (1990) is careful to note that neither Piagetian nor Vygotskian principles of the learning process have been theoretically or empirically analyzed, a sobering thought indeed. Glaser's basic premise is that it is time to view comprehension strategy research and knowledge structure research within a broader perspective. He suggests that learning be studied directly within both natural settings and carefully designed instructional settings. He concludes that a closer coupling between theory and well-conceived practical applications may be the key to understanding how humans learn, a point well made by Goldstein in the area of language intervention.

The research that Glaser reviews holds import for those language researchers who have looked to cognitive science for guidance in the understanding of mental representations, problem solving, scaffolding, and discourse in the service of language comprehension and production.

Implicit in the preceding papers has been the underlying premise that the language scientist must construct hypotheses following certain and, in this case, differing theoretical orientations. Obviously, the selection of a small number of variables versus multiple variables reflects these researchers' theoretical stance. Both maintain a foothold in the positivist paradigm, requiring that quantitative data be analyzed through statistical means.

Some who eye the research scene see language research paradigms as following those in other disciplines that is, moving away from a positivist paradigm to a postpositivist paradigm Garrison (1986). Empirical studies that have been the norm for some time in speech-language pathology slowly may be giving way to ethnographic, naturalistic research since, in the post-positivist world, relationships are more likely to be viewed *in situ*. This move toward more qualitative research methods is less prevalent in speech-language pathology, but there is evidence that our discipline, too, may modify its research agenda to accommodate the conflicting streams of research emanating from psychology, education, anthropology, sociology, child language acquisition, and medicine.

That the clinician have a theoretical perspective regarding how language is acquired, how appropriate goals are selected, and the appropriate means for increasing communication abilities, is stressed by Olswang and Bain (in press), as well as by Fey and Goldstein. Olswang and Bain agree that intervention remains an elusive and difficult task, and warn that the application of "normal processes" models to language-impaired children, in terms of developmental stages and language facilitation, may sometimes lead us astray. They advocate future research that will vary from this model, noting that because selection of a treatment delivery model and teaching strategies are extraordinarily complex tasks, clinicians may only have general guidelines to assist in making treatment decisions. Olswang and Bain conclude by recommending single-subject designs (e.g., multiple-baseline designs) as a helpful means for assessing ongoing effectiveness.

Wilson and Risucci (1986) recognize the important contribution of different subtypes of language disorders. Their model provides a clinical quantitative classification of language-disordered preschoolers. They suggest that subtype studies of this population can be useful in systematically evaluating the efficacy of intervention. Wilson and Risucci note that although recent work in typological research has been within the tradition of quantitative, multivariate approaches, rather than clinical-inferential, they propose that such approaches are at variance with the clinical approaches utilized by medicine. In essence, they suggest that a model in which clinical and quantitative methods are blended might be most appropriate for language disorders, specifying that quantitative approaches that rely on retrospective, multivariate analyses of large quantities of psychometric databases in search of statistically homogeneous subpopulations can be supported by clinical approaches using observations and consensus among professionals, with little reference to standardized assessment or empirical evaluation. Wilson and Risucci cite Aram and Nation's 1975 study as the first to explore subtypes within developmental language disorders (DLD) to be based on *quantitative* procedures. Some years later, Rapin and Allen (1983) used clinically derived variables and a psycholinguistic approach in a medical setting, contextualized by an ongoing evaluation of videotaped interactions of DLD children's language with parents or other adults, thus reflecting a combined processing and psycholinguistic model.

Another approach to treatment comes from Norris and Hoffman (1990), who purport that naturalistic therapy is consistent with the principles of whole language learning. They stress the importance that speech-language pathologists obtain positive results by providing organizational structure, as they facilitate meaningful communication. Norris and Hoffman concede that although whole language approaches may seem to be unstructured, they maintain it is the clinician's duty to provide implicit structure in this intervention approach.

Evaluating discourse structure, Jackson (1986) uses another method of analysis in a naturalistic context. Conducted primarily through the method of analytic induction, which constitutes both discovery and testing, this method is driven by a falsification model. It requires that empirical claims be tested through active, procedurally diverse searches for counterexamples.

At the earliest stages of language, Ingram (1989) provides us with a useful discussion of the historical differences between researchers of *language acquisition*, who have concentrated on the *theory of language*, and *child language researchers*, who have concentrated on the *theory of acquisition* based on children's actual discourse. In discussing the possible reconciliation of these two viewpoints that is, *combining* acquisition *data* with a *theoretical perspective*, Ingram also sets forth the three positions researchers appear to hold on the nature of language acquisition. Fey and Goldstein have also provided some insight into those perspectives: (a) behaviorism, (b) maturationism, and (c) constructionism.

Rice (1989) noted that the study of child language "sits at the interface among linguistics, developmental psychology, sociology, anthropology, and education, and it links basic questions about the nature of human intellectual competencies to applied questions of how best to teach young children" (p. 149). Lest speech-language pathologists and scientists think that divergence is common only in our discipline, readers should note Rice's comments that the area of language acquisition is among the most contentious in the developmental literature, with little consensus and lively debates.

The above is a brief examination of the two preceding papers within the broader context of conflicting versions of language science and research. It serves to set the stage for the need to expand the influence of our research to audiences other than ourselves.

CASTING OUR RESEARCH SHADOW

Are there other audiences beyond our own professions? In the larger scheme of things, our own professions are far from the largest, and our output must be viewed as having relatively little social or political impact. At the individual level, the university researcher is constrained by higher education's institutional review boards (IRB) in conducting research with humans, while more basic researchers may find an even closer monitoring of their use of animals in biomedical research. Both IRB activities stem from society's perception of the conduct of research as filtered through activist groups and governmental regulations. Much of the research in language disorders is supported by only a few of the federal or state agencies, and even fewer foundations. Thus, researchers may apply, and appeal, to specific entities that may well have already defined their areas of research need or the acceptability of certain research practices.

Little of the language scientist's work has had an impact on social policy, although it is this author's view that language, its acquisition and its disorders, is the very stuff of literacy, which, in turn, has captured the public's attention and that of various funding organizations. Interestingly enough, it may be our profession's recent engagement with literacy (Stewart, 1989), with written as well as oral language (and its importance in bilingual and Limited English Proficiency arenas), that may result in not only a higher national profile as to the benefits (and limitations) of language research, but also a setting of national social policy.

As we know, research may either precede or follow nationwide movements or agendas. The national movement in the late 1960s and 1970s toward mainstreaming in the schools was largely based on a *philosophy* of appropriate placement, as is the current interest in reading circles based on "whole language." Neither has been subject to serious scrutiny prior to its implementation in our nation's schools. Currently, the body of research on family systems and young children has been translated into federal regulations incorporated in P. L. 99-457 as a requirement for an Individualized Family Service Plan (IFSP).

Most researchers are quick to surmise the prevailing winds of federal support, and recognize that it is difficult, as Cronbach (1982) has noted. Research that runs contrary to current belief systems is unlikely to be supported to any significant level. There appears to be a desire on the part of Congress, for example, to support research that meets immediate social objectives. It has frequently been extremely difficult to obtain support for well-designed longitudinal studies, even though language scientists have long suspected that these might be much more valuable than cross-sectional studies. The outcomes of early otitis media with effusion (OME) (Friel-Patti, in press) on language acquisition and development have only recently been the subject of intensive research. The same is true of language development from infancy through late adolescence. Only recently have we begun to realize the importance of investigating the changing course of children's language and its manifestations in written and read text over time (Wallach & Butler, 1984).

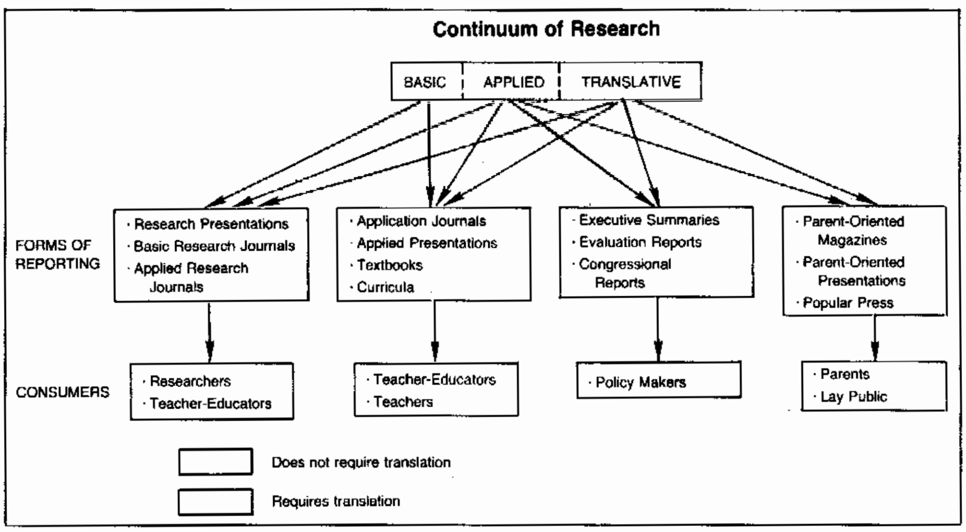

FIGURE 2. Relationship of research and consumers in early childhood special education. Continuum of research. From S. L. Odom. (1988). Research in early childhood special education, in S. L. Odom and M. B. Karnes (Eds.) *Early intervention for infants & children with handicaps* (p. 7). Baltimore: Paul H. Brookes. P.O. Box 10624, Baltimore, MD 21285, Toll free no. 800-638-3775. Reprinted by permission.

As Odom (1988) points out, there is a special need for the translation of research to consumers and the public. He notes that the continuum of research extends across basic, applied, and what Odom terms "translative." Figure 2 shows his conception of the research continuum.

Language science (as well as speech-language pathology) researchers have been conversing largely among themselves, and on occasion with members of other disciplines. It has only been in the past two decades that the profession has made a concerted effort to involve itself at the social, political, and governmental levels. We have begun to talk with others and to translate our findings to them and policy makers—a good beginning, but there remains much to be done.

SCIENCE AND SERVICES: A DYNAMIC DUO?

We can conclude from Fey's and Goldstein's papers that we *are* developing our own scientific base, although we truly share our interests and our research with many disciplines. As Siegel and Ingham (1987) maintain, communication sciences and disorders "sits at the intersection of . . . many other biological and behavioral sciences [and that we] share and contribute to their methodologies and theories" (p. 103) as well as draw from them. They too note the clinician's dilemma, since science often lags behind practice in clinical fields, and Monday mornings always come early (Butler & Wallach, 1984; Perkins, 1986).

Nevertheless, Butler and Wallach (1984) have long maintained that there is clinical gold in the pristine hills of research:

> Mining that gold is sometimes troublesome, since basic understanding of language learning . . . may lie within neurological and cognitive substrata. It is likely that appropriate assessment and intervention, derived from yet to be developed postulates, will be neither simple nor direct. . . . The very complexity inherent in language acquisition, the development of cognition, and the achievement of academic success mitigate against easy solutions, (p. 361)
>
> Although a surge of optimism has swept through a number of professional fields regarding the expanding knowledge base and its eventual application to practice, all too frequently discipline boundaries contribute to fragmentation of information. . . . (p. 362)

Yet, it is at the boundaries of the various disciplines that we may well do our best research and theory building, and at the same intersection that we may well find relevant intervention strategies and service delivery options.

Surely, theory, research, and clinical practice in language acquisition and its disorders are exciting endeavors. More importantly, they also reflect "society's stake in the utility of language in communication between individuals and between societies, [as well as] the very essence of language itself: its role in thinking and learning" (Butler, 1986, p. 57).

When language interventionists toil in the clinical vineyard, they are engaged in both an art and a science. When service and science can blend together, they make a significant contribution to the human condition. As Strachy (1927) commented: "Perhaps of all the creations of man, language is the most astonishing. Do we not all agree?" (p. v).

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RESEARCH PAPERS: C. SPEECH

SPEECH RESEARCH 21: PEERING INTO THE FUTURE

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The directive given to me by the American Speech-Language-Hearing Association (ASHA) was to discuss future trends in speech research and their impact on clinical practice. To meet the challenge presented by this formidable directive, I decided to conduct an investigation in which I surveyed the ideas and opinions of a large group of speech researchers. As I had hoped, this experience broadened my view of the future and provided me with new, and sometimes surprising, insights into what may lie ahead.

METHOD

Participants

Twenty-one speech researchers were interviewed for this investigation. Although 21 may seem an odd number (which, of course, it is), I chose it to signify the fact that we are moving toward the 21st century.

These 21 individuals were selected to represent a wide range of research interests, work settings, and professional ages. Their interests included normal function and various disorders (e.g., orofacial anomalies, alaryngeal voice, stuttering, dysarthria, spasmodic dysphonia), encompassed all the speech production subsystems (respiratory, laryngeal, velopharyngeal-nasal, and oral), and involved a variety of approaches (physiology, acoustics, perception, and synthesis). Most were affiliated with universities, whereas others held positions in private practice, research institutions, and hospitals. Professional age, defined as the number of years since obtaining the Ph.D. degree, ranged from 6 to 39 years. Although most participants were not clinically certified speech-language pathologists, nearly all had a history of affiliation with ASHA. For example, among them were a past president, past editors, members, life members, fellows, and honorees of the Association. Table 1 contains a list of the participants and provides information about their professional backgrounds.

Procedures

To recruit participants, I sent letters to 21 individuals. In these letters I requested the opportunity to speak with the recipient about future trends in speech research and their impact on clinical practice. I followed these letters with telephone calls during which I either conducted the interview or made an appointment to call again. I was pleasantly surprised when the first 21 individuals I contacted agreed to be interviewed. Interviews typically lasted 45 minutes to 1 hour.

After obtaining preliminary information (see Table 1), I began the interview by asking the question, "What do you see for the future of speech research?" This allowed participants the freedom to discuss any topics they thought were relevant. During the remainder of the interview, I asked directive questions regarding several issues that I believed to be important to our future. These issues were (a) the clinical applicability of basic research, (b) the education of prospective researchers, (c) multidisciplinary research efforts, (d) the impact of technological advances on research, and (e) the concentration of research efforts on specific subject populations. In many cases, I did not need to ask questions about these issues because the interviewee discussed them spontaneously. Each interview was tape recorded (with permission) and transcribed. A penultimate draft of this report was sent out to each participant to obtain permission for the inclusion of quotations and any other direct references. No substantive changes were made as a result of their reviews of the penultimate draft.

RESULTS AND DISCUSSION

In the pages that follow, there are instances in which I have referred to participants by name, such as when using direct quotations, when an idea was unique, or when I thought the reader might find it interesting to know who contributed a particular idea or opinion. There are other instances in which I have presented findings without reference to individual participants. Usually this was

TABLE 1. List of participants and information about their professional backgrounds.

Name	Position	Affiliation	Ph.D. conferrred Institution	Year	ASHA certified
Ronald J. Baken	Professor	Columbia University	Columbia University	1969	no
Steven M. Barlow	Associate Professor/ Scientist	Indiana University	University of Wisconsin	1984	no
Eric D. Blom	Speech-Language Pathologist	Head and Neck Surgery Associates, Indianapolis, IN	University of Maryland	1972	yes
Raymond G. Daniloff	Clinic Director	Louisiana State University	University of Iowa	1967	no
John W. Folkins	Professor/Chair	University of Iowa	University of Washington	1976	no
Katherine S. Harris	Distinguished Professor Vice President	City University of New York Haskins Laboratories	Harvard University (Psychology)	1954	no
Thomas J. Hixon	Professor/Head	University of Arizona	University of Iowa	1965	yes
Arthur S. House	Retired	Institute for Defense Analyses Princeton, NJ	University of Illinois	1951	no
Patricia K. Kuhl	Professor	University of Washington	University of Minnesota	1973	no
Charles R. Larson	Professor	Northwestern University	University of Washington	1975	no
Christy L. Ludlow	Research Speech Pathologist	National Institutes of Health	New York University	1973	yes
Peter F. MacNeilage	Professor	University of Texas	McGill University (Psychology)	1962	no
Fred D. Minifie	Professor	University of Washington	University of Iowa	1963	no
Ronald W. Netsell	Staff Scientist	Boys Town National Research Hospital	University of Iowa	1969	yes
Robert L. Ringel	Vice President/Dean	Purdue University	Purdue University	1962	yes
Ralph L. Shelton	Professor	University of Arizona	University of Utah	1959	yes
Thomas Shipp	Research Speech Pathologist	VA Medical Center, San Francisco, CA	Stanford University	1963	yes
Anne Smith	Professor	Purdue University	University of Iowa	1978	no
Kenneth N. Stevens	C. J. LeBel Professor	Massachusetts Institute of Technology	Massachusetts Institute of Technology (Electrical Engineering)	1952	no
Ingo R. Titze	Professor Director of Research	University of Iowa Denver Center for the Performing Arts	Brigham Young University (Physics)	1972	no
Gary Weismer	Professor	University of Wisconsin	University of Wisconsin	1975	no

done because an idea or opinion was shared by several, so that to list them would be cumbersome.

Research Directions

The first interview question, "What do you see for the future of speech research?" elicited a diverse set of responses, most of which involved rather specific remarks about future research directions. To illustrate, Baken focused on the application of nonlinear dynamic approaches to the study of speech and voice production, Harris stressed the importance of making observations of movements of the vocal tract structures, and Kuhl emphasized the progress being made in cracking the speech code from a perceptual standpoint.

Personally, I found this diversity refreshing. I think diversity is healthy for our discipline, just as diversity in any living system helps ensure its survival by increasing flexibility and adaptability (Capra, 1982). I was also impressed by the enthusiasm conveyed by these scientists when discussing their own research. Enthusiasm—or what Titze referred to as passion—is the force that drives good research. Lewis Thomas (1974) recognized the power of this force when he wrote in his essay on natural science, "I don't know of any other human occupation . . . in which the people engaged in it are so caught up, so totally preoccupied, so driven beyond their strength and resources" (p. 118).

Despite differences in individual perspectives, there were certain ideas that emerged repeatedly during the course of these interviews. One such idea, shared by over half the participants, was that a strong emphasis should be placed on attempts to understand how speech production is controlled by the nervous system. Approaches to the study of neural control as discussed by these scientists primarily pertained to whole-organism and system levels of observation. There were exceptions, however. For example, one approach, championed by Barlow and Larson, involves examination of the neural control of vocalization and vocalization-related behaviors in animals, and includes observations not only at whole-organism and system levels but also at cellular and molecular levels. Presumably, it will become increasingly possible to make such observations in humans as well. An example of a recent cellular-level investigation in humans (cited by Barlow) was that conducted by McClean and colleagues (1990) who performed detailed single unit study of the thalamus during speech production. Assuming that speech research follows present trends in neuroscience, more attention will be paid to the cellular and molecular levels of organization, and phenomena such as central pattern generators, synaptic reorganization, and neural regeneration will assume increasingly important roles in our understanding of neural mechanisms underlying the speech process.

In addition to emphasizing the study of neural control, a number of researchers stressed the need for making simultaneous observations at multiple stages of the speech process—including the neural, muscular, structural, aeromechanical, acoustical, and perceptual stages. As pointed out by Shipp, only by examining the speech process at various stages of its realization will we be able to construct a comprehensive picture of how spoken communication is accomplished. This comprehensive picture is critical to understanding normal and abnormal function and to developing rational approaches to the evaluation and treatment of individuals with speech disorders.

The usefulness of multistage observations was viewed from another perspective as well, that of defining the nature of the transforms between the stages of the speech process. For example, how movements of structures translate acoustically has been a major topic in speech research for decades. Weismer reminded me of how important it is to the clinical process that we define these transforms when he said, "The more you understand about the transformation, the better you can function perceptually," and "perceptually" is how most clinicians are forced to function, with their ears as their primary clinical tools.

Whether working toward defining phenomena at a single stage or attempting to obtain greater specificity of the transforms between stages, modeling promises to make substantial contributions to our knowledge of normal and abnormal speech processes. An example was offered by Stevens who said that by synthesizing the speech of someone with a disorder he is able to glean information about the nature of the speech signal, make inferences regarding certain physiological events responsible for its production, and identify its salient perceptual features—in other words, perform analysis by synthesis. Such modeling techniques should result in improved parameterization of the speech process.

In addition to discussing *what* we should be researching, some participants raised the more general issue of *how* we should be researching. Here, a controversy arose. There were those who advocated that a concentrated effort be made to collect descriptive data. In the words of House, "The important missing elements of our knowledge are still descriptive. Our field skipped over the pre-science age, the descriptive age, and has tried to be theoretical and experimental prematurely and possibly incorrectly." In contrast, there were several others who asserted that our research should be more theory-driven, and expressed concern that, as Ludlow put it, our field is "stuck in the descriptive stage."

The perception that much of our research lacks a theoretical base was perplexing to me, particularly because it seemed easy to find examples of theory-driven research represented in the work of the present group of scientists. For instance, there is the research of MacNeilage, who is studying speech from a standpoint of evolutionary theory, and that of Netsell, who is investigating normal and abnormal speech production from the perspective of neural oscillators. Another example is the speech breathing research in which I have been involved. As I see it, this research is both descriptive and theory-driven in that it has provided nearly all the available descriptive data on speech breathing, yet its design has taken into account theories of aging, development, or whatever other theoretical perspective was relevant to the population under investigation. Perhaps "theory-driven" means different things to different people, and therein lies my confusion.

I suppose if I were to take sides on this issue, I would support the view that we need more descriptive research at this stage in the development of our discipline. A theory-bound perspective, if adopted too early in the investigative process, can constrain experimental design to the point of limiting the scope of observations. In extreme cases, strict adherence to theory can have tragic consequences, such as those resulting from the use of "objective" data to support prevailing prejudices regarding the intellectual capacity of certain disadvantaged groups as defined by their race, class, or sex (Gould, 1981). On a lighter note, using a less constrained approach to research may improve our chances of making serendipitous discoveries (Roberts, 1989).

Whether one believes that our research should be more descriptive or more theory-driven, some of these scientists advised that it should be much more selective. Although there are innumerable investigations and experiments that could be conducted on questions related to speech production, speech, and speech perception, it is clear that our resources, both human and financial, are extremely limited. Therefore, our research questions should include only those that are crucial to our understanding of the speech process.

Clinical Applicability

Most of these scientists agreed that for speech research to have a greater impact on clinical practice, more effort should be devoted to translating knowledge gained from basic research into its potential clinical applications. However, they disagreed as to how this should be done. For example, Titze was of the opinion that basic researchers should assume responsibility for determining the practical relevance of their work. In contrast, Daniloff, Hixon, and Larson adamantly opposed such a suggestion, indicating that basic researchers should be free to explore the nature of Nature with no clinical strings attached. As support for this view, Hixon cited evidence that the vast majority of the clinical treatments in medicine have emanated from basic research and not from insights gleaned from clinical practice. Another sentiment, expressed by Blom, was that the most effective way for research to impact clinical practice is through the contributions of individuals who are both clinicians and researchers. Despite these differences, most participants agreed that one means of helping to bridge the gap between basic research and clinical application is to provide environments in which basic researchers and clinicians have the opportunity to interact on a routine basis.

Several interviewees underscored the need for more clinical research. As mentioned previously, some believed that we should collect substantial descriptive data, including data on clinical populations. Also, there were those, Ringel and Shelton in particular, who emphasized the need to document the efficacy of our clinical treatments, given that so little is known about what treatments are effective, for what types of disorders, and under what conditions. The call for treatment efficacy research also has been sounded in a recent issue of *Asha* (Olswang, 1990).

Education

Inadequate preparation in basic sciences was seen as the weakest link, or perhaps the fatal flaw, in our educational programs. Most of those interviewed believed there to be a serious deficit in our preparation in the physical sciences (e.g., physics, mathematics, engineering, computer science), biological sciences (e.g., physiology, biochemistry, genetics, embryology), and neurosciences (e.g., neurophysiology, neurochemistry, motor control). This need for a stronger science background was seen as critical not only for students pursuing a Ph.D. but also for those master's students whose interests are in providing services in medical settings. This reflected the general attitude among these scientists that speech-language pathologists who work in medical settings require more extensive and specialized preparation than speech-language pathologists who provide services in school settings.

Although it may seem that the most straightforward approach to solving our educational problem would be to add science courses to our curricula, the solution may not be so simple. As some participants observed, our better master's programs already require a 2-year commitment and many of our doctoral programs entail a 4- to 5-year commitment beyond the master's degree. It seems unrealistic to expect students to extend their programs to accommodate more coursework, particularly given the high cost of education as compared with the meager remuneration commanded following graduation. One solution, offered by Harris, might be to eliminate the redundancy of the information presented in the clinical courses offered at the undergraduate, master's, and doctoral levels, thereby allowing more time to complete coursework in the basic sciences.

A few of those interviewed suggested that doctoral education in our field could be improved by developing programs that begin at the undergraduate level and culminate with a Ph.D. Such a program is now in place in my own department at the University of Arizona and is being met with enthusiastic support. Students in this program spend most of their undergraduate time concentrating on coursework in the basic physical, biological, neural, and social sciences. In the graduate portion of the program, coursework and research training are provided by faculty from several disciplines. Key features of this program are the strong science base and the multidisciplinary training component.

Another major concern expressed was that the quality of our educational programs varies too widely. There were several participants who suggested that certain programs be closed so that available resources could be funneled into those programs with the greatest potential for excellence. In fact, Weismer went so far as to suggest that over half of the existing programs should be eliminated. Once a core of high-quality programs was established, new programs of comparable caliber could be added as more resources became available.

Improving the quality of our educational programs would improve our capacity to attract bright students into our field. However, we, along with others in science-based disciplines, are faced with the reality of a national trend in which students are moving away from the sciences in favor of more lucrative careers such as business and law. Although we may not be able to reverse a national trend, it was suggested that we attempt to lessen its impact on our discipline by developing and implementing aggressive campaigns to recruit talented students. In Barlow's words, we should "do a harder sell job."

Finally, the topic was raised as to whether or not postdoctoral study should be encouraged. Usually this topic was raised by me, and usually the response was affirmative. However, my impression was that postdoctoral study was not perceived to be an essential component of the educational process. In fact, some participants seemed to see postdoctoral study as merely a delay mechanism to allow time for newly graduated scientists to begin building a research record before having to assume the responsibilities of a faculty position. It was my general impression that the most enthusiastic support for postdoctoral training came from those who themselves had a history of formal postdoctoral training (Folkins, Hixon, Kuhl, Larson, Ringel, and Smith). I will return to this topic later.

Multidisciplinary Research

Our field, by its very nature, is multidisciplinary in that it draws on knowledge from many different disciplines. Nevertheless, according to the majority of those interviewed, we are too often insular in our approach to research. When asked what might be done to stimulate interactions between our scientists and those in other fields, respondents offered two possible suggestions. One was that we develop more doctoral programs that emphasize multidisciplinary education. Such education, particularly if it includes laboratory experiences outside of our field, should help to prepare students to conduct multidisciplinary research during their careers as scientists. Another suggestion was that we establish more research centers in which scientists from different disciplines are housed together to work in a common area of investigation. It seems as though centers of this type would stimulate multidisciplinary collaboration merely by the physical proximity of laboratory facilities and the frequent opportunities for scientists to engage in informal exchange.

My own experience has convinced me of the value of a multidisciplinary approach to research. At the University of Arizona, where I obtained my doctoral training and where I am now a faculty member, I have been fortunate to be involved in a campus-wide Motor Control Group consisting of scientists from a variety of fields—including physiology, neurobiology, systems engineering, exercise and sports sciences, biopsychology, and medicine—who meet on a weekly basis to discuss their work. My interactions with members of this group have resulted in several collaborative research projects. I rate these projects among the most valuable of my doctoral and postdoctoral career.

Although we as a discipline may have a long way to go before we are multidisciplinary in our approach to research, there are indications that we are moving in that direction. For example, as reported by Smith, recent progress in the area of stuttering can be attributed, at least in part, to the emergence of new forms of collaborative efforts that cross disciplinary lines. It appears that a multidisciplinary approach to understanding the speech process will be the wave of the future.

Technology

In the collective opinion of these researchers, there were two types of technology that have made, and will continue to make, a profound impact on our science. Not surprisingly, computer technology was one. Computers have contributed to our progress in a variety of ways. They have enhanced our ability to acquire, process, and manipulate large and complex data sets. They have made it possible to create elaborate models of the speech process involving physiologic, acoustic, and perceptual variables. Computers also have made an important contribution in the area of communication. For example, by using computers in conjunction with phone lines, we now can transmit data almost instantly from university to university, and send entire manuscripts to out-of-state coauthors. As pointed out by Folkins, such ease of information exchange is important in developing and fostering long-distance cooperative research efforts.

The other technology identified by these scientists as having potentially far-reaching consequences on our understanding of the speech process is that of brain imaging. Techniques such as positron emission tomography (PET) and brain electrical activity mapping (BEAM) have revolutionized our ability to visualize the nervous system in action. Dynamic magnetic resonance imaging (MRI) also is generating excitement, although at present it is too slow to be adequate for the study of speech processing.

There was another technological issue raised by these scientists that involved the use of instrumentation in evaluating and treating clients with speech disorders. Although, as Weismer pointed out, there has been substantial "lip service" paid in support of integrating instrumentation into clinical settings; in truth, not much progress has been made. It seems to me that one of the major obstacles to the clinical use of instrumentation stems back to deficiencies in our educational programs. Clinicians graduating from many of our existing master's degree programs may not have adequate grounding in basic sciences for dealing with the concepts underlying instrumentation or its applications. Nor do they have adequate practical training in the use of instrumentation in many cases. Nevertheless, the development of clinically oriented resource materials, such as those provided in the book written by Baken (1987), should facilitate the learning of clinical measurement procedures and perhaps stimulate more interest in instrumental approaches to evaluation and treatment.

Specific Populations

When asked if future research efforts should target specific populations, some respondents just said "no."

However, the majority of those interviewed identified groups that they believed should receive special attention during future years. Several participants stressed the need to concentrate on normal subject groups representing the extremes of the life span, with the goal being to understand the influence of development and aging on the speech process. Rationales for targeting these groups seemed to reflect prevailing clinical priorities. That is, understanding developmental processes was seen to be paramount to improving intervention approaches for young people with speech disorders, just as increasing our knowledge of the aging process was seen as critical to increasing the efficacy of our interventions for senescent clients. The quality of clinical services for the senescent segment of the population will become increasingly critical to our nation's social and economic health as the "graying of America" continues. Projections indicate that by the year 2030 more than one fourth of the nation's population will be over the age of 60 (U.S. Bureau of the Census, 1984). My home state of Arizona already reflects such an age distribution.

Culturally diverse groups also were identified as important targets for future research, with the acknowledgment that we have a tremendous amount to learn about dialectical differences, cultural considerations, and service delivery models. These issues will become increasingly important as culturally diverse groups represent a progressively larger share of our nation's population over the next several decades (U.S. Bureau of the Census, 1989).

When interviewees were asked which subject groups with speech disorders should be targeted for study, responses varied widely. However, most participants mentioned disorders with a neurogenic basis (e.g., dysarthria and stuttering). This emphasis on neurogenic speech disorders was consistent with the strong interest in the neural control of speech production expressed by this group of scientists.

Funding Mechanisms

Speech research, like all such endeavors, requires financial support to survive. So the question of how to maximize our ability to compete successfully for funding is critical. During the course of these interviews, suggestions were offered as to how we might increase our funding potential. For instance, Daniloff advised that we incorporate grant-writing activities into our doctoral curricula so as to arm future scientists with the skills necessary for preparing competitive funding proposals. Another suggestion involved the development of standardized research procedures. Titze, as a result of his experience as a member of review panels for the National Institutes of Health (NIH), has grown to believe that our lack of standardization places us at a disadvantage relative to other fields with which we compete for funds. In his opinion, the fact that our procedures are often untried and lacking in paradigm makes our funding proposals more vulnerable to criticism.

Another important funding-related question that arose during these interviews was: What are the needs of speech research and what types of funding mechanisms can best meet those needs? Some participants expressed concern that our young scientists operate at a disadvantage with respect to the prospect of obtaining funding for their research endeavors. This is a problem that affects all young scientists today, not just those in our field, as indicated by the report that new investigators (those seeking federal funding for the first time) are only about half as successful in obtaining grants as those who apply to renew existing funding (Palca, 1990a, 1990b). Fortunately, there are federal funding mechanisms in place designed especially for young scientists. The Clinical Investigator Development Award (CIDA) and the First Independent Research Support and Transition (FIRST) award are two such mechanisms offered through the NIH. As a grateful recipient of a CIDA, I am convinced that such awards are valuable contributors to the development of a research career.

Also mentioned by some interviewees was the need for mechanisms which would help retain senior scientists who are at risk for "burning out." To circumvent the burnout problem, it was suggested that research chair positions be created that would be secure, well-remunerated, and free of fund-generating obligations. The offering of such a position might entice a talented senior scientist to continue his or her research work for an extra decade or more.

One frequently raised concern was that we are progressing too slowly in our quest to understand the speech process. Hixon suggested that the use of a new type of short-term government contract might help us progress at a faster pace. Such a contract would call for a group of experts to convene for a relatively short period of time to work collaboratively and intensively on some speech-related problem. As the top scientist of the Manhattan Project might have said, "You have all these great minds, but they're all dancing to a different tune. You bring them together in one place, isolated with no distractions. You create an atmosphere of stress, creative stress, everyone competing to solve one problem . . ." with the result being that the problem would be solved, and solved quickly (quote from the character of J. Robert Oppenheimer, played by Dwight Schultz in the movie *Fat Man and Little Boy*, Paramount Pictures, 1989).

Although grant-funding rates at the NIH and the National Science Foundation (NSF) are at all-time lows (Palca, 1989), appropriations for the newly established National Institute on Deafness and Other Communication Disorders were up by approximately 25% last year over the previous year, according to Folkins. Therefore, at least for now, the funding outlook for research in communication disorders appears to be relatively bright.

The Role of ASHA

Although I did not question participants about their views on ASHA, several commented spontaneously about the role that our Association plays—or does not play—in science. A frequently expressed sentiment was that ASHA does not support its scientists, as evidenced by the fact that so few scientists are members of ASHA—only 0.9% of ASHA members list research as their primary professional function (American Speech-Language-Hearing Association, 1990). As Minifie stated, "There is the danger that, if our scientists don't advance knowledge in the communication sciences and disorders, someone else will."

Perhaps most outspoken on this topic was House, a self-described "thorn in the side of ASHA." According to his account, ASHA made a decision many years ago to turn its attention away from science in favor of the professional side of our discipline. Nevertheless, in his words, ". . . every time the Association tries to do anything to call attention to its existence in the world, it puts great emphasis on science, and I think that this is incorrect, it's presumptuous, and it really ought to stop."

On a more positive note, House suggested that ASHA could play an integral part in the acquisition of new knowledge by acting as the primary driver behind the creation of a national data bank. ASHA's role in its creation might include several activities—it could form committees of scientists to develop standardized procedures for data collection and analysis, it could sponsor workshops for training researchers in these procedures, and it could set up a centralized system for storing and accessing the data obtained. From this data bank could come the descriptive information and perhaps even treatment efficacy documentation called for by some of the scientists interviewed for this investigation. I like House's idea of creating a national data bank and believe that, if carefully orchestrated, it could make an unparalleled contribution to our discipline and to individuals with communication disorders. However, I disagree with him on one point. Whereas he envisioned clinicians to be the primary contributors to this bank, my bias is that the responsibility of data collection should be left to researchers. The debate as to whether or not clinicians should conduct research dates back decades (Jerger, 1963, 1964) and will not be resolved here.

Reflections

Before I began interviewing these 21 researchers, I had some preconceived notions about what they might say. For example, I suspected that they would be interested in neurologically based research questions (in keeping with the spirit of the "Decade of the Brain") (U.S. Congress, 1990), supportive of efforts to translate basic research findings into clinical applications, concerned about improving the quality of our educational programs, enthusiastic about multidisciplinary research, impressed with the impact of computer technology on our field, and in favor of concentrating resources on the study of specific subsets of the population such as the aged, the culturally diverse, and those with neurogenic speech disorders. Most of these preconceptions were borne out. However, there were some surprises as well. One surprise was that, although these scientists generally supported the concept of postdoctoral training, their support was not as strong as I had anticipated. In contrast, the prevailing attitude in the multidisciplinary environment in which I work is that postdoctoral training is essential to a scientist's education. Postdoctoral study offers many benefits—for example, it promotes multidisciplinary collaboration, equips scientists with specialized skills, and socializes young researchers into the world of science. Perhaps an even more compelling reason to pursue postdoctoral study relates to employability. Case in point, the Faculty of Science at the University of Arizona, which includes my own department, is hesitant to consider anyone for an assistant professorship who does not have formal postdoctoral experience. It seems to me that we should encourage doctoral candidates in speech, language, and hearing sciences to pursue postdoctoral study.

Another surprise came in the form of comments about ASHA. To be honest, I had not thought much about the role of ASHA in science before undertaking this project. Now that I have, I see some ways in which the Association might offer more support to its research membership. For example, ASHA could reinforce its talented scientists by presenting awards in recognition of a history of valuable research contributions (in the way the DiCarlo Award is given in recognition of clinical contributions). ASHA might further demonstrate support for its scientists by taking an active role in setting guidelines and policies related to scientific ethics and fraud. It could also take the lead in creating a national data bank, as House suggested. Through the American Speech-Language-Hearing Foundation (ASHA's "charitable arm," as Minifie called it), ASHA could raise money to support productive research laboratories, fund research chair positions for talented senior scientists, and support research and educational goals of promising young scientists in our field.

Happily, there are indications that ASHA is becoming more active in its support of science. For example, recent efforts have culminated in the establishment of the Dennis Klatt Memorial Fund for the support of postdoctoral careers. Furthermore, the director of ASHA's Research Division, Cynthia M. Shewan, recently published a long list of suggestions as to how ASHA might expand its role in science (Shewan, 1990).

Before closing, I would like to suggest that we as scientists also might expand our role by playing a more active part in the global community. As stated in the preface to a book authored by a group of American and Soviet scholars (Gromyko & Hellman, 1988), international collaboration among scientists clearly benefits science, but most importantly it contributes toward a more peaceful world. Through participation in traveling professorships, student exchange programs, and visiting scientist arrangements "connections and friendships are built up, and channels of understanding are opened that can survive the dramatic upheavals of modern history" (p. xviii).

I am optimistic about our future—to borrow Kuhl's words, I think it looks "very, very rosy." I expect speech research to continue with a passion, chiefly because speech is intrinsically interesting to those of us who use it, but also because speech is essential to most everything we do. As Lewis Thomas (1983) wrote, "As a species, the thing we are biologically good at is learning new things, thanks to our individual large brains and thanks above all to the gift of speech that connects them, one to another" (p. 163). I am proud to be among those who are seeking to understand this gift of speech.

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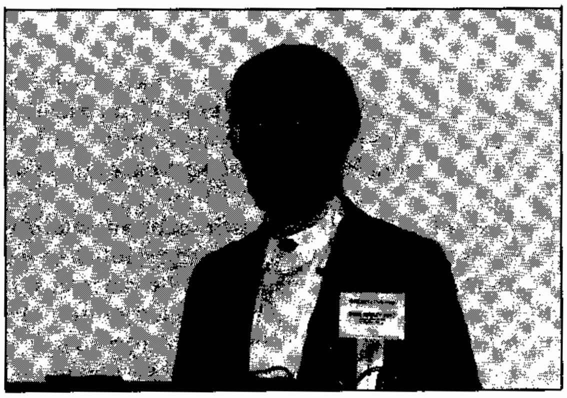
CROSS-DISCIPLINARY ADVANCES IN SPEECH SCIENCE



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The purpose of this paper is to present several areas of research in speech science in terms of the important issues and results just emerging from the laboratory, and to attempt to forecast important research activities in these areas in the future. Three areas have been selected which are cross-disciplinary and reflect the contributions made to speech science by other disciplines, and vice versa. These areas are focused primarily on the acoustic aspects of speech perception and production, although occasional allusions to motor control are included.

The three topics discussed are speech perception, speech training aids, and phonological development. In general, each section is self-contained. These topics are connected with a few common threads, such as categorical perception and the underlying units of speech. The first two sections reflect the author's current commitment to these areas and include recent experimental results to illustrate the issues raised. The final section on phonological development is an overview of a few important cross-disciplinary topics and a forecast of their future directions.

PSYCHOACOUSTICS AND SPEECH PERCEPTION

A central issue in speech perception has been to understand the process of the *identification* of speech sounds in normal discourse. Although identification itself may be broadly defined to include word or phoneme identification, lexical access, or even the identification of a specific talker, the discussion presented here will focus on phoneme identification. The selection of phoneme identification should not imply, however, that the phoneme is assumed to be the basic unit of speech. Rather, phonemes, that is the consonants and vowels of a particular language, have been selected because they are readily identified by native speakers regardless of whether they are viewed as units either derived from some larger unit (e.g., a syllable) or units synthesized from smaller ones (e.g., features). In any case, phonemes have served as the stimuli most frequently manipulated and studied in speech perception research.

Early speech perception research conducted by psychologists and linguists examined the auditory processes of the normal, adult listener of speech. The assumptions were that the subject was already a highly trained listener of his or her native language and should be tested in conditions that were similar to normal listening conditions. This research tradition is strongly associated with the Haskins Laboratories and will be referred to as *speech perception* research. The strong claim emerging from this tradition is that the auditory processes for speech are specialized and distinct from those used to identify other acoustic events (Liberman & Mattingly, 1989).

In contrast to the speech perception approach is one that arose from psychophysical research with nonspeech, acoustic stimuli. In psychophysics, acoustic stimuli are carefully manipulated under highly controlled listening conditions using a set of well-studied methods. Results of these experiments provide a description of the relation between properties of stimuli and the associated behavioral responses which permit insights into the presumed auditory processes (Watson, 1973). Some of these methods were applied to speech-like stimuli in the earliest days of speech research (Flanagan, 1955a; Flanagan, 1955b), but very little subsequent research has followed this course (Pastore, 1987; Repp, 1987b). One reason for this hiatus is that knowledge of the capabilities of the auditory system for processing simple stimuli logically precedes the investigation of more complex sounds like speech. The research described here has followed standard psychophysical methods and will be referred to as *psychoacoustics of speech* research, or psychoacoustic research for short.

The distinction between speech perception and psychoacoustic research is not meant to imply that persons doing speech perception research are not interested in auditory processing of speech sounds or in the relation between speech and nonspeech processing. In fact, the issues of the representation of speech in terms of auditory models (Carlson & Granstrom, 1982) and of the relation of psychophysics to speech perception (Schouten, 1987) have been the focus of several published conferences, including a recent one at the 120th meeting of the Acoustical Society (Greenberg & Kluender, 1990). The distinction made here is simply that these have been two distinct research traditions, and the kind of information that is derived from the results of psychoacoustic experiments is different from that of most speech perception experiments.

The purpose of the psychoacoustic experiments discussed below is to determine the basic capabilities of the auditory system to process speech. The long-range goal is to bridge the gap between processes which represent the basic capabilities of the auditory system to respond to acoustically complex speech signals, and the perception of speech in normal discourse. In the following section, a brief review of a completed study of the categorical perception using this approach is presented (Kewley-Port et al., 1988; Watson & Kewley-Port, 1988). Several ongoing experiments are then described. The effectiveness of auditory models to explain these results is discussed. Finally, some projections for the future of psychoacoustic research in relation to speech perception will be made.

Categorical Perception

The categorical perception of sound continua is considered one of the essential phenomena that support the theory that speech is processed by a specialized phonetic module (Liberman et al., 1967; for a review, see Repp, 1984). Categorical perception is demonstrated for a sound continuum if the number of categories that are identified limits the *discrimination* between stimulus pairs such that good discrimination is only obtained for pairs that belong to different categories. Generally, it has been shown that categorical perception is obtained for many continua which manipulate the acoustic cues used to discriminate consonantal phonemes (Fujisaki & Kawashima, 1968; Liberman et al., 1957; Miller & Eimas, 1977). For vowel continua, however, clear evidence for categorical perception is not usually obtained (Fry et al., 1962; Stevens et al., 1969).

Although categorical perception has been exhaustively studied for over 30 years (Harnad, 1987), only a few studies have applied psychoacoustic methods to examine the auditory capabilities for discriminating speech sounds (Carney et al., 1977; Goldberg, 1986; Sachs & Grant, 1976). Nonetheless, several theories explaining categorical perception have claimed that a threshold in the auditory system is the basis of the discontinuity observed in the discrimination functions (Miller et al., 1976; Pisoni, 1977; Stevens, 1985). Our studies of the perception of temporal onset differences in speech (voice onset time or VOT) and nonspeech (noise-buzz) continua challenge these theories (Kewley-Port et al., 1988; Watson & Kewley-Port, 1988).

To study the auditory capabilities for processing the temporally varying continua, two important task variables were controlled (Watson & Foyle, 1985). The first task variable was *subject training*. Subjects in our experiments were trained for thousands of trials to assure that performance approached asymptote. (This contrasts with the typical 1- or 2-hour speech perception experiments.) Second, the level of *stimulus uncertainty* was controlled (Watson & Kelly, 1981). In the experiment designed to obtain the best resolution of temporal acuity, stimuli were presented under minimal stimulus uncertainty (i.e., only a single pair of stimuli to be discriminated was presented within a block of trials). In other experiments the level of stimulus uncertainty was systematically varied. These included the typical, high-uncertainty ABX paradigm administered to untrained subjects that is generally used in categorical discrimination experiments.

The results of the Kewley-Port et al. (1988) and Watson and Kewley-Port (1988) studies are shown in Figure 1. The top panel shows results for a bilabial /ba-pa/ continuum varying in VOT. The bottom panel shows results for a noise-buzz continuum patterned after the nonspeech continuum studied by Miller et al. (1976). The categorical discrimination peak was obtained for the /ba-pa/, ABX task as expected. When task conditions changed by providing training and feedback in a high-uncertainty, same-different (S-D) task, discrimination improved for the short VOT stimuli, and the categorical peak disappeared. When subjects listened under minimal stimulus uncertainty, discrimination improved everywhere. The same pattern of results was shown for the noise-buzz continuum, although categorical discrimination was not obtained in the ABX test.

These results demonstrate that under minimal stimulus uncertainty, no discontinuity (or threshold) in the discrimination functions was obtained for VOT. Of course, under the more "ordinary" listening conditions of the ABX high-uncertainty task, the categorical peak is present for the speech continuum. That is, as task variables place more demands on central processing capacity, categorical discrimination occurs. These results support theories that central and not peripheral auditory processes are the locus of categorical perception.

Turning to another result, the temporal acuity for similar speech and nonspeech stimuli was the same for trained subjects at the limits of resolution. Under minimal stimulus uncertainty, no discontinuity (or threshold) in the discrimination functions was obtained. The discrimination functions were entirely representative of discrimination for other auditory dimensions such as frequency or intensity. Thus at this level of auditory processing, no "special" processing of speech signals is observed.

Capabilities of Auditory System to Process Vowels

Vowel detection. An analogous series of psychoacoustic experiments is in progress for vowel sounds. Vowels may be described as spectrally complex sounds with a spectral shape that is the result of vocal-tract filtering of a harmonic series. The peaks or resonances of the filter (i.e., formants) are believed to be the spectrally salient properties used to identify vowels. Only a few studies have investigated the detectability of vowels (or any speech sounds) or the discriminability of frequency differences

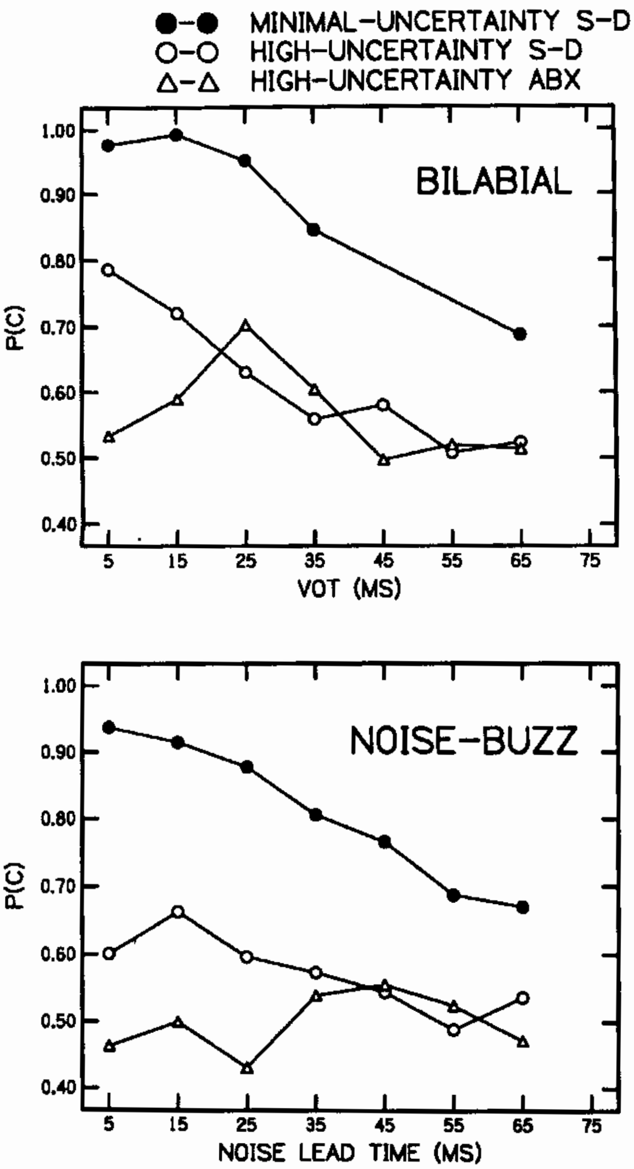

FIGURE 1. Average percent correct discrimination of stimuli differing by 10 ms is shown for three discrimination tasks, a minimal-uncertainty, same-different; a high-uncertainty, same-different; and a high-uncertainty, ABX task. The upper panel shows discrimination results for speech stimuli differing in VOT. The lower panel shows discrimination results for nonspeech, noise-buzz stimuli (see text).

for vowel formants. That is, little is known about listeners' abilities to "hear out" fine acoustic details in the waveforms of speech. It is essential that we determine the limits of processing for the spectral, temporal, and intensive properties of speech sounds in isolation if we are to gain a better understanding of their processing within the context of other speech sounds.

A series of experiments to determine the threshold of detectability of vowels, both in isolation and in sequences of vowels, has been completed. The vowel set included the 10 English monophthongal vowels. They were synthesized as steady-state vowels produced by a female talker. Thresholds were obtained from well-trained, normal-hearing listeners using an adaptive-tracking paradigm.

The first study investigated isolated vowels under minimal stimulus uncertainty (Kewley-Port, in press). Vowels were calibrated for equal rms sound pressure at the earphones. Detection thresholds differed by 20 dB across vowels as shown in the bottom panel of Figure 2 for four subjects. Explanations for these very large, 20-dB differences in thresholds will be examined in the auditory models section. The following sections discuss the effects of manipulating stimulus uncertainty for vowel sequences and relate those results to nonspeech stimuli.

Research on tone sequences consisting of a series of brief tones presented one after the other to form single "word-length" patterns has been conducted for many years by Watson and his colleagues (Watson, 1987; Watson & Foyle, 1985; Watson et al., 1976). Some of those experiments have now been replicated with sequences of steady-state vowels (Kewley-Port & Watson, 1986). A vowel sequence consisted of permutations of the ten synthetic vowels, each 40 ms long. One goal of these experiments was to learn whether the factors that govern the detectability of patterns of pure tones have similar effects on spectrally complex vowels. Significant variables that determine the discriminability of tonal sequences include length of the tones, position of the target tone in the sequence, and frequency of the tone.

First, the detectability of vowels under very high-stimulus uncertainty was investigated. In this condition a new vowel sequence was presented on every trial. The sound level of the nontarget vowels was 77 dB SPL. Thresholds for detecting the presence of the target vowel in the sequence differed only slightly across positions within the vowel sequence, but differed considerably across subjects and vowels. Results averaged across position are shown in the top panel of Figure 2 for four subjects. Two important findings are: (a) obtained thresholds for individual subjects differed greatly, about 43-dB from the best to the worst subject; and (b) vowel thresholds in sequences exhibited the same pattern of 20-dB differences seen across the isolated vowels (the dashed line in Figure 2). Taken together the results for vowel sequences are similar to those obtained for the nonspeech, 10-tone sequences, in terms of the time-course of learning, differential detectability of the stimuli, and unexpectedly large individual differences among the normal-hearing listeners (Watson et al., 1976).

In the second study, the effect of high versus minimal stimulus uncertainty on the detectability of vowels in vowels sequences was examined. In the high-uncertainty condition, a catalogue of 48, 10-vowel sequences was constructed. In the minimal-uncertainty condition, asymptotic detection thresholds were estimated for one vowel within a single sequence.

Thresholds obtained for target vowels in each of the 48 sequences were similar to those in the high-uncertainty experiment. Individual differences among the three subjects in this experiment were within the range for subjects shown in Figure 2. Taking all 7 subjects together, under

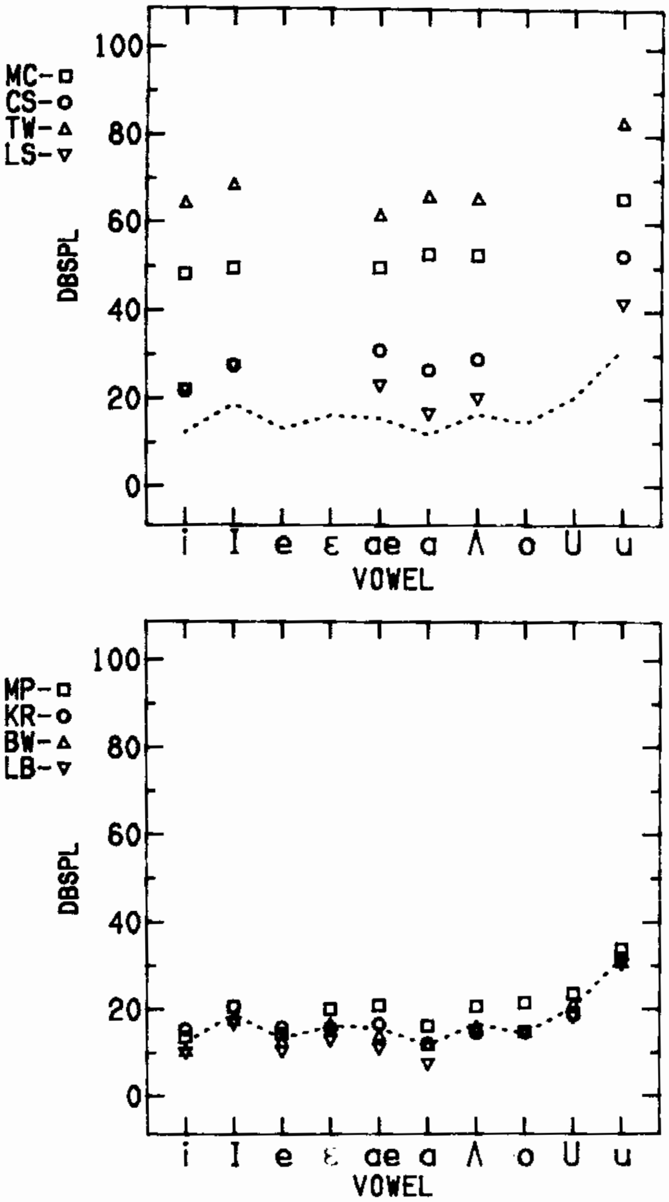

FIGURE 2. Thresholds in dB SPL for detecting one of 10 vowels. Symbols indicate data for different subjects. In the top panel, vowels were embedded in a 10-vowel sequence presented under very high stimulus certainty. In the bottom panel, vowels were presented in isolation. The dashed line in both panels is the average of isolated vowel thresholds for the four subject shown in the bottom panel.

high-stimulus uncertainty the detectability of vowels in sequences was much *worse* than the isolated vowel thresholds. Differences ranged from 8 dB to 51 dB, with an average of a 31-dB increase in the threshold.

To account for this 31-dB elevation in the threshold, the amount of *energetic* masking was estimated. Energetic masking is the intensity increment needed to detect the vowel in the presence of the context vowels relative to the isolated vowel threshold. Energetic masking was estimated under minimal stimulus uncertainty conditions for three different sequences from the vowel catalogue. The amount of energetic masking observed ranged from 9 dB to 25 dB, depending on the position within the vowel sequence. The increment in masking from minimal to high uncertainty represents the amount of *informational masking* contributed by increasing stimulus uncertainty. In this case, a particular pattern occurs only once every 48 trials. Informational masking for vowel sequences was about 25 dB, an increment analogous to that reported for tonal sequences (Watson, 1987; Watson & Kelly, 1981).

In conclusion, the increment in the amount of informational masking obtained between minimal-uncertainty and high-uncertainty conditions in sequences of sounds appears to be similar for sounds that are spectrally simple (tones) or spectrally complex (vowels). These experiments, in general, support a theory of speech perception that assumes that subtle, high-information-bearing portions of speech waveforms may become salient as a result of prolonged experience rather than because of any inherent properties of the acoustical waveforms of speech. Evidence for an independent "phonetic module" proposed by Liberman and Mattingly (1989) that is distinct from the auditory processes for frequency and intensity is not found in these results, nor in the results of the psychoacoustic studies of VOT described earlier.

A comparison of the subject variability shown in the two panels on Figure 2 has some important implications for how vowels are processed. For isolated vowels under minimal uncertainty (bottom panel), the capability for detecting vowels is nearly the same for these normal-hearing subjects. As stimulus information and the cognitive load of the task increase dramatically, as shown in the upper panel, remarkable individual differences become apparent. Based on conclusions formed from the more extensive testing conducted for tonal sequences, (Johnson et al., 1987; Watson, 1987), it appears that these individual differences relate to limitations on informational or pattern processing for complex stimuli that are not apparent in performance with simple stimuli, such as pure tones. These limitations may reside in either short-term memory (Johnson et al., 1987) or in basic cognitive processing capabilities (Watson, in press; Watson, 1990), or both. These differences may have strong clinical implications when services to be provided need to be adjusted depending on the level of the client's auditory capabilities. Some steps have been taken to measure the auditory capabilities of hearing-impaired persons for complex nonspeech stimuli (Espinoza-Varas et al., 1989; Tyler et al., 1983) and speech stimuli (Bacon & Brandt, 1982; Leek & Dorman, 1987; Turner & Henn, 1989; Turner & Van Tassell, 1984). However, the baseline data describing the capabiliites of normal listeners to perceive speech is one of the areas where future research must fill in the gaps.

Auditory Models

There is another way in which psychoacoustics and physiological acoustics contribute to our understanding of speech perception. Recently these disciplines have provided general models of how spectral-temporal information in the auditory system is transformed, models which can help us interpret behavioral measures of auditory capabilities. Models have been developed by Delgutte (1990), Humes and Jesteadt (1989), Moore and Glasberg (1986; 1987), and Zwicker (1974), among others. Some models produce representations of the speech signal that closely match actual recordings of speech made in the eighth (cochlear) nerve (Delgutte, 1990; Young & Sachs, 1979). Other models represent the transformed acoustic signals in terms of *excitation patterns* (Moore & Glasberg, 1987) which can be thought of as "auditory spectrograms" (Carlson & Granstrom, 1982).

These models have been applied to a number of problems, for example, to explain how the phonetic features or properties of speech are processed (Bladon & Lindblom, 1981; Delgutte, 1982; Kewley-Port & Luce, 1984; Searle et al., 1979), or how speech sounds are discriminated by normal and the hearing-impaired listeners (Bacon & Brandt, 1982; Leek & Dorman, 1987; Turner & Henn, 1989). Another application of these models is found in speech processing algorithms. For example, coding of speech for digital telephone transmission has incorporated models of auditory masking to maximize efficiency (Schroeder & Atal, 1985). Improved performance in speech recognition algorithms has resulted from auditory models (called "auditory front-ends") that process the acoustic signal (Blomberg et al., 1982; Cohen, 1989; Hermansky, 1990).

Auditory models can assist in the interpretation of data collected concerning speech processing capabilities of the auditory system. Sometimes only relatively simple information about auditory processing is required to provide explanatory power for a set of data. For example, Kewley-Port and Atal (1989) examined the relationship between vowels in a small portion of the F1 × F2 space. Using multidimensional scaling, perceptual spaces describing the perceived distances between vowel stimuli were obtained for several sets of vowels. The perceived vowel distances appeared to be warped along some type of frequency dimension. Transforming the center formant frequencies from Hertz to Barks, the Euclidian distances correlated so highly with the perceptual distances (r = 0.94) that only a small amount of variance was not accounted for. The Bark scale proposed by Zwicker and Feldtkeller (1967) is based on a physiological model of the frequency resolution along the basilar membrane. In this experiment a representation of formant frequency in Barks was a better model than any other tried, including, for example, the Mel scale for pitch.

A more powerful auditory model was applied to the data from the experiments on detection thresholds for vowels. The average thresholds in the lower panel of Figure 2 are reproduced in Figure 3. They show that the thresholds for isolated vowels differed by 20 dB when equated for equal sound pressure at the earphone. At threshold, all just-detectable vowels should be approximately equal in loudness. Kewley-Port (in press) examined several auditory models to explain the 20-dB differences in intensity. First, some of the differences in threshold can be accounted for by the audibility of the vowel spectra (i.e., lower frequencies in the vowel spectra need to be more intense to be detected). Audibility was estimated using the A-weighting filter on the sound-level meter. The recomputed thresholds (labeled A-weighted thresholds) are shown in Figure 3. The overall range of the A-weighted thresholds was flatter, but considerable variability remained.

Additional modeling transformed the vowel spectra into excitation patterns. One model (called Average Excitation) calculated the average intensity in dB over the entire excitation pattern. The Average Excitation thresholds (Figure 3) are quite flat and can be reasonably described as being "equally loud." For this set of data, then, large apparent differences in the capability of the auditory system to detect spectrally complex vowels can be explained by a model of auditory processing. Thus, auditory models, derived from physiological and behavioral measures of the transformation of *simple* acoustic signals in the auditory periphery, seem to provide some explanatory power for speech processing. It should not be construed, however, that speech processing takes place in the "ear." As the psychoacoustic experiments on speech reported above have shown, selective attention (resulting from long-term training) can make portions of a complex signal more salient by 15 to 25 dB. The determination of which portions of the speech waveform are made salient by peripheral versus central processing is a long-term goal of psychoacoustic research.

Future Research in Speech Perception

It is not outrageous to claim that many issues concerning the perception of phonemes have not changed a great deal since the early work on categorical perception at the Haskins Laboratories (Liberman et al., 1957; Liberman et al., 1967) or the pioneering psychoacoustic studies of speech by Flanagan (1955a). Liberman and Mattingly (1989) continue to make strong claims that the auditory processes for speech are specialized and distinct from those used to identify other acoustic events. Although there is currently a renewed interest in the application of psychophysical techniques to determine the thresholds of performance for speech (e.g., Greenberg & Kluender, 1990; Schouten, 1987), so little research in this area has been conducted that a great deal remains to be learned.

It seems to me that progress in resolving many issues in speech perception will be based on results from psychoacoustic experiments with speech stimuli. Baseline data on the capabilities of the auditory system provide, by definition, the foundation for understanding speech perception. The required data must be the result of systematic studies that vary the task variables and always employ well-trained subjects. Recently such studies have been motivated by attempts to understand speech processing in the hearing impaired (Leek & Dorman, 1987; Turner & Henn, 1989). With the arrival of the computing

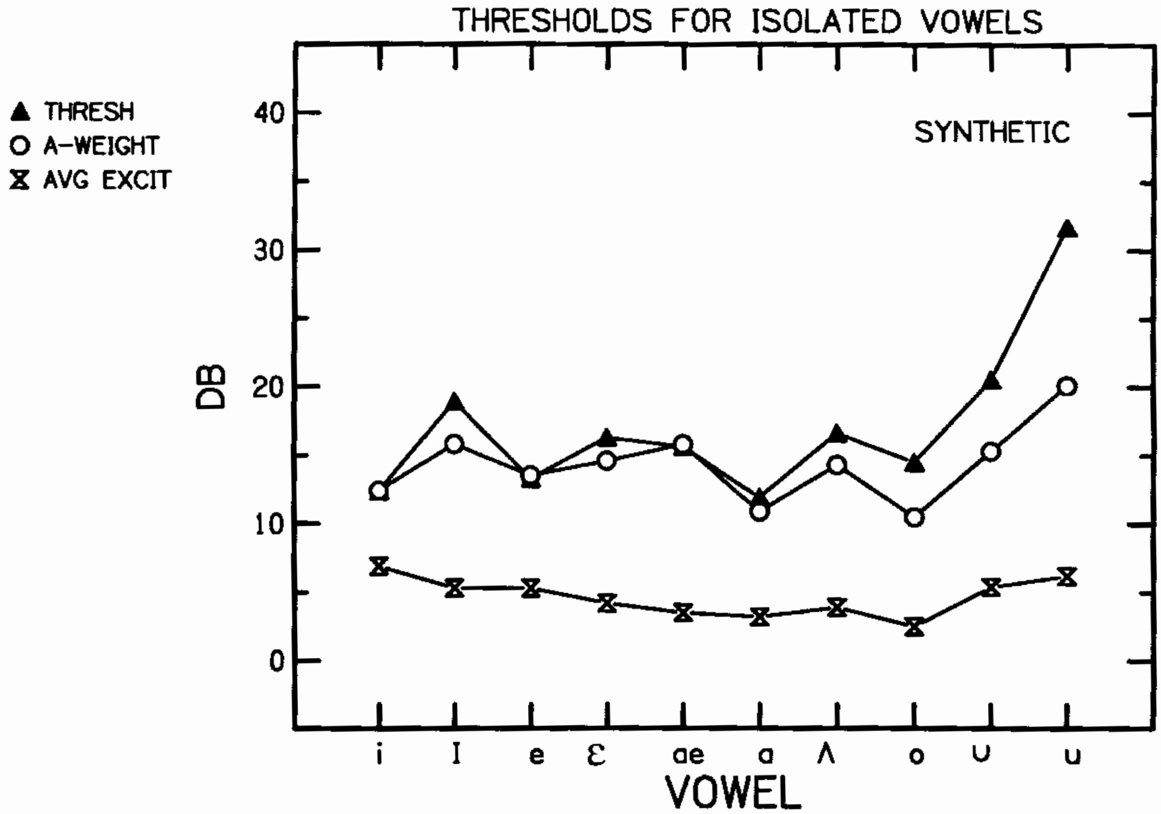

FIGURE 3. Thresholds in dB for isolated, synthetic vowels. Thresholds for vowels equated for equal sound pressure are shown with closed triangles. Thresholds adjusted for the A-weighting filter as an estimate of the audibility of the vowel spectra are shown with open circles. Thresholds calculated as the average intensity in dB calculated from excitation patterns are shown with Xs.

power available in digital hearing aids, we can expect considerably more research to be focused on this area.

How then will the bridge be made from the baseline capabilities of the auditory system to understanding speech "phenomena" such as categorical perception, duplex perception (Liberman & Mattingly, 1989), verbal transformations, and illusions (Warren et al., 1990)? One approach being taken is to study systematically the identification and discrimination of common nonspeech sounds, such as clapping (Repp, 1987a) and walking (Li & Pastore, 1990). These studies will help determine how the auditory system processes familiar nonspeech sounds. Another approach is to use auditory models, as demonstrated in a recent paper by Hermansky (1990) who presented several successful applications of an auditory model to speech perception. The bridge will ultimately be made, however, from the results of a family of psychophysical experiments covering detection, discrimination, and, finally, identification of complex stimuli, both speech and nonspeech. Psychoacoustic experiments of speech perception phenomena, similar to those reported by Kewley-Port and Watson (1988) for categorical perception, will determine those aspects of perception which are peripheral and "hard-wired." We will then be better able to investigate and explain other aspects of perception that are more plastic and central in origin.

COMPUTER-BASED SPEECH TRAINING AIDS

Turning our attention to another topic, it is obvious to the speech and hearing professional that the use of microcomputers is rapidly growing. Computer applications in speech, language, and hearing are clear examples of the benefits that cross-disciplinary collaboration can produce, in this case in the area of service delivery. Important applications in service delivery include diagnostic and treatment systems and communication devices. Because my research has concerned treatment, and in particular interactive speech therapy, the discussion that follows will focus on that topic. However, a few general remarks on the future of service delivery in the other areas can also be made.

Some of the earliest applications of computers in the clinic involved speech diagnostics. Systems that input and analyzed various parameters of speech were developed first as analog systems or on laboratory computers. Many have now been converted to packages available for microcomputers. Currently Kay Elemetrics' Visipitch and Nasometer systems are widely used in clinics. Speech analysis workstations, including the Kay Elemetrics' DSP Sono-graph and the GW Instruments MacSpeech Lab II, will be commonplace clinical tools as prices decrease.

Note that these systems present visual information concerning a client's speech, but do not *evaluate* it. (For a discussion of the *validation* of these instruments, see the section on Clinical Evaluation below.) The goal of computer-based diagnostic systems in the near future should be to incorporate knowledge of speech disorders, possibly through the use of *expert systems*, which can then be employed to evaluate the speech analysis data. The longer term goal, probably a decade away, is the development of interactive speech diagnostic systems that will both diagnose and recommend alternative treatment plans for some clinical problems.

In the area of alternative and augmentative communication, there is likely to be an explosion of low-cost, computer-based systems that are tailored to the needs of different disabilities. This is the result of major strides in the technology for speech synthesis, analysis, and recognition made during the 1980s. This technology includes the development of digital signal processing algorithms for speech and their inexpensive implementation in specially designed computer chips (i.e., DSP chips, for example, the TSM320 series from Texas Instruments or the DSP32C from AT&T). High-quality, low-cost speech synthesis (e.g., Best Communications) or text-to-speech systems are now available and are a substantial improvement in intelligibility and naturalness over the speech heard from most current augmentative devices. Soon these improved synthesizers can be tailored to produce a voice quality desired by the client. Speech recognition algorithms are still far from general purpose, but microcomputer systems are improving and becoming lower in cost. (The Introvoice VI from Voice Connexion with speech synthesis is $700.) Thus speech recognizers can be reliably used as input drivers to computers for many disabled persons, including the speech impaired (Carlson et al., 1988). (The use of speech recognition in a speech training aid is described below.)

Computer-Based Speech Training Systems

Computer-based speech training (CBST) aids are several decades old, and several microcomputer systems are now commecially marketed. (For reviews of CBST systems, see Bernstein et al., 1988; Bernstein, 1989; Braeges & Houde, 1982; Lippmann, 1982; Watson & Kewley-Port, 1989.) CBST systems may be described as having several properties. First, a CBST system is programmed to process one or more signals (acoustic, articulatory movement, electrophysiological, etc.) derived from the speech of a client, for the purpose of providing feedback about the utterances. This may be contrasted with a system that simply prompts a client to speak, but without providing feedback. These systems are available. Second, a CBST should be able to keep detailed records of the course of speech training. Third, CBST systems, now and in the foreseeable future, should be designed as *training aids* which supplement and presumably enhance speech training under the guidance of a speech-language pathologist. Although some systems may be suitable for providing speech drill conducted independently by the client, the general use of the CBST system should be considered in relation to the tasks that will be conducted by the human teacher or clinician. This relation between human and computer is not often explicitly considered but it is an essential factor in the use and potential success of computers in the speech training.

Currently, the development of CBST systems seems driven by available technology rather than by more theoretically based considerations that might focus on the global relationship between technology and speech training. Two recent articles in *Volta Review* have examined several theoretical issues underlying the development of CBST systems (Bernstein, 1989; Watson & Kewley-Port, 1989). As background for our *Volta Review* article, more than 30 CBST systems were reviewed. One outcome of this effort was a general taxonomy for describing *feedback* in CBST systems (Watson & Kewley-Port, 1989). This taxonomy and some important implications of it for the future of CBST systems are described below.

Feedback in CBST systems

Feedback in CBST is often presented in a video-game format; however, the relations between a monkey climbing a palm tree and throwing coconuts and either the speech parameters represented in the graphics or the specific aspect of speech to be learned by the client may be very complicated (example from IBM SpeechViewer, see Ryalls, 1989). Moreover, the optimal forms of feedback for speech training may be quite different for different speech disorders. In the case of hearing-impaired individuals, the computer-based feedback must serve as a *substitute* for those portions of the natural feedback (sidetone) in the auditory channel that they cannot detect. For normal-hearing, misarticulating persons, feedback should probably provide information to *augment* that derived from the auditory channel.

A general taxonomy for describing feedback has been proposed by Watson and Kewley-Port (1989). Types of feedback are described by three discrete dimensions, or *properties*. The first property is the *physical measurement* on which the feedback is based. Three possible measures of the vocal response are its electrophysiology, articulatory movement dynamics, and the acoustic output itself. The second property of feedback in the taxonomy is the *standard* of speech performance, in relation to which the measured speech production is evaluated. For example, the standard might be a normative sample, that is, one that is representative of normal speech production, as produced by the speech teacher (e.g., Visipitch). The third property of feedback is the *level of detail* that is presented to the client. This property of feedback is further subdivided into feedback that provides a high level of detail (i.e., a spectrogram) versus feedback that provides a low level of detail (i.e., a single acoustic dimension, like fundamental frequency).

Altogether, various combinations of properties in the three dimensions represent at least 48 possible choices for speech feedback. In reviewing the 30 plus CBST systems that have been developed, less than a quarter of the possible feedback options were tried. While it is probably not worthwhile to investigate systematically all 48 options, an awareness of the alternatives, and the theoretical and empirical arguments favoring various choices may lead to the development of important new techniques in speech training. For example, systems that provide evaluative feedback, such as the Indiana Speech Training Aid (ISTRA, see below), are in the minority. In the ISTRA system each new utterance is evaluated against a standard, in terms of speech quality, and the feedback is a representation of that quantitative measure. The difference captured here is whether the client observes a spectrogram, for example, of his or her utterance and must learn how to determine whether that particular spectrogram means a good or a poor production, as opposed to cases where computer algorithms evaluate speech quality, so that the feedback can consist of words or graphic displays indicating "excellent," "good job," or "poor—try again" (e.g., the baseball video game in Figure 4).

Why is it that the majority of current systems require that a client learn how to evaluate the feedback in CBST? Is it because it is difficult to develop algorithms to make an evaluation between a standard and a new utterance? Or is it because clients might make use of nearly any portion of feedback that is rich in information, compared with the drastically reduced information contained in a positive or negative evaluation? Or is it because it is difficult to validate evaluative feedback, which requires a *correlation* between the quality metric calculated by the computer and the ratings of speech quality by human listeners, for the same speech utterances (e.g., see Watson et al., 1989)? Whatever the reasons, it seems obvious that CBST systems must in the future develop and validate evaluative feedback. Clearly, if the system provides the evaluation, the speech-language pathologist will be able to use the system more quickly and effectively because the computer can take over that aspect of training. In addition, evaluative feedback may make it possible for clients to learn faster and work more independently from the speech-language pathologist, thus increasing the number of clients who can be given effective speech training by clinicians with overwhelming caseloads.

Indiana Speech Training Aid

The specific CBST system that we have been developing over the past five years is called the Indiana Speech Training Aid (ISTRA). The ISTRA system is based on a system originally developed at the Boy's Town National Research Hospital (Lippmann & Watson, 1979), which used a computer-based, speaker-dependent speech recognizer. The current ISTRA system, implemented on a PC-compatible computer (Kewley-Port et al., 1987; Kewley-Port et al., in press; Watson et al., 1989), provides feedback to the client concerning the *quality* of speech production. This feedback is derived from a goodness-of-fit metric output by a speech recognizer. This metric is an estimate of the similarity between a new utterance and a stored template representing the best recent efforts to produce those same utterances. Syllables or words used to form the templates are periodically updated as speech intelligibility improves. Speech-language pathologists use standard diagnostic and treatment methods to implement ISTRA speech drill as outlined in a speech training curriculum (Kewley-Port et al., in press). The speech drill is presented to the client in the form of video games (see Figure 4), with nine games currently available at different training levels. Records of all training for a given client are stored on floppy disk and can be easily reviewed by the speech-language pathologist in printed or graphic form.

Role of Clinical Evaluation

The ultimate test of the success of a computer-based speech training system is verification that the disordered speech of an individual improved significantly as a result of training on that system. Without such evaluations the enthusiasm of clinicians or clients tends to become relied upon as the sole validation of the method. Before such systems are placed into general use, however, speech-language pathologists should demand that speech training systems are carefully evaluated and that the results are appropriate documented. Although many developers of CBST systems will probably agree on the importance of clinical evaluation, confusion abounds over how, when, and by whom the critical clinical tests should be conducted. Some of this confusion arises because CBST technology is new, and, therefore, methods by which to conduct such evaluations are not available in the literature. A description of several types of clinical evaluation along with some examples from the ISTRA project are

FIGURE 4. Photograph of a hearing-impaired child conducting speech drill on the ISTRA system. The Baseball drill game provides feedback in terms of singles, doubles, etc., based on levels of the goodness of production as measured by a speech recognition metric.

presented here. (For more information, see Kewley-Port et al., in press; Kewley-Port & Watson, in press.)

Clinical evaluation of CBST systems may be systematized by identifying four reasonably distinct levels of evaluation: (a) Tests of the *acceptability* of the system by speech-language pathologists and clients; (b) Tests of the *clinical effectiveness* of the device to improve speech; (c) *Beta-test* evaluations at off-site locations; and (d) *Independent verification* of clinical effectiveness.

Acceptability evaluations. The first level of evaluation involving clients is the study of how acceptable the CBST system is for speech training. Evaluations are based on observations or discussions with the clinical users about the appropriateness of the feedback displays for age or type of client, choice of microphone, etc. The suitability of a drill to a particular type of therapy or the flexibility of training for a particular client should be determined by a speech-language pathologist. Thus, it is intrinsic to the goals of acceptability evaluation that as wide a variety of clients and clinical settings as possible be included. Note that the *data* collected in this type of evaluation are primarily *testimonial.*

In later stages, which may be part of beta tests, training logs or questionnaires may be used to collect the testimonials in a more systematic way. In a recent example, IBM undertook a huge acceptability study of SpeechViewer by placing systems in clinics in 29 countries[1] (Ryalls, 1989). Testimonials collected in such studies should not, however, be regarded as adequate substitutes for clinical evaluations involving control groups, or other rigorous testing methods, to demonstrate actual speech improvement, which is the goal of the next level of evaluation.

Clinical effectiveness. Studies of clinical effectiveness using experimental and control groups are rarely used because clients in need of similar speech therapy are frequently quite different from one another in terms of variables such as age, education, etiology, presence of multiple disabilities, etc. (see McGarr et al., 1989). Therefore, *single-subject designs* are commonly used (McReynolds & Kearns, 1983). In most studies of clinical effectiveness, judgments of the correct production of speech are made by the speech-language pathologist providing treatment. As such, these judgments represent the clinical record of treatment progress. Studies of the clinical effectiveness of speech therapy require a great deal of time and effort. It is therefore not at all surprising that many developers of CBST systems have not conducted evaluations of effectiveness, and have sometimes presented the results of acceptability evaluations (e.g., testimonials from recipients of free systems) in place of difficult and time-consuming clinical research.

Independent verification of clinical effectiveness. Consider two levels at which clinical effectiveness of speech training on a CBST can be independently verified. First, it seems obvious that studies conducted by persons not associated with CBST developers would have less potential experimenter bias than those directed by the developers themselves. In addition, there exists a problem of how data collected in speech therapy can be assessed independently from the judgments of the speech-language pathologist who is the person conducting the speech therapy. On some occasions, an independent judge can make a second evaluation. (See McReynolds & Kearns, Chapter 5, 1983.) Nonetheless, because the speech-language pathologist is the person most closely working with the client, there exists a potential bias towards demonstrating improvement in speech production.

Truly independent verification of the clinical scoring can be done by listener juries who are "blind" to the purposes of the project. That is, word lists spoken by the clients can be recorded before, during, and after therapy, randomized, and presented to listeners for evaluation. Although we have attempted to follow this procedure in the ISTRA project, these listening studies are very time-consuming, mostly because the randomization requires digitization and computer editing of large numbers of utterances. Thus, in one report, out of 11 single-subject design studies using ISTRA, independent jury evaluation was completed for only three of them (Kewley-Port et al., in press).

There is clearly a lack of information in the literature on optimal methods to obtain reliable and valid measures of speech improvement. This is a critical issue to developers of CBST systems. Often the first question asked is, "How does computer training compare with training conducted by a human?" Not only are methodologies lacking to conduct this comparison, there are very few published studies comparing different speech training techniques conducted by humans (see Gierut, 1990; McGarr et al., 1989). Clearly this will continue to be an important area of research, and is even more important as more and more computer-based training systems for use with speech and language disordered are offered to clinicians.

Beta-test evaluations. A main purpose of beta-test evaluations is to *fine-tune* the device to speech-training tasks in environments in which it will most frequently be used. Sometimes off-site evaluations of computer-based speech trainers can begin quite early in development. In fact, developers are often forced to begin acceptability evaluations off-site because they have no on-site access to clients. In this discussion, however, off-site testing for any of three types of clinical evaluation described above are to be distinguished from beta-test evaluations that are conducted just prior to general distribution (or sales) of a system. Feedback given to the developers during beta testing is usually testimonial, in terms of logs recording applicable information, or surveys.

Clinical Evaluation of ISTRA at a Beta-Test Site

Clinical evaluation of ISTRA has been conducted on-site in the Indiana University Speech and Hearing Clinic and off-site in an elementary school in Bloomington, Indiana. Altogether over 60 children have participated in

[1]"SpeechViewer. A Guide to Clinical and Educational Applications." Available from IBM in November 1989.

training at both sites. A preliminary report of three clients who participated in the on-site evaluation studies of ISTRA drill has been published (Kewley-Port et al., in press). Although we were pleased with the amount and apparent success of this effort, the difficulty of collecting data that is judged sufficiently "clean" for publication under the duress of system development may well serve as a cautionary example to others.

The off-site testing of ISTRA, however, illustrates an unusual example of beta testing. In cooperation with the IBM and International Voice Products (hardware was on loan from the latter companies), and Mrs. Anne Summers (CCC-SLP), an ISTRA system was installed at Fairview Elementary School, which serves the deaf children in the Monroe County School Corporation, Bloomington, Indiana. The ISTRA system was at Fairview for 2 years beginning in fall, 1987. (The system is still in service with Mrs. Summers at a different school.)

This speech classroom served the typical function of a beta-test site (i.e., to fine-tune the software, improve the documentation, and evaluate the acceptability of computer-based drill in schools). Children with several different speech disorders used ISTRA drill in the classroom, including normal-hearing, misarticulating children, hearing-impaired and profoundly deaf children, and mildly mentally handicapped children. About 16 children participated each year. These children met in their regular group sessions for articulation therapy, usually twice weekly for 20 minutes. As part of these sessions, Mrs. Summers selected vocabulary words for the children to drill on the ISTRA computer, including making the training templates as needed. Children then returned to the speech classroom once a week to conduct speech drill independently on the computer for about 30 minutes. This additional drill increased the amount of articulation therapy received by each child by at least 100%. (We realize that this amount of therapy was probably considerably less than what would have been desirable for many of these children.)

As is typical of beta testing, testimonial data on ISTRA were collected. At the end of both years of testing at Fairview School, surveys were administered to both the teacher and the children to collect information on the acceptability of the system for speech training. The main responses to the survey questions were in the form of rating scales, where 1 meant "poor" and 5 meant "exceptional." Questions for the teacher included, "Was ISTRA a good speech training technique for each child?" and "Was the child motivated to work on ISTRA?" and a rating of the child's speech improvement. The results of the teacher's survey for both years produced average ratings between 3 = normal and 4 = good. For example, the average rating of the motivation level of the child was 3.9, and the average rating of speech improvement was 3.5. The children indicated that they liked the system (preferred to conduct speech drill on the computer system twice as often as with the teacher) and believed their speech improved (average percent "yes" was 91%). Although these survey results are not intended to represent objective data on the success of using ISTRA in the school classroom, they do indicate that the system was well received as part of the school speech therapy program.

It was during the second year at Fairview that an unusual study was designed to obtain an independent evaluation of whether the children's speech was improving with the use of computer-based drill. Sixteen children used ISTRA to drill on seven types of target sounds, including /s/, /r/, and /j/. For each child, these target sounds were part of a vocabulary of five words used in computer drill. To determine whether speech improved, these five words were incorporated into a larger list of 15 words. This word list was randomized and repeated. Each child was tape recorded reading the 30 words on two occasions, the first on March 7, 1989, and the second on May 22, 1989, less than 3 months later. The recordings were judged in a counter-balanced order by two advanced master's-level students in speech and hearing who were not familiar with the children's speech. The judges were asked to attend to the target sound(s) and to score each word as a correct or incorrect production. The data are reported as the average percent correct productions for the word lists produced by each child.

In this type of beta-test evaluation, treatment factors cannot be controlled. Here, it was found that four children had near-normal performance on the first recording, which was not improved for the last recording. (Both averages were equal to 96% correct.) To evaluate instances of improved performance, these four subjects were removed from the analysis. For the remaining 12 clients, significant overall average improvement of 17% correct productions was obtained from the first to the second recording (correlated t test, $t = 6.47$, $p < .001$). Results are shown for individual children in Figure 5. While two children showed negligible improvement, most others improved substantially.

Results in this study showed that in a rather short period of time, under 3 months, a significant improvement in correct speech production was obtained for this group of 12 children. While these children continued to receive their normal group therapy, they spent the same or a greater amount of time drilling on ISTRA. Unfortunately, to our knowledge there is no study in the literature for children in school therapy programs with which to compare our results. The surprising and discouraging fact that controlled studies of the effectiveness of speech therapy in school settings are not readily available highlights a great need for future research. An elevation of the status of speech-language pathologists as healthcare professionals in the schools cannot be achieved unless techniques for conducting evaluations in the school classrooms are established, and a body of objective, well-controlled research demonstrates that speech, and more importantly communication, improves as a result of treatment.

Future of Speech Training Aids

The discussion above focused on the evaluation of speech training and speech training aids because it is a

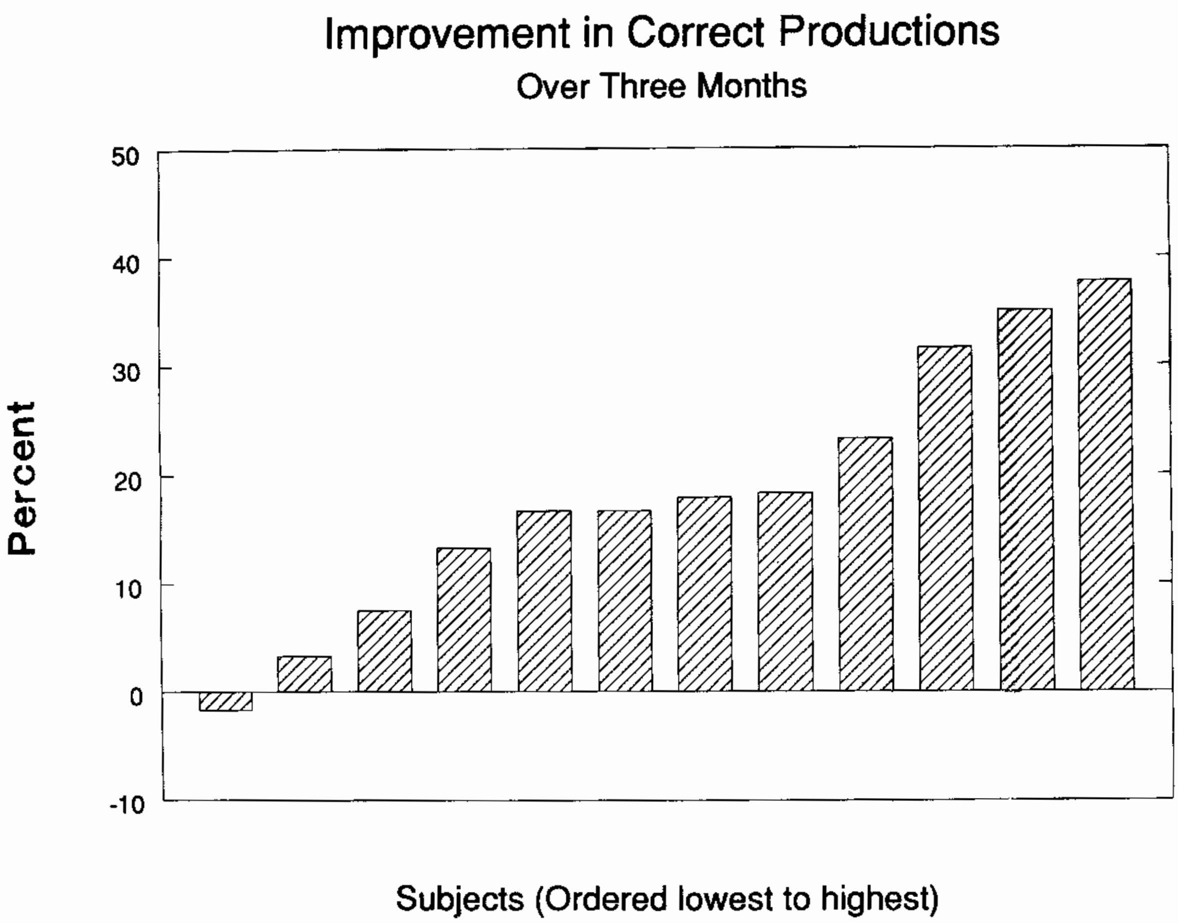

FIGURE 5. Average percent correct improvement in the production of probe words produced by 12 children. Improvement over 3 months' time is shown for children who supplemented their school speech therapy with ISTRA drill.

critical issue for currently developing systems and one that impacts strongly on potential success of these devices in the future. Some projections about other aspects of speech training systems follow.

If we envision a broad picture of the types of speech training conducted by typical speech-language pathologists, then the training provided by any existing speech training aid seems very narrow. Reasons for this include the difficulty of requisite engineering problems and the necessity for a strong interdisciplinary team actually to develop and test the final device, a team which may be hard to assemble. The future then seems to lead down several paths. On the one hand, new and different kinds of devices can be expected. For the hearing-impaired or motor-disordered patient, devices that are based on physiological signals may be very useful. Current systems include palatometers and glossometers (Fletcher, 1989) or sensors for voice parameters (Ferguson et al., 1988; Yamada et al., 1988). In the future these physiologically based systems should move out of the laboratory, and presumably different measures of articulatory movement will be explored. It is more likely, however, that new devices will be based on analyses of the acoustic signal. At AT&T Bell Laboratories, algorithms for converting the acoustic signal into midsagittal sections of articulatory structures are now being tested for reliability (Sondhi, 1990). Elsewhere, new algorithms are being developed to detect phonetic features that are in error in a child's speech, such as breathiness or "soft" burst releases.

An obvious area for expansion of speech training systems is simply to combine several existing systems into one unit. This is not trivial, as systems use a variety of instrumentation to sample speech. Moreover, it is not necessarily an advantage to have one very complicated system. Multifeatured systems will necessitate the careful development of speech training curricula to assist speech-language pathologists in the correct use of the device. In fact, speech curricula have rarely been developed for computer-based systems although they are essential for their efficient use (Kewley-Port et al., in press). Nonetheless we may expect computer-based systems to provide more general training using a variety of speech drills in the future.

Finally, algorithms which can produce *evaluative* feedback for the client will become more commonplace. Although this comment is similar to that made for diagnostic systems, it is important to restate it for speech training aids. Most training aids rely on a method of visual comparison between a clinician's correct production and the client's effort to match that production. For the client to interpret displays of pitch [Visipitch, SpeechViewer] or breath control [Cafet (Goebel et al., 1985)], etc., and especially if he or she is to work independently, the client must be "instructed in speech science." In the future, algorithms will be developed and tested against human judgments (Watson et al., 1989) to evaluate these comparisons as the basis of feedback.

PHONOLOGICAL DEVELOPMENT

Introduction. The final topic of this paper, phonological development, relates to the previous two in several ways. Research in phonological development is currently a very active area and one which has made significant advances because of the cross-disciplinary contributions of speech-language pathologists (Elbert & Gierut, 1986), linguists (Dinnsen, in press), and developmental psychologists (Jusczyk, in press), among others. In the study of phonological development, however, there is an interesting potential for reciprocal contributions to other disciplines. Collaborators from other disciplines are closely examining the unique insights provided by the studies of developing phonological systems to find answers to some of their own underlying questions.

This section is brief compared with the other two and grows out of some of my earlier research interests (Belmore et al., 1973; Kewley-Port & Preston, 1974). The following discussion highlights some current and future research in phonological development, particularly as it relates to issues raised earlier in this text.

Linguistic aspects of phonological development. The purpose of early studies in infant speech production was, by necessity, to establish inventories describing the development of sound production. Achievements to date include descriptive feature systems of vocal development that are applicable to the progression of vocalizations from vegetative to speech sounds during the first year of life (Oller, 1980; Stark, 1980; Stark, 1986). These prelinguistic feature systems overlap with descriptions of the emergence of phonetic features (Kent & Bauer, 1985; Kewley-Port & Preston, 1974) and the meaningful use of speech as phonemes, covering babbling through first words (Ferguson & Farwell, 1975; Lieberman, 1980). Finally, as a sufficient number of words (50) can be produced, and word imitation is established in the infant, a process of acquiring the phonological structure of the meaningful words in language begins (Locke, 1986; Stoel-Gammon & Cooper, 1984; Vihman et al., 1986). In parallel with investigations of sound inventories have been attempts to describe theoretical (universal) structures of these emerging sound systems. Although these structures are frequently based on phonological features (Jakobson, 1968; Locke, 1983), the domain of the features may also have its origin in motor, biological, perceptual, or cognitive aspects of development. Phonological systems have also been described for both normal and disordered speech (Dinnsen, in press; Stoel-Gammon & Dunn, 1985).

Two observations concerning the applications of phonetics and phonology to the emergence of speech seem in order. First, while tremendous progress has been made over the past 15 years, theoretical accounts of the development of phonetic and phonological systems must be considered preliminary. The database on which these developmental theories rest is too small. Considering that the database must include samples from 0–48 months (in the minimum) for both normal and disordered speech, the task of simply collecting the samples is enormous (Stoel-Gammon, in press). Thus, we can anticipate an effort to increase significantly this database in the near future. Fortunately, electronic networking will make it easier to share these databases over long distances. For example, the CHILDS database to archive language samples and an associated phonetic code is under development at Carnegie Mellon University (MacWhinney & Snow, 1985).

Another observation refers back to comments made earlier concerning the phoneme. A long-term quest in disciplines that relate to language—from engineers developing speech recognizers to speech-language pathologists describing disordered speech—is to discover the underlying unit of speech. Only a few are willing to promote the phoneme as the unit (Liberman & Mattingly, 1989). Phonological features are not widely accepted outside of linguistics. It now appears that studies of phonological development will play an essential role in providing data that can establish such a unit of speech. For example, the *syllable* is considered the primary unit in the studies of the "canonical syllable" by Oller and his colleagues (Oller, 1986; Oller & Lynch, in press), while *motor control patterns* are the units being investigated by Kent (in press) and Vihman (1990). Nittrouer et al. (1989) have proposed a syllabic unit founded in perceptuomotor coordination. Of course *phonological features* have received strong support from linguists who have demonstrated that they are a powerful tool for describing disordered speech (Dinnsen et al, 1990; Elbert & Gierut, 1986). Thus, research to discover appropriate units for explaining phonological development may be incorporated into the parent disciplines, which are themselves in search of the underlying unit of speech.

Psychology and the development of speech perception. The relation between the baseline capabilities of the auditory system and speech perception is a particularly acute one in understanding the development of speech. Not only must the relation between peripheral and central auditory processes be mapped out over the first years of life, but the relation between the control of speech production and the level of speech perception abilities is important as well. Because it has proven difficult to obtain reliable procedures for measuring various aspects of hearing and speech perception in infants, our knowledge of these processes is not as advanced as that of speech production.

Interestingly enough, more is known about how very young infants discriminate speech than nonspeech sounds. (See Kuhl, 1987, for review.) The auditory capabilities required to detect and discriminate tones appear to be somewhat poorer for infants than for adults in most cases, although exceptions have been reported (Olsho, 1984; Sinnott et al., 1983; Trehub, 1973). For speech stimuli, very young infants can discriminate a large variety of phonetic contrasts, many of which do not appear in their language-learning environment (Eimas et al., 1971; Morse, 1974; Streeter, 1976; Werker & Tees, 1984). It has, however, proven more difficult to determine at what age phonetic categories are established such that minimal pairs of syllables can be reliably *identified.* Phonetic categories for some sound contrasts have been shown for 2 ½ year olds (Oller & Eilers, 1983), but results for infants nearer 1 ½ years are ambiguous (Werker & Pegg, in press). Thus, we can anticipate a future emphasis on collecting data on speech perception capabilities in the 1- to 2-year age range. These data will complement efforts described above on mapping the development of phonological systems. Probably the hardest research program to carry out, but one that would be very fruitful, would be to obtain both perception and production data for the same infants, particularly between 1 and 2 years.

Returning then to the first topic of this paper, how can we account for the emergence of phonetic categories and the corresponding phenomenon of categorical perception? What are the primitive structures of perceptual categories (e.g., acoustic parameters, phonetic features, motor gestures) and are these primitives learned or innate? Although the lack of an adequate database may make theories about the emergence of categorical perception premature, the implications of these theories for investigations of phonological development and methods to remediate disordered systems are so important that such theories abound. It is appropriate to begin with the theory expounded by two of the "grandfathers" of categorical perception, Liberman and Mattingly (1989). They propose that the motor gesture is the primitive (of both perception and production) and that these primitives are innately available to the infant through the evolutionary development of speech in humans. According to them, there is a specialized, innate phonetic module, independent from auditory processing mechanisms invoked for nonspeech signals, that preempts and processes these primatives.

If the Liberman and Mattingly (1989) theory is an example of the extreme position to dissociate speech and nonspeech functions in the auditory system, then it appears that few others have followed their path. For example, on the other extreme, Jusczyk (1985; in press) proposes that general auditory processes are the basis for the surprisingly good phonetic discrimination observed in infants. A shift from these general, psychophysical processes to phonetic ones develops with exposure to the infant's language, according to Jusczyk. This shift is the result of assigning or tuning of weights to different properties of the acoustic signal that occur in both speech and nonspeech signals. This theory is compatible with current theories that model psychological processes, including learning, as distributive "neural" networks (Lippmann, 1989). Jusczyk's model is also compatible with that of Wode (in press). Wode links the emergence of categories to the development of heightened sensitivity in the auditory system, a concept that needs further consideration in light of the results of Kewley-Port et al. (1988) described earlier.

A theory somewhere between the two extremes is presented by Lindblom (in press). Lindblom's model, which has been simulated on a computer, takes as input the innate capabilities in the auditory system to discriminate speech sounds and principles for learning the articulatory gestures of a particular language. Given these, his model has demonstrated that a process of self-organization of information over time can lead to the discovery of the phonemic principles of a language.

In the future we hope that the current widespread application of computer simulations to the development and evaluation of theories of phonological development will pay major dividends. It is clear that in these models, the most important assumption is the characteristics of the primitive auditory structures that are assumed present at birth. An interaction between computer simulations and research on adults and infants across the different speech disciplines will help select a likely candidate for the true auditory primative.

Another topic that may be illucidated by use of computer simulations is that of the *learning principles* which underlie phonological development. Until recently, the principles of learning invoked in theories of phonological development did not have to be clearly expressed. With computer simulations, however, these principles must be made explicit as computer algorithms. Learning strategies derived from neural networks will also be investigated. Thus, we can expect that the role of learning and its careful study in phonological development will be a future area of active investigation.

CONCLUSIONS

Advances in speech science usually arise from cross-disciplinary collaboration because the science itself is too large and complicated for any single individual or discipline to grasp adequately. Given the topics discussed in the present manuscript, the following three issues stand out as particularly important areas for future research.

An exciting research area, especially for theoreticians, is that of phonological development. Given the small size of the database in phonological development relative to the overall data space, each new finding is an important piece in the puzzle as well as new grist for the theoretician's mill.

Research on the development of interactive computer-based systems for the diagnosis and treatment of speech disorders presents an enormous challenge to speech scientists and their collaborators. The necessary technology, from computer hardware to the speech processing algorithms, require the state-of-the-art solutions in engineering. In addition, sufficiently detailed analyses of the knowledge to be incorporated in the diagnostic or training components of these systems are needed in order to assist the human speech-language pathologist in these demanding tasks.

Research studies to compare human and computer-based techniques for speech remediation are vitally needed. Future research must both develop appropriate procedures for obtaining independent measures of speech performance and collect performance data in a variety of treatment settings.

Certainly the future offers many challenges to speech science as we try to apply knowledge gained from research on normal adults to the multitude of problems found in communication disorders.

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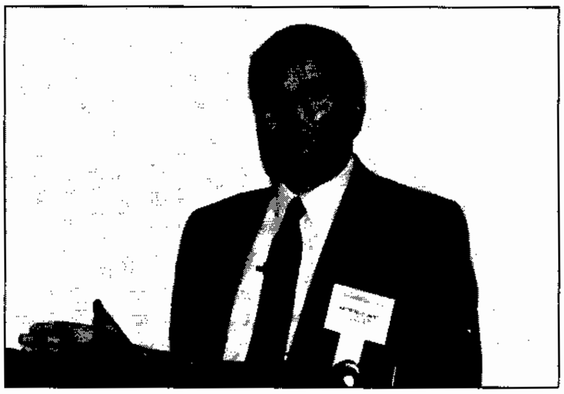
SENDING SPEECH SCIENCE INTO ANOTHER CENTURY: Comments on the Papers by Jeannette D. Hoit and Diane Kewley-Port



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The papers by Drs. Hoit and Kewley-Port are timely appraisals of the state of the science in speech production. The two papers take a different approach but converge on certain issues and complement one another in their assessment of research in this field as another century approaches.

Dr. Hoit's paper addresses the future of research in speech production by seeking expert opinion through interviews with 21 prominent speech scientists. The topics of the interviews generally seem to fall into three categories. The first pertains to science itself, that is, the use of scientific methods to discover knowledge about speech production. The second concerns scientists, the people who practice science. The third deals with the support system for science, including financial, technical, and educational support, and the various other ways by which society contributes to science and scientists. The remarks that follow address the first two of these issues directly and the third more indirectly.

THE SCIENCE OF SPEECH PRODUCTION

An example of an issue pertaining to science itself is the degree to which speech research is descriptive or theoretical. On this topic, Hoit records a difference of opinion, both as to the *current* state of affairs and as to the *desired or ideal* state of affairs given the nature of the field. Some maintain that descriptive work is the appropriate, perhaps even necessary, approach, given what is currently known—and not known—about speech. Others argue that theoretically driven research is preferable.

There seems to be a general feeling that theoretical foundations need shoring up. This is not a new feeling: It is one that I have heard in various discussions ever since I was a graduate student. What does it mean? It cannot mean that speech science is without theories. Theories abound in both speech science perception and speech production. A sampling of theories in speech perception (and its close relative, word recognition) reads: Motor theory (presented in revised form in 1985 by Liberman & Mattingly), TRACE theory (a connectionist theory proposed by McClelland & Elman, 1986), Cohort theory (Marslen-Wilson, 1980, 1987), Logogen theory (Jackson & Morton, 1984; Morton & Broadbent, 1967), LAFS (Lexical Access from Spectra; Klatt, 1979), a fuzzy logical model (Oden & Massaro, 1978), LAME (Lateral Access from Multilevel Engrams; Warren, 1983), Autonomous Search theory (Forster, 1976), Auditory-Perceptual theory (Miller, 1984), and Quantal theory (Stevens, 1989).

Speech production also has several theories to choose from. Its inventory includes: Motor Program theory (Sternberg, Knoll, Monsell, & Wright, 1988; Sternberg, Monsell, Knoll, & Wright, 1978), Motor Schema theory (Kent, 1981), Adaptive Model theory (Neilson & Neilson, 1987), Action (or Task Dynamics) theory (Fowler, Rubin, Remez, & Turvey, 1980; Kelso, Saltzman, & Tuller, 1986), Dynamic Gestural Patterning theory (Saltzman & Munhall, 1989), various forms of connectionist theories (Dell, 1986; Jordan, in press), "Iceberg" theory (Fujimura, 1986), and Quantal theory (Stevens, 1989).

These theories differ in fundamental ways, so that investigators do indeed have a choice, not an echo. Moreover, a large number of additional theories apply to subsystems. For example, in speech production, models have been developed for the respiratory subsystem, the laryngeal subsystem, the velopharynx, the tongue, the lip-jaw complex, and even the jaw.

Perhaps the dissatisfaction that emerges in Hoit's interviews is that the theories have not often enough yielded *critical* experiments. All of the theories can account for some part of the extant data, and all make some interesting but not necessarily immediately testable predictions. At their greatest points of frustration, researchers sometimes speak of the "theory crisis" or the "data crisis." Those who speak of the theory crisis are distressed that the theoretical developments do not effectively guide the research process. Those who speak of the data crisis are concerned that the database is simply too limited to enable an adequate test of available theories. Some might suggest an underlying crisis, a "tool crisis," in which the research tools don't generate the kinds and amounts of data needed for the desired pace of research progress. (There is also a phenomenon that might be called "tool fascination," in which the research is driven by available tools and the questions to which they might be applied.)

However one looks at the problem, it is instructive to try to compile the failed or rejected theories in the study of speech. After all, one view of science is falsification. Progress is determined in part from the rejection of theories that are not sustained in empirical test. We do have some striking failures. Husson's neurochronaxic theory of vocal-fold vibration was discounted and stands as one example of scientific progress by the falsification criterion. Even though Husson was wrong in his conception of vocal-fold vibration, his theory made testable predictions and therefore advanced the understanding of vocal-fold physiology. The myoelastic-aerodynamic alternative survived and was an effective precursor to elegant models in the current literature. On the other hand, some theories seem either to evolve well or resist experiment well enough to maintain their viability in the hostile world of empirical observation. For example, in speech perception, Motor theory weathered various assaults until a revised form of the theory was published in 1985. But revision counts as scientific progress too.

A possible reason for the discontent that some scientists have with the theoretical foundation of speech research is that theories tend to disappear more from disuse or lack of investigator interest rather than from outright empirical rejection. One gets the feeling that our research closet is full of theories, many out of fashion, but most of them still wearable in the right company.

Currently, connectionist models are commanding much attention in many fields, communication sciences included. Connectionist models are based on networks of large numbers of simple but multiply interconnected units. Typically, the connections can be weighted by a process akin to learning. In this way, each connection can modulate the activity that it transmits. The entire network has a behavior that depends on the initial state of activation of its units and the weights of its connections. These models often "work" in the sense that they can generate the desired output (patterned responses or rule-like behavior) from a specified input and the opportunity to "learn." However, especially when these models operate with hidden-unit nets, the question arises if the models are psychologically plausible. If they are, then another question is how one can choose among alternative architectures (see discussions of Hanson & Burt, 1990).

Similar observations can be made about the theoretical foundations of therapy for speech and language disorders. The problem is not so much a lack of theories as a means to their evaluation.

Speech Scientists

Concerning scientists, Hoit's interviews give us some hints about a part of our research future that is easily identifiable and probably most within our means to do something about. I am speaking of the next generation of scientists. The future of science is in the hands of a forthcoming generation of scientists—young people, and perhaps some career-changing older people—who will carry on the work. Hoit's interviews brought up the questions of scientific background and postdoctoral study. Many, if not nearly all, speech scientists would agree that general scientific education is desirable. But exactly what is speech science? Is it computer modeling, or the study of movements, or the study of events at the level of the motor unit, or is it the study of neural pathways, or is it the study of acoustics? It is all these and more. Speech is a confluence of disciplines, however bromidic that may sound. Yet our educational programs continue to fall short of the broad-based scientific training that seems appropriate. Speech science is too often a smattering of introductions to selected sciences, few of which are learned well enough to support independent inquiry.

Unlike several other disciplines, communication sciences and disorders regards postdoctoral study as a seldom-taken path to a research position. Many graduating doctoral students can find employment readily without participating in the postdoctoral interim so common in the physical and biological sciences. However nice it is to have the choice of doing postdoctoral study or not, there is a consequence that might be felt in the competition for research funds. Postdoctoral study often serves as a kind of research apprenticeship in which the postdoctoral fellow learns more about research but also about enabling research. One good way to enable research is to learn to write successful research grant applications, particularly the haloed RO1, or individual research grant application. If our discipline has relatively few individuals engaged in postdoctoral work, a possible consequence is that our scientists will lack the polish that scientists in other fields have by dint of their apprenticeship.

CLINICAL SPEECH SCIENCE

The topic of clinical application appears in both the Hoit and Kewley-Port papers. It is also a topic I recently pondered for a conference sponsored by the National Institute on Deafness and Other Communication Disorders. The conference was titled "Assessment of Speech and Voice Production: Research and Clinical Applications" (henceforth, Assessment Conference). I concluded that for the area of motor speech disorders, auditory-perceptual measures (*listening to speech*) and nonspeech maneuvers (movements like kissing, jaw wagging, and tongue protrusion) constitute the preferred battery of clinicians who have responsibility for dysarthric clients. At least this was the conclusion drawn from the Department of Veterans Affairs Dysarthria Survey (Gerratt, Till, Rosenbek, Wertz, & Boysen, in press). In this survey, 66 respondent clinics rated the frequency and value of various assessment tools. The highly favored auditory-perceptual measures usually are performed with little more than the clinician's ears and eyes. They probably define the mainstream of contemporary clinical practice, not just in dysarthria but in speech disorders generally.

The Department of Veterans Affairs Dysarthia Survey revealed that instrumental procedures, the kind of procedures generally used by Hoit's scientist informants, were rated lower than auditory-perceptual measures and nonspeech maneuvers in both frequency of usage and clinical value. Although the *predicted use* of the instrumental procedures exceeded the actual or current use, none of the ratings of *predicted* use of the instrumental procedures approached the rated *current* use of *nonspeech maneuvers*, which, of all methods rated, had the highest frequency of usage and the highest clinical value. There seems to be an irony in that fact, particularly because some research has shown a dissociation between impairments of speech and nonspeech functions of the same musculature.

It seems then, that contemporary clinical practice is built on a two-part foundation. The first part, consisting primarily of auditory-perceptual measures and nonspeech maneuvers, is both frequently used and highly valued. These procedures are easily implemented and involve little, if any, instrumentation. The items in the second part are used less frequently and valued less by the contemporary practitioner, but at least some clinicians believe that the future value of these procedures will exceed the currently perceived value. The items in this second component nearly always involve specialized instrumentation and a sophistication in the use of that instrumentation. A fundamental need in the future of assessment is to enfold these two parts together to produce an efficient and informative protocol. Unless and until these two parts cohere in clinical practice, their separation is emblematic of a continuing fissure between the primary activities of clinicians and the activities of researchers.

An undercurrent of the Assessment Conference was the possibility of bringing some standardization to research and clinical procedures. The subject was regarded with a mixture of trepidation, indifference, and enthusiasm. The current lack of standardization in speech and voice assessment is a symptom of the way in which research currently relates to clinical application. Will standardization be introduced to voice and speech assessment in the next century? Assuming that there was consensus on the desirability of standardization (which there may not be), we are left with difficult issues such as: What body or agency should take responsibility for standardization? Should standardization be introduced only narrowly, in well-defined areas that have been extensively studied? What are minimum requirements for the development of standards? Perhaps another conference is needed to encourage deliberate pursuit of standardization.

PLURALITY OF THE SPEECH SCIENCES

One striking aspect of Kewley-Port's paper is its breadth, ranging over the perception of complex sounds, the development of computer-based speech training aids, and the study of phonological development by acoustic and psycholinguistic methods. The cross-disciplinary or even cross-topic approach to research is distressingly rare in speech research, no matter how much we recite the well-rehearsed bromides about the interdisciplinary nature of the problem and its solutions. One of the areas discussed by Kewley-Port is a good example of the barriers and fragmentation in contemporary research on speech. The area is phonological development. It seems logical to me that this area demands the combined expertise from the fields of linguistics (including, but not restricted to, phonology), motor development, speech (complex sound) perception, speech production, developmental psychology, and so on. It is all too rare that even the two most obvious of the mutually concerned fields have a productive confluence. I refer to perception and production. Those who study perception often do so with singular focus, and those who study production do likewise. The isolation is not restricted to data collection but to theorizing as well.

In his recent book *The Remembered Present*, Gerald Edelman (1989) presents the kind of model that challenges our ability to overcome our isolationist research programs. Taking an approach based on evolution, neurophysiology, and linguistics, Edelman models language as a nested set of reentrantly connected functions (Figure 1). Edelman proposes that phonological, syntactic, and semantic levels should be able to interact in various combinations. To enable such interaction, he sees the need for special memory systems that are related to words

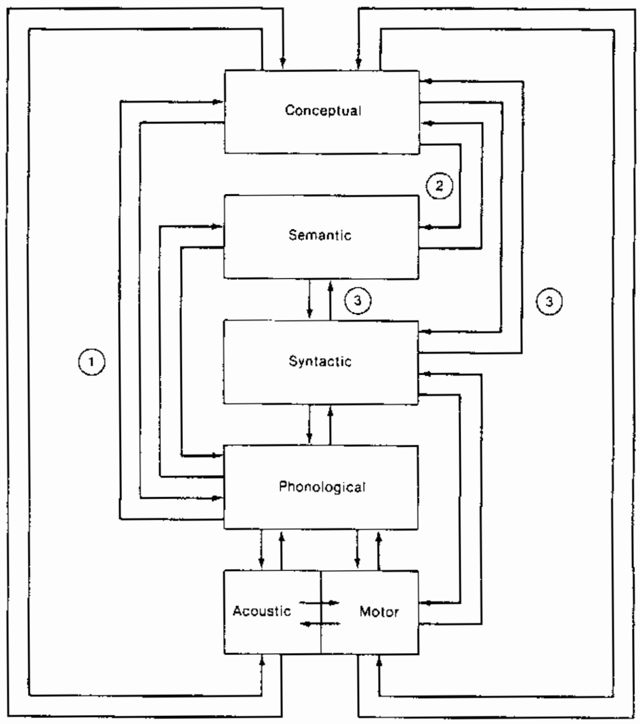

FIGURE 1. Edelman's diagram showing a strongly nested set of reentrantly connected language functions. From: G. M. Edelman, *The Remembered Present* (New York: Basic Books, 1989).

and sentences at each of these levels. The key element that does the enabling is *reentry*. Reentrant connections among the cortical regions concerned with phonological coding, utterance production, and conceptualization "would act to unite a particular sequence much in the same manner that multiple visual areas . . . that are functionally segregated can yield the basis for a particular coherent 'image' " (p. 177). In a sweeping summary of the matter, Edelman writes, "Speech rhythm, segmentation, categorization, and reentrant cycling may all have been closely linked in the evolution of the powerful speech specialization" (p. 179).

The purpose of these remarks is not to review Edelman's proposal in detail, nor to evaluate its promise for speech research. Rather, the intent is simply to cite a model of the kind that demands a holistic view of speech as a human behavior that is conceptual, linguistic, perceptual, and motoric. Attempts at bridging are difficult enough, and rare enough, even in the subdomains. In the first part of her paper, Kewley-Port addresses the gap between our understanding of the auditory system's ability to respond to acoustically complex speech signals and our understanding of the perception of speech in normal discourse. She summarizes results that converge on a central conclusion, namely support for a conception of speech perception in which the salience of portions of the speech waveform is determined by substantial experience rather than by some inherent acoustic features. In other words, we have to learn to process the complex acoustic signal of speech. However, I wonder if the "word-length" sound patterns used as stimuli differ from natural speech in dimensions that may in fact underlie some kind of inherent acoustic molding of attentional and perceptual processing. For example, the syllabic crests of natural speech may be a coarse temporal feature, readily detectable from the envelope of the speech waveform, that directs attention.

Kewley-Port points to an intriguing example of how converging knowledge from different areas of investigation might lead to improvements in both the tools and substance of our science. The example is Hermansky's (1990) derivation of an analysis technique called Perceptual Linear Predictive (PLP) analysis. This technique enfolds three psychophysical concepts with an autoregressive all-pole spectral modeling to yield a type of analysis that appears to have several advantages over the more conventional LP techniques. This is a good example of a kind of symbiotic relationship between science and technology. It is also an example of how knowledge from different domains can converge in scientific progress. The convergence can take form in the humanization of technology. This humanization refers not only to the use of technology to aid us in our human pursuits but also to the ways in which technological progress often is marked by the machine simulation of human characteristics. Because talking is easy and natural, efforts are underway to give us voice input to personal computers. Approaches such as Hermansky's yield analysis tools that have humanlike features. A likely benefit is that the visual displays of speech in computer analysis systems will become increasingly easier to associate with our auditory experience of speech. As this happens, eye and ear will unite in a compatible perceptual phenomenon, making it easier to develop clinical instruments for speech analysis and for speech training.

The promising results described by Kewley-Port for computer-based speech training aids may presage a future in which computers serve the clinician as more than a device to display signals. Indeed, the decision-making potential of computers is an exciting arena. As Kewley-Port points out, there is much that remains to be done just in obtaining, processing, and interpreting acoustic and physiologic signals. Certainly, progress is being made in problems such as derivation of the vocal-tract configuration from the acoustic signal. I share Kewley-Port's optimism that acoustic analysis will enable near-future clinical applications.

But I also have pestering thoughts about the obstacles that stand in the way of widespread clinical application. First, the Veterans Affairs Dysarthria Survey gives the sobering advice that many clinicians don't expect much from instrumental techniques and that their laboratories *usually* are not equipped with even the basic A/D board. Add to this the fact that most of the data on speech production are based on the adult male talker. The data base of speech science needs to be expanded to include data on women, children, and individuals with diverse cultural and linguistic backgrounds.

And it is not only the data but theories as well that are shaped by the historical pattern of adult male focus. Titze (1989) commented, "One wonders, for example, if the source-filter theory of speech production would have taken the same course of development if female voices had been the primary model early on" (p. 1699). Klatt and Klatt (1990) commented on the same point: "Informal observations hint at the possibility that vowel spectra obtained from women's voices do not conform as well to an all-pole model, due perhaps to tracheal coupling and source/tract interactions" (p. 820). I believe that many implications of sex and age differences in respiratory, phonatory, and articulatory functions have not been adequately described. Those who criticize the relative exclusion of women from research supported by the National Institutes of Health (NIH) have grounds for their complaint. Our database for half (or more) of the population is not satisfactory. In my own research on both normal-speaking and dysarthric individuals, I am becoming convinced that the sex differences can be important. Not only are men and women not uniformly vulnerable to various neurologic diseases, but they may differ in the effects of the same disease.

One last concern: Our models of speech production remain stubbornly fixed to phonation or articulation (and rarely both together). Perhaps Dr. Hoit would agree with me that respiration is sadly neglected in most of the published models of speech production. It is taken for granted.

SOME THINGS TO DO

These preceding comments are intended to be a kind of charge for the future. I agree with the directions charted by Drs. Hoit and Kewley-Port, but I would add the following elements for a research agenda:

1. The acoustic and physiologic database on speech production should be augmented by increasing the number of women and children who serve as research subjects. Many of the procedures developed for speech research are noninvasive or present little risk to human subjects. We can comfortably study women and children. Speaking is not a male prerogative, nor are speech disorders exclusively a male frailty. Speech also needs to be studied in individuals who represent diverse cultural and linguistic backgrounds.

2. Steps should be taken to introduce instrumentation and the sophistication to use it in clinical settings. Clinical protocols, dominated as they are by auditory-perceptual methods, continue to stand in sharp contrast to contemporary research protocols. Demonstration clinics, model programs, and standardization will help to bring technology into clinical and human utility.

3. Fight fragmentation. In a well-intentioned effort to make things manageable, scientists tend to separate speech from language, speech from hearing, and then further subdivide speech into the typically nonjoined systems of respiration, phonation, and articulation. But the clinician always encounters them bound together in human form with no dotted lines to mark boundaries and no well-defined boxes with connecting arrows. Efforts should be made to unite speech perception and speech production in both theory and data. Speaking and listening have been studied separately for too long. Communication fares better when they are both present. In like manner, language formulation and speech production should be melded. We should heed the ambitious effort of Willem J. M. Levelt, who tries to unite at least some of the asundered pieces of communication in his book *Speaking: From Intention to Articulation* (1989). What havoc he wreaks with most theories of speech production when he asserts, "The construction of a preverbal message is a first step in the generation of speech" (p. 107). Preverbal? I know a few speech production experts who are uncomfortable with anything phonological, let alone preverbal.

4. Prepare the way for those who will follow us. A radical prescription is to overhaul the educational system, from undergraduate to doctoral level. It appears that discontent with the current educational system is growing, even among the professoriate. James J. Duderstadt, president of the University of Michigan, has spoken of a reform movement that will result in substantial changes in higher education. He is quoted as saying, "That happened in this country 100 years ago, and it will happen again in the 1990's" (Grassmuck, 1990). We need curricular revision that seeks to give students serious opportunities to learn problem solving skills. Perhaps the classroom lecture, the default model of modern education, needs to be discouraged. More modestly, steps could be taken to increase scientific consciousness and scientific ability at all levels of the educational process. Our discipline ought to get away from the sloppy thinking that science means study of the normal communicator. Communication science should embrace the normal and the disordered in combined or paralleled research programs and in theories that make normal and disordered cohere in a common understanding. There is a need to give more honor to teaching. The way to that honor is to take teaching seriously, as seriously as we take research grant applications and papers submitted to archival journals. Teaching should be peer-reviewed, in the same spirit of examination and expectation that is given to peer reviews of research grant applications and scientific articles.

5. As argued earlier, the time is right for efforts at standardization. We need to exercise some control over the current chaos of clinical assessment procedures and measures. Researchers and clinicians should come together to work out a systematic plan for the development and promulgation of assessment standards.

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