Chapter 15

Research in Audiological Rehabilitation: The Challenges

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The primary challenge facing rehabilitative audiology is to establish a sound empirical basis for clinical practice, yet numerous obstacles impede attainment of this goal: philosophical differences between researchers and practitioners, the distinct goals of research and practice, and conflict between researchers' emphasis on general laws and principles and practitioners' focus on the individual client. Several research methodologies are reviewed with the dual goals

of illustrating how practitioners can contribute to the establishment of scientific knowledge and how the results of audiological research can be applied in clinical practice. New models of mentorship and collaborative, interdisciplinary approaches can vitalize clinical research and engage practitioners in the research enterprise.

Audiologists and professionals in related disciplines provide a wide range of services designed to enhance the communicative and interpersonal functioning of individuals with hearing impairment. The extent to which these habilitative and rehabilitative services are grounded in scientific evidence of their efficacy varies considerably. The preeminent challenge for audiological rehabilitation in the foreseeable future is to establish a sound empirical basis for clinical practice in all its variety and complexity. The scope of this endeavor must include, *at a minimum:* (a) validation of procedures used to select and individualize instruments such as hearing aids, tactile aids, cochlear implants, and assistive listening devices; (b) documentation of the ultimate benefit that is derived from sensory aids in everyday life; (c) establishment of the benefits obtained from various types of training, such as auditory training or training in speechreading, assertiveness, or communication strategies; and (d) demonstration of the efficacy of behavioral and psychosocial interventions, such as adjustment counseling, stress management, and environmental management. Current trends and future directions in these substantive areas of research are the focus of several chapters in this monograph.

The call for a comprehensive research agenda in audiological rehabilitation is not new, nor, with few exceptions, has it been heeded (Carney, 1993a). The challenge is twofold: first, to generate a knowledge base that has implications for habilitation and rehabilitation and, second, to translate that knowledge into intervention strategies that are employed in clinical practice. The obstacles to meeting this challenge are not insignificant. They are grounded in philosophy of science and in the differing objectives and world views often held by researchers and clinicians. In this chapter we first consider the philosophical context within which research questions are framed, and then discuss more specific conceptual, methodological, and pragmatic issues that must be addressed in order to meet these challenges.

THE RELATIONSHIP BETWEEN RESEARCH AND PRACTICE

Integration of research and clinical practice within health and human-services professions is an ideal that can be realized in various ways. The *scientist-practitioner model* promotes integration by providing graduate training both in the methods of scientific research and in the practice of a discipline. This model was initially developed for clinical psychology at the Boulder Conference of 1949 (Raimy, 1950) and, despite some controversy over its merits and success (see Barlow, Hayes, & Nelson, 1984, Chapter 1, for a review), it continues to be

the dominant training model in clinical psychology today (Belar & Perry, 1992; O'Sullivan & Quevillon, 1992). The scientist-practitioner may function primarily as a researcher, primarily as a practitioner, or, ideally, may integrate both functions. In the context of professional practice, he or she directly applies research-based knowledge and uses scientific principles in information-gathering, reasoning, decision-making, and evaluation.

A *dual-track model* recognizes distinct career paths and distinct training tracks for researchers and practitioners, but nonetheless directs practitioners to be consumers of research-based knowledge and to apply that knowledge in clinical practice. Practitioners' training emphasizes acquisition of clinical knowledge and skills, but they also receive training in scientific method so that they can critically evaluate research and identify its clinical relevance. Likewise, researchers may receive basic training in diagnosis and treatment of disorders, but more emphasis is given to research methodology, data analysis, statistical inference, and technical writing. The dual-track model reflects the fact that the vast majority of practitioners do not conduct research. Thus, integration of research and practice is primarily achieved through transmission of knowledge *from* researchers *to* clinicians rather than through active participation by practitioners in the research enterprise.

Despite the obvious importance of achieving integration of research and practice, and regardless of which integration model one favors, there are three differences between these two domains that create impediments: different philosophical approaches to knowledge, different goals of research and practice, and different emphases on general knowledge versus knowledge of individuals.

Philosophical Perspectives

Historically, the philosophical perspective that has guided scientific research is *logical positivism*. In this view, knowledge of truth and reality are obtained from directly observable events, which are communicated quantitatively in the form of measurements. Scientific laws are inferred from regularities in observed relationships among variables (or *constructs*), and these theoretical propositions are then tested for their generality and explanatory power, and confirmed or revised in accordance with experimental outcomes. Knowledge is accrued through formal methods of inquiry that impose stringent procedural requirements so as to ensure the validity of the conclusions drawn. These positivistic themes are common topics of courses in research methods, experimental design, measurement, statistics, and data analysis.

Hoshmand and Polkinghorne (1992), attempting to redefine the science-practice relationship in psychology, point out that practitioners depend not only on formal, research-based knowledge, but also upon knowledge gained through experience. *Experiential knowledge,* which is essential for effective clinical practice, has more immediate impact on the judgments and decisions made with individual clients than does research-based knowledge. The training of practitioners in many fields implicitly recognizes the distinction between content-

knowledge and practical, applied knowledge by the dual curricular mechanisms of course work and clinical practica. However, this experiential knowledge, which appears to emerge from trial and error and to be unsystematic and unpredictable, does not conform to the positivistic standards for scientific research. It is passed on through an oral tradition and is referred to, often pejoratively, as clinical "lore." Hoshmand and Polkinghorne argue that a greater integration of science and practice can be achieved if a more constructivist philosophy is adopted wherein research methodology is broadened to permit mutual exchange of knowledge among researchers and practitioners:

> We need an epistemology of professional inquiry that (a) can bridge the gap between the formal knowledge base of research and the knowledge processes of practice and (b) would allow practitioners to contribute to the knowledge base of the profession. (p. 56)

The key element in this position is its affirmation that knowledge gained clinically, through interaction with individual clients and evaluation of their treatment outcomes, is a legitimate source of knowledge in its own right. Indeed, this constitutes a third model for the integration of research and practice, one of *mutual influence* between scientist and practitioner and bidirectional transfer of knowledge. This approach fosters openness to a variety of methodologies, particularly those that are qualitative and descriptive, which have not traditionally been used in the more theory-driven, quantitative positivistic approach. Clinicians can therefore be encouraged to join the research enterprise, not just by conducting traditional research that tests hypotheses and theoretical predictions, but also by codifying and systematically documenting, for a larger audience, what they now communicate informally to one another. In principle, these ideas are completely consistent with what was envisioned by the earliest proponents of the scientist-practitioner model (Barlow et al., 1984, pp. 8-10) and is still advocated by its adherents today (e.g., Stricker, 1992). They simply highlight the powerful, and different, philosophical bases for knowledge acquisition that in fact typify researchers and practitioners.

Goals of Research and Practice

Regardless of whether the roles of researcher and practitioner are integrated within the same individual, there is consensus across many disciplines that the goals of these two endeavors are different. The goal of research is to obtain an answer to a research question. The question is usually cast within the context of existing knowledge and with the explicit purpose of adding to it. Toward that end, observations are carefully structured, so as to be verifiable, objective, and reliable. Extraneous variables that might confound interpretation of relationships are identified and controlled to the greatest extent possible. Moreover, sufficient data are gathered to ensure generalizability of the results to a target population. In short, there is adherence to the canons of scientific method.

In contrast, the goal of practice is to provide the best possible outcome for

the client. The activities involved in diagnosis, treatment formulation, treatment implementation, and treatment evaluation resemble in many ways the activities of systematic research. Indeed advocates of the scientist-practitioner model conceptualize practice as application of scientific principles: formulation of a hypothesis about the nature and degree of a presenting problem, evaluation of the hypothesis on the basis of systematically gathered evidence, development of a treatment plan tailored to the individual, and systematic evaluation of treatment effectiveness. Nevertheless, in clinical practice these activities are driven by the needs of the client and by the ethical obligation to provide the best treatment possible. At each stage, achievement of the best possible outcome for the client is the overriding consideration.

The different goals of research and practice impede their integration in two ways. First, the generalizability (i.e., external validity) of laboratory research findings to clinical settings may be limited by elements of research design. Subject selection criteria, uniform testing procedures and treatment protocols, control of extraneous variables, and other considerations necessary for rigorous testing of research hypotheses result in findings whose validity can only be assumed under those conditions. Moreover, the subtle effects of volunteerism and subjects' awareness of participating in a research study may further limit the generalizability of the results. Thus, when a practitioner seeks to be a consumer of research-based knowledge and to translate that knowledge into clinical practice, its relevance to the clinical context, which differs in many ways from that of the research context, may be quite low. One frequent criticism of many research designs (to be discussed in more detail below) is that they are based on groups of subjects with conclusions formulated in terms of differences between group means, whereas the practitioner provides service to individual clients and must make treatment decisions on an individual basis.

The second way in which the goals of research and practice impede their integration is more serious and much more difficult to overcome. It concerns the conduct of clinical research within the context of clinical practice. In many instances, the goals of research and of practice can be served simultaneously, but integration of research activities into clinical practice is easily thwarted by their incompatibilities. Siegel and Spradlin (1985) note that formation of control groups and random assignment of subjects to groups may be logistically impossible or objectionable on ethical grounds. Even the use of delayed-treatment control groups is problematic because a potentially effective treatment is withheld for a period of time. Single-subject designs, which have recently been widely advocated for use in the clinical setting (Barlow et al., 1984; Connell & Thompson, 1986; Kearns, 1986; McReynolds & Thompson, 1986) and which address some of the concerns about generalizability of findings from group designs, are not without problems. Siegel and Spradlin note, for example, that establishment of a stable baseline is important for some time-series designs, but if the length of time required to accomplish this is great, the resultant postponement of treatment is not in the best interests of the client.

Barlow et al. (1984) have been at the forefront in promoting the fullest integration of scientist and practitioner roles. Indeed, they have stated, "In some sense it can be argued that good practitioners are already doing evaluations of potential scientific value with clients they see, if they follow the guidelines for good professional practice" (p. 158). Barlow et al. distinguish four activities on a continuum between pure treatment and pure research and show how they relate to the goals of research and practice respectively (see Table 1).

Client outcome is the only concern in *pure treatment,* but in *treatment evaluation* the goal is also to determine whether the treatment per se was effective and, if so, what components of the treatment were responsible. The potential contribution of this type of systematic evaluation of a knowledge base for clinical practice is obvious and is what the authors allude to in the statement quoted above. However, because the clinical goals are primary, procedures (such as shortened baseline periods or midcourse changes in the treatment protocol) may not be optimal for achieving research goals. In *treatment research* these priorities are reversed, with client needs being relevant, but secondary to the requirements of research design. Random assignment to treatments and delayed-treatment controls can thus be justified if it is explicitly acknowledged that the scientific goals are primary and client outcome secondary. A corollary of this position is that the client's informed consent will be obtained so as to address the ethical issues that are entailed. In *pure research,* there is no goal of improving outcomes for the clients/subjects who participate in the research. The researcher is free to design the investigation in accord with the highest standards of scientific inquiry without regard to whether benefit will be obtained by individuals who participate. Ethical standards that govern the use of human participants in research provide the necessary safeguards against adverse effects, and again, informed consent is obtained. These distinctions are extremely useful because they show how the tension between the disparate goals of research and practice can be alleviated by identification of which goals are primary and which are secondary or irrelevant in a given situation.

Table 1

The Distinction Between Treatment, Treatment Evaluation, Treatment Research, and Research

Goal	Pure treatment	Treatment evaluation	Treatment research	Pure research
Pursuit of better organized scientific statements	None	Secondary	Primary	Primary
Pursuit of better client outcome	Primary	Primary	Secondary	None

Note. From *The Scientist Practitioner: Research and Accountability in Clinical and Educational Settings* (p. 284) by D.H. Barlow, S.C. Hayes, and R.O. Nelson, 1984, New York: Pergamon Press. Copyright 1984 by Pergamon Press Inc. Reprinted by permission.

Levels of Analysis

Scientists and practitioners differ greatly in the extent to which they are concerned with general principles as opposed to specific instances. In the physical sciences there is a notable tradition of success in formulating general laws that hold true across a vast diversity of contexts and conditions. Such laws involve measurement of constructs and determination of the mathematical functions that relate them. Weber's Law and Stevens' power laws in psychophysics, are familiar examples from hearing science.

In the human behavioral sciences, the unit of observation is a person. General laws of behavior must apply to all individuals or must in some manner accommodate individual differences. Yet each individual is unique and represents a complex, dynamic system within which general behavioral laws are realized. In addition, there may be lawfulness and regularity within the behavior of an individual that are superimposed upon, and possibly independent of, the laws that hold across individuals. The terms *nomothetic* and *idiographic* are used to characterize general versus individualistic levels of analysis in understanding and predicting behavior (Allport, 1937, p. 22).

The contrast between nomothetic and idiographic concerns may be the single most important difference between research and practice that impedes their integration because scientific research is almost invariably nomothetic in the kinds of questions it asks, whereas clinical practice is, by definition, idiographic. A researcher may demonstrate that a program of analytic training in speechreading results in a statistically significant improvement in performance, but a clinician must decide whether to recommend such training based on a hypothesis about whether or not a particular client will be one of those who show such benefit. A researcher may demonstrate that Treatment A tends to produce better outcomes than Treatment B for a particular disorder, but a clinician must decide whether Treatment A or Treatment B should be prescribed for a particular client with that disorder. A researcher may show that there is a significant negative correlation between client age and benefit received from a hearing aid, but a clinician will need to decide whether to prescribe a hearing aid for a particular 80-year-old client.

In each of these examples the researcher's question, and the answer obtained, pertain to average or expected effects. It is unlikely that every individual in the hypothetical speechreading study showed an improvement with training, but, on the average, performance after training was reliably better than before training. Similarly, in the comparison of two treatments, on the average one may produce better outcomes than the other, but it is unlikely that in every individual case this would be true. Other variables, not incorporated in the research design, may account for individual differences in the relative effectiveness of the two treatments. But the clinician receives no guidance from the research report as to what these variables might be because the focus is on the general finding that Treatment A is superior. Lastly, in the hypothetical correlational study, which does involve individual differences among clients (both with regard to age and

hearing aid benefit), the result is still that as age increases, on the average, benefit decreases. Unless the correlation is nearly perfect, there will be considerable variability in benefit at every age and the clinician will have limited guidance, without additional data, as to whether *this* 80-year-old client is likely to do better or worse than predicted.

Because the implications of research-based knowledge are not straightforward in clinical practice, other sources of knowledge are also utilized. First, the clinician draws upon his or her own cumulative, experiential knowledge, which includes prior cases who have received speechreading training, prior experience with Treatments A and B, and prior experience fitting hearing aids to 80-year-old clients. This experience will have resulted in an informal personal evaluation of the effectiveness of the various interventions, as well as implicit hypotheses about the characteristics of individual clients that may have had an impact. Second, the clinician will draw on the wealth of diagnostic and case history information available on the client. This detailed assessment of the client provides an idiographic context within which research-based and experiential knowledge can be combined, and a clinical judgment about the preferred course of action can be made.

Implications for Audiology

The three differences between the domains of research and practice that have been discussed here can be mutually exacerbating, and this can lead to a complete breakdown in the integration of research and practice. Diagnostic and rehabilitative audiology are less vulnerable to such a breakdown than some other human service professions because the clients' presenting problems typically have a sensory/perceptual basis. There is a strong foundation in hearing science for understanding normal and impaired auditory function. Diagnostic tests are sophisticated, ranging from behavioral to electrophysiological, and there is a strong theoretical and empirical basis for technological interventions, such as hearing aids and assistive listening devices. The knowledge base derived from pure and applied research clearly informs clinical practice and will continue to do so.

However, the challenge identified at the beginning of this chapter is to provide a similar scientific basis for those aspects of rehabilitative practice that do not currently enjoy such a strong foundation. It is in precisely those substantive areas that audiology is most similar to other human service professions such as psychology, rehabilitation counseling, education, and social work. Within these disciplines there has been considerable discussion about the forces that tend to dissociate research and practice, and strenuous efforts have been made to resolve the dilemmas surrounding integration of the two.

CLINICALLY RELEVANT RESEARCH METHODOLOGIES

In the second part of this chapter research methodologies with relevance to clinical practice are examined. A guiding assumption in this presentation is that

every audiologist has a role to play in meeting the challenge to empirically justify all aspects of rehabilitative practice. Few may choose to fully integrate the roles of scientist and practitioner (although such integration is the ideal), but every audiologist has the obligation to incorporate relevant research-based knowledge into clinical practice, and the majority can contribute to documentation and formalization of experientially-based knowledge.

In the sections below we begin with consideration of relatively neglected descriptive methods and end with the classical experimental method. Examples of these methods in the audiological literature are cited to highlight the clinical relevance of various types of research and also to illustrate successful integration of research and clinical practice.

Descriptive Methods

Because the dominant positivistic philosophy of science so highly values testing theoretical propositions, the hypothetico-deductive approach to research is strongly preferred over mere observation and description (Cronbach, 1975). We stand in strong opposition to this unfortunate value system because description, however unpretentious its origins, is the foundation upon which solid theoretical structures must be built. Moreover, careful, systematic, and comprehensive description directs attention to phenomena worthy of more rigorous experimental investigation and suggests the hypotheses that merit subsequent empirical test.

Qualitative methods. This term is applied to methods of inquiry that define phenomena from the perspective of those experiencing them and that derive the structure of a particular domain inductively rather than imposing an a priori conceptualization upon it. Qualitative research involves data collection procedures such as interviews and direct (field) observation, methods of coding and interpreting interviews or field notes, and techniques of theory-building that are grounded in concepts and relationships inferred from the data (Stern, 1980; Strauss & Corbin, 1990). Descriptive research often combines qualitative and quantitative methods.

Practitioners engage in qualitative assessment when they gather information about the specific nature of a presenting problem, the situations in which it expresses itself, or its impact on the individual and significant others. The *clinical interview* is probably the technique most widely used for this purpose, and if information is gathered solely for the purpose of client assessment and treatment planning, it is considered a tool of professional practice rather than of research. However, if a *structured interview* is developed and consistently used, the information gathered for an individual client can be combined with that obtained from other clients so as to characterize the clinical population seen at a particular facility or to characterize a target population in general.

Erdman, Crowley, and Gillespie (1984) have described a related method of qualitative assessment in which a *problem-solving assignment* was given to participants in a group aural rehabilitation program. Clients were instructed to describe three situations in which their hearing loss affected their communication

ability and to include information about the environment, the purpose of the communication, their behavioral and emotional reactions, and those of the other persons in the situation. Not only was the information used to structure a problem-solving approach to rehabilitation, but systematic review of 300 cases that were acquired during the ongoing program permitted a description of the prevalence of various behavioral and emotional reactions within the clinic population. Such information proved useful for planning and structuring the rehabilitation program itself, but was also the foundation for the development of a quantitative assessment of adjustment to hearing loss (Demorest & Erdman, 1986, 1987). This example illustrates how qualitative information gathered as an integral part of clinical practice can be analyzed so as to enhance service delivery and also contribute to the knowledge base regarding adjustment to hearing loss.

Other investigators have used qualitative approaches to describing handicap associated with hearing loss (Hétu, Riverin, Lalande, Getty, & St-Cyr, 1988) and strategies for managing hearing impairment (Hallberg & Carlsson, 1991). Because these studies were designed from the outset to meet research (as opposed to clinical) objectives, considerable attention was given to the structure of the information-gathering interview, the training of coders who performed formal content analysis of the verbatim transcriptions, and the interobserver consistency (i.e., reliability) of the results. Interestingly, these two studies produced descriptions that were generally consistent with one another and with the results obtained by Erdman et al. (1984). Thus, despite the differing goals and techniques of the clinical and research-oriented descriptions, convergence in their findings has contributed to a broad-based understanding (spanning three countries and three languages) of the problems experienced by hearing-impaired adults and the mechanisms of adjustment they utilize.

Surveys. Comprehensive and systematic description of an individual can be undertaken via interview, but a comparable level of description for an entire population is more likely to be based upon a survey. Surveys are typically questionnaires that use a closed-response format which is easily codable for subsequent data analysis. Some surveys, such as those conducted by telephone, strongly resemble interviews, but differ in that there is uniformity in the wording and in the sequence of questions asked. Standardization takes precedence over flexibility because a survey's goal is to characterize the population rather than the individual.

In the context of audiological rehabilitation, surveys are often used for formal or informal *program evaluation* (a topic discussed in more detail by Hyde & Riko in Chapter 17 of this monograph). Clients who have received hearing aids or who have participated in a rehabilitation program are surveyed to ascertain their satisfaction with services received, their adjustment to amplification, or their perceptions of benefit received and any residual difficulties experienced. An excellent example of this approach is the series of surveys conducted in Great Britain over a period of years to evaluate the National Health Service's hearing aid distribution programs (Bicknell & Davies, 1968; Brooks, 1972, 1981; Car-

stairs, 1973; Grier, 1968; Kodicek & Garrad, 1955; Rice, 1966). A more recent example is the national survey of graduates of auditory-verbal habilitation programs conducted by Goldberg and Flexer (1993). Their survey examined the social validity of auditory-verbal treatment: Did such treatment achieve its long-range goal of educating children in regular learning and living environments and enabling them "to become independent, participating, and contributing citizens in mainstream society" (p. 190)?

Surveys conducted at Veterans Administration hospitals (Hutton, 1980, 1982, 1983), teaching hospitals (Alberti, Pichora-Fuller, Corbin, & Riko, 1984), and in the military (Erdman, 1984; Scherr, Schwartz, & Montgomery, 1983; Surr, Schuchman, & Montgomery, 1978) illustrate the use of this approach for local program evaluation. Although assessment of individual clients was not the goal of the National Health Service surveys, the other surveys cited served the dual purposes of program evaluation and routine follow-up of clients for individual treatment evaluation. These studies support the contention that data generated in ongoing clinical practice can be aggregated and analyzed so as to contribute significantly to a knowledge base with broader application than that for which the data collection was originally undertaken.

Databases. An emergent methodology for population description that is increasingly available as an automatic accompaniment to routine clinical practice is *database query*. Clinical records that include demographic and case history information are often stored electronically and computer-based diagnostic testing (e.g., Hunter & Margolis, 1992) and hearing-aid prescription (e.g., Sims, Knight, & Austin, 1992) are clinical realities. Various forms of self-assessment are being converted for computer administration (e.g., Palmer, 1992a), and even among those that are administered in a paper-and-pencil format, software systems are available for scoring and report generation (Demorest, 1987; Demorest & Erdman, 1984; Palmer, 1992b). Database systems can also be created specifically for accrual of normative data on clinical test procedures such as auditory brainstem audiometry (Weber, 1992). Tye-Murray (1992) and Sims (1988) describe computer-controlled, interactive speechreading training, wherein records of an individual's responses can be captured for subsequent analysis.

All of these examples involve the generation of databases as a natural outgrowth of diagnostic and rehabilitative services provided by practitioners. Because the clinical records are stored electronically, it is extremely simple to query the database and summarize patterns within the data that hitherto would have been accessible only through tedious (and probably unfeasible) review of paper records. Base rates for various diagnostic categories, norms for clinical tests, documentation of client progress on behavioral and self-report measures of disability and handicap, and patterns of association among all such variables can now be obtained simply and inexpensively. Thus, the practitioner is in a position (a) to formalize experiential knowledge that has immediate application in day-to-day clinical practice and (b) to contribute to a comprehensive description of clinically observed phenomena. The bidirectional model of knowledge transfer

envisioned by Hoshmand and Polkinghorne (1992) is achievable. Moreover, a fully-functioning clinical arena (rather than a laboratory setting) is the source of this information flow.

Psychometric Research

Psychometric research (which can utilize all of the methodologies discussed in this section) focuses on the development and evaluation of tests. Testing is an integral part of audiological practice whether it focuses on measurement of hearing sensitivity, word recognition, sentence identification in noise, speech-reading ability, hearing handicap, or hearing aid benefit. Standardization of stimulus materials, instructions, protocols for test administration, and scoring is essential for maximizing test reliability and validity and for minimizing the effects of extraneous variables on test scores. Once a test has been developed, however, clinical application and test-score interpretation must be grounded in solid, scientific evidence of the test's psychometric characteristics. If audiological rehabilitation is to have a firm, empirical basis, test developers (or others) must conduct the necessary psychometric research, and practitioners must be knowledgeable about psychometric principles and conversant with the research literature on the tests they use. This section reviews, very briefly, the kinds of information practitioners need for clinical practice and cites illustrative examples of relevant psychometric research in diagnostic and rehabilitative audiology.

Instrument development and evaluation. Tests that assess individual differences represent operational definitions of hypothetical constructs. Pure-tone audiometry measures auditory sensitivity; nonsense syllables, monosyllabic word lists, and sentence lists measure various aspects of auditory and visual speech perception; self-assessment inventories and questionnaires measure constructs ranging from handicaps that may accompany a hearing loss, to psychosocial adjustment, to hearing aid benefit.

Every stage of test development and evaluation provides information that is relevant to the use of tests in clinical practice. *Construct definition* begins with the test developer's conceptual definition and ends with precise specification of test content. Test selection by the practitioner must be guided by evidence (a) that the construct the test claims to measure is appropriate for the intended clinical use, and (b) that the test content adequately represents the construct. It is not uncommon for instruments with similar measurement objectives to define a construct quite differently (Demorest & Walden, 1984).

One striking example of differences in operational definitions is provided by two distinct approaches to defining hearing aid benefit. Cox, Gilmore, and Alexander (1991) compared the Profile of Hearing Aid Benefit (PHAB), in which respondents indicated the frequency of communication problems in various situations, with and without their hearing aid, to the Intelligibility Rating Improvement Scale (IRIS), in which respondents rated the percentage of speech understood in those situations with and without their hearing aid. Cox et al. found that comparable subscales from the PHAB and IRIS correlated only modestly

(i.e., .76, .39, .34, .29, and .54). Clearly, each of these approaches to assessing hearing aid benefit provides a logical and reasonable operationalization of the construct, but a clinician would have to consider his or her measurement objective carefully in order to decide between them.

Results of *item analysis* and *factor analysis* provide additional, detailed information about the construct(s) that tests assess. For example, Demorest and Erdman (1986) analyzed the items and scales of the Communication Profile for the Hearing Impaired (CPHI) and determined which items best represented the content of each individual scale. They also factor analyzed the scales (Demorest & Erdman, 1989) and uncovered five common factors underlying them. Such information can be used to develop factor scores and is directly applicable to the interpretation of clients' profiles.

Evaluation of test score *reliability* and its counterpart the *standard error of measurement*, can take many forms, each with different implications for test interpretation and use. Estimates of internal-consistency (e.g., split-half reliability, coefficient alpha) tell the clinician whether the score obtained on a particular test form is form-specific or generalizable. Such information is important when clinicians must decide whether test scores obtained under various testing conditions are significantly different. For example, Thornton and Raffin (1978) developed tables of critical differences for percentage-correct scores on 10-, 25-, 50-, and 100-item monosyllabic word lists based on a binomial model for observed test scores. Their results, together with tables of confidence levels for score differences obtained with 25- and 50-item tests (Raffin & Thornton, 1980), have been presented in a form that maximizes their clinical applicability and hence contributes greatly to the integration of scientific knowledge in clinical practice.

Research on *retest reliability* over hours or days and *retest stability* over weeks, months, and years, provides a basis for inferring significant change in test scores over time. Examples from rehabilitative audiology are provided by a series of studies of retest reliability of the Hearing Handicap Inventory for the Elderly (HHIE; Newman & Weinstein, 1989; Weinstein, Spitzer, & Ventry, 1986) and the Hearing Handicap Inventory for Adults (HHIA; Newman, Weinstein, Jacobson, & Hug, 1991). Using a retest interval of 6 weeks and a face-to-face administration procedure, Weinstein et al. estimated that a change of more than 18% in the total score on the HHIE would be needed to infer a true change in handicap. Malinoff and Weinstein (1989) subsequently used this value to infer that 19 clients in a sample of $N = 25$, showed significant reduction in handicap after 3 weeks of hearing aid use. Although more precise estimates of reliability and standard error of measurement might be derived with larger, more representative clinical samples, the important point to note is that this psychometric research was conducted in a clinical setting and that it provides essential information for clinical interpretation of HHIE scores.

The most important type of psychometric research for practitioners is that which provides evidence of test *validity*, or, more properly, evidence that sup-

ports the validity of inferences drawn from test scores (*Standards for Educational and Psychological Testing,* 1985). Often, however, the relevant data are not presented as validation data per se. For example, many researchers have reported correlations between hearing handicap scales and audiometric measures. The correlations have been used both to support the theoretical prediction of a positive correlation between degree of hearing loss and degree of handicap, and also to demonstrate that hearing sensitivity, speech perception, and hearing handicap are not synonymous. Another type of data that is relevant to test validity is obtained when clients are followed for a period of time following an audiological intervention. Tests and measures obtained at the time of treatment can be evaluated for their prognostic value in relation to outcomes measured at a later time. Such information is germane to *construct validity* and *criterion-related validity,* but is often not labeled as such. It is important for researchers to identify and communicate the psychometric implications of their results and for practitioners to be alert to the psychometric relevance of all research that utilizes standardized tests.

Hayes, Nelson, and Jarrett (1987) have proposed that assessment procedures should be formally evaluated for their *treatment utility*. That is, it should be demonstrated that the assessment actually has an impact on treatment outcome. This might occur because the assessment provides information that is diagnostic and that dictates differential treatment, or it might occur because the assessment itself has therapeutic value.

Normative studies. Most tests used in audiological practice are designed to assess individual differences among clients. Yet very few tests have been standardized on large, heterogeneous clinical populations. Hence, *general norms* are lacking. Some speech recognition tests have been formally standardized (e.g., the Speech Perception in Noise test [Bilger, 1983, 1984; Bilger, Nuetzel, Rabinowitz, & Rzeczkowski, 1984; Kalikow, Stevens, & Elliott, 1977]), but instruments used by rehabilitative audiologists generally have not. Speechreading tests and self-assessment scales that quantify handicap have not been systematically administered to large samples of clients for the purpose of describing the distribution of scores in a general clinical population.

Although there is obvious clinical utility in normative data such as means, standard deviations, and percentiles for an entire target population, large normative samples also make it possible to develop *subgroup norms* which provide information about more homogeneous subgroups within a clinical population. For example, it would be possible to determine whether handicap scores vary systematically with gender, age, type of hearing loss, occupational status, and/or combinations of these factors, and if so, to develop separate norms for each group.

Perhaps the most feasible and useful type of norms for practitioners to develop on a given test or measure is *local norms,* that is, norms for a local clinical population. Through clinical use of an instrument over time, a database emerges that characterizes a particular clinical population. Aggregation of several such sets of norms across clinics can be used to determine whether the norms gener-

alize across clinical populations and, conversely, to evaluate the sensitivity of the test to known differences among the populations. For example, Erdman et al. (1990) and Montano and Malinoff (1990) presented norms for the scales of

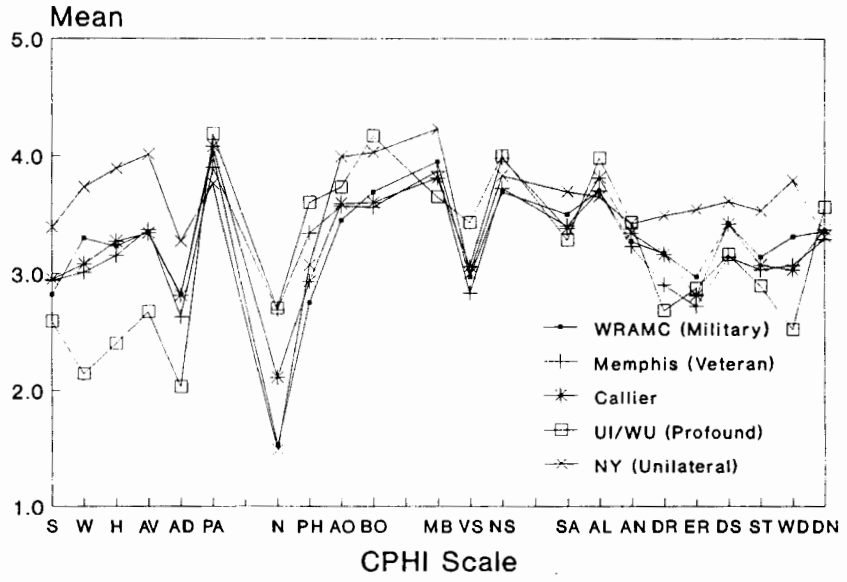

Figure 1. Mean scores on 22 scales of the Communication Profile for the Hearing Impaired (CPHI) for five clinical samples. WRAMC = 1,226 active-duty military servicemembers with predominantly noise-induced hearing loss from Walter Reed Army Medical Center; Memphis = 57 veterans from the VA Medical Center in Memphis, TN; Callier = 64 clients tested at the Callier Center for Communication Disorders, Dallas, TX; UI/WU = 29 cochlear implant candidates with profound sensorineural hearing loss from the University of Iowa Hospitals and Clinics, Iowa City, IA, and Washington University Medical Center, St. Louis, MO; NY = 25 clients with unilateral hearing loss from Manhattan Eye, Ear and Throat Hospital, New York. CPHI scale abbreviations: Communication Performance, S = Social, W = Work, H = Home, AV = Average Conditions, AD = Adverse Conditions; Communication Environment, N = Need, PH = Physical Characteristics, AO = Attitudes of Others, BO = Behaviors of Others; Communication Strategies, MB = Maladaptive Behaviors, VS = Verbal Strategies, NS = Nonverbal Strategies; Personal Adjustment, SA = Self-acceptance, AL = Acceptance of Loss, AN = Anger, DR = Displacement of Responsibility, ER = Exaggeration of Responsibility, DS = Discouragement, ST = Stress, WD = Withdrawal, DN = Denial. From presentations by S.A. Erdman, M.E. Demorest, D.J. Wark, C.R. Lansing, M.A. Henoch, L. Corn, and S.M. Binzer ("Factors Affecting Adjustment to Hearing Loss," 1990, *Asha, 32* [10], p. 171) and by J. Montano and R.L. Malinoff ("Effects of Unilateral Hearing Loss on Adults," 1990, *Asha, 32* [10], p. 182) at the Annual Convention of the American Speech-Language-Hearing Association, Seattle, WA, November, 1990. Adapted by permission.

the CPHI from five clinical populations (see Figure 1). Although mean scores on most of the scales were quite similar for the five samples, scores on the Communication Performance scales and Communication Need were quite different. Clients with unilateral hearing loss reported better communication performance and those with profound hearing loss reported worse communication performance than the other samples. Again, it is important to note that these data were accumulated during ongoing clinical practice and that they provide clinically useful information even in the absence of a formal standardization of the CPHI.

Single-Subject Methods

Intensive study of individual clients may be undertaken for purposes of treatment, research, or a combination of the two (see Table 1). It is the most idiographic approach to research in the clinical setting because it permits description of the interrelationships among variables within the individual and, in some cases, permits strong conclusions to be drawn about treatment efficacy for the individual.

Case studies. Detailed description of an individual client's history, presenting problems, and response to treatment intervention comprise the elements of a case study. Although case studies nearly always lack the kinds of procedural control that permit strong scientific conclusions, they are an important complement to experimental research. Case studies contribute to research when they suggest specific hypotheses about etiology and treatment, or when they provide a counterexample to some prevailing scientific principle (Kazdin, 1980).

Case studies can also be valuable when they provide an unusual opportunity to address a question that cannot be studied experimentally. For example, Hawkins (1982) documented a case of overamplification in a client with bilateral hearing loss. Audiometric test records from two clinics over a 10-year period clearly indicated threshold shifts of 30-40 dB in the aided ear and essentially stable thresholds in the non-aided ear. The scientific value of this case study was enhanced by the initial bilateral symmetry of the client's hearing loss, the availability of test data that ruled out other competing explanations for the hearing loss (e.g., conductive or retrocochlear problems), the large number of hearing evaluations conducted during the 10-year period, and the fact that the output of the hearing aid was set below the client's loudness discomfort levels. This unique combination of circumstances rendered the case important in demonstrating that overamplification can occur even at output levels generally thought to be safe.

Single-subject experiments. Single-subject experiments can be performed in a variety of ways. The key elements involve direct manipulation of one or more independent variables and repeated observations of the subject under different experimental conditions. A psychophysicist who systematically varies the parameters of a signal and measures a subject's thresholds is performing a single-subject experiment, although the terminology is not usually applied to such studies.

Most researchers who use the term "single-subject design" are referring to

the *time-series designs* associated with behavioral analysis. Excellent texts are available (e.g., Barlow et al., 1984; Barlow & Hersen, 1984; Kazdin, 1982), which explain the underlying rationale for evaluation of internal and external validity and which describe the elements from which complex designs are built. In an important series of tutorial papers, McReynolds and Thompson (1986), Kearns (1986), and Connell and Thompson (1986) provided an overview of these principles and emphasized the flexibility of single-subject designs for clinical research in communication disorders.

Single-subject experiments permit practitioners to demonstrate treatment effectiveness and to rule out other competing explanations for a client's improvement. They permit evaluation of treatment versus no-treatment, comparison of alternate treatments, and determination of the components of a treatment program that are most effective (Kearns, 1986). Because they are so intimately linked to clinical practice and its focus on the individual, they represent a medium through which research and practice can be integrated. As Connell and Thompson (1986) explain in detail, single-subject designs are extraordinarily flexible and do not have to pit scientific rigor against clinical concerns (cf. Siegel & Spradlin, 1985, cited earlier). Moreover the design can be modified within the course of a study, if necessary.

Although time-series experiments have not been used frequently in audiology, studies by Lesner, Lynn, and Brainard (1988) and Brainard and Lesner (1992) illustrate the viability of the approach for evaluating amplification systems. Lesner et al. used a reversal design (i.e., ABAB) to evaluate an FM assistive listening device in reverberant conditions with four normal-hearing subjects and found that all four subjects showed slightly better performance on a tracking task with the device. Brainard and Lesner compared performance on continuous discourse tracking in four subjects with hearing impairment who used each of two telephone amplification devices. They utilized an alternating treatments design in which both devices were evaluated over eight treatment sessions. Although there was some suggestion that one of the devices resulted in superior performance initially, across all sessions there was no evidence for the superiority of one device over the other for any subject. Thus a single comparison of devices made on one test day could easily have led to a conclusion different from what was obtained over time. This methodology is particularly appropriate for evaluating devices that may require some learning or adaptation on the part of the client. Interestingly, these changes over time are consistent with findings obtained by Walden, Schwartz, Williams, Holum-Hardegen, and Crowley (1983) in their test of the assumptions underlying comparative hearing aid evaluation.

Clinical case replication. When a treatment program is applied in clinical practice, it is unlikely that it will be equally successful with all clients. This is true regardless of whether evidence for the effectiveness of the treatment comes from clinical research based on groups of subjects or from single-subject research. Practitioners who elect to use a particular treatment therefore have the opportunity to observe, firsthand, its success with a heterogeneous clientele.

When such observations are made informally, hypotheses regarding which client or environmental factors influence treatment outcome simply become part of "clinical experience."

In contrast, Barlow et al. (1984) urge practitioners to be actively engaged in clinical replication, which they describe as follows:

> Clinical replication is a process wherein practitioners using a clearly defined set of procedures or "treatments" intervene with a series of cases that have a well-specified and measured problem. . . . In the course of this series, the practitioner observes and records successes and failures, analyzing, where possible, the reasons for these individual variations (or intersubject variability). This process . . . takes advantage of the strength of practitioners, specifically their observational skills, in the most important context of all: the treatment setting. (p. 58)

It is only through this type of intensive observation by practitioners that the variables which interact to modify treatment effectiveness can be identified. Once identified, however, they can in turn become the focus of formal clinical research investigations. Clinical replication represents a major step toward overcoming some of the limitations of group designs for clinical research.

Correlational Methods

In this and the following section we briefly consider those research methods most strongly associated with nomothetic research: correlational methods, which are often used to study the structure and lawfulness of individual differences, and comparative and experimental methods, which are used to describe natural and experimentally induced differences among groups. These two approaches are often criticized because their conclusions are derived from aggregations of individuals and are based on statistical abstractions such as means and coefficients of correlation. Integration of the knowledge they produce into clinical practice is not always obvious or straightforward, but they remain the prevalent methodologies for addressing clinical research questions.

Correlational research methods investigate covariation or association between variables. They determine whether there is a systematic (typically linear) relationship between the values of one variable and the values of another. Although one variable may be designated "independent" and another "dependent," in the absence of experimental manipulation of the independent variable and control of extraneous variables, no interpretation of cause and effect is justified.

Correlational models range from simple zero-order *correlation* between two variables, to *multiple correlation* between several independent variables and a dependent variable, to more complex multivariate techniques such as *factor analysis* and *canonical analysis*. Theoretical models that postulate causal relations among several constructs can be tested using such techniques as *path analysis* and *linear structural equation modeling*. The latter approach is especially powerful because it permits the researcher to include several measures of each construct and it provides for evaluation of psychometric assumptions con-

cerning measurement error.

The correlational models that have the most direct relevance to clinical practice are those based on simple and multiple correlation. Once it has been established that one or more independent variables are correlated with a particular dependent variable, then it is possible to develop a *regression equation* for predicting the values of the dependent variable. For example, in audiology there have been numerous studies of hearing aid benefit and its correlation with variables measured at the time of hearing aid fitting. Several approaches to defining benefit have been employed including hearing aid use, comparisons of aided and unaided performance on speech recognition tests, and self-reported satisfaction with the hearing aid. Each of these is a legitimate outcome measure against which a clinician might wish to evaluate the success of his or her practice. If it were shown that audiometric, demographic, and/or self-report measures obtained at the time the hearing aid was dispensed were correlated with one or more of these outcome measures, the clinician could predict, with a known margin of error, the amount of benefit a given client was likely to obtain.

For correlational research to have an impact on clinical practice, several conditions must be met. First, the measures used as predictors and as outcome indicators must be psychometrically sound. That is, they must be reliable and they must be appropriate operational definitions of the construct. Second, the correlational research must be based on a sufficiently large and representative clinical sample. Large samples are necessary because the correlations must be accurately estimated if the regression equation is to be valid. Sample sizes of *at least* 50 subjects, and preferably 100 or more, are needed for this condition to be met. Representative samples are important because correlations are sensitive to the range of individual differences in the sample and the degree of correlation in the target population may be under- or overestimated if the sample is not typical of the population. Third, the degree of correlation must be clinically, as well as statistically, significant. Large samples make it possible to detect correlations that are non-zero, but of no practical value. Finally, the results of the regression analysis must be presented in a manner that makes them readily applicable. The regression equation must be given, and, equally important, the *standard error of estimate* should be provided. This permits the clinician to estimate the margin of error in the prediction by placing a *confidence interval* around the predicted outcome.

Comparative and Experimental Methods

The utility of group designs in clinical research, particularly research on treatment effectiveness, has been challenged on a number of grounds. Siegel and Young (1987) have thoughtfully reviewed the criticisms of group designs, which essentially claim "that single-subject research is more practical and comes much closer than a group design to capturing the essence of therapy procedures" (p. 196). After considering five specific objections to group-based research, Siegel and Young concluded that it is not inherently less appropriate for clinical research

than single-subject designs, and that the two approaches "are, instead, competing strategies that are defended as appropriate for very much the same questions" (p. 198).

The logic of between-group comparisons derives from identification of an independent variable on which the groups differ. This variable may be manipulated by the researcher (e.g., Treatment A vs. Treatment B) or it may be a characteristic that is not under the researcher's control such as the subject's gender, age, degree of hearing loss, or type of hearing aid worn. If the groups are formed on the basis of a variable that is not under the researcher's control, this results in a *comparative design*. Manipulation of the independent variable, together with random assignment of the subjects to groups, results in an *experimental design*. In either case, observations are made on some dependent variable and the research question concerns whether or not the groups differ on that variable.

Comparative and experimental designs permit stronger conclusions than can be drawn when a single group is studied. Although it was noted above that descriptive methods such as surveys can contribute valuable information regarding treatment effectiveness, inclusion of a comparison group considerably strengthens the inferences that can be drawn. This point is nicely illustrated in a recent experiment by Kricos, Holmes, and Doyle (1992), who evaluated the effectiveness of a communication training program for elderly adults with hearing impairment. There was significant mean improvement for experimental subjects who received 4 weeks of training. Had this been the only group studied, it would be tempting to conclude that the training had an effect. However, equal mean improvement was made by control subjects who received no training. Despite the fact that description of a client sample before and after intervention provides important information (particularly if there is *no* change), comparison of treated and untreated groups (or groups receiving different treatments) provides a more rigorous logical basis for conclusions regarding treatment effectiveness.

One common criticism of group designs in clinical research is that conclusions are based on statistical analysis of group means and that statistical significance does not guarantee clinical or practical significance (Siegel & Young, 1987). Although this is a true statement, it is gratuitous because there is no reason why information about effect magnitude cannot be directly estimated in a group design. Group means, which are routinely reported, can, and should, be interpreted with regard to their practical significance. Moreover, if one is concerned about the magnitude of effects in relation to the magnitude of individual differences, there are numerous techniques for estimating the percentages of variance accounted for by treatment effects and individual differences. Statistics such as the intraclass correlation and ω^2 (Winer, 1971) or standardized mean differences (Cohen, 1969) should be routinely reported in comparative and experimental studies, particularly those that claim effects with clinical applicability.

Perhaps the most persistent and important complaint about group designs is

that they ignore individual differences which are readily apparent from the variability among subjects within groups. The conclusion that a treatment is effective pertains to the group mean and, as was noted in the introduction, does not imply that every individual within a group responded in the same manner. Having acknowledged that point, what are the implications for the relevance of group mean differences and how can individual differences be accommodated in group designs?

If the magnitude of a treatment effect is very small, relative to individual differences in treatment outcome, little useful knowledge is gained. In the language of experimental design and analysis, individual differences in the effects of an independent variable imply a statistical interaction. Cronbach (1957, 1975), speaking from the perspective of educational psychology, referred to this as "aptitude × treatment interaction." In the context of clinical research, it is usually termed "client × treatment interaction" (Barlow et al., 1984).

Cronbach (1957, 1975) and others have argued forcefully such interactions must be studied in their own right. One way to accomplish this is to identify potentially important characteristics of individuals that may interact with treatments. Group designs can then be expanded to include subgroups defined on the basis of these additional variables. For example, benefit from training in communication strategies might be different for individuals with bilateral as opposed to unilateral hearing impairments. A group design could evaluate this hypothesis by examining two independent variables (treatment and symmetry of hearing loss) and their interaction.

In order to meet the criticism that group designs do not address individual differences, it is necessary for research designs to be expanded so as to incorporate important client characteristics. It is precisely when the researcher wishes to know which characteristics to explore that the clinician can provide insights gleaned from experience and thereby guide the formulation of clinically relevant research questions. Once again, it is clear that bidirectional transfer of knowledge between researchers and clinicians (Hoshmand & Polkinghorne, 1992) is not only possible, but is highly desirable.

CHALLENGES TO IMPLEMENTATION OF THE RESEARCH AGENDA

Beyond identifying specific research needs and developing relevant research strategies a more critical challenge exists. Specifically, how can prevailing attitudes towards research be modified? Because traditional scientific methods are not always applicable in the clinical arena, and because the clinical relevance of research findings is not always immediately apparent, those with clinical interests all too often view research with skepticism, or even disdain. Ironically, clinicians are in an optimum position to identify areas in need of research and to gather clinical data. Nonetheless, because of preconceived notions about research, many have an aversion to doing so. Considerable attention is now being given to ways in which this trend can be reversed through research men-

torship. Emphasis is being placed on horizontal models of mentoring, collaborative research through interdisciplinary mentoring, and practitioner mentoring (Bates, 1993; Boysen, 1993; Carney, 1993b; Dubno, 1993; Krantz, 1993).

The need for new and innovative models of research mentoring is also driven by the need for increased clinical research, which as Jerger (1993) has pointed out, must, of course, be conducted in the clinic. Hence, at a minimum, clinicians have a professional responsibility to make known clinical issues in need of research and to ensure that adequate investigations ensue. Ideally, clinicians will participate in those research efforts. Throughout this monograph clinicians and researchers discuss the research needs facing rehabilitative audiology. Even the most clinically oriented among us can identify with the issues and questions raised by the authors. This collaborative effort has permitted clinical questions that warrant systematic investigation to be identified, and research findings and research methods that are relevant and applicable in the clinical arena to be discussed. In our view, ongoing collaboration of this nature in academic, clinical, and research settings will enable rehabilitative audiologists to meet the research challenges facing them.

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