these scores will be negligible for young people and large for elderly people, it would seem that the difference score might better estimate the age component of HH.

Kryter's proposal changes the current 5:1 weighting in favor of the better ear to 3:1, and suggests that this is supported by data discussed in "reviews of related research." In fact, there are no peer-reviewed published data that support a 3:1 better ear weighting. One of the "reviews" he cites (King et al, 1992) never mentioned 3:1 weighting and ultimately recommended a variable weighting (when one ear is normal, and the other is deaf, their weighting is approximately 5:1). The other, his own book (Kryter, 1994; chapter 7), actually recommends a 2:1 weighting based on two papers reviewed: one by Kryter and Archer listed as "in preparation" (a MEDLINE search showed no such publication to date) and another by Harris and Myers (1974), which is an unpublished US Navy report, apparently relating to the influence of simulated unilateral loss on speech reception in extremely poor speech-to-noise conditions. As previously noted, performance deficits seen under the most adverse and unusual listening conditions cannot be considered representative of "everyday" communication. Such data could be given some weight, appropriate to the frequency and importance of the conditions they represent, but should not be the sole basis for determining the characteristics of a method for HH estimation, such as low fence or the better ear:poorer ear ratio.

Robert A. Dobie
The University of Texas Health Science Center at San Antonio
San Antonio, TX

REFERENCES


Reply to Dobie
I appreciate the opportunity to reply to the comments of Dobie regarding my paper "Evaluation of Hearing Handicap" (JAAA 9:141–146, 1998). I will attempt to respond in the approximate order in which his comments were given. All bibliographic references are as given in my paper and Dobie's comments.

Dobie properly criticizes the statement in my abstract that the 1979 AAO method "underestimates measured hearing handicap by . . . 17 dB to 35 dB," without citing in the text where those numbers come from. The numbers represent the general magnitude and trend of the difference between the measured versus AAO predicted percentage hearing handicap as a function of threshold losses, as shown in Figure 3 of the paper.

Albeit, the Nett et al "paper" is an unpublished progress report; it deserves weight, of course, only if it is consistent with findings, as it is, from published research.

Ward (1973) suggested that the data of Kell et al (which I had discussed in an earlier paper) were "simply irrelevant to the issue [assessment of speech hearing handicap] at hand. . . ." However, Ward did not—as also, apparently,
Dobie does not—recognize the point made by the Kell et al data, which, as noted by Kryter (1973, p. 1249), indicate some invalidity of the AAOO estimation procedures. Kell et al found that 64 percent to 80 percent of a sample of industrial weavers had difficulties with speech communication in many everyday situations. Kell et al also found that the speech level had to be 5 to 20 dB greater for the weavers than for same-aged control subjects to understand 50 percent of the words in a speech test. These findings are hardly indicative, in my opinion, of a “slight hearing handicap with difficulty only with faint speech,” as predicted by AAOO procedures.

Lutman and Robinson's (1992) discussion of methods for rating percent hearing for sounds (deafness) does not constitute, as implied by Dobie, an “acknowledgment” that inappropriate research methods were used in the speech hearing handicap studies of Robinson et al (1984) and Lutman et al (1987). Dobie confuses studies of assessments of hearing handicap for everyday speech (Robinson et al and Lutman et al) and assessments of disability for hearing sounds (Lutman and Robinson).

Dobie offers some remarks concerning Figures 1 and 2 that are somewhat misleading and gratuitous. For example, his statement that reported speech-to-noise ratios (SNRs) in the Pearsons et al study would have been 3 to 4 dB higher had they measured speech “peaks,” as done in the Suter experiment. Although what is meant by speech peaks is different for these two studies, the SNRs from both studies are actually of numerically comparable magnitudes. The magnitudes of SNRs are to be taken as scaled indexes, with differing meanings, depending on the methods used for measuring the speech and noise signals (see discussion in Kryter, 1994, Chapter 6).

I am mystified by Dobie's comment that “Even Kryter eventually backs away from the curves in Figure 3... No data are offered in support of the new 15-dB low fence; it appears to be simply arbitrary.” Also, I am somewhat dismayed by Dobie's contention that “Ultimately, common sense and clinical experience compel us to reject the notion that people who have 0 dB HL have a 15 percent HH.”

A reasonable, it is suggested, rationale for a low fence is that it is largely an indicator of, and adjustment for, the effects of real-life noise masking of everyday speech. The proposed low fence of 15 dB HL (average of 0.5, 1, 2, 3 kHz) rests upon the functions in Figure 3 that show about 15 percent speech misunderstanding and perceived handicap in real-life listening conditions by people with 0 dB, or better, hearing level. Accordingly, the range of 60 dB between the 15-dB low and 75-dB upper hearing level fences map 0 to 100 percent hearing handicap at the rate of 1.67 percent per decibel hearing level above the lower fence. (The linearity of this function follows the AAOO and AAO-ACO simplifying convention on this matter.)

Perhaps, it is conjectured, the low fence of 25 dB, which was recommended by AAO-ACO, arose from the fact that speech listened to in the relative quiet of an otologist's office or clinic would usually be perfectly intelligible to patients with HLI of up to 25 dB or so, as shown in Figure 2.

Dobie's endorsement of “common sense and clinical experience,” and apparent eschewal of research findings, appears also to be the general approach involved in the preparation of the subject AAOO and AAO-ACO documents. To the best of my knowledge, no discussion of, or references to, published or unpublished research data relevant to the bases or validation of the procedures therein promulgated has ever been published.

The proposed “adjustment for normal presbycusis” is faulted by Dobie as being “unfair.” I stated (p. 144) that the proposed procedures can be used to estimate hearing handicap due to overall losses in hearing sensitivity, including presbycusis. As a matter of possible scientific and practical interest, not “fairness,” an additional, optional, procedural step is proposed to permit estimation of percent hearing handicap due solely to a nonage-related cause, such as exposure to noise.

Dobie's comments on the question of “better ear vs poorer ear” weightings for binaural listening do not, I believe, represent a duly considered evaluation of available research evidence on that matter.

Although Dobie's comments are welcomed, they do not persuade me to make any substantive changes in my paper, or the hearing handicap assessment procedures proposed therein. However, for explicitness, the last words of the paper, starting with “-1 to 17 dB,” should be changed to read: “-1 to 6 dB in hearing levels, with adjustments for presbycusis, ages 25 versus 60 years, and 10 to 17 dB without adjustments for presbycusis.”

Karl D. Kryter
Department of Communicative Disorders
College Health and Human Services
San Diego State University
San Diego, CA