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Letters to the Editor

The limited accuracy of bone conduction audiometry
(*Journal of Laryngology and Otology,* 1991, **105,** 518–521) (July)

Dear Sir,
A distinction has to be made between diagnosis of ear disease and the measurement of hearing impairment. The diagnosis of middle ear disorder does not rest on the demonstration of an air-bone threshold difference alone, but is the sum of the clinical evidence. It would be absurd to claim that someone with sub-total perforation of the eardrum did not have a conductive defect because his air-bone gap failed to reach some arbitrary figure. All that should be said is that he has a conductive abnormality, but the air-bone gap is small.

In border-line cases a statistical test may be needed to decide whether an air-bone gap is to be accepted as a random occurrence or as evidence of a disorder, but the air-bone difference is the only measure of conductive function and to arbitrarily reduce it when the results turn out to be inconvenient is to introduce a bias which underestimates the middle ear component. The problem that measurements are unreliable remains, but it is one which applies to audiometry generally and by no means only to bone conduction measurement.

The 'true' value of a measured entity is the target at which management is aimed. There is an important difference between measurements in the psychological domain and those in the physical sciences. It may not be assumed that the true threshold of hearing is the same for all otologically normal persons. True threshold is generally not zero even when hearing is normal; true air-bone difference is generally not zero even if the conductive apparatus is without defect. The existence of a modest air-bone gap in the absence of pathology should cause no embarrassment. It is a perfectly acceptable outcome of even the most refined audiometric measurement.

'Measured', as distinct from 'true' thresholds inevitably have systematic and random uncertainties associated with them. The latter, however, have nothing to do with the dispersion of thresholds within that population which gave rise to audiometric zero and it is not correct to assert that a particular confidence limit based on this dispersion represents uncertainty in the air-bone gap. It is meaningless to compound this confidence limit arithmetically with an (arbitrary) estimate of uncertainty in the measured values of air-bone threshold difference.

The point at which uncertainties in the audiometric expression of impairment should be taken into account is in its conversion to a numerical expression of disability.

Yours faithfully,
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Reply

Dear Sir,
We are grateful to Mr Williams and Dr Haughton for ventilating their thoughts on the interpretation of bone-conduction thresholds, which provides us with an opportunity to clarify our standpoint.

Our paper clearly states that its concern with audiometric measurement of bone conduction is exclusively in the context of disability assessment and not in any way with the use (or misuse) of such measurements in diagnosis.

We have sought in this particular matter, as well as in other aspects of quantitative disability assessment, to discourage reliance on unquantifiable opinion and to make maximum use of objective principles and facts. At the same time, it has to be remembered that the situation in compensation cases is not neutral. It is incumbent upon the defendant to prove, if he can, that the plaintiff's claim in unsubstantiated. In the example they cite, the pathology (drum perforation) is overt and the existence of a lesion is indeed beyond doubt. But that is not the same thing as saying that there is a conductive hearing loss which is numerically equal to the observed air-bone gap (ABG). The same would apply, *a fortiori,* if one observed the same small ABG in a case where there is no overt pathology. In that case the measurement (lying well within the limits of normal hearing) might or might not betoken some hidden pathology: it certainly could not be said to indicate hidden pathology 'on the balance of probabilities'. If one were to take the equivalence of ABG and conductive loss to its logical conclusion, we should reach the *reductio ad absurdum* that, in the same way that a positive ABG would entail abatement of damages, a negative value should automatically attract an enhanced award! The truth of the matter is that the compensable loss which everyone is aiming to quantify is the *true cochlear loss,* but in practice we have to infer a value from the difference between the AC threshold and the ABG, and it is well known that this difference is influenced in an uncontrolled manner by several extraneous factors.

Of course, if one had before and after measures of the ABG, these extraneous factors would cancel out, and the shift in ABG would be a reliable indicator of the conductive loss. Since this is not available in practice, the only escape from the dilemma of enhancement versus abatement is to impose a dead band. The matter then reduces to this. If the ABG falls within the dead band, one bows to the inevitable that no conclusion can be drawn. If, however, the ABG is sufficiently large, one prefers the hypothesis that the measurement has disclosed a real conductive loss to the alternative hypothesis that the result is simply a normal (no conductive loss) case at the extreme of the distribution. It is, admittedly, a matter of judgement where the limit of the dead band should be set.

In connection with negative ABGs, it should be recalled that the commonly adopted rules for masking in BC tests lead to a bias towards positive ABGs. For the more accurate measurement of BC thresholds required for medicolegal purposes, masking should always be used, thereby achieving consistency with the test conditions in the experimental studies on which national and international calibration standards are based. Then as many negative ABGs as positive ones will appear in sensorineural cases: indeed, there would be something wrong with the calibration standards if they did not.

Turning to the statistical argument, we would go along with our critics' statement that 'it is not correct to assert that a particular confidence limit based on this dispersion [the inter-subject dispersion of ABG values] represents uncertainty in the air-bone gap'. We should more correctly have written about confidence limits on the deviation of a measured ABG from the (normal) population mean. From this point of view, the position that an individual occupies in the distribution of all individuals is indistinguishable from a random variate. With regard to the extra allowance for random errors of measurement, it is a fine point whether this should be added in a linear as opposed to a root-mean-square manner. We might well have set the dead-band limit at three standard deviations and ignored the possible measurement error, and still arrived at the same value of 15 dB. The fact is, as already stated, that the particular number selected is a matter of judgement related to the 'balance of probabilities' between the two hypotheses above. In practice, we do not feel that there is much leeway either side of 15 dB if one is to avoid, on the one hand, an excessive burden of proof and, on the other hand, falling into the trap of the *reductio ad absurdum:* 20 dB is, surely, evidence enough to justify the assumption that a real conductive loss is present, whilst 10 dB is still well within the normal inter-subject range and defies any firm conclusion.

Finally, our critics point out that uncertainties in the audiometric expression of impairment should be handled in the domain of disability. We agree, and that principle applies not only to the uncertainty attaching to the true cochlear impairment, but equally to the air-conduction HTLs. However, as a practical matter, it turns out to be very difficult to write an explicit formula for the percent disability equivalent to so many decibels of uncertainty in ABG. This is not a difficulty of principle, but arises simply because the formula entails two cumbersome non-linear transformations: ABG to conductive threshold shift (Carhart's effect) and hearing threshold level to percent disability, respectively. The algebraic complication can, of course, quite easily be outflanked in any given case, by evaluating the disability percentage for two values of ABG separated by their estimated decibel uncertainty. For example, one could insert the measured ABG and the measured value minus 15 dB and see how the difference between the resulting estimates of disability comes out. In effect, this is close to our own way of dealing with the problem, except that we recommend a built-in margin and taking the single output value as the most appropriate measure.

All the complications of BC audiometry would evaporate if only someone would invent and perfect a way of directly measuring the acoustical input to the cochlea.

Yours faithfully,

R. R. A. Coles, M. E. Lutman and D. W. Robinson

The feeding pharyngostomy

Dear Sir,

We read with keen interest the article 'The feeding pharyngostomy: an alternative approach to enteral feeding' by D. G. John and C. P. Fielder (*Journal of Laryngology and Otology,* 1991; **105,** 451–453).

During the last five years we have done over 150 pharyngostomies to feed patients with upper aerodigestive tract cancer in the post-operative period and in advanced stages of the disease. Our technique, though basically similar to the one described, has some differences:

1. In some patients with advanced cancer, it is not possible to pass a feeding tube down, due to obstruction by the tumour. So we first attempt to pass the tube down into the stomach through the mouth, and only if it succeeds do we make an incision in the neck to bring out the proximal end of the tube through the wound in the neck. This avoids unnecessary trauma to the neck of patients in whom a feeding tube cannot be passed down the oesophagus into the stomach. An alternate method of feeding is employed in such cases.
2. The feeding tube is brought out of the skin at the anterior border of the sternomastoid muscle near the angle of the mandible. The proximal end of the tube is caught between the jaws of a Mixter dissecting forceps which is directed laterally and superiorly behind the posterior tonsillar pillar. An incision is made at the point where the forceps tents up the skin, and the proximal end of the tube is brought outside. As the track through which the tube traverses the neck is inclined from the pharyngeal mucosa to the skin, dependent drainage does not occur through the track. So the chance of a persistent cervical fistula after removal of the tube is very low. In fact we have not had even one case of persistent leak from the neck after removal of the tube, till now.

Yours faithfully,

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Reply

Dear Sir,

I was most interested in the comments of Profs. Sebastian and Cherian concerning their large series of pharyngostomies.

I would agree with their first point, that in patients where oesophageal or post-cricoid obstruction is a possibility a nasogastric tube must be passed first. Since writing the paper I have performed two pharyngostomies for neuromuscular disease, under local anaesthesia and in these cases it is also absolutely necessary to pass the tube before making any external incision.

In their second point it is suggested that the tube is