

LETTERS TO THE EDITOR

TO THE EDITOR,

The Journal of Laryngology and Otology.

DEAR SIR,—May I be permitted to attempt to answer some of the points raised by Dr. Dan McKenzie in his review of the recent publication of the Medical Research Council entitled “ Psychological Experiments on the Effects of Noise.” I will confine my attention to his criticisms of that portion of the work for which this department was responsible.

I should like to make it clear that I do not write this letter because I object to the way in which Dr. McKenzie makes his criticisms, save at one point. It is a pity that his dislike for our work should be made the medium of an attack upon the Medical Research Council. To have done this is like condemning a whole cargo of fruit, or even the complex operations of an entire shipping company, because one small orange is alleged to be rotten. This apart, I fully recognise that he has every right to his views, and though I believe that they can all be shown to be seriously mistaken, they are at least expressed with refreshing candour and clarity.

Dr. McKenzie urges that our experiments were badly controlled because: (1) the observers were not given preliminary auditory tests; (2) the degree of noise in decibels is not given; (3) a silent chamber was not used for control experiments; (4) a large variety of noises were employed.

(1) Actually, practically all the observers whom we used had already been submitted to all the usual auditory tests that form part of a laboratory course on the special senses. These include tests of auditory acuity, of bone conduction, of differential response to frequency variations, and of reaction to auditory rhythms. The auditory capabilities and characteristics of the six observers who gave us the bulk of our results were very fully known as a result of earlier experiments, many of which, Dr. McKenzie will be relieved to know, were conducted in a properly constructed sound-proof chamber. It is thus about as certain as possible that all our observers were well within the limits of normal variations as regards these characteristics. More than that was not necessary, since the experiments were merely designed to bring out the general trend of the effects of noise upon performance in a relatively unselected group of the kind of population with whom we were dealing. That there may be abnormalities of auditory reaction which yield effects that will not fit well into our picture we recognised and specifically stated. We stated also that these lay outside our survey. Since, after all, the work was done in a laboratory which is largely

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concerned with special sense experiments, it seemed reasonable to assume that this statement would be interpreted as it was meant to be by any ordinary reader. We did, in fact, take care to exclude from the experiments individual significant pathological disturbances.

(2) The decibel convention concerns the variation of noise, and of tone, in one respect only: that of their physical intensity. Rightly or wrongly our primary interest was not in the effects of variation in this respect alone. We were interested in the gross effects of irrelevant, or unwanted, auditory stimulation upon certain specific tasks. The attempt to correlate decibel scales directly with scales of annoyance or of distraction is at present a highly controversial matter. In any case, the main significance of the decibel notation in relation to the apparent loudness of sounds lies in its general connection with the Weber-Fechner law. And obviously most of the stimuli that we were using lay well outside the range of application of that law. We considered the question and decided to sacrifice a certain, probably illusory, appearance of definiteness in order to avoid any suggestion that a specific variation of auditory intensity can, because of the intensity factor alone, be forthwith related to disturbance, distraction, annoyance, or any other such subjective factors resulting in spoiled work.

(3) That a sound-proof chamber was not used for controls was no accident, but due to a definite decision on our part; and I still think we were right. Absolute silence is an even more abnormal condition for work than loud noise. To shut up a man in silence is not merely to remove noise, but to establish new positive conditions of reaction. We could perfectly easily have used such a room, for we have a particularly efficient one ready to hand. Few people can have spent more hours conducting auditory experiments in such a room than I and my students have done. We did not use it because we knew by often repeated experience that if we had done so we should have started new and disturbing processes of physiological and psychological adaptation. The controls needed were rather those of the relatively quiet laboratory conditions to which our observers were already accustomed. Such controls were employed, as we stated, in every single set of experiments we carried out.

(4) Certainly a variety of sounds were used, but Dr. McKenzie fails to note that in each case the sounds were definitely chosen and presented to investigate specific points, and that they were controlled and given in exactly the same way to the various observers throughout any given group of experiments. The noisy workshop was used *only* in the early stages, when we were trying to find out what technique would be most likely to yield fairly definite results. We reverted to the laboratory precisely because all the sounds

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produced there could be more regularly repeated and controlled. The clicks were used to investigate the effect of synchronous and non-synchronous sound upon regularly recurrent movements. The imitation of the noisy workshop was employed when we were trying to see if the indication that noise may have different effects upon mental and motor performance could be confirmed. The gramophone records were used both because they produce sounds that can be exactly repeated and controlled, and because, having already dealt with a variety of relatively "meaningless" stimuli, we wished to see whether a more significant type of stimulation would affect performance in a markedly different manner. That one record concerned an attractive topic was due to no "confusion of thought", but was meant to illustrate how the effects of auditory stimuli upon work may vary, not only with their physical characters, but also and dominantly, with their "significance". Absolutely the only respect in which the results of these varieties of stimuli are compared and treated "in the lump" is that they all had a less detrimental effect upon performance than had seemed likely.

Dr. McKenzie disapproves of the "occupations or exercises" performed by our observers. So do we, in the sense that we agree that better ones might be devised. But it is not easy. Any such occupations must satisfy at least three criteria: (a) they must be interesting and yet not absorbing; (b) they must have a learning curve whose normal characteristics are well known and thoroughly established, and (c) they must be such that any performance can be exactly scored. It is odd that the only one of our "occupations" that Dr. McKenzie approves is the one which least satisfies these necessary conditions. It was this that made us uncertain about its results, and not the neglect of "quiet" controls; for as I have said, such controls were always employed.

Dr. McKenzie scores a fair debating point when he objects to our phraseology in one of the sections of the Report dealing with the "suggestion" effects of conventional views about noise. Yet, unless it is wrested wholly from its context, it is pretty obvious that the expression which he thinks would outrage the eminent Shade of T. H. Huxley is meant to be no more than the affirmation of a strong opinion. Indeed if we should have benefited—as perhaps we might—by calling in a medically trained expert, here is a case in which Dr. McKenzie could certainly have avoided unnecessary confusion by consulting a psychologist. For no psychologist would ever suppose that "suggestion" can create something out of nothing. The Christian Scientist is supposed to say: "There is no pain; there are no bad effects; there is only suggestion." Whether he does say that is not our concern, but all that we say is: "There are bad effects, but, owing to widely current views, the sufferers connect some of these with wrong causes." I submit that it takes a good

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deal of twisting to make these two views resemble each other, and that neither we nor the Medical Research Council can be charged with promulgating opinions at all closely akin to those attributed to Christian Science.

Never once in this Report have we said that the bad—or the good—effects of noise are due to suggestion. We have said only that it is probable that some bad effects are, by force of suggestion, wrongly attributed to the direct operation of noise. But since our work was completed a further series of experiments has been carried out here. These were in charge of another investigator, from another laboratory in another country, who is fully trained both in the technique of this kind of experiment and in the physiology of the special senses. He was familiar with our results and, like Dr. McKenzie, doubted their validity. His observers were different from ours, though of the same social class as ours. Their auditory reactions were all first fully tested, in our case, and there were no marked deviations from the normal. They were required to carry out a fairly difficult mental task with a controlled and persistent noisy background. After several months of work he found his results thoroughly consonant with ours. He also found that, by a careful arrangement of experimental instructions, he could significantly alter the disturbing effects of noise. By suggestion some of them came, by suggestion some of them disappeared. Apparently we might have been more emphatic about this matter than we were.

Dr. McKenzie has obviously read our Report. I am therefore wholly unable to understand how he can charge us with neglecting the facts that the threshold of disturbance in the case of sound varies from person to person, and with “ the degree of preoccupation of the higher cerebral centres ”. Both of these facts—particularly the second—were pointed out by us several times in the Report, demonstrated, and discussed. Also nowhere do we deny that sound stimuli can become “ so insistent that . . . they engross the entire consciousness.” But to investigate this case, as a persistent phenomenon, was clearly outside the limits of the purpose of our study.

Nor were we unmindful of the necessity for definition, as the review indicates. Throughout, as we stated, we took noise to mean “ excess of irrelevant or unwanted auditory stimulation ”. In every set of experiments our conditions conformed to this definition. Certainly we could have defined noise differently. We could, for example, have taken noise to mean that degree of character of auditory stimulation which is definitely detrimental to the carrying out of coincident mental or muscular work. If we had done so our problems would have been different, and we should have begged many questions that we wished to investigate.

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Our Report is not so innocent of conclusions as the review suggests. These conclusions are set forth in detail and can be studied by anyone who cares to consult the Report. I must repeat that we have not said that noise, particularly loud discontinuous noise, does not prejudice efficiency. We have stated and demonstrated the exact opposite. We have also stated and demonstrated that the direct effects of noise upon work are often exaggerated. We have stated and demonstrated how rapidly some of these effects can be countered by physiological and psychological processes of adaptation.

My impression is that Dr. McKenzie thinks that experimental work on the special senses ought to be carried out by, or under the direction of, the medically trained specialist alone. The assumption is apparently that only the medically trained specialist can possibly have the necessary knowledge of the physiology of the special senses. The history of scientific investigation in this field, a history in which this Department has taken some share, does not bear out such an opinion. I must protest that, whatever the value of this particular Report may be, the Medical Research Council are not to be blamed because it did not occur to them to adopt so extraordinary a view.

Yours faithfully,

Cambridge.

F. C. BARTLETT.

TO THE EDITOR,

The Journal of Laryngology and Otology.

SIR,—The critical review of the Report of the Hearing Tests Committee scarcely presents the facts, which will justify what its author, Mr. C. S. Hallpike, calls a communication of destructive tone, for his claim that the Committee have omitted all references to the facts in Wegel's Chart is not supported, when we find the line of minimal intensity on this chart described on page 15, XXI, ii., paragraph 2, of the Report, and a warning issued against the use of notes of great intensity in the "effects of pitch on auditory fatigue", page 20.

Nor will his readers be stimulated by the suggestion that we should record the air conduction on a graph, and bone conduction separately, on the grounds that we know little more of the path of bone conducted sound than in the time of von Bezold. Nor again will they follow his attack on the Committee when he says that the Committee confuses the definition of deafness in a surprising way and quotes as evidence, between inverted commas, a sentence which is nowhere to be found in the Report. It is clear that it is not the Committee, but Dr. Hallpike who is creating the confusion.

The issue raised by Dr. Hallpike's paper is whether otologists should continue to record the number of seconds for which they hear a fork longer than a patient, or whether they should convert