THE SCHOTT TREATMENT OF HEART DISEASE.

SIR,—No one has read with greater interest than I the correspondence which has been evoked by Dr. Saundby's important paper on the Schott treatment of heart disease, and I feel sure that many of your readers share with me a sense of obligation to Dr. Poore for the mathematical formula which he offers for the approximative estimation of the actual shrinking of a dilated heart which is represented by the remarkable and rapidly induced diminutions of areas of dulness which have been attested by so many observers that their reality has been placed beyond the range of reasonable question. That such shrinkage may be actually less than the diminished area of dulness would at first sight suggest Dr. Richard Greene's acute anatomical reasoning makes sufficiently clear; but, as he well remarks, clinical facts far outweigh both explanations and objections. Indeed, the restoration to health, in the course of a few weeks, of sufferers who have been treated for months or years with little or no success by the methods which have hitherto held the first place among therapeutic resources is one of those facts which is too stubborn to admit of cavil, however wide the field which it may offer for discussion and explanation.

It must, however, be said that Dr. Poore's reasoning on the subject of lung expansion seems to be based on a misconception of the views which have been advanced by Dr. Schott and his followers. Unless I misunderstand Dr. Poore, he is under the impression that it is contended that "the lungs undergo no extra expansion." Such is not the case. What is contended is that the migration of the apex beat towards the mesial line is not the result of an overlapping of the pulmonary margin induced by more complete inflation of the lung.

The argument which I adduced in a communication to the BRITISH MEDICAL JOURNAL in March last appears to me to be conclusive on that point; for the interposition of an air cushion of inflated lung tissue, however thin, between the apex and the chest, would of necessity diminish the force of the blow communicated to the thoracic wall, whereas the opposite is the case. If the apex beat be modified, as it often is, it increases in force. Moreover, it appears to me that the argument deduced from the greater weight which would be incumbent on the lower lobes in consequence of the inclination of the body of the bather is not consistent with the results of clinical observation. The apex beat is never vertically depressed as the result of the baths or the exercises, but it may be frequently noted to rise obliquely in the direction of the sternum.

The inference, therefore, is that the lungs, by inflation, advance passively to occupy the vacuum which would otherwise be created by the shrinking heart. As a matter of fact, it is not uncommon for the patient to volunteer the remark that he can breathe more deeply, and in such cases the respirations are found to have diminished in frequency. On the other hand, Dr. Schott would be the last to contend that deeper inspirations do not, in their turn, assist a burdened heart to expel the contents of its distended chambers.—I am, etc., W. BEZLY THORNE.

Upper Brook Street, Dec. 2nd.

CHITRAL RELIEF FORCE.

SIR,—In the BRITISH MEDICAL JOURNAL of October 19th a letter appears under the above heading, signed by Staff-Surgeon Kirker, R.N., in which he refers to an article of mine published on August 10th, entitled "Waterborne Typhoid." In that contribution I tried to draw attention to the common fly as a probable carrier and disseminator of morbific products.

Your comments at the end of Staff-Surgeon Kirker's letter are I think apt to be misunderstood, hence I take this earliest opportunity of asking you to kindly elucidate them. You state that "Dr. Battersby's observations are of course theoretical, and based on the hypothesis of *omne ignotum*;" I do not think this expression is quite correct, as I am as much a believer in the waterborne theory of enteric fever and cholera (under certain circumstances) as you are, but as the result of practical experience and personal observation, I am trongly impressed with the belief that in certain outbreaks ther primary causes have to be looked for. With reference to the common fly, my theory is based upon the argument by analogy. Having witnessed the marvellous manner in which the cholera bacillus is capable of multiplying on agar jelly within a few hours of inoculation, I say it is probable that the typhoid organism may do likewise when implanted by flies on suitable pabulum. Since my letter of August 10th, I have read an account in which some of our Continental friends have actually demonstrated the existence of typhoid bacilli in the dejecta of flies previously fed on typhoid excreta. So after all my observation may not be so theoretical, nor based upon the hypothesis of "*omne ignotum*."—I am, etc., J. BATTERSBY, M.B., D.P.H., Surgeon-Major, A.M.S.

Rawal Pindi, Punjab, Nov. 11th.

THE SECRET OF CENTENARIANISM.

SIR,—In your criticism of Sir B. W. Richardson's recommendation of total abstinence as one of the conditions for longevity, you state that "a strictly temperate use of alcohol tends to prolong life," and you cite Sir George Humphry's statistics of centenarians and octogenarians as showing that abstainers have "only a slight advantage in point of longevity over the non-abstainers." I wish to point out that these statistics are overwhelmingly in support of the advantage of total abstinence. Thus, of 45 centenarians, 12 were abstainers and 30 were moderate drinkers. These numbers must not be compared together, but with the number of the class from which each was taken. The number of abstainers 100 years ago must have been very small, probably not 1 per cent. of the population, yet these furnish more than 25 per cent. of the centenarians. The same reasoning applies to the 689 persons between 80 and 100. The abstainers supplied 12 per cent., while it is noted that a considerable percentage besides took very little and not regularly. It is perfectly clear that if the whole population had been abstainers for the last 100 years, the number of centenarians would have been enormously greater than it is, and with the spread of total abstinence we may reckon on an increase in their number and of the average age at death. This can only operate gradually, but every one who abstains from intoxicating liquors is surely helping to increase his own longevity and that of his posterity who walk in his steps. I need hardly say that the statistics of life insurance societies entirely support Sir Benjamin's dictum.—I am, etc.,

J. JAMES RIDGE,
Honorary Secretary British Medical Temperance Association.

SIR,—In the annotation on "The Secret of Centenarianism" in the BRITISH MEDICAL JOURNAL of November 30th, I read: "The most trustworthy statistics on this subject are those of Sir George Humphry. Of 45 cases of centenarians collected by him only 12 were total abstainers, while 30 were moderate drinkers, and 3 were heavy drinkers," going on to say, "the abstainers would appear from these figures to have only a slight advantage in point of longevity over the non-abstainers."

Sir George Humphry found that between 80 and 100 of the abstainers were a fraction over 12 per cent., whilst the percentage of abstainers amongst the centenarians had risen to 26 per cent., rather more than "a slight advantage." If we knew the proportion of abstainers between 50 and 60, 60 and 70, 70 and 80, 80 and 90, and 90 and 100 we would have material which would show whether it is or is not the case that the proportion of deaths amongst non-abstainers are in all periods higher, and that therefore the percentage of abstainers would steadily increase with age. The statistics of Sir George Humphry, as far as they go, support the affirmative.

Your annotator also says that "what evidence is available on the subject seems to show that a strictly temperate use of alcohol tends to prolong life, for the excellent reason that it assists digestion and therefore promotes health." I am deeply interested in the subject but do not know any evidence to the above effect.

Finally, may I recommend your readers to a leading article dealing with this matter in the BRITISH MEDICAL JOURNAL of October 19th?—I am, etc.,

Dublin, Nov. 30th. E. MACDOWEL COSGRAVE, M.D.