the leading articles are the only things in the *Journal* of any use to us. "Use" is, of course, the operative word if medicine attempts to be an applied science. The *Practitioner*, on the other hand, treats the G.P. (and the non-teaching specialist) with dignity. It gives the *ex cathedra* treatment for the conditions for which we want to know the science to apply. And it also gives the arguments for such treatments with even, in some cases, the alternative and less successful treatments. It is then up to the individual G.P. to apply such treatment and see whether he can get a better result than he did heretofore.

The more medicine approaches science, the greater the amount of treatment will become formalized and dogmatic rather than the transfer of spirit and personality. Even now, does the writer of a paper on the methonium compounds know so very much more about how those drugs work than the G.P.?

May I give you the reason for the appearance of the *Medical World*? It is essential because of the uselessness of the *British Medical Journal* to us "old sticks . . . the applicators of medicine." I do not know the *Lancet* and have not seen enough of the *Medical World* to form an opinion on it. All three of the journals, however, are trying to sort out the successes from the failures of the empirical therapeuticists and present it free from the experimental embellishment.

You say the multiplication of journals for general practitioners "... has behind it the assumption that the general practitioner is intellectually inferior to his colleagues. . . ." The assumption is, of course, correct. There is no need to remind us of it. The G.P. is a G.P. because he has neither enough brains nor influence (brains in my case). If I had got a first-class honours degree and knew the right people, do you think I would be seeing sixty to seventy people at my surgeries and twenty calls daily? When the day is over, one can feel a little tired and unwilling to read wrangles about abstruse medical problems. The capacity of any one human brain is, after all, finite.

If the object of the *British Medical Journal* is both immediately and ultimately to improve the practice of medicine, it should consist of two parts, one part devoted to the evolution of biology and therapeutics, and one part devoted to the nature of disease as it has at the moment of writing been established and the methods of treatment of disease which appear at the moment of writing to be best to the writer.

Until such a reform of your *Journal* is carried out I will continue, as do the vast majority of my fellow G.P.s, to read the summary of each article and the leading articles, with a glance through the parts given over to medical politics. I will, as I always do, read the *Practitioner* from cover to cover. It would be simpler to give my membership of the B.M.A. up and save my six guineas. After all, I can become a founder member of the "College" for ten guineas and have my interests possibly better catered for.—I am, etc.,

Folkestone.

J. M. B. MORWOOD.

SIR,—It is difficult to follow your argument in the leading article (February 28, p. 496) under the above heading. You mention that the *B.M.J.* is not a journal for general practitioners, and then deplore the multiplication of journals for general practitioners. The very fact that you claim that the "Refresher Course" series caters especially for the general practitioner shows how little you understand his needs; very few of these articles are of any value to him—they are written by consultants and appear to be based, at best, on hospital out-patient experience.

Your circulation of 78,000 copies does not mean a similar number of readers. Most of us receive the *B.M.J.* free in virtue of our membership of the B.M.A., and all too many copies are never unwrapped. A man is far more likely to read a journal that he has selected and paid for; any excessive multiplication will always be checked by financial considerations.

How can a journal be "what its contributors make it" when all contributions have to pass the scrutiny of one section only of the profession, the consultants? Certain it is that in 1953 it is easier for a young registrar to have access to your columns with a description of two cases of an extremely rare disease than for a general practitioner "at the peak or over of middle age" to be able to report a discovery similar in value to that of digitalis. We general practitioners need a journal run by men who have shared our experiences and who are "on the same wavelength."

We would be the first to resent any suggestion of intellectual inferiority, but already much of "medical science in the making" is not seen by us because of the multiplication of scientific journals that the B.M.A. has found it necessary to sponsor.—I am, etc.,

Bath.

CHARLES HAGENBACH.

Chlorophyll as a Deodorant

SIR,—I have read with interest Dr. J. C. Brocklehurst's paper entitled "An Assessment of Chlorophyll as a Deodorant" (March 7, p. 541). I hope that you will publish these comments on it.

Dr. Brocklehurst starts by committing an error, very common in chlorophyll literature, by failing to define his materials. Certainly he makes a show of doing so, but it is meaningless in that he does not give the concentration of any of his samples, except "solution D," which he says is reported to contain "12.5% water-soluble chlorophyll." One would want to know the values, based on spectrophotometric assay, in order to assess the worth of his results, as I shall illustrate below. Secondly, Dr. Brocklehurst appears to have discovered an entirely novel, not to say amazing, method for the preparation of methyl mercaptan. Perhaps this accounts for the fact that his product also had novel properties in that 30 mg. had a gaseous volume of 130 ml.—instead of the 14 ml. that one would expect on the usual assumption that a gramme molecule has a volume of 22.4 litres at S.T.P. These aberrations undermine one's confidence in the paper as a whole.

However, assuming that he did in fact have methyl mercaptan, I consider that his first two experiments are valueless, since they failed to comply with the requirement that the water-soluble chlorophyllin and the mercaptan were in intimate contact for a sufficient period of time. Further, since one does not know how much actual chlorophyllin was used in his tests, there is no means of knowing whether the amount of mercaptan was more than could under any circumstances have been deodorized. It is doubtful also whether contact between the chlorophyllin and the odorous materials in his third experiment would have been sufficient, only his fourth experiment being adequate in this respect. Here he fails to define even the amount of mercaptan he used. The failure to deodorize garlic in this case could, from my experience, only arise from the use of insufficient chlorophyllin. Substances such as ether and perfume materials are not deodorized by chlorophyllin under any circumstances.

I have not experimented with methyl mercaptan, but instead have used benzyl mercaptan. If one adds to 100 ml. of water containing 1 g. of sodium copper chlorophyllin (100% by N.N.R. spectrophotometric assay¹) 0.4 g. of benzyl mercaptan in 2 ml. 90% v/v alcohol and leaves the mixture for 18 hours at 37° C., or at room temperature, it becomes completely deodorized (even if then acidified with sulphuric acid). The green solution now contains a suspension (c. 0.4 g.) of colourless, odourless crystals readily shown to be dibenzyl disulphide, m.p. 71° C. (This approaches the maximum amount of benzyl mercaptan that can be deodorized under these conditions; a smaller quantity is of course much more rapidly deodorized, but contact for an hour or more is still required.) A control experiment at the same alkaline pH (9.5), but without the chlorophyllin, reaches a similar result only after about 10 days. Replacement of the air in the vessels by nitrogen results in virtually no change in 21 days or more. Thus it appears that the deodorization of benzyl mercaptan due to aerial oxidation is greatly accelerated by the presence of an appropriate amount of sodium copper chlorophyllin. It is not a catalytic reaction, since the chlorophyllin can deodorize only a limited amount (max. c. 0.5 g.). On recovery, the chlorophyllin shows no apparent change in properties or colour, except that the wavelength of maximum light absorption is altered from 405 μ to 402 μ , but it still will not deodorize any more benzyl mercaptan or, for that matter, any garlic oil.

One might therefore reasonably expect 1 g. of 100% sodium copper chlorophyllin to transform about 0.2 g. of methyl mercaptan to dimethyl disulphide. This latter, unlike the dibenzyl compound, would still have an offensive odour (and incidentally reduce iodine). Whether, being a liquid and perhaps more readily contacted than the very insoluble dibenzyl disulphide, it would react with a further quantity of chlorophyllin to be deo-

dorized (like the diallyl sulphide of garlic oil) remains to be determined. This I propose to try.

Garlic oil differs from benzyl mercaptan in that its main constituent is already a disulphide, the diallyl ester. Here 1 g. of 100% sodium copper chlorophyllin will in 24 hours completely deodorize only about 0.005 g. of the oil. Presumably it is again an oxidizing action, but I have not been able so far to identify the product(s). That it is a true deodorization is confirmed by the fact that our parent company now regularly uses sodium copper chlorophyllin to deodorize distillation plant after distilling garlic oil. Such cleaning formerly took 14 days, but is now effected overnight with great saving in expense. Our experiments have revealed that water-soluble chlorophyllins *in vitro* have little or no effect on any substances that we have so far tested, other than odorous sulphur-containing compounds, against which they are highly effective. Since such odorous compounds may often be present in the body as breakdown products of sulphur-containing proteins, it does not seem unreasonable that chlorophyllin should deodorize mouth or breath smells. Systemic deodorization seems less likely. I prefer to leave the clinicians to decide such matters. It is to be remembered, however, that the weight of such materials normally exhaled would presumably be very small indeed as compared with the quantities used in the *in vitro* tests.

I commend Dr. Brocklehurst to try the experiments mentioned above. I shall be very surprised if he fails to confirm my results—that is, assuming that he is using the appropriate chlorophyll derivatives. I shall be happy to supply him with suitable samples on request. Meantime, his present experiments have, in my opinion, proved nothing.

As a mere chemist, I do not feel competent to discuss his clinical results. However, their interpretation may have been biased by the results of his unsatisfactory *in vitro* tests. Further, they constitute only an isolated set of unfavourable subjective results to be set against a much larger number of positive results obtained elsewhere—and must be so regarded.

Finally, while I hold no brief for the wick type of deodorizer, and have already questioned² the part chlorophyllin plays in such preparations, I am interested to note the reference in your annotation to the presence of crotonic aldehyde in some of them. What authority exists for this statement? Again, in your references to Dr. A. H. Corwin's criticism, would it not have been fairer also to refer to his subsequent retraction³ of most of his criticism?—I am, etc.,

WM. MITCHELL,

Chief Chemist,

The Allen Chlorophyll Co. Ltd.

London, N.1.

REFERENCES

- ¹ *J. Amer. med. Ass.*, 1951, **34**, 146.
- ² Mitchell, W., *The Export Review*, 1952, **13** (Nov.), 35.
- ³ *Chemist & Druggist*, 1952, **159**, 238.

SIR,—As a research chemist in the chlorophyll field I feel that a few comments are called for on the article "An Assessment of Chlorophyll as a Deodorant," by Dr. J. C. Brocklehurst (March 7, p. 541) and also on your annotation (March 7, p. 554), especially in view of the subsequent wireless broadcast publicity.

To begin with, under the heading "Chemistry" the formula of chlorophyll is given incorrectly. The magnesium should be directly attached to the nitrogen atoms. After saying that often no information was supplied about the chemical composition of the types of chlorophyll used in proprietary preparations, the author falls into the same error. Admittedly in solution "A" he defines the chlorophyll used, but no indication is given of the purity. Obviously a material testing 60% by the spectrophotometric method would be less effective than a material testing at 120%. Both these strengths are commercially available. In solution "B" one assumes that a crude form of sodium copper chlorophyllin is used, but here again the precise nature of the material is left undefined. Further, the amount of chlorophyll used in preparations described as solution "C" are so small as to be inadequate for the deodorizing experiments subsequently described. Solution "D" again consists of undefined chlorophyll—no information is given concerning the kind or purity.

It is in the experiments, however, that I feel serious criticism can be levelled. In experiments 1 and 2 the time of contact between the mercaptan and the chlorophyll is very short—too

short, in fact, for any deodorization to take place, even under the most favourable conditions. For example, work in our laboratories has shown that a period of approximately one hour must elapse for the odour of benzyl mercaptan to be completely removed by sodium copper chlorophyllin at 37° C. Further, the figures obtained are so variable that no reliance can legitimately be placed on them. The number of results reported are totally inadequate having regard to the comparatively crude nature of the experiments. The author admits the magnitude of the experimental error, but we feel his results are practically useless from the point of view of a critical evaluation.

Experiment 3, it is claimed, "involves the use of very much smaller quantities of mercaptan and other odorous vapours," but are the quantities very much smaller? In experiment 1, between 12.6 and 18.5 ml. of 0.1 N iodine solution was reduced, which is equivalent to about 60 and 90 mg. of methyl mercaptan respectively. In experiment 2, 5 ml. of iodine was used, which is equivalent to 24 mg. This compares with 15+30 mg. used in experiment 3. The great weakness of experiment 3, however, is in the author's own admission "that no attempt is made to record the degree of smell present, as the method is too subjective to provide reliable quantitative results." According to the author's technique, even if 90% of the odorous substance were deodorized by the chlorophyll the latter would be classed as ineffective. The Table provided shows that in one case a deodorant effect did occur, but this is dismissed because of the presence of the strong smell of chlorophyll. But surely this would apply equally so to many of the other examples given, particularly the results recorded previously. Comparing these two experiments, it is obvious that, while deodorization had not taken place in 22 hours, it had taken place in 46 hours (even when a smaller amount of chlorophyll was present), and, instead of being dismissed as without significance, it is surely highly significant and should have been made the basis of further experiments.

I am not sure why Dr. Brocklehurst included the experiments with ether, as not only do they bear no relation to the deodorization of breath or body odours, but in any case excessive amounts have been used, running into many hundreds of mg. I do not know the density of methyl mercaptan, but arguing from theoretical considerations—that is, the gramme molecular volume—130 ml. seems to be an excessive volume for 30 mg. of methyl mercaptan. In experiment 4 the results obtained with perfume are quite irrelevant, as it has never been claimed that chlorophyll deodorizes perfume. Further, it is not reasonable to suppose that chlorophyll would penetrate the cell tissue of a slice of onion in sufficient quantity to react with all the odorous substances present. Again no information is given concerning the relative concentrations of chlorophyll and mercaptan used, nor is any definition given of "syrup of garlic."

Finally, the clinical trials described in the paper consist of three people; the first one was given 400 mg. of 10% chlorophyll solution, which means an intake of 40 mg. of chlorophyll of undefined strength daily. From our own experience we know this to be inadequate for this sort of condition. A more likely quantity is 200 to 300 mg. per day of 100% material.

On the subject of the annotation, I should like to point out that the report of the National Bureau of Standards recently quoted by Dr. A. H. Corwin is from section 4, "Control of Odours in Households and Places of Business," subhead 6, "Masking Odours and Their Uses." The subject of the entire circular is space deodorization. The specific reference is to whether chlorophyll can by photosynthesis "purify" enough air to have any distinguishable effect on atmospheric odours. This reference has no application whatsoever to chlorophyll as a contact deodorant. As used by Dr. Corwin, this quotation gave the inescapable impression that the National Bureau of Standards had found chlorophyll valueless as a contact deodorant.

In conclusion, may I reiterate that, though chlorophyll is not a panacea for all kinds of odours, work about to be published will show beyond any doubt that in certain specific cases chemical proof is forthcoming that odorous substances can be changed into odourless compounds.—I am, etc.,

L. DEADMAN,

Leatherhead.

Chief Chemist, Ashe Laboratories, Ltd.

SIR,—In the annotation on chlorophyll (March 7, p. 554) reference was made to crotonaldehyde and formaldehyde. We feel sure your readers will be interested to know that neither of these substances is present in "airwick" fluid.—I am, etc.,

B. E. CHANELL,

Slough.

Managing Director, Airwick, Ltd.