Brit. J. Psychiat., **116**, 387–98) and the Editors of this *Journal* (Correspondence **117**, pp. 119) have misunderstood Dr. Mawson's criticisms of the above paper (idem Vol. 1A p. 117), and misunderstood him in a particularly interesting fashion.

Mawson states that the above investigation was not conducted in such a way as to furnish evidence for the final conclusions, and feels that such unsubstantiated findings should no longer be published in this *Journal*.

He argues that the following methodological flaws have been committed, and that the case made for the use of antidepressants in the treatment of phobic states is therefore not proven by this investigation. Briefly: the data were bias-prone since they were derived from the clinical records of the doctors prescribing treatment, the data were assessed retrospectively, there having been no previously established standardization of data collection, and the appropriate corrective procedures such as independent rating of clinical records and the use of control groups were not undertaken.

It would have been reasonable to expect that the authors, in their reply, would argue that the faults had either not been committed or did not imply that their conclusions were unwarranted. Sadly, they have done neither. They merely reiterate the findings of their investigations, and state that the potential value of their treatment regime has been established to their satisfaction.

The point of interest is that the effects of attention and placebo reaction are generally assumed to be present in all treatment regimes and are not, in these studies, the object of investigation. Therefore, what matter if the first uncontrolled study yields favourable results? This may be only the result of the above factors. A proper investigation will have to be done anyway (N.B. the prospective study being carried out by Kelly), and could profitably have been done in the first place.

The Editors themselves raise a number of equally interesting points, among them that in their view Dr. Mawson 'expects too much', not only of their *Journal*, but of psychiatry as a whole.

The first point, by implication, is that because criticisms can be made of any work, no one piece of work is better than any other. Their lament: 'Even controlled drug trials contain a large make-believe element, since serum levels of the drug are not monitored over the trial period', prompts the reply 'Monitor the serum levels'. One does not say that a clearly malfunctioning watch is, after all, a reliable timepiece simply because even a more precise one has its own, far smaller, error. Implicit in their comments is in fact the view that some investigations *are* better than others (witness the initial 'Even' above) and of course the whole point of Mawson's criticisms is that some procedures make findings move further up into the regions of comparative acceptability than do others.

And further, the Editors then reiterate precisely what is in question: 'It is not possible to get, by giving standardized doses at set intervals over a fixed length of time to an arbitrarily selected group of patients, the same results from a psychotropic drug as can be obtained by a clinical expert sensitively selecting his patients and dosages, individual by individual, on a basis of experience'. The point of interest here is that despite the undisputed non-comparability of the results, all the adjectives would suggest that the results of the clinician are 'better' (along an impermissible scale). The factors which make such comparisons dubious have been set out in Mawson's letter.

That these confusions are not confined to the authors of the papers and the editorial board is shown by Dr. Freeman's (*Journal*, September 1970) misunderstanding of the whole problem, since he takes as demonstrated precisely what is at issue, the value of M.A.O.'s in phobic states, and then berates Mawson for 'the neglect of practical and humane considerations,' namely suggesting that the supportive evidence is unsatisfactory. At this level it is intellectually arrogant to refuse to dispense imaginary goods.

Finally, one regrets the publishing of methodologically flawed investigations in any journals, but since standards in these matters are open to vigorous discussion, perhaps correspondence rather than static editorial policy will provide the necessary corrective.

Above all, we must move away from the position in which it would seem that the ultimate criterion of the veracity of psychiatric findings is that they should arouse in the investigators a feeling of satisfaction.

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DEAR SIR,

My departure to work overseas for a few months has prevented me from replying earlier to Dr. Mawson's latest letter. However, as well as giving time for further reflection, this interval has provided me with a completely new dimension of psychiatric experience, which has very much reinforced the views I expressed in your September issue.

The most important of the 'real issues at stake' was Dr. Mawson's castigation of your own editorial

policy in publishing the paper by Kelly *et al.*, which he found offensive to the purity of his statistical conscience. I was then concerned to point out that contributions such as this are in fact of considerable value from the *clinical* point of view and that you would be doing the majority of your readers a great disservice in excluding them. Between Sutherland's speculative-type articles on the one hand and reports of scientifically rigorous double-blind trials on the other, there is an important middle ground of extended and well-recorded clinical experiences which often deserve publication. If they are not reported there may be very long delays before procedures of clinical value become widely known and before those centres which are more fortunately placed for resources undertake detailed trials of them. (One might have imagined that this process would have worked the opposite way, but, like the Duke of Plaza-Toro, the teaching hospitals lead the psychiatric profession from behind.) Therefore, my primary aim was to support the claim of Dr. Kelly and his colleagues to be heard, rather than that they should wait for a state of double-blindness to be achieved first.

It was not my intention to make a personal attack on Dr. Mawson (whom I do not know) or on any other individual, but only on certain viewpoints. The extremes to which these can lead are well illustrated in the latest exchanges over lithium therapy (1). The full complexities of that particular argument have become extremely difficult to follow, but I suspect that, having got into a posture of being statistically holier-than-thou, the critic concerned is presently motivated more by obstinacy than by scientific reason in this case. Meanwhile, I find lithium carbonate, like phenelzine, to be a very useful therapeutic weapon.

In his passage about arguments *ad* this and *ad* that (which sounds like an extract from 'Up Pompeii'), Dr. Mawson is presumably implying that I am unhappy about the values which psychiatrists in training are likely to absorb at the Maudsley and at the MacMaudsley in Edinburgh. If so, he is perfectly right. He asks me to say what I consider are the 'desirable direct products' of academic psychiatry, and I believe these to be well trained clinicians, motivated primarily by compassion for their patients and with a deep awareness of the social and economic background to psychiatric disorders. Unfortunately, some of those with the most encyclopaedic knowledge of Bleuler and Schneider are also marked by a total inability to communicate with patients on a human level. Certain teaching centres have by now developed a firm tradition, not of healthy scepticism, but of a negativism which ignores the fundamental human concerns of psychiatry. It is as harmful in its way as the Freudian stranglehold which paralysed American psychiatric teaching from the 1940s until just recently.

As a result, the astonishing progress of psychiatry in the last few decades has not come from these intellectual powerhouses but from the research of pharmaceutical companies (who are beneath contempt as far as most teaching centre personnel are concerned) and from the pioneering activities of a number of people in unfashionable mental hospitals and general hospital units. It is now almost twenty years since the development of community-based district psychiatry began in Lancashire, which has not only been of enormous influence in national planning within Britain, but has constantly drawn professional visitors from throughout the world. Yet in all this time I am aware of only one consultant and one registrar (myself in 1959) who actually went from any of the main teaching centres to see what was going on in this area. Another 'desirable direct product' of academic psychiatry might be the systematic study of these services, yet only Hoenig and Hamilton have ever attempted it (2). And if some of the bright young men who jostle in the corridors of power had a livelier sense of the human needs of the community which supports them, they might decide to come and work in this badly understaffed region; but I have seen no applications from them.

To finally demolish my credibility (see the exclamation mark) Dr. Mawson mentions that I have criticized the Dunlop or Scowen Committee. This must have really knocked them in the aisles in the Maudsley Common Room. But until it acquired a psychiatric member this Committee had no authoritative basis whatever for commenting on psychiatric drugs—which did not prevent it from doing so. Its performance over MAOI drugs matches that with the contraceptive Pill, where it managed to combine the maximum of unnecessary public alarm with the minimum of useful information. A very few people have died through the use of MAOIs, as they have with the Pill, but the invalidism and deaths which result from *not* using these drugs are enormously greater, though the Committee will never say so. On the use of combined antidepressants, Dr. Sargant has consistently been proved right and they have been proved wrong.

I respect scientific method, statistical sophistication and academic knowledge, and I am not unaware of placebo effects or of the long history of discarded treatments in medicine. Where I differ from Dr. Mawson is in my sense of priorities and in my ideological approach to the practice of psychiatry.

I wrote last time on the basis of over nine years' work in the industrial North of England—very different from Dr. Mawson's sheltered workshop conditions. At this moment, I am the only psychiatrist for an island with more than 100,000 people; there has rarely been a trained psychiatrist here in the past, and none for at least two years. There are many urgent problems of psychiatric morbidity in this area, but the most inescapable is that of schizophrenia. It seems as plain as a pikestaff to me that in a situation like this, where primary medical care and social services are almost non-existent, the best way to help these people is to get nurses to give them regular long-acting phenothiazine injections.

Whether I know I believe this or believe I know it is a semantic point I will leave to the sages of Denmark Hill. What I know and believe is that if I do not take this action *now*, and persuade other doctors to do the same, thousands of unfortunate people will languish unnecessarily in the snake-pit conditions of Caribbean mental hospitals, or perhaps in even worse circumstances elsewhere. If Dr. Mawson still considers this a piece of self-deception, he could come and try for himself.

Renée Dubos has pointed out (3) that while we concentrate so much of our resources on acquiring new knowledge, we fail to make practical application of existing knowledge which, even though incomplete, would be capable of solving most of our currently pressing problems. This is certainly the case in psychiatry today. In the course of several visits to the U.S.A. I have seen untold wealth poured down the drain in the name of 'research', whilst the most crying human needs are ignored. If the NIMH had never existed, if not a single American psychiatric journal had ever been published or any thesis written, if there had been no conferences, 'workshops', seminars or evaluative meetings, if not a single dollar had been spent on any form of non-commercial research, would any patient have been really worse off? On the other hand, if the whole of this immense investment had gone into the actual provision of clinical facilities and services, would not the American public have benefited immeasurably?

This apparent digression is very relevant to the difference of attitude between Dr. Mawson and myself. In the U.S.A. the fact that resources are dictated by intra-professional goals (and whether these are financial or intellectual makes little difference) has resulted in the creation of what has been called with some justice a 'professional mafia'. Enormous sums, both public and private, go into the system, but little emerges to help the patient as a human being. We have to avoid such a situation in Britain by ensuring that academic medicine in general is firmly anchored in community needs. I believe that this should involve, amongst other things, a greater respect for the therapeutic openings which are made by practising clinicians, such as Dr. Kelly and his colleagues.

I agree, of course, with the quotation from Sir Denis Hill and wish that more evidence of such 'partnership' came from the university departments themselves.

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[This correspondence is now closed. Ed.]

DEPERSONALIZATION AND ESTRANGEMENT: INDIVIDUAL OR SOCIAL PROCESSES?

DEAR SIR,

The organic, psychological, psychoanalytical, and general clinical psychiatric theories of depersonalization, recently surveyed by Dr. Sedman (1) have this in common: they concentrate on the individual person, using concepts of a more mental or of a more structural functional model, as the case may be. Accordingly, depersonalization is contrasted against the conditions and processes effective in the growth and maintenance of personality and in establishing the perceived, or self-perceived, coherent personal identity.

One would wish, however, to take into consideration that the sense of self-identity and the reliable feelings of a quasi-permanent image of the own coherently consistent person, together with their impairment in depersonalization and estrangement, all point close links with the transpersonal processes of communication. Expressive-interpretative interchanges proceed at all levels: verbal, pre-verbal, and non-verbal (e.g. postural or autonomic-vegetative, as in blushing, paleing, or missing a heartbeat); interpersonal identification phenomena play a discernible role in shaping the features of the individually and personally sensed self image.

For this reason, observations about communication processes could serve to supplement and reconcile some seemingly opposed views: on the one hand,