

Psychologists: Non-medical and medical*

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May I start with a dream? It is about a student who wanted to register for a higher degree. Eyebrows had been raised because of a couple of unusual entries on his application form. In announcing his “field of study”, instead of writing “psychology”, like any normal person, he had put “psychologists”. And then, in the space labelled “title of proposed thesis”, he had said, “a highly critical investigation of their nature”.

Now, his plan could easily have been faulted, not because there were any actual rules against that kind of thing, but—far more important—because there were no precedents. However, his scheme filled me, the dreamer, with joy. Furthermore, I felt glad that I was convenor of the higher degrees sub-committee, and that I had been left alone empowered to act on behalf of my colleagues, who by this time had gone round to a place called the Marlborough. In their absence I decided to recommend, very strongly indeed, that the applicant should have his degree immediately. And it seemed right that it should be a proper one. There should be no mere Master’s degree for him. Even a Ph.D. would be embarrassing in its inadequacy. Nothing less than a D.Sc. or a D.Lit. would do for such bold, discerning intention. Here was a man ready to grasp nettles. Let him be placed, without delay, among the top people.

Like a rocket—a Russian rocket—my recommendation shot with split-hair precision along the corridors of the Senate House. Before you could say “Id”, the candidate was being given his scarlet gown. A high dignitary of the University helped him into it in our gracious Chancellor’s boudoir, where it had apparently been waiting for him, behind the door, on a little psi-shaped peg. Of course, the poor new graduand, who happened to be a scraggy ectomorph, was in a frightful state. And he looked quite dotty in his ridiculous hat. But, then, they all do. As he fainted [2]and I woke up, the whole business seemed to me to be tremendously gratifying.

I hope you think my recommendation on the award of the degree was sound. For in my opinion we psychologists are really rather negligent about the study of ourselves. We rush around to meetings and conferences, in Rome one week and Birmingham the next; and while we breathlessly read papers at each other on *other* people’s quirks—those of children, workers, patients, rehabilitees, prisoners, consumers, and Uncle Tom Cobleigh—we hardly give a thought to our own. Of course some psychologists claim that we cannot hope to know ourselves well without being analysed; but it

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seems to me that that is a bit far-fetched. Anyway, I believe that by ordinary and less expensive means—even by straight forward self-examination, conducted in the blunt and unrefined manner of the Church—we can discover quite enough about ourselves to be going on with. A long time ago, after I had written somewhere that it was desirable for vocational advisers to seek awareness of their own prejudices one of my colleagues said, rather solemnly, “If we all did that, we, and psychology too, might be very different”. I suspect he was right.

It is particularly important that we should have considerations of this sort in mind when we talk on matters of high controversy. As I shall be touching on some such matters—though delicately, I hope—in the next forty minutes, it seems fitting that in doing so I should try to bring some of my own relevant presuppositions into the light of day. (If I may, I shall call them “presuppositions” rather than, pejoratively, “prejudices”.) But I ask you now to note the wording of my title. It reads “Psychologists: non-medical and medical”, with the adjectives in that order. And, as you will see later, I do not mean simply “psychologists and psychiatrists”.

Do I give any secrets away, I wonder, when I hint that I am not the only person in this room who came to psychology by chance, or at least through what must in honesty be called a rather dicey sequence of educational acrobatics? Heading in the long run for my father’s trade—ears, noses and throats—but having already detected in myself a strong distaste for surgery, I opted out of medicine when I was eighteen, during my Second M. B. course. I then spent a summer vacation on an exercise which was probably mainly taxonomic in purpose, though at the time I would have not called it that. I wrote a short textbook of invertebrate zoology. You will not find reference to it in any publisher’s list, though it was, I thought, quite good. Undoubtedly it performed a useful extricatory function; for the writing of it made me decide that what I really wanted was what seemed to be called anthropology.

So, back in Cambridge, I announced to my tutor that I had finished with medicine and intended to take up anthropological studies without delay. He asked whether I had read the regulations about anthropology. I had. Then would I perhaps be good enough to go away and read them again? It turned out that—like many others, no doubt, in many universities, before and since—I had not grasped the implications of a tiresome requirement that reduced my plan to ruins.

That same evening I tackled *The Student’s Handbook* and discovered [3]the moral sciences tripos, of which I had never heard. It had a “part one”, consisting of logic, philosophy, ethics and psychology; and a “part two”, in which you could specialize. It sounded attractive, not least because it would apparently lead me into fields touched on lightly in the last chapters of one of my now favourite books, Woodger’s *Elementary Morphology and Physiology*. So I took a midnight plunge off my flying educational trapeze, at the manic end, and at nine the next morning, without another word to anyone, I landed at my first moral sciences lecture. It was given to five of us in the dining-room of his house in Millington Road by our logician, Mr. W. E. Johnson. I recall that he was trimly bearded but, wrapped in his ancient travelling rug, rather dowdily beshawled.

The performance began punctually when Mr. Johnson’s sister, Fanny, looked in for a moment to place in front of him—in a businesslike and perhaps slightly disapproving

way—a packet of ten cigarettes and two boxes of matches. As with quivering fingers he spent three matches lighting his first cigarette, he asked his first question. It came upon us with almost brutal gentleness. Could he assume that we had all mastered Mill's *Logic*? I cannot now remember whether Mr. Bernard Babington Smith was able to answer, Yes. Perhaps he was. I know I was not. I had heard of John Stuart Mill, because my rather starchy history master had frothed at some of his libertarian ideas; but of the existence of his *Logic* I had been, until that moment, entirely ignorant.

A bewildering and indeed terrifying hour followed. It was devoted mainly to an exposition of Mr. Johnson's views on that most fundamental of all questions in logic, "What is a proposition?", one I soon recognized to be not only fundamental but also charged with dynamite, which in those pre-nuclear days was really something. Afterwards I leapt on to my bicycle and rushed back to Trinity Street to buy my Mill. The very look of it increased what Saint Paul and Freud might jointly have called the length and breadth and depth and height of my traumatic experience. In fact, it shook me to the core. It seemed to have shaken three previous owners too, for it had all their names in it but had obviously not been read. In this it differed from my copy of another "set" book which I bought at the same time, Ward's *Psychological Principles*. I found that my Ward had been annotated freely—and with very considerable pungency—by its last possessor, a certain John Bowlby.

It was not until my year of specialization came along that things were sorted out satisfactorily. Then, even statistics began to make sense. I think this was partly because Mr. Udny Yule, not being a psychologist himself, disentangled his subject from what went on in the psychological laboratory. Much of the laboratory work in those days well deserved the comment of Mr. Bartlett (who had not yet become either Professor or Sir Frederic) that it was dull and trivial. It is perhaps appropriate that I should add here, at a conference attended by a number of laboratory technicians, that my most effective guide in the laboratory was unquestionably our senior technician, Mr. Sillitoe.

The rest of the psychology I found exciting. MacCurdy's *Common Principles in Psychology and Physiology* had just been published; and so [4] had *The Basis of Sensation*, whose author, Adrian, was only a few yards away, through the wall. Bartlett's *Remembering* was on the stocks; and we were all reading Head's *Aphasia*, or some of it. Of course, many of the things we talked about—image functions, physiological gradients, epicritic and protopathic sensibility and the like—have all but perished, or have at least been given new names. "Rigidity" had not yet come in: we had to make do with such ideas as McDougall's "tendency to prefer the familiar" and Bartlett's own "tendency to conservation", though—if we were inclined to heresy—we could always add Spearman's "perseveration". Moreover, none of us "designed" experiments: in those pre-Fisherian days at the tail-end of the twenties we just "planned" them.

The adventurousness of it all had, I think, its drawbacks. In the event, I must confess, I left Cambridge knowing not very much about what was called psychology by the people I began to meet who had been to other universities. My attention had been fixed on certain borderline problems of psychology and physiology and there had been some social psychology, especially of primitive groups, on the periphery. That

was about the lot, apart from a little psychopathology and something—not enough, I now think—on *bewusstseinslagen*. But I had enjoyed it, and I soon learned to put up with the anxiety created in me by the troublesome erudition of those whose psychological education had been more broadly based.

Shortly before I came down from Cambridge I had been fixed up with an assistant lectureship elsewhere which I did not want. I was relieved when I was rescued from it by C. S. Myers. He was our external examiner, and in my short *viva* he took up the matter of my employment. His contribution to the discussion was, as I discovered later, typically Myersian in form. It consisted of long pauses, friendly grunts, a few short questions and occasionally a remark belonging to the last question but two. In less than no time I was swept on to the staff of his National Institute of Industrial Psychology. In consequence, I spent the next year in Wormwood Scrubs Prison.

I feel bound to say that on the whole I was very happy in prison, not only because my work there gave me a chance to *do* something but also because it seemed to fit in well with views I now held about both religion and politics. For me, these had long been relevant to my psychological studies, and recently they had become more so. This was partly because I had been greatly influenced in both fields by a fellow of my college, Dr. Joseph Needham, a chemical embryologist of considerable distinction whose later books of essays—especially *The Sceptical Biologist* and *Time the Refreshing River*—delight me still. In accord with his, my religious opinions had moved firmly from low to high; and my political opinions, orthogonally, from right to left. For a high church socialist, with what we called “neo-mechanistic” leanings, Wormwood Scrubs seemed just the place.

However, I was in for another shock, comparable with the one I had at the time of my start on the moral sciences tripos. The new trouble arose because I had been flung, at the age of twenty-two, into a situation in which I became directly—and, indeed, solely—responsible for formulating [5] advice on the vocational training of 400 young men who had been sentenced to Borstal detention. I soon realized that I knew virtually nothing about either employment or crime, and very little about psychology that had even apparent relevance. True, when the results of the Borstal vocational guidance experiment were calculated two or three years later, it was found that the new method of allocation tried out had proved very significantly superior to the old method, when both were judged by the previously agreed global criterion. But its apparent success turned out to be small consolation for my conviction that the experiment was difficult to defend ethically, because of my own inexperience; and scientifically, because it made an assumption which, though still commonly made by psychologists, I believe to be untenable. The untenable assumption is that in psychological inquiry we can manage well enough with a *semblance* of co-operation.

It was at this point that I really began to think not only about psychology but also about the whole business of being a psychologist. The outcome does, I imagine, bear traces of its Cambridge origins; but I should not like Sir Frederic Bartlett to be held responsible for it. Let me set out briefly half a dozen relevant “working beliefs” I now hold and begin to weave in some observations on my theme.

First, I believe that psychology is best regarded primarily as a biological science, concerned chiefly with the explanation of individual human behaviour. Without doubt

it should be closely related to sociology and economics, as several speakers at this very conference have strongly urged; but in my opinion it should be even more closely related to physiology and anthropology. It seems, from the American letter in *The New Scientist* a fortnight ago, that the National Academy of Sciences shares this view. Whether the University of Rome does so too, I cannot say; but I was interested to notice last week that there psychology and anthropology are as closely knit physically as psychology and physiology are at Cambridge.

This conception of psychology implies close links with medicine, if only because the medical sciences themselves are in the biological group. But to leap to the conclusion that in these circumstances psychology should be regarded as a handmaid of medical practice would be to commit *en route* several logical fallacies.

Now, however, for my second working belief, which to some of you may seem distressingly narrow. It is that, *vis-à-vis* physiology, psychology is profitably regarded as what I call a “residual” science. It deals essentially with physiology’s “left-overs”, and takes up the explanation of behaviour at the point at which the physiologist has to say, “I’m sorry, but I can’t do any more—not yet, anyway”. As I have put it elsewhere, crudely perhaps, “the task of the psychologist is primarily one of clearing up after the physiologist”. The still-popular distinction between physiology and psychology on “part-whole” lines, despite its respectable McDougallian and Woodworthian sponsorship, seems to me to be the reddest of red herrings and a rather objectionable one at that; for it introduces, quite unnecessarily, an odour of defeatism. The division of labour between the [6]two sciences is not to be arranged on such a basis, surely, nor yet on the basis of the older distinction between “bodily” and “mental”, though that was preferable, but rather on differences between the kinds of concepts they use in the explanation of behaviour.

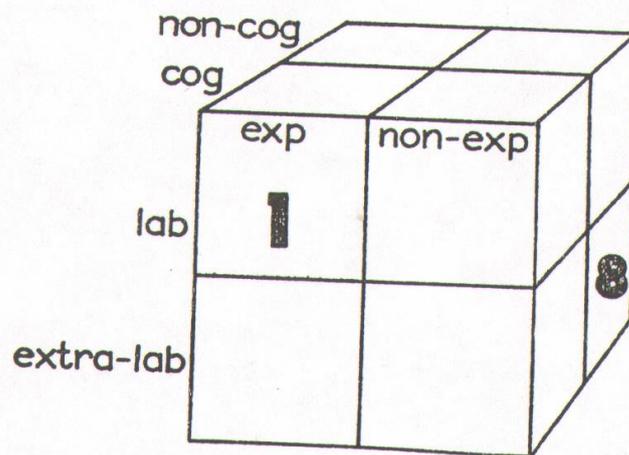
Recently the committee of the medical section of our Society insisted that the job of the medical section should be to study what they called “psychological processes”. If by that they mean that they should be studying behaviour with non-physiological concepts, I am with them entirely. I would go further, much further, and say that indeed is what psychology is about. But if they mean that there is some behaviour which does not call for the exploration of physiological, neurological, pharmacological and other conditions that may be associated with it, then I think they are up the pole. On the view I am taking, we do not use psychological explanations for the sheer fun of the thing: we use them *faute de mieux*. When we postulate non-physiological determinants of behaviour, we do so because, apparently, physiology cannot take us as far as we want to go, and because we shall be stumped unless we can think up something which will enable us to do a reasonable job of prediction. Of course, physiology may *never* succeed in taking us as far as we want to go. I am not making any assumptions about that. I am merely advocating a conscientious use of Occam’s razor, with the assumption that attempts to find physiological explanations should be made with dogged persistence.

This leads me directly to my third working belief. It is that the psychological explanations we think up, the psychological concepts we use, should clearly depend on the kind of behaviour we are wanting to predict, and on the precision with which we are wanting to predict it. Here we have justification for diversity in our explanatory concepts. Egos and ids, intelligence and attainments, factors of this and

that, skills and attitudes— all these and many more may have a useful part to play. Their defensibility depends on what they are used for; and on how well, in the predicted event, they are found to have served their purpose. Different problems of human behaviour call for different levels of explanation, and for different degrees of precision; and these may be equal in scientific respectability. The danger we must guard against is that of using concepts unsuited to our problem; and of condemning others for using concepts which may be well suited to their problems but not to ours.

Of course, the more comprehensive in their application a set of concepts turns out to be, and the more compatible they are with other sets of concepts, the greater may be their chance of contributing to the advance of psychology. As we cannot apply these criteria of comprehensiveness and compatibility without knowledge of what our colleagues are up to, it is our business as psychologists, whether we are psychologists with medical qualifications or without them, to be well-informed. The dedicated psychologist who has given himself to one particular problem, or to one particular set of concepts, may easily become an unprogressive stick-in-the mud, pure or applied.

My fourth working belief is that many of the most highly desirable psychological inquiries cannot be carried out in the laboratory, cannot be [7]tackled experimentally, and are primarily non-cognitive. I think we academics should be ready to face these facts and their implications for our work. Let me produce for you, shyly, a very simple diagram, roughly (I repeat, roughly) indicative of what seems to me to have been going on. It is based on three dichotomies: cognitive v. non-cognitive, representing the “what” of psychological investigation; experimental v. non-experimental, representing the “how”; and laboratory v. extra-laboratory, representing the “where”.



I think it is indisputable that there has been a tendency for psychologists whose interests lie mainly in Cube 1 to regard themselves as a bit better than other people, and especially than those whose interests lie mainly in Cube 8, who are almost in the untouchable class. (Mind you, the Cube-1er may occasionally make a dash for Cube 8 himself, perhaps staying just long enough to do a little consumer research, possibly of the deep down kind. But that is by the way.) My point is that if we academics

encourage our students to think that Cube 1 is the only “proper” one, we shall be doing serious disservice to psychology. The danger of this is, to my mind, ever-present, because a special interest in Cube 1 is, I venture quite respectfully to suggest, a schizoid inclination; and so is an interest in academic appointments. Of course, if a psychologist—again, whether or not he is medically qualified—turns out to be particularly good at tackling Cube 1 problems, it seems fair enough that he should stick to them. But he should be brought to realize that he is choosing just one room in a rather large house, and that there are others.

This takes me to my fifth working belief, which is that in most psychological inquiry a necessary condition of precision is the co-operation—the wholehearted, unrestrained co-operation—of the subject; and that a necessary condition of this co-operation is the expectation that it will produce help. The connection between this proposition and the others I have put forward may seem remote. I do not think it is. The psychologist, it seems to me, differs from other scientists—possibly from all others except the social anthropologist—in the degree of his dependence on the [8]understanding and goodwill of most of those whose behaviour he studies.

Sometimes we want people to work at the ceiling of their performance perhaps in a test of suitability for a job. Sometimes we want them to give us a sample of their habitual behaviour, perhaps in a diagnostic test in a hospital. Sometimes we want to elicit from them the thoughtful truth in an attitude or opinion survey. How likely are we to achieve these important ends without giving something in return? Our subjects may themselves be content with the help they receive from the feeling that they have had a cosy session with a sympathetic person. But are we not inclined to be over-optimistic, complacent or casual about this?

You may recall that I referred to the untenable assumption that in psychological inquiry we can manage well enough with a *semblance* of co-operation. This problem appears to me to be fundamental, but even in the places where one would most expect to find it ventilated—for example, in such text books on testing as those of Anastasi and Cronbach—it receives perfunctory treatment. It is not, to my mind, a problem to be swept aside by the quotation of test-retest reliability coefficients or the building-in of “truth checks” in questionnaires. The advancement of psychological knowledge and its fruitful application depend, I suggest, on the skill of the psychologist in eliciting co-operation; and the development of that skill depends on his having something to offer in return for the co-operation. We must not imagine that we are anywhere near accomplishing a data-collecting task in a research investigation merely because somebody has “given us” (as we say) a group of workers, or school children, or patients.

In the first of a series of lectures he gave in London last term, Professor Oliver Zangwill touched on this point vividly. Speaking of work done at the National Hospital, Queen Square, he said (I hope I do not misquote him): “Good scientific work is not possible without therapeutic intention. . . You cannot get good co-operation unless the patient feels that you are trying to do something for him”. That is my argument too. And I would add that a psychologist’s competence must be greatly dependent on his awareness of its implications. No amount of expertise in the design of investigations, or in the computations that follow their execution, can atone for failure in this regard.

I have one more point to make about the eliciting of co-operation. Should we be as satisfied as we sometimes seem to be about group testing and group questionnairing procedures, where, if what we want are peaks of effort and truth, we may have as many different attitudes to manipulate—attitudes of apathy, anxiety, resentment and the rest—as we have people in our group? Should we be as willing as we sometimes are to delegate testing to people who have had little chance to understand what it is all about, or to practise it? How much of the misclassification of secondary school children we hear about is due not to the unsuitability of the test materials but to the failure of testers to grasp the importance of creating testing conditions which will foster in *each* child an attitude of what I call “quiet urgency”, testing conditions which are meant to evoke a peak performance without “flap”? I am sorry we did not hear more [9] about this problem in the long correspondence that took place just over a year ago in the columns of *Education*.

Let me make it clear that I am not putting up a “closed shop for psychologists” argument. I have no objection in principle to the employment of non-psychologist testers. I had two or three hundred of them on my staff when I was in charge of the Admiralty psychological department during the war. With careful training and individual supervision most of them did very well. What I am worried about is the use of tests by people who lack adequate initial instruction and continuing direct guidance, particularly when they think that they themselves happen to be exceptional people who do not need attention which they admit would be quite appropriate for others. If I were to say that many medical psychologists who use tests seem to need as much training and supervision as non-psychologist testers, and left it like that, I might be suspected of bias; so let me remark that the best lesson I ever had in psychological testing was given me by Dr. C. J. C. Earl, who later became medical superintendent of the Monyhull Hall Colony here in Birmingham.

When I began speaking about the importance of co-operation in psychological inquiry I made the comment that the connection between this topic and my earlier ones was perhaps not as remote as it might seem. What I was driving at was the contention that this matters greatly, *whatever* cube or cubes we are working in. Whether we are trying to induce effort in the performance of a cognitive task, or to elicit truth in a non-cognitive one, whether we are doing this in the laboratory or the clinic or the school or the factory; whether we are doing it experimentally, in controlled conditions, or without the benefit of experimental method—in all these situations we are dependent on the co-operation of our subjects. I believe that stress should be laid on this in our practical courses in psychology, right from the beginning. Indeed, I would argue that the first aim of even an elementary course should be to show students how to identify, describe, classify, measure and manipulate determinants of co-operation in psychological inquiry. To my way of thinking, there is no more fundamental task for the student. I wonder whether some of the *naivetés* perpetrated, some of the disappointing results obtained, in both research and application, are not due to an insufficiency of emphasis upon it?

I hardly needed any reminder of the importance of this crucial problem, once it had reared its head in prison; but I had one some years later. Shortly after the beginning of the war, I paid a short and ill-prepared visit to the Midlands, to test unofficially part of the second battalion of the Derbyshire Yeomanry. It came about because their

commanding officer happened to be married to one of the daughters of Professor Spearman. The scene of the operation was a disused brewery in Long Eaton; and the time was late on a wet afternoon in November. A moment or two after take-off, I found myself standing on the top of a huge beer vat, delivering instructions lustily to about 250 rather reluctant soldiers. Professor Spearman himself, aided by Dr. William Stephenson and Dr. C. B. Frisby, obligingly speeded round the touchline explaining to the yeomen the finer points of the highly inappropriate tests we gave them. Afterwards, [10]the four of us all did sums.

My sixth working belief—the last I shall mention—is that it is as convenient in psychology as it is in some other disciplines to make reasonable use of the distinction between science and technology, between the accumulation of knowledge and the application of it. Do not many of our arguments about psychologists and psychiatrists derive from a neglect of such a division?

The reason why I have chosen the title “Psychologists: non-medical and medical” is this. Psychologists may be scientists or technologists; and they may be, also, non-medical or medical. A psychiatrist—as his label suggests—is one kind of medical technologist, who belongs to the same category as the paediatrician, the gynaecologist, the aural surgeon and other specialist practitioners. He is a medical technologist who happens to be concerned with the diagnosis, treatment and prevention of abnormalities of behaviour which do not appear to be mainly physical in origin. He will probably have some interest in “the theory of it” too, and will to that extent turn up on the scientific side of the fence as a psychopathologist.

But psychiatrists and psychopathologists are not the only medical psychologists around. Among our pioneer British psychologists were several who were medically qualified but who were not primarily either. I am thinking here of Rivers, McDougall and Myers. Even MacCurdy, though he held for many years a university lectureship in psychopathology, had interests of such breadth that it would be hard to recognize him as a psychopathologist first and foremost. We have successors to men like these in our Society now; but they are few, too few.

Let me put this point another way. Psychology, as I see it, is one. But it has its scientific and technological sides, the pure and the applied, the theoretical and the practical. It has a need, on both sides, for people representing a diversity of other training and experience. It does not really matter whether they have qualified previously in medicine or in engineering or in law or in anything else, provided they are able to contribute in some way to the advancement of our science and the development of its various technologies.

The fact that there has been, in the last half-century, greatly increased activity in the particularly important technology called psychiatry has, I suggest, given rise to misunderstanding about the relation between it and the rest of psychology. Nor has the situation been improved by the further fact that many—perhaps very many—psychiatrists seem to know of psychologists only, or at least chiefly, as non-medicals whose function is to give them diagnostic aid. In short, psychology is not to be regarded as a handmaid of psychiatry. Psychiatry is one of psychology’s own technologies, just as it is a medical technology; and psychopathology is the branch of

scientific psychology which is specially concerned with the theoretical foundations of that particular technology.

Let me finish by putting forward, on the basis of what I have said, some purely personal reflections on the future of what has for many years been called “the medical section” of the Society. As most of us are well aware, we have been through troublous times. I shall make no attempt to describe [11]them. It is enough to say that a few months ago fourteen past chairmen of the medical section signed a letter—written to me, as president of the Society—in which they urged that the main interest of the section had long lain in the field of treatment. They expressed the opinion that it should stay there. They did not state explicitly that it was an interest in treatment carried out mainly in accordance with Freudian and Jungian concepts and methods, but it was generally understood that that was what they meant. Now it is quite well known that there are some—possibly many—in the Society who disagree with this view, and who would like to see the aims of the section broadened considerably. A deadlock has been reached. Indeed, it was reached long ago. It shows no sign of being broken. But broken it must be, somehow.

Although the compromise solution that readily springs to mind—that of having two separate sections, or two sub-sections of one section—is apparently regarded unfavourably by many, I wonder whether it might not be the best? The present medical section has undoubtedly had for years the character ascribed to it by the fourteen past chairmen. When I decided to apply to join the medical section myself, some twenty years ago, I asked Dr. Myers whether he would propose me. He said he was quite prepared to do this. “But,” he asked, “wouldn’t it be better if you asked one of the Tavistock people?” The medical section has been, in a sense, a society within the Society, largely out of touch with the rest of the organization. But could we not let it remain there, pursuing its chosen aims? And could we not have, additionally, a section with rather different aims, concerning itself more with psychopathology than with psychiatry; that is, more with science than with technology?

It would not be necessary for either section to be called “the medical section”. Indeed, we might well drop the adjective “medical” altogether; and, as the present section appears to prefer the word “psychotherapy” to the word “psychiatry”, one might be the psychopathology section and the other the psychotherapy section. Perhaps psychiatrists of persuasions other than those strongly represented in the present section might be inclined to attach themselves to the psychopathology section; but I doubt whether that would cause either difficulty or harm.

Admittedly, the creation of another section in this field would raise problems for the *British Journal of Medical Psychology*—problems not only of nomenclature but also of finance and control. But that is another matter. I suggest—and this is my very last point—that our concern in dealing with this particular matter should be to see that all members of the Society who are interested in disturbances of behaviour, whether or not those members are medically qualified, and whatever may be the balance of their interests between theory and practice, feel that they have been provided with a satisfactory forum for the full discussion of their problems.