

Correspondence.

"Audi alteram partem."

THE STUDY OF APPLIED PSYCHOLOGY.

To the Editor of THE LANCET.

SIR,—During the last few months several papers on the psychology of shell shock and cognate conditions have been published in your columns. One striking feature is the attraction which such problems have for a wider intellectual circle than the professed psychologist or psychiatrist, a fact which is well exemplified by the case of the distinguished anatomist, Professor Elliot Smith, whose contribution to THE LANCET emphasises not only the interest but the profound importance, both social and individual, of the topic which he deals with. Your leading article of April 15th contrasts the admirable paper of Dr. Bernard Hart on the psychology of rumour with the spiritualistic fantasies of M. Maeterlinck, which are rightly censured, though with regret at the degradation of a great poetic genius. Two books, one by Sir Martin Conway, the other by a well-known surgeon, Mr. Wilfred Trotter, have recently been published on the psychology of the crowd.

The newspapers teem with material of surpassing interest to the psychologist. There we see the peace-bred politician, accustomed to be passively impelled along his victorious path by the united opinions of a great party, now confronted with the mandate to lead, a rôle which his education and experience render him singularly unfitted to fill. We see the permanent official, inured to routine and a wholesome dread of responsibility, suddenly called upon to reorganise his department on new lines and to expand it to meet the urgent requirements of a critical situation, a task which he has never contemplated and is usually little adapted to. We see the multitudinous critics, accustomed to the apparent freedom of individual thought which is the offspring of a democratic environment, voicing their panaceas in the press with little result but to make confusion worse confounded. Even our scientific experts, Fellows of the Royal Society though they may be, are so narrowed by the cramping influences of lives passed in the laboratory and class-room that they show a lamentable inability to realise the broader aspects of the struggle, and, above all, fail to do justice to the practical problems which it has been their habit to view with Olympian indifference. Every one babbles of causes and effects with a logical accuracy which is only stultified by false premises. In spite of the proved failure of the theorists of the French Revolution and after—Rousseau, the Utilitarians, and *hoc genus omne*—and because of the growth and predominance of rationalism and its many favourable and advantageous results, we are more than ever convinced that man differs from the lower animals in being endowed with reasoning faculties and works out his salvation by them.

Yet we are equally convinced that not only our bodies but our minds have evolved from those of lower species, and that even the individual in some modified manner recapitulates his phylogenetic history in his own development. Comparative psychologists like Professor Lloyd Morgan have done much to elucidate the mentality of lower animals and the parts played by instinct and intelligence. Genetic psychologists like Professor Mark Baldwin, whose escape from a dastardly death at the hands of the Germans we all rejoice in, show how great a part instinct, modified indeed but clearly recognisable, plays in the mentality of the child. Social psychologists like Professor William McDougall further demonstrate how instinct permeates and pervades life from the cradle to the grave. Anthropological psychologists like Durkheim and his school, and especially Lévy-Bruhl, emphasise the corporate mentality of primitive races, its emotional responses, its "prelogical" indifference to incompatibles, and, above all, its predominant group-unity. The evolution of the individual mind and its gradually increasing segregation from the communal mind has yet to be traced down the course of the ages. When that is done we shall have attained a scientific psychology of history which will replace the speculative philosophies of history, which are the natural outcome of transcendental philosophy. Thus shall we lead up by slow but sure progress to a really scientific comprehension of the motives,

and therefore of the actions of individuals in their social relations. Psychology, after centuries of sterile individualistic introspection, has at last become a practical science, with all knowledge for its realm. Sooner or later its teachings must be recognised as the only sure foundation for education in all its branches. By it alone can any truly scientific guidance be given to democratic thought and action.

This long preamble is a brief and inadequate review of some of the reasons why I think the time is ripe for the formation of a society to deal with the innumerable problems of what may be called applied psychology. Why do I ask you, Sir, to ventilate this suggestion? First, because I think that scientific psychology must be founded on physiology, and that those who have been well trained in physiology and neurology must be the guiding spirits of any such enterprise. Second, because morbid psychology has advanced farther along the practical lines of an applied psychology than any other branch of the subject. Third, because the problems of this nature which the war has so strikingly brought to light have revealed the widespread interest in the subject as well as, to many people, the inadequacy of the methods available for dealing with them. Fourth, because I think this is the best way to obviate the greatest danger which such a society would be confronted with—viz., over-popularity. It would be fatally easy for it to degenerate into a medium for pseudoscientists, faddists, and fanatics. Already scientific eugenics has had reason to pray to be saved from some of its friends. To avoid such a disaster I think (1) that the society must be run on oligarchical lines—the council must have complete control; and (2) that at least half of the council must consist of professional psychologists and psychiatrists of established reputation, the remainder representing other interests. It would be easy to enumerate specially qualified anthropologists, classical scholars, historians, and so on. To avoid the dangers of too autocratic a government I would advocate a large committee on the above lines, with a small executive subcommittee. Such a society holds out an alluring prospect of useful work in many spheres of public endeavour. The argument that the present time is little suited for embarking upon a project of this nature is worthy of deferential consideration, but I would submit that there are obvious reasons why it would be wise to coördinate the discussions of the many problems which the war has exhibited in an acute form. The universal cry of the moment is for organisation of effort. Doubtless we shall muddle through now as of yore, but we are no longer content to muddle on indefinitely. Now is the time to lay the foundations of future organisation, and I would point out that the organisation of a democracy like ours is a far slower, subtler, and more complicated task than the organisation of a pack of wolves (like the Germans) or a flock of sheep.—I am, Sir, yours faithfully,

J. HERBERT PARSONS.

Queen Anne-street, W., April 15th, 1916.

INFLUENCE OF FEBRILE CONDITIONS ON INOCULATION AGGLUTININS.

To the Editor of THE LANCET.

SIR,—In recent articles Professor G. Dreyer and his co-workers have commented adversely on certain methods in common use for measuring agglutination, and also on certain results obtained in persons inoculated with B. typhosus. I should like to refer to three of these questions—(1) the value of the microscopic method; (2) the effect of febrile conditions on inoculation agglutinins; and (3) the comparative value of a positive reaction to B. typhosus in inoculated and non-inoculated persons.

1. The value of the microscopic method. Dr. E. W. Ainley Walker in THE LANCET of Jan. 1st argues that this method is unreliable. None of his reasons applies specially to the microscopic method, and every argument he advances applies equally to the macroscopic or any other method which involves the use of living cultures. Bacteriologists are quite aware of these difficulties, and have to be constantly on the look out to avoid them, and whatever their importance in theory, in practice undoubtedly they are avoided. These methods, microscopic, macroscopic, and mixed, have been in use for many years, and their results are confirmed by the diagnosis of clinicians and by post-mortem examination. If Dr. Walker wishes us to believe