

MEDICINE AND PSYCHOLOGY.

To the Editor of THE LANCET.

SIR,—Certain aspects of modern psychology have been presented in your columns recently.

May I draw attention to the following? In Dr. W. McDougall's lucid introduction to *Psychology in the Home University Library* he defines psychology as "the positive science of the behaviour of living things. To accept this definition is to return to the standpoint of Aristotle, and to set out from generally recognised facts, unprejudiced by theories."

From the standpoint of this definition there is evidently a very wide field of investigation open to psychology to-day. And in considering some of the problems with which we in the medical profession are confronted there seems special need to maintain a balanced judgment as free from bias as possible. A hasty process of deduction can be of little use in this respect; and the slower and surer method of induction would appear to be more hopeful of fruitful results. To quote Bacon:—

Man, being the servant and interpreter of Nature, can do and understand so much and so much only as he has observed in fact or in thought of the course of nature (*de naturæ ordine re vel mente observaverit*): beyond this he neither knows anything nor can do anything. —(*Novum Organum*: Aphorism I.)

It cannot be that axioms established by argumentation should avail for the discovery of new works; since the subtlety of nature is greater many times over than the subtlety of argument. But axioms duly and orderly formed from particulars easily discover the way to new particulars and thus render sciences active. —(*Aphorism 24*: Book I.)

And to carry the principle a step further—into the realm of psychotherapy—I cannot help thinking that its application will prove both illuminative and fruitful.

There is ample material at hand in practical results which have already been achieved in England, America, and on the Continent. And these results bear intimately upon general practice as well as upon some of its special departments.

..... Even if the breath of hope which blows on us from that New Continent were fainter than it is and harder to perceive; yet the trial (if we would not bear a spirit altogether abject) must by all means be made. there is hope enough and to spare, not only to make a bold man try, but also to make a sober-minded and wise man believe. —(*Aphorism 114*.)

I am, Sir, yours faithfully,

HERBERT SHORE.

Esmond-road, Bedford-park, W., May 11th, 1913.

NECROSIS IN ECLAMPSIA.

To the Editor of THE LANCET.

SIR,—I have read with much interest the paper by Professor R. Jardine and Dr. A. M. Kennedy¹ on symmetrical necrosis of the cortex of the kidney in eclampsia, and more particularly the two tentative conclusions of the latter—namely, that he is "inclined to the view that the cause of the necrosis in the kidneys is the same as that which causes the necrosis in the liver" and that "the thrombosis in the smaller cortical renal arteries is secondary to the necrosis of the cells." Dr. Kennedy then asks what it is that initiates the necrosis, and submits that it is some factor in the eclamptic toxin. I am very glad to agree with him, but hope that it may be possible to go a little further and to define this factor more closely.

My own observations on necrosis in eclampsia have been confined to the liver, where alone have I seen it. In previous communications² I have strongly urged that this necrosis is largely if not entirely due to hæmagglutinative thrombi—that is to say, to an actual occlusion of the vessels with massed red cells without the intervention of fibrin. That these occur in the livers of eclamptic cases has been demonstrated by Flexner, Welch, and myself. Further, Pearce has shown that an experimentally induced hæmagglutination is capable of causing a focal necrosis of liver cells, and has very clearly proved the interrelation of this hæmagglutinative with a hæmolytic element in the production or prevention of necrosis. The full references will be found in my papers quoted.

It appears that this hæmagglutination occurs most readily where the circulation is very slow, and although such

thrombi have not to my knowledge been described experimentally in the kidney, still the site of the focal necrosis as given by all writers on this form of renal destruction is much in favour of such an origin. It would be extremely interesting and important to learn from Dr. Kennedy whether any appearances of this nature are to be seen in the third and earliest of the three cases now reported. In the microscopical report of one of the two livers examined Dr. Kennedy comments on the very great dilatation of the capillaries that was present. This may be similar to the extreme congestion noted by Pearce in his experimental work before the onset of necrosis.

In conclusion, I have to congratulate Professor Jardine on his unique experience of six cases of this very rare condition.

I am, Sir, yours faithfully,

H. LEITH MURRAY.

Liverpool, May 13th, 1913.

ARTIFICIAL PNEUMOTHORAX — AND AFTER? THE MECHANICAL FACTOR IN THE TREATMENT OF PHTHISIS PULMONALIS.

To the Editor of THE LANCET.

SIR,—In view of the organic change which within the past 12 months has radically modified the conditions of our professional existence, it is necessary to review our relation to the problem of consumption, which we are now under contract to cure. Henceforth not only have we a responsibility to pathology and therapeutics, we are responsible also to the State, which, now judge as well as paymaster, is an interested partner in our pathological and therapeutical problems, not alone financially, but under a conscience clause. It has undertaken the welfare of the population, hitherto from time immemorial under our sole charge, and it stands committed not to deny to any individual under the rates the genuine cure. We can no longer escape a categorical statement of the endorsable value of our therapeutical assets. Our pathology is monumental. How much longer must it remain ornamental? As to our therapy, having discarded for a while the entire pharmacopœia, we are reduced to a game of battledore and shuttlecock between syringe and sanatorium. It is enough for a fair critic that Batty Shaw is still unconvinced by the one while Wilkinson still distrusts the other. The sanatorium is at best a mere preventorium—opens its doors to every earliest case; yet how many are still discharged with progressive phthisis! Tuberculin scorns it, and bargains for "neither too late nor too early." Yet within that interval of its own selection it fails more often than not.

How much longer shall we wait for the authoritative jury which ought to decide between fact and opinion? Meanwhile, is it not time to resume our premicrobic consultations in the grotto, where Bodington left them, and to question Nature once more whether there might not be some other cure elsewhere? Forlanini had consulted her in 1882, just within the bacillary era, but eight years before bacteriotherapy had shed its elytra. "Is it to be excision or artificial pneumothorax?" The surgical proposition was pre-Listerian, and therefore problematic. The other was well supported by solid fact, ripe for discussion, but seemingly incapable of it in a microbic atmosphere, except by him alone; yet brilliantly acted upon in 1885 by W. Cayley, in England, so slow to follow up his pioneer operation, although Carson long before had laid an experimental foundation for it. Forlanini's non-bacillary, mechanical cure was a singular instance of a method arising *d'emblée*, the offspring of a master thought, in full panoply and ready for immediate action, but too heroic, too magnificent, for practical tactics, fraught with limiting risks greater than have yet been registered, and therefore bound to pass away as ineligible for the universal cure, yet imperishable in its teachings, which I venture to believe contain that cure. At the latest hour, when the profession, and in this country the State, are pressing for a decisive answer, it has cut the Gordian knot, and it now demonstrates to us that there is a cure elsewhere—the cure of Piorry (1833), the cure of Freund (1858), the cure of Forlanini (1882); and that that cure, though this had not been hitherto specified categorically nor yet perhaps thoroughly realised, is the mechanical cure.

¹ THE LANCET, May 10th, 1913, p. 1291.

² Journal of Obstetrics and Gynaecology of the British Empire, October, 1910. Ibid., February, 1913. British Medical Journal, January, 1911.