DUALITY OF THE MIND.

To the Editor of THE LANCET.

SIR,—May I trespass on your space for the purpose of rectifying an error as to the nature and object of my paper on the duality of the brain,—an error arising from the change in some parts from the third person to the first, made by the transcriber, I presume, for the purpose of rendering the whole more continuous.

I expressly stated that it was part only of the introductory chapter of a work I was preparing for the press; that I only made assertions and did not offer proofs, for which one, or even half a dozen evenings, would not suffice; that I did not ask for assent till a graduated series of evidence should make it impossible to withhold it; and that my object was only to excite attention and record the theory, that no other person might claim a priority therein. Of course the objections raised had no meaning, for the parties could not possibly know what they were opposing.

The facts and inferences I have to bring forward form a mass of proof so superabundant that I cannot conceive it possible they should fail of producing conviction. As to the novelty, it cannot be questioned, since there is not a single writer in this or any other country of Europe, who has given the remotest hint on the subject, if, indeed, Dr. Holland be not, to a certain extent, an exception. I am, Sir, your obedient servant,

A. L. WIGAN.

London, April 20, 1844.

THE NON-DUALITY OF THE BRAIN.

To the Editor of THE LANCET.

SIR,—An article in last week's LANCET, headed "Duality of the Mind Proved," &c., has arrested my attention, and with a true spirit of philosophic inquiry I take the matter up, hoping that the author will satisfactorily account for a certain phenomenon, hereafter to be related, which appears to me to be a very strong proof of the non-duality of the mind.

At page 39 (commencing at the fifth line from the bottom) the author says, "The two hemispheres of the brain are, strictly, two distinct and perfect organs, at least as distinct, and each respectively as perfect as the two eyes." He means that the hemispheres are distinct and perfect organs in the performance of their *mental* functions, as may be seen in his fourteenth proposition, where he compares the functions of both hemispheres to the eyes. He says that "one cerebrum may be entirely destroyed," &c., "may be annihilated, yet the mind remain quite perfect, as at any former period, and capable of exercising its functions in the same manner and to the same extent that one eye is capable of exercising the faculty of vision, when its fellow is injured or destroyed." I doubt that its *complete and entire destruction* ever happened, that is to say, that the hemisphere has been destroyed *in toto*, to the very origin of the nerves.

But now to the point of objection as to the mind's duality. How is it that an *immediate* comatose state supervenes in depression of the skull on one small portion of a hemisphere, and on removal of that pressure, that the mind's activity returns? To elucidate the matter more fully I shall quote Sir Astley Cooper on the subject of depression. He says, "In order to ascertain whether the symptoms arising from depression would come on *immediately* after the accident, I tried the following experiment:—I applied the trephine to the cranium of a dog, and took out a portion of the bone; I then separated the dura mater from the bone; for I found that I could make no impression on the brain until I had done so, and then pressed upon it with my finger. At first the animal did not seem to feel it, but, upon pressing more deeply, it produced pain and irritation. Upon still increasing the pressure he became comatose and fell; I kept him in that state for five or six minutes, when, upon removing my finger, he got up apparently little worse for the operation."

Now this seems to me to prove the mind's non-duality or synchronous mental action of both hemispheres, or

why should *complete* insensibility be induced in the aforementioned case of compression? It may be said that the depressing power was transmitted to the other hemisphere, or that the depressing power had acted on the circulation of the blood, and so affected the cerebral mass; but such ideas are not admissible when we consider the soft and yielding substance of which the brain is composed. It may be likewise said that the other hemisphere sympathises with the one compressed; but if it so far sympathises as to annul its power of receiving impressions, then it can no longer be said to act independent, as in the case of the eyes. One eye may be injured or cut out without destroying the function of the other *completely*, though it may suffer much from sympathy; but such does not seem to be the case respecting the hemispheres of the brain.

We know that considerable portions of a hemisphere have been lost, and complete mental activity remain; but still this goes but a little way to prove the mind's duality. Nay, we have reason to suppose that portions of *both* hemispheres have been lost and complete mental activity remain, when we read Sir A. Cooper's case of transverse fracture (opening rather) of the os frontis, where a considerable portion of brain was lost, related in his lectures under the head, "Wound of the Brain," although Dr. Wigan says that the least injury of both hemispheres is incompatible with mental activity.

Hoping to be more enlightened on the subject through the medium of your valuable journal, I remain, Sir, your's very respectfully,

MICHAEL RYAN, M.R.C.S.

Ashley, Market Drayton, Salop,
April, 6, 1844.

VITILIGO INFANTILIS.*

To the Editor of THE LANCET.

SIR,—Allow me to make a few remarks on the case of vitiligo infantilis, as it is one of unusual interest; first, from the novelty of the disease attacking so young a subject and so very extensively; secondly, from the incorrect diagnosis formed of it by the author of the notes, and by many others who had an unusual opportunity of judging correctly of it. The proper time, I must acknowledge, for making my strictures, was when the child was exhibited to the members of the Medical and Chirurgical Society; but not being present is my excuse for omitting to do so.

May I ask, how was it that the authors on cutaneous diseases, who were present, and had presented to them a living case, thereby removing any doubts, as simply the notes or histories of cases are frequently calculated to do, were silent? At the nick of time they might have proved the value of their authority, and, no doubt, from the profession, in due time, would have reaped the fruits of it.

Dr. Gregory, in his notes, says the disease commenced at four months old, with some smooth, white, shining elevations. Now, if we substitute the word tubercles for elevations, we have Dr. Willan's definition of the commencement of vitiligo, which will not do, unfortunately, for the present case; and as it had existed fourteen months when the Doctor first examined it, he is not prepared to prove that it did begin so, nor by information derived from the mother. If I am correctly informed, Dr. A. T. Thomson, after comparing the case with numerous plates, inferred that it must be vitiligo (the word derived from "vituli," the white and glistening appearance bearing some resemblance to the flesh of calves); the present case does not even tally with the simple definition of the name. Dr. Gregory candidly tells us that his notions of the disease were very confused, not having seen one before; therefore he must excuse my contradicting and stating that the disease rather commenced with small round elevations, remaining for some little time stationary in their growth, and at this stage of the disease requiring the eye of a connoisseur to discriminate it from the commencement of lepra; but practical experience will readily afford the proper clue, for as the disease progresses the pathognomonic character of the true class to which

* Properly, Erythema Sulcatum.

it belongs becomes more beautifully developed, readily allowing it to be assigned to its proper order, the third of Dr. Willan, erythema; but in order to make it more apparent to the profession it would be necessary to rewrite the article "Erythema," which is very faulty; therefore, at present, it must suffice to say that I have frequently met with the disease, presenting always one uniform likeness, the reticulated or sulcated appearance of the skin being increased in proportion to the diseased thickness of the cutis and length of time it has existed, as well as the size of the patches, the patches sometimes consisting of many, at other times of a few only, such as two or three, or a solitary patch of no small magnitude, according to its situation, the favourite ones being the back of the neck, over the loins, and the fore-part of the thighs, accompanied with more or less irritation, and shaded with a dirty chocolate colour. Like erythema marginatum and other chronic forms of erythema, this species may last, if not treated by appropriate remedies, for years, to the no small annoyance of the patients, and the sooner that Dr. Gregory sets about in earnest to effect a cure the better, and it need not interfere in the least with the important function of teething. Nature will rather thank him for his assistance hereafter, if he do his business in a master-like style, which will be proved by the enamel of the teeth; should this be faulty we may rest assured that when this is forming nature is overtaxed,—therefore assist her.

Two important things prevent my saying more, your space and my time. I have the honour to be your obedient servant,

J. H. HORNE, Surgeon.

Princes-street, Coventry-street.

ON THE PRESENT STATE OF MEDICAL PRINCIPLES AND PRACTICE.

To the Editor of THE LANCET.

SIR,—I was much pleased to observe in your Journal on several occasions lately some able remarks on the present state of medical principles and practice. I happened to be engaged at that time upon this very subject, and hoping, from the general circulation of your periodical, that the medical public are aroused to a serious consideration of the present state of the profession. I beg you will oblige me by inserting the following remarks.

Dr. Mackin's letter (page 606) points out some of the grave errors into which the medical theorists have fallen, and the temptations, not easy to resist, of extending without judgment a favourite principle beyond its due bounds; on these subjects, therefore, I need not enter.

"Fas est ab erroribus doceri."

It must be admitted on all hands that the prevalence and success of barefaced quackery in all ages is one of the strongest, if not the strongest, argument which can be brought against the so-reputed scientific practice of the regular physician. We may justly refer the origin and continuance of this widely-spread evil to three causes,—popular ignorance of the importance of medical knowledge in the cure of disease, credulity in the power of nostrums, and the backward state of medical science itself. Perhaps the last of these evils is the least difficult to remedy, and from whom is it more proper that that remedy should come than from us? Is it not as deeply to be deplored as it is strange to believe, that furnished as we should be with the works of centuries, and assisted by the great sciences on which our doctrines are founded, we are still, in the nineteenth century, as far as the precise results of our practice go, almost as uncertain as those to whom anatomy was unknown, physiology a fable, chemistry a fancy, and surgery a charm!

Medicine, which has been cultivated and encouraged by many of the great and learned in every age, remains still in a condition worse than infancy. Why is it not improved in proportion to the progress made in anatomy, physiology, and materia medica, or why has it not kept

pace with those great physical sciences with which it is vaunted to rank. Now these latter, I need hardly say, date their existence almost, and certainly owe their advancement, to the employment of a summary philosophy, recommended and practised by the founder of modern science, who, rejecting the fanciful theories of the schools, declared himself a partizan of no sect, but the humble attendant on and interpreter of nature, not presuming to give laws to, but contented to derive laws from her.

Nor has any one ever yet convinced us that these rational principles, to which natural philosophy, in all its branches, owes its elevation, are applicable to everything but the constitution of man and his diseases.

Let us inquire further wherein consists the difference between a physical theory and what is now called a medical one. The former, well founded on incontrovertible facts, derived from one invariable nature, is certain and sure; the latter resting, if it rest at all, on observations respecting the treatment of individuals, no two of which are alike, even to themselves at different times; thence arises uncertainty and doubt when we wish to apply them to a particular case. The difference above stated is the same as that between a register of experiments and a statistical table. The first is distinctly true in particularity, and therefore admits of no variation in the inference; the second is a collection of averages, and but subserves the purpose of calculating probabilities. And so we find, in fact, the theory of medicine, as at present constituted, is but a doctrine of probability. "Tout est généralité dans les auteurs, tout est particularité dans la pratique."

"It being the nature of the mind of man, to the extreme prejudice of (medical) knowledge, to delight in the spacious liberty of generalities, as in a champaign region, and not in the inclosures of particularities."

Our practice, when successful in one case, is hastily made a rule for the whole race, because we wait not to mark with jealousy and caution all that may have influenced our success, the difference of circumstances,—those differences in the patient himself which make him what he is, his peculiar temperament and constitution, his habits, his idiosyncracies, all those important though minute things which seem to mould each individual of our species, and whether in health or disease make him truly an independent whole, an isolated microcosm. For "the subject of man's body is, of all other things, most susceptible of remedy, but then that remedy is most susceptible of error. For the same subtlety of the subject doth cause large possibility and easy failing, and therefore the inquiry ought to be the more exact." We must believe it of consequence that we should know well the constitution of our patient before we can draw correct inferences as to the probable influence of our remedies. Again, in another point of view, each individual has his nature so different from another that the same morbid cause, as it is called, will be followed by varied results, although applied under the same apparent conditions. Take cold, for instance, as a common yet very important one of these, apply it to the feet of several healthy persons, the results may be, the first will eventually complain of common chilblains, the second of rheumatism, the third of catarrh, the fourth of sore throat, the fifth of colic, the sixth of headach, the seventh of nothing. Can it be denied that temperament, or habit, or original constitution, has nothing to do with the diseases following this simple application. And yet in our reports of cases, drawn up and published for the benefit of future practitioners, no mention is made of these differences when observed,—we dare to write of disease, but disregard the patient, to whom its character is often due. We triumphantly extol our recipes and treatment, but neglect to state what modification they have suffered after administration. Is this "the ancient and serious diligence of Hippocrates?" Some of these reports are like the *ex parte* statements in a court of justice, as if the author had a secret interest in advocating and making good the cause of the accused theory at the bar of public opinion. But in questions like these it is not becoming there should be "two sides," nor would there be if we reserved our judgment. Reports such as these last but their little