ing, common lepra broke out over the body, proving our diagnosis to have been incorrect. The treatment was altered, and the patient slowly recovered.

Authors on venereal diseases usually lay great stress on the colour of the eruption. M. Ricord, however, has shown how frequently this may deceive the practitioner; when the copper colour is present, it may assist the diagnosis, but the disease may be no less syphilitic when that tint is absent. In the commencement of true syphilitic affections, the general surface of the skin may present an earthy unhealthy colour, but the areola about the eruption may be, and will be, found generally rosy. As the eruption declines, it will (particularly if not treated) become darker coloured than in other affections, and present the appearance of boiled ham, as Fracastor, and, recently, Ricord, have observed. If mercury, however, has been used for the primary symptoms, or if iodide of potash has been employed, this discoloration does not occur, and we lose at once the supposed diagnostic mark of the specific disease. In many instances I have known practitioners hesitate in concluding an eruption to be syphilitic, on account of this absence of discoloration, apparently unaware of the effect of previous treatment in modifying tints. We should recollect, that in these affections of the scalp, distinctions of colour are far less visible than on the body, and must be looked for on those parts of the head uncovered with hair. Notwithstanding the exceptions just spoken of, great dependence may be placed on the colour, if conjoined with the other symptoms we are about to mention.

In public practice, and in cases of patients who have not been treated with any form of medicine, we usually find the cervical glands situated in the posterior cervical region swollen and enlarged; and M. Ricord has lately called the attention of the profession to this symptom, as a valuable diagnostic mark in secondary symptoms, on account of its universality. In forming a diagnosis, we should look for traces of the indurated primary sore, which may be often found months after the disease is supposed to be cured.

In private practice, when the primary symptoms have been treated with mercury, we must not expect to find induration, and much must depend on the history which the patient gives us; by the history, I mean the course and relation of one symptom to another, and the period at which they occur one after the other, or of their combination for some months previously. Practical surgeons place little confidence in patients' statements, particularly those of women, who generally have not observed, or purposely mislead, on the points of primary disease. Aware of this, the medical man, before notifying to the patient his suspicions, had better let him trace back the relapses, if any, noting well the intervening period, and the parts of the body on which the eruption appeared, particularly inquiring about the throat. Having, then, thus obtained the information, he has next to consider if it tallies with the usual course of syphilis, and with present symptoms. This mode of inquiry is far preferable to the usual plan of asking a patient (whom you suspect) if he or she has had primary symptoms.

Queen Ann street, Cavendish-square.

(To be continued.)

ANATOMY, PHYSIOLOGY, AND PHRENOLOGY.

By E. J. HYTCHE, Esq., Surgeon, Islington.

ALTHOUGH I do not doubt that the fairly-expressed paper of Mr. Marshall will elicit a reply from some phrenologist who is versed in cerebral anatomy, yet there are one or two points mooted in that communication to which I must crave liberty to reply. The statement of Leuret, that "the organ of destructiveness is larger in some herbivorous than in carnivorous animals, and that the wolf ought to be more submissive than the domestic dog," does not invalidate the conclusions of phrenology, even if his manipulation were correct. The phrenologist never compares the skull of a man with that of a dog, nor even the brain of a cat with that of a dog; but, guided by the light afforded by comparative anatomy, he compares species with species. The correctness of this mode of procedure is shown by the fact, that in man there are parts of the brain, the rudiments of which can alone be traced in certain species, and in others of which they are altogether absent. Such being the case, the impropriety of comparing animals in which there is no true analogy is obvious. Again, that difference in facial conformation, and in the anatomical structure of the head generally, which mere difference in habits rendered necessary, would at once serve to indicate the fact, that the localization of the faculties in different species

would vary; and hence, mental seats, which might be traced in one class, would be entirely erroneous if predicated of another. Following, then, the course so admirably pursued by Vimont, the phrenologist compares specie with specie; and he finds, more or less, a distinct system of organology pertaining to each. If Leuret had found that the largeness or smallness of the organ of destructiveness in two individuals of the same species, and in the same condition, offered no index to the power of the feeling, then, indeed, he would have shown the fallacy of the phrenological doctrine; as it is, he merely restates a fact, which the phrenologist was the first to report, and which he believes to offer no obstacle to the application of the science.

I must confess that I felt startled at the assertion of Mr. Marshall, that the "phrenological view of the function of the cerebellum is unsupported by physiology, and by pathology." It does appear to me, that the ascertained facts that the cerebellum is considerably less proportionally developed in the child than in the adult—that, at the age of puberty, its growth is rapid, and the intensity of the amative feeling correspondingly progressive—that in the female it is much less proportionally developed than in the male head, and the passion less manifest, together with the many cases of diseased cerebella, combined with morbid manifestation, which have appeared in the medical journals presents irrefragable evidence in favour of the localization of that organ. Mr. Marshall has not, however, supported his assertion by any fact; and the value of the objection of Leuret, that he found the "weight of the cerebellum much greater in geldings than in either stallions or mares," will depend on the question, whether the brains compared were of equal or unequal size; in other words, whether the stallion was of less dimension than the gelding.

Again, the statement of Lelut, as cited by Mr. Marshall, that he "found, on an average, in examining 100 brains of idiots and imbeciles, that the posterior and not the anterior part of the head was the one which had suffered most diminution," may be correct, and yet phrenology would remain unaffected. It has been found by Voisin and others, that the idiotic is generally a small head; that, though a large head may be in a state of idiocy, yet that no male adult head under thirteen inches is capable of healthy mental elimination. The cineritious substance is also found of much less thickness and consistence in the idiotic than in the fully developed brain. It is scarcely necessary to add, that there are two kinds of idiocy—the one intellectual, and the other affective; the one connected with defective intelligence, the other with feeble animal energy. If, then, the cases cited by Lelut were those of animal idiocy, his facts illustrate and confirm, rather than overthrow, the conclusions of phrenology.

And here the fundamental principle whereby phrenology has been substantiated, may be stated—it is simply a deduction from matured observation of the brain, or of the forms which have been impressed upon the cranium by the cerebral mass. In pursuing his observations, Gall carried out the Baconian mode of philosophizing; for he observed, and not till he had observed hundreds of cases did he declare his deductions to be sound. Take for example his first phrenological observation, that which pertains to the organ of language. He observed, that those of his schoolfellows who excelled in the acquirement of language possessed prominent eyes; the same phenomena were observed at the university where he studied; and after these observations had been continued for years with the same result, he concluded that the relation was not accidental, but that an intimate connexion subsisted between the portion of brain which is situate over the super-orbital plate, and the power of verbal memory. This principle he never abandoned; but observation, and not the assumption of fitness, indicated the seat of the mental faculties. The question, then, is, if this train of observation be fallacious, wherein does the fallacy consist? Gall took nothing for granted, and arrived at no opinion without years of untiring research. If, for example, he had stated that the appreciation of hues was assigned to the portion of brain immediately over the middle of the superciliary ridge, but deduced the opinion from the observation of one fact only, then, indeed, his dogma would have been worthless; for the connexion might have been accidental, and the deduction overthrown by a larger array of facts. But the case of Dalton is but one of innumerable cases wherein deficiency of the power of discriminating colours has been found concomitant with a feeble endowment of the organ.

It has indeed been stated by Mr. Marshall, that the knowledge of phrenologists respecting the cerebral convolutions is "too incomplete to enable them to assign precise limits to their assumed cerebral organs, or to identify equivalent parts

of the brain in different cases." It is true that the cerebral organs have not been dissected nor developed, nor does the phrenologist profess to possess the power. The manner in which the brain acts, whether considered as a whole, or as a congeries of organs, is at present occult; for though electricity may be the prime mover, the fact has yet to be proved. The organs may be so distinct, that when our knowledge of the cerebral structure is enlarged, the organs may be developed by a skilful manipulator; or it may be that the undulations consequent on the flow of blood to a specific part of the brain may suffice for the performance of its appropriate function. What do we know of cerebral action, save the fact derived by Cooper and others from observation of fractured skulls, that when the mind is inactive, the brain is quiescent, but that in connexion with mentalization, there is cerebral excitement. It may be, then, that there is partial as well as general excitement, though the motion can only be submitted as a supposition. After all, the fact that the organs have not been dissected, does not disprove their existence; anatomists had dissected the nerves for years before it was shown that the nerves of motion and sensation were distinct. The only question is, have we, irrespective of anatomy, sufficient evidence to warrant the assertion that the brain is a congeries of organs?

In addition to the evidence which is derivable from an observation of the connexion which subsists between mental quality and cerebral endowment, other proofs might be submitted which partake of a physiological character. I might detail the cases which have fallen under my own notice, wherein there has been a morbid manifestation of some faculty, accompanied by acute pain in the region of brain assigned as its seat. Yet, inasmuch as such cases do admit of error, seeing that the precise locality—especially in affections of the organ of language—may be mistaken, I shall merely submit two or three cases wherein structural alteration has been found concomitant with morbid mentalization.

Dr. Balfour relates the case of a man who laboured under religious mania; every curse levelled at a sinner in Holy Writ he applied to himself, and his views of futurity were fearful. After failing in one attempt, he succeeded in poisoning himself. His head was examined, and the right organ of veneration—one of the chief ingredients in the religious sentiment—was found to be in a state of chronic inflammation. The membranes covering the organ also presented every sign of disease. Dr. Silliman, of America, attests the case of the late Sir Robert Liston. He had been remarkable for kindness of demeanour; but subsequently his manners experienced a striking change, without any apparent cause. In lieu of that benevolence of spirit whereby he had been characterized, he became hasty and passionate, and his language violent, if not frantic, in the extreme. It may be mentioned that his intellectual faculties were unaltered. On the dissection of his brain, the frontal lobe was found unimpaired; but the organs of language and combativeness were found to be diseased.

Cases like these present such forcible proofs that, whether the organs are ever to be dissected or not, they are really distinct, that to adduce other evidence is unnecessary. All the phrenologists ask is, presuming these cases to be reported correctly, wherein does the fallacy consist of thinking that the disease of the cerebral organ and the morbid mentalization bore the relation of cause and effect? Far be it from us to ask our opponents to follow, implicitly, the doctrines of Gall and Spurzheim, and believe because they assert. If their doctrines will not bear the test of a fair examination—not the objections which arise amidst book-study, but those which are found on an appeal to nature—then let the science fall; for if phrenology be against facts, it loses its sole title to respect. The same field is open to the antagonist as to the believer; and we claim an adherence to phrenology, not because Gall and Spurzheim believed, but because God has inscribed its truth on the forehead of man!

Hemingford Cottages, Islington, Dec. 2nd, 1845.

ON THE ATOMIC THEORY AND ELECTRIC CONDUCTION.

By G. P. T. HILL, Esq., Filey.

HAVING lately had the pleasure of perusing the fourth lecture of Liebig on "Organic Chemistry," as published in your journal of April 14th, 1844, my attention was forcibly drawn to the remarks of Dr. Faraday, closely following it, in which, assuming the truth of the atomic theory, he would represent the space between the ultimate atoms as the true medium of electric conduction.

The following objections to this hypothesis I should be

obliged by your publication in *THE LANCET*, if you consider them worthy a space in so valuable a journal:—

I. Water is a conductor of electricity, but if a compound salt, or a portion of sulphuric acid, &c., be added to it, its conducting power is materially improved, though at the same time its density is also increased. If space be essential to conduction, we ought to expect a corresponding diminution of this power, but the reverse happening, it is clear that this faculty does not depend on the existence of space between the atoms of a body.

II. A rod of metal is a conductor, a similar rod of glass a non-conductor. But space is in both these substances continuous throughout the mass, therefore in the one case it exhibits properties entirely different from the other. Neither can its conducting or non-conducting power be ascribed, in the one case to its union with a metal, or in the other to its union with glass, for it is inconceivable that there can exist any difference in the space between the atoms of glass and that between those of metal. The conduction by bodies is therefore a peculiar force solely resident in themselves, which force is not possessed by all.

III. Some bodies become conductors when heated. This cannot be attributed to the increase of space between their atoms, but may be easily accounted for by considering the close connexion that subsists between the particles of heat and those of electricity, and the one may thus be said to render a substance susceptible of the other.

IV. Potassium is a conductor. Its oxide is a non-conductor. The latter has also a greater specific gravity than the former. This affords no argument for the necessity of space for conduction, for we may surely ascribe its non-conducting power to the change of properties induced by the union of the metal with oxygen. This view is of course applicable to the compounds of sodium &c. with oxygen.

Filey, Dec. 6th, 1845.

FACTS AND ARGUMENTS IN SUPPORT OF "THE HUMANE SYSTEM OF NON-RESTRAINT IN THE TREATMENT OF THE INSANE."

By WILLIAM SMITH, Esq., late House Surgeon to the Lincoln Lunatic Asylum, and to the County Hospital at Lincoln.

ALTHOUGH the principle of non-restraint may now be considered as firmly established, and the practice formally adopted in some of the largest and best regulated asylums in the kingdom, still the principle is faintly contested by many superintendents of public asylums, and not a few of the proprietors of the licensed houses.

That insane persons still suffer from the baneful effects of instrumental restraint, the following graphic description, from the official report of the Commissioners in Lunacy to the Lord Chancellor, will abundantly demonstrate:—

"At the asylum of West Auckland, first visited on the 5th of December, 1842, there were thirteen males and sixteen females; each sex had one sitting-room, with windows that did not admit of any prospect from them, and the violent and quiet, and the dirty and clean, were shut up together; there was only one small walled yard, and when the one sex was in it, the other was locked up. One of the male patients said that they were made so tender by their confinement that their health was destroyed. There were two small grass closes belonging to the house, but they appeared to be little used for the employment of the males. In the small, cheerless day-room of the males, with only one unglazed window, five men were restrained by leg-locks, called hobbles, and two were wearing, in addition, iron handcuffs, and fetters from the wrist to the ankle; they were all tranquil. The reason assigned for this coercion was, that without it they would escape. One powerful young man, who had broken his fetters, was heavily ironed, and another was leg-locked and handcuffed, who was under medical treatment, and in a weak state. One woman was leg-locked by day and chained to her bed at night; chains were fastened to the floors in many places, and to many of the bedsteads. The males throughout the house slept two in one bed.

"I have seen one secured in a dark and loathsome shed, lying extended upon straw, (for the space did not admit of his standing erect,) in a state of filth that I dare not describe; a second was fettered and manacled, and basking in the public street, exposed to the rude gaze of the thoughtless passers by; a third I have seen led about the streets, and even to church, in the restraint of a strait-waistcoat; a fourth" (I copy from a letter now before me, of a justice of one of the Welsh counties) died some time ago, in the most deplorable state, having been for about fifteen years chained."