

Examination of the objections made in Britain against the doctrines of Gall and Spurzheim / by J.G. Spurzheim.

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EXAMINATION
OF THE
OBJECTIONS
MADE IN BRITAIN
AGAINST THE
DOCTRINES
OF
GALL AND SPURZHEIM.

BY
J. G. SPURZHEIM, M. D.

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1817.

EXAMINATION

OF THE

OFFICERS

MADE IN BRITAIN

AND

REMARKS

ON THE

RESULTS

OF THE

EXAMINATION

1811

INTRODUCTION.

DISCUSSIONS, properly conducted, are of great utility. For that reason, I am always ready to examine every objection against our doctrines. But I am sorry to observe, that scientific pursuits are so often degraded by selfish passions and the spirit of party;—that literary publications are employed for the purposes of calumny and detraction;—that invectives are used instead of arguments;—and that, by praising friends and blaming rivals, the progress of the arts and sciences, and the improvement of man, are mightily retarded.

Such behaviour I will never imitate; nay, the illiberal and uncandid manner in which some British Reviews have taken up our investigations, has hitherto prevented me from attempting justification. As, however, many persons have no inclination, and a greater number no time for comparing the original works with the reports of the critics; and as in science the majority of readers believe, without examining for themselves, I cannot entirely avoid controversy. We have never published a separate answer to single pamphlets, but merely considered the objections in our Lectures or in our Works, when treating of the respective objects. Our maxim is, never to fight with darkness, but to endeavour to bring light.

I am now to submit to the public some observations on the objections of our principal antagonists in Great Britain, confining myself to the points in question, and depending on the moral sense, the judgment and observation, of my readers. In short and concise expressions I will state the real object of our inquiries, and the true import of our propositions, and then compare the interpretations of the chief Reviews, especially of the literary gospel of Edinburgh. At the same time I will mention an antagonist, who was at first anonymous, but did not long conceal himself; who then appeared as an author on the structure of the brain, and at last as a historian of the anatomy of that organ.

The Edinburgh Reviewer speaks (NO. 49. p. 229.) of "a conscientious discharge of duty on this occasion;" it therefore is right to name him accordingly. The author of the Treatise on the Brain, in a pamphlet, asserts, that the anatomy of the brain is imperfectly known, even to the distinguished teachers of the medical art in Edinburgh; that the persons, I have addressed, never perhaps have completed their studies in this department, (p. 4.); that I have shown the corpus dentatum to spectators, most of whom had never seen it before, and not one of whom had rendered himself familiar with its appearance by dissection," p. 73.* Hence, if there be only

* As the reader may wish to know who my auditors were, I will mention the names of some gentlemen. At the first demonstration were present, Dr John Thomson, Prof. Regius of Military Surgery; Dr Barclay, Lecturer on Anatomy and Surgery; Dr Duncan, junior, Prof. of Medical Jurisprudence; Drs Emery and Irvin, of the Military Staff. At the second, were Dr Rutherford, Prof. of Botany; Dr Home, Prof. of Materia Medica; Dr Thomas Brown, Prof. of Moral Philosophy; Prof. Jamieson; Drs Farquharson, Dewar, Sanders, Anderson, and a great number of professional gentlemen. At the Physical Society I gave the demonstration in presence

one person in Edinburgh who can judge of the appearances of brown and white, he deserves the name of anatomist *par excellence*. As in his Treatise on the Brain he states (Pref. ix.), that he has scrupulously avoided the introduction of any physiological matter; and as in the pamphlet he maintains, that the anatomy of the brain, *in a physiological point of view*, is fortunately not of essential consequence in the practice of medicine, (p. 3.) I will style him a mechanical dissector. Another name which he merits, is that of historian, because he has compiled facts, excellent indeed,—concerning the history of the anatomy of the brain.

The profession of a Critical Reviewer is acknowledged to be very extensive; his infallibility is understood:—Hence, without any previous study, he can decide all questions on Anatomy, Physiology, Pathology, Philosophy, the Arts, and, in short, on all the branches of knowledge; nay, he can criticise books without reading them. He is never at a loss, and arrogates at least the appearance of talents. If his own authority is not sufficient to impose on the public, a sacred band of literary Oligarchs answer for his correctness, and, for that reason, he assumes the mighty WE of sovereignty.

Every one will perceive, that our adversaries are very witty men. They deal extensively in the ridiculous; and when they have leisure to become serious, they speak of

of Dr Monro, junior, Prof. of Anatomy and Surgery; Drs Rutherford, Barclay, and Sanders; Mr Bryce, President of the College of Surgeons; Mr George Bell, and a numerous audience of medical gentlemen. Since that time, I have often repeated these demonstrations in private parties, and always to the satisfaction of the spectators. *It is worthy of notice, that the essential point alluded to, was, whether there is brown matter in the corpus dentatum?* This had been denied by the *Edinburgh Review*, p. 264.

the motives and *dangerous consequences* of our inquiries; but their generous minds need not be apprehensive, since they declare our doctrines "incredible and disgraceful nonsense, absurd theories, trash, and despicable trumpery." If that is the case, while, as they admit, we make proselytes, they have, indeed, very little confidence in the discernment of their countrymen. Why do they not rather listen to our constant declaration, that one fact, well observed, is more decisive to us than a thousand opinions, and all the metaphysical reasoning of the schools; and that facts alone can expel such intruders as our doctrines?

These observations will be divided into three Chapters. The first will contain Anatomical, the second Physiological, and the third Philosophical considerations.

OBSERVATIONS, &c.

CHAPTER I.

ANATOMY.

SECTION I.

WE have examined the anatomy of the nervous system in general, and of the brain in particular, *in strict relation to physiology and pathology*. This we have repeated in our demonstrations and in our works. When we delivered the Memoir on our Anatomical Inquiries to the French Institute in 1808, we mentioned in a letter to that learned Society, that we present “*Une description du Systeme Nerveux, moins d’après sa structure physique, et ses formes mécaniques que d’après des Vues Philosophiques et Physiologiques que des hommes habitués à des considérations superieures ne refuseront point d’acquiescir.*” The same idea is expressed in my work on Physiognomy, p. 13.; and in the article CERVEAU for the *Dictionnaire des Sciences Médicales*. Vol. iv. Paris, 1813, § 1. and 2. In our works we have positively stated, that physiological and pathological facts have induced us to examine the structure of the brain.

The conscientious critic, however, instead of examining our views, and of judging accordingly, thought it his duty only to abuse our propositions, (or, in their polite phraseology, to cut them down), and to declare that "in this department we have displayed more quackery than in any other; and that our bad faith is here the more unpardonable, that it was so much more likely to escape detection," p. 254. The anatomist *par excellence*, has scrupulously avoided the introduction of any physiological matter. He confines himself to descriptions of mere mechanical forms, measurements, and shades of colour of individual and isolated parts.*

* There are, however, many discoveries of that kind in his book, which ought not to be overlooked. He, for instance, has discovered, that the dura mater must be excluded from the membranes of the brain; because "it seems more natural to regard it as forming a part of the sides or walls of the cranium," p. 150.; while other anatomists speak of two lamellæ of the dura mater; one of which belongs to the internal sides of the skull, and the other to the brain.

Another great discovery of the mechanical dissector is the number of cul-de-sacs in the encephalon. A small one is mentioned, p. 84. shaped like a point of a writing pen; another, in p. 98. about a sixth of an inch deep; a third, in p. 99. of a conical shape; a fourth, p. 104.; and two more, p. 112. A deep triangular pit is mentioned, p. 180.

Other anatomists speak merely of two sorts of substance in the brain, of a grey or cineritious, and of a white. The mechanical dissector has first described a variety of colours, such as a brown, a wood-brown, nearly the same as a nut-brown, a dark-brown, a greyish-brown, a reddish-brown, a wine-yellow; a white, an orange-white, a yellow-white, a reddish-white, and a bright white.

Important discoveries with respect to the supposed cerebral nerves will be mentioned afterwards. Here I will only notice his discoveries concerning the brain. He imagines, that, in the natural "situation of parts, the anterior commissure is seldom more than a tenth of an inch in length," while it is continued to the middle lobes; and "he imagines also, that it is placed anterior to the pillars of the fornix, and seems to unite them together,"

Willis, Vieussens, Haller, Vicq d'Azyr, Prochaska, Soemmerring, Reil, Bichat, Cuvier, Portal, Sabatier, and all living anatomists of distinction, examine conjointly the structure and functions of the parts, and even intersperse pathological remarks. Every practical man of the profession will agree with Mr Lawrence, (*Two Introductory Lectures*, p. 116.) when he speaks of separating anatomy and physiology from one another; and says, "What would you think of a person who should describe to you a watch or a steam-engine in this way? who would exhibit to you all the parts, and show their position, without any explanation of their uses; without any reference to that nice adjustment and mutual action, which render the one subservient to the important purpose of marking the division of time, and enable us by the other to execute the most stupendous movements of human labour, and to produce the most striking results of human ingenuity? As I cannot for my own part discern, what purpose of utility, much less what end of interest or amusement, could be answered by a merely anatomical detail; and as the separation of the science of organization from that of life, seems to us *most violent* and *unnatural*, I shall not disjoin anatomy and physiology."

p. 100. while it is quite detached from them. He has discovered, that the appearances which may be seen without actual dissection, or with very little dissection, or by removing the cerebellum, may be called external, p. 95.

An important discovery consists in the invention and application of new names. By this discovery, every thing appears new in the description, at least so far as the names are concerned; and that you may not suspect that you are reading about things which you knew before, the old synonymes are suppressed. This is particularly the case with the description of the ventricles, p. 104. Indeed, such discoveries as the preceding cannot fail to amuse the man of mechanical genius,

Our ingenious mechanician affords novel information, when he tells his readers, that anatomical knowledge of the brain, in a physiological point of view, is *fortunately* not of essential consequence in the practice of medicine; and that skilful and eminent practitioners are satisfied, *and justly so*, with a general view of this organ, p. 3.; and that anatomy of the brain may be studied less with a view to refined physiological research, than to the practice of physic, p. 183. All other physicians, however, of sound judgment, at all times have admitted as a principle, that pathology is to be founded on physiology, and that without understanding the functions in the state of health, it is impossible to judge of their derangements. Who believes, that in the practice of medicine it is of no importance to know the anatomy and physiology of the heart, of the lungs, liver, stomach, &c.? Are the structure and functions of the five senses not of equal importance? And will those of the brain and its parts be deemed less worthy of consideration? Shall the most delicate or most complex organization be declared useless? If, on the contrary, the brain alone explains the various instincts of animals, and all the modified manifestations of the human mind; if it alone accounts for the innateness of genius; if it is certain, that each species of manifestation of the mind has its appropriate part in the brain; if all manifestations of the mind, in the state of health and disease, find their explanation only in the cerebral organization; if the influence of the affections and passions on the bodily constitution is indubitable, and *vice versa*; how is it then possible, that a lecturer on the institutions of medicine can separate the structure and functions of the organization? can maintain, that a skilful physician does not need accurate knowledge of anatomy and physiology? is justly indifferent with respect to the

structure and functions of the brain, as well as to the connexions of its parts with each other, and with the whole body?

Such notions will not, I trust, induce those of the medical profession to neglect the most interesting study of all, viz. that of man. Indeed, the examination of the nervous system is not only important, because all functions of the body, such as digestion, circulation, respiration, nutrition, secretion, and excretion, depend on it, but also because the five senses, all inclinations and sentiments, all moral and intellectual faculties, and all the characteristics of humanity, are evinced by means of the nervous system alone. Thus, the medical profession is not only interested in studying the human mind with respect to bodily health, and particularly with respect to insanity; but it is their province to improve the knowledge of the mental powers, since these can be discovered only by the study of the brain and its parts. No profession is better prepared than that of the physician by accessory knowledge, and by the study of nature in general; nor is any one so frequently and so seriously admonished to revise opinions, and to forsake hypothetical reasoning, in order to follow the simple method of experience. No philosopher is more intimately convinced, that all our knowledge ought to be reduced to a rational mode of judging from experiment and observation; while a speculative philosopher thinks, that “the labours of metaphysicians can only be rewarded by attentive and patient reflection on the subjects of their own consciousness.”—(*Dugald Stewart's Elements of the Philosophy of the Human Mind*. 5th Edit. p. 8.) According to such a precept, every one has a right to take himself as a standard for the rest of mankind: A Caraib metaphysician may find, that destruction is the first moral principle.

The physician, besides, is placed in circumstances the most conducive to a profound and certain knowledge of man. No one has such opportunities of observing men at all times, and in all situations. He alone is present during the night and the day, to witness the most intimate concerns, and the most secret events of domestic life. Good and bad men, when sick, with difficulty conceal from him their true sentiments. Who desires not the friendship of the man, whom he trusts with his own life, or with that of his wife, and of his children? To such a man, as knowing all that belongs to our nature, we unfold the most secret thoughts, and we acknowledge our frailties and our errors, in order that he may judge truly concerning our situations. There is consequently no man more called upon, no man more necessitated to study mankind, than the physician. I leave it now to the reader, and to those who practise the healing art, to decide, whether a person contributes to the celebrity of his profession by inculcating such doctrines?

Thus, only according to a philosophy, which states that every one may take his own consciousness as the measurement of that of all men, are our British antagonists excusable—according to such a philosophy alone, is it unnecessary for them to study the spirit of our inquiries. As they cannot raise their minds above mechanical forms and shades of colour; as they do not even feel the necessity of considering the parts of the nervous system in connexion; as they even invent artificial divisions; how could their judgment of our investigations be sound, equitable, and just?

SECTION II.

IN our anatomical views, which are always connected with physiology, pathology, and philosophy, the first point to be considered is, that there is no common origin of the nerves; that all descriptions of the spinal cord as a prolongation of the brain, are incorrect; that no nerve, and no cerebral part, owes its origin to any other; but that all of them, on account of their mutual influence, are in communication. (*Vide* Memoir to the French Institute, sect. 1.; *Dictionnaire des Sciences Médicales*, Art. CERVEAU, § 3. NO. 1, 2, 3, and 4.; Physiognomical System, p. 13—18.)

Such considerations have entirely escaped the conscientious Reviewer, and mechanical Dissector.

SECTION III.

THE second point to be considered is, that the general form and arrangement of the nervous system are modified in different beings. In the superior animals, it is divided into the nerves of the abdomen and thorax, the spinal cord, the supposed cerebral nerves, and the cerebellum and brain. The spinal cord is composed of a series of swellings between two undulatory lines. These swellings are proportionate to the nerves, which go off.

The conscientious Reviewer is satisfied with stating, that our descriptions of the spinal cord “abound in conjectures, and assumptions, and inaccuracies,” p. 267.—The mechanical Dissector has not attended to comparative anatomy, and does not mention any thing of that kind. The Historian is in unison with the Reviewer, and merely declares, that our statements are unfounded,

p. 179.—Comparative anatomy, however, shows great modifications in the general form and arrangement of the nervous system, as in the caterpillar, lobster, frog, fish, bird, or quadruped. At the Physical Society, and in Dr Barclay's lecture-room, I have shown to my auditors the swellings of the spinal cord of a calf. As our statements are not attacked in detail, I do not repeat what is mentioned in our works.

SECTION IV.

THE next points to be examined concern the medulla oblongata, and the supposed cerebral nerves. The medulla oblongata does not belong to the spinal cord, and the supposed cerebral nerves have different origins from what anatomists generally believe.

The literary gospel does not embrace these points; I have only to consider the respective discoveries of the mechanical Dissector. He believes, that the medulla oblongata, though situate in the head, belongs to the spine; he calls it the cranial portion of the spinal cord, and fixes its termination to the lower edge of the pons Varolii, p. 175.

In our views, a great portion of the medulla oblongata belongs to the greatest number of the supposed cerebral nerves; the rest to the cerebellum and brain. In my second demonstration in Edinburgh, before a numerous and respectable audience, the mechanical Dissector repeatedly protested against my stating, that the medulla oblongata is not interrupted, but continued to the cerebellum and brain, or rather that both, by means of the medulla oblongata, are in communication with the nervous mass of the rest of the body. The gentlemen who were present will recollect, that I twice asked the Dissec-

tor, whether he could show the interruption of the pyramids, since he protested against their continuation? Now, as a historian, four months later, he tells us, that the idea of that very communication of the pyramids with the crura cerebri has been known a century and a half. The man of duty either was or was not acquainted with the fact. In the first case, why did he protest against it? and why did he not state it in his book, professedly written on the brain? There he terminates the brain at the upper edge of the pons, ascribes the mass of the pons to the cerebellum, and the medulla oblongata to the spinal cord. In the second case, he has learned it since, though he might have found in our works the same authors quoted, whom he, as historian, now appeals to, to prove that the idea is not original. More of this tergiversation afterwards.

This discoverer calls the abductor, trigeminal, facial, and auditory nerves, *cerebellar*, p. 202. and places their origins in the peduncles of the cerebellum, p. 207—210. viz. in the lateral portion of the annular protuberance, p. 112. Comparative, as well as human anatomy, however, shows the contrary. These nerves exist in fishes and birds, though these animals have no annular protuberance, and in the greater number of quadrupeds these nerves go off behind the pons; how then can they originate from the pons? Even in the human brain, we can trace the fifth pair through the pons to the corpora restiformia of the medulla oblongata. I have done it in presence of many in Edinburgh, as well as in other places.

He has further discovered that the facial and acoustic nerves originate from the same spot, p. 209, 210. while they go off at quite different places, the facial nerve at the external edge of the corpus olivare, and the auditory nerve

behind the medulla oblongata in the fourth ventricle. He has also discovered, that the optic nerve arises from the anterior corner of the commissure of the tractus optici, p. 205. viz. "from the part situate before the pituitary gland and infundibulum," p. 83. while even in the infancy of anatomy, the optic nerve has been traced farther back. Comparative and morbid anatomy amply elucidate this point. In many fishes, the optic nerves are placed only over each other without adhesion; and in quadrupeds and man, when one of the optic nerves is injured and diminished in size, the diminution is not only visible as far as their union or partial decussation, but passes across to the opposite side, backward, and proceeds to the anterior pair of the corpora quadrigemina.

By comparative views we have proved, that the optic thalami in birds and quadrupeds have been confounded, and the same name given to quite different parts of the brain; and that the optic thalami in quadrupeds do not belong to the optic nerves, but to the brain proper.

SECTION V.

THE fourth consideration is with respect to the communication of the cerebellum and brain with the rest of the nervous system.

The conscientious Reviewer, and Anatomist *par excellence*, had nothing to say in this respect; but the Historian, "after a painful research" of four months, (p. 3.) has contrived to find matter for opposition. He maintains, "that it is impossible to trace any fibres, either from the corpus restiforme or from any other part of the medulla oblongata, into the corpus dentatum," p. 3.

The Historian is wrong in ascribing to us the discovery of the communication between the cerebellum and

the corpus restiforme. During his "painful research," he might have found the history of this communication, as well as that of the brain with the medulla oblongata. This very name *oblongata*, is only explained by the medulla of the brain and cerebellum having been considered as continued to the spine. A great number of anatomists speak of prolongations, or crura, or processus cerebelli ad medullam oblongatam, and distinguish them from the crura or processus or pedunculi cerebelli ad pontem. We consider this ancient view of communication as correct; the ancients only erred in imagining that one part gave origin to another. In fact, the connexion between one bundle of the corpus restiforme and the corpus dentatum of the cerebellum, is easily shown in scraping off the auditory nerve from the external surface of the corpus restiforme, and following the direction of the bundles. I have shown it in Dr Barclay's lecture-room, and I am ready to do so to every one who procures a fresh brain.

The communication of the brain with the rest of the nervous system, requires more full exposition. Here the Dissector appears in *his proper light and colours*. He himself calls the attention of the public to the second demonstration. I therefore must be excused for speaking of it. When I demonstrated the decussation of the pyramids, he began the controversy with the question, Whether we maintain to have first discovered the decussation? As Historian, he tells us, that he thought it *his duty*, in justice to preceding anatomists, to make their claims known to my audience, p. 74. My answer was, that our works show the contrary, and that we have given the history of the decussation. I then remarked, that before us, many anatomists have spoken of a decussation of the nerves, because injuries affecting the brain are often

propagated on the opposite side of the body; that, however, there are other observations on record, where injuries of the brain are visible on the same side with the injury; that we have first discovered, that *only a part* of the brain is in communication with the opposite side of the nervous system, and the other part with the nerves of the same side. He was obliged to allow, that this distinction is new.

I beg leave to make a few observations on this occasion. The man of duty, when he wrote professedly on the brain, did not mention a single author who had spoken of the decussation. He himself speaks of "two or three ridges, which would hardly have been worthy of particular notice, were it not for the absurd theories with which they have often been connected in physiological writings," p. 177. On the other hand, in our works, the names of all the authors, whom he, as historian, quotes, are given, and many more. He speaks of Mistichelli as the first, while in our Memoir we have stated, that the decussation has been described by the most ancient anatomists, such as Aretæus and Cassius; that afterwards it had been neglected; but that pathological facts called again the attention of Fabricius de Hilden to it in the year 1581. We have quoted Mistichelli, in 1709, Petit, Lieutand, Santorini, Winslow, Soemmerring, and Portal. Has now the man of duty, as historian, a right to accuse us "*of neglect and ignorance against every preceding inquirer,*" p. 2. while he, on this occasion, as author, does not quote one, and we have quoted them all, and a greater number than he as historian? Is it not rather our duty to mention the preceding authors when we write a book, than when we give outlines of a demonstration, and in an oral communication?

This is not yet the whole. The Historian says, p. 69. "The structure in question (decussation of the pyramids) has been taken notice of, ever since its discovery, in elementary works of the highest reputation, and such as anatomists still daily consult; and it has been particularly mentioned in the best and most generally known treatises on the brain, so that there is as little room for maintaining that it has been overlooked by modern anatomists, as that *the description of the corpora pyramidalia themselves has been forgotten.*"

Does the man of duty not accuse himself by this passage? Let us admit the case to be as he says; I then reply, that he had no reason to put his question. If the decussation is so generally known, no one could be mistaken. In that case, he could have asked me with the same propriety, whether we maintain to be the first who have described the pyramidal bundles, since, according to his own words, "the decussation is as little overlooked as the description of the pyramids?"

But the reader would be mistaken, did he think the decussation as generally known as the Historian alleges. To prove that this anatomical point was not sufficiently understood, nor completely ascertained by the modern anatomists, I shall examine a few works of those authors whom the Historian has quoted. Vicq d'Azyr, for instance, did not know the true decussation, nor did he represent it. He speaks of such a thing, and points out a place where he looked for it; but there it does not exist. This is evident, from comparing his own passages with nature. In explaining the 22d plate, he says, "Lorsqu'on écarte le sillon 12, 15, entre les corps pyramidaux, on aperçoit de petits cordons blanchâtres et médullaires qui se portent d'un coté à l'autre comme autant de petites commissures dont la direction varie." In ex-

plaining the 23d plate, he marks the same place by b. b. b. b. and says, that these are transverse fibres. Plate 17. fig. 1. 57, and 58, he says of the pyramidal bundles, "Elles sont séparées de la protuberance annulaire par un petit enfoncement 82, 82, et entre ces corps se trouve une fente ou division longitudinale 59, 59, au fond de laquelle on voit, lorsqu'on écarte les bords, plusieurs cordons blancs qui se dirigent d'un coté à l'autre en maniere de commissures, les uns transversalement, les autres obliquement."

Dumas and Boyer maintain that palsy of the opposite side in injuries of the head is not at all explained by the anatomy, because the decussation of the medulla oblongata can by no means be proved, "qu'il n'est rien moins que prouvé par l'anatomie."

Sabatier quotes the passage of François Pourfour du Petit; but he adds, that "le prétendu entrecroisement des fibres de la moelle allongée n'est rien moins que certain."

Chaussier, who with Vicq d'Azyr, belongs to the few quoted by the Dissector, also quotes the passage of du Petit, and speaks of Santorini and his plates. "Mais, dit il, en examinant les objets de plus près, en suivant attentivement les progrès de la préparation, les changements que produit l'écartement, le tiraillement des parties, il nous a paru que ces prétendus faisceaux des fibres transversales ou obliques sont uniquement le resultat de la traction que l'on exerce sur le tissu de la partie, qui avant de se déchirer, s'allonge et prend l'apparence fibreuse," p. 142.

How could the impartial Historian overlook such passages in books he quotes? and if he did not overlook them, how can he say, that the decussation was generally known? I can affirm, that at the universities and colleges

where we have demonstrated the brain in Germany, Denmark, Holland, France, Great Britain, and Ireland, the decussation was not shown to the pupils before the publication of our works. The French commissioners felt the truth, and allowed that we had recalled the attention of physiologists to the decussation of the pyramids, though they deny us the merit of having discovered it. They ought to have said, that we had not discovered it the first. We can assert, that we were not taught it in the school, nor had we learned it from books. Pathological facts alone called our attention to it. Without pathological considerations it must appear indifferent. For that very reason, the mechanical Dissector speaks of it as scarcely worthy of particular notice. He himself, also, may still become acquainted with some modifications which the decussation presents. The description of two or three ridges is very incomplete. We think that our mode of demonstrating it is preferable to that of Santorini, who employed a long and peculiar maceration, while, by our mode of proceeding, we can show it in every fresh brain.

SECTION VI.

THE fifth point which may be discussed, is our method of dissecting the brain. The common way consists in slicing it, whether to begin from above, as most commonly is done, or from below, or from the sides; or in cutting off small portions, and showing their mechanical appearances. Every one who has attended anatomical lectures, or will look at anatomical works, is aware that I speak truth. The descriptions given by the mechanical Dissector himself, and the macerated pieces which he

showed in the second demonstration, prove the same statement.

We consider the parts in connexion with, and in relation to one another: we observe what is general or constant, and we are as much convinced of the modifications of every part of each brain, as of those of every other part of the body. We always begin the dissection at the medulla oblongata, and examine the successive additions and distributions towards the convolutions. We seldom cut, but mostly scrape; because the substance, on account of its delicacy, when cut, does not show its structure. The conscientious Reviewer had suggested, that our proceeding is limited to the use of the handle of the scalpel alone. The Historian adds, "the blade of the scalpel, and the points of our fingers;" but he calls this proceeding rude, p. 26. It seems he had forgotten what he wrote on the previous page 17. There he has said, "Every anatomist, who has enjoyed frequent opportunities of examining the recent brain, must have observed, that there are particular portions of the white substance, which *tear* much more readily in one particular direction than in any other; and that the surfaces of the *lacerated* parts in the former case, but never in the latter, put on an appearance similar to that exhibited by a piece of muscle, or of any other fibrous nature, when torn in the direction of the fibres." May I not suppose, that this hero of the scalpel tears and lacerates with his fingers; and that, if he had used them more dexterously, he would have made fewer mistakes. I sometimes make use of my fingers, to obviate an objection which has been made in Germany, France, and even in Edinburgh, viz. that we artificially form the appearances in the brain by the handle of the scalpel, or that we play a trick on the spectators. The conscientious Reviewer himself main-

tains, that we must know the incorrectness of our assertions, and show to our less knowing pupils the fibrous structure of the white matter in some portion of the brain, where, in consequence of the two kinds of matter, the white is disposed in threads through the brown, p. 256.

For the demonstration of many parts, we prefer fresh brains. The structure of others may be better seen, when they are previously macerated in diluted acids or alcohol. Our works attest, that we have employed various means, especially in examining the structure of the convolutions. Several adversaries in Germany, particularly Prof. Ackermann at Heidelberg, objected against the preparation of the brain by maceration. They maintained, that this appearance is not natural, but the result of a chemical process. An example may be mentioned with glass, which is an uniform mass. In the southern countries, in Paris, for instance, windows exposed to the sun and moon split into innumerable scales; this appearance is not natural, but the result of a chemical process. To obviate that objection, we prefer proving our statements on fresh brains. At the same time, we have always answered, that the white substance of the brain must have naturally a fibrous disposition, because the appearance is the same under all the very various circumstances, whether, for instance, examined fresh or coagulated.

It is, however, conceivable, that in towns, as in Edinburgh and Halle, where we cannot procure a number of fresh brains, the dissector may prefer to keep the parts in spirits. Even in towns where there is a great facility of procuring fresh brains, we get some which are entirely unfit for demonstration. If we unfortunately meet such a one, shall we draw the inference, that in no fresh brain whatever the structure can be seen? Indeed, in the dis-

secting rooms at Halle and Edinburgh we may be induced to say with Reil, that our method in dissecting fresh brains is not sufficient, and that the cerebral mass is too pulpy and too deliquescent, (*zu breyigt und zerfliessbar*) for being examined in connexion. The conscientious Reviewer, p. 236. quotes this passage of Reil; and the only meaning is, that Reil at Halle found the brains too soft, and thought it necessary to prepare them by maceration. The Historian must know very little of the German language, on account of his erroneous interpretation of this passage, p. 188. If ignorance of the language be not the cause, he has invented a story worthy of a conscientious Reviewer. I shall afterwards give the history as it happened between Reil and us. In answer to Reil, I here only state, that in London, Dublin, Paris, and Vienna, we can easily procure brains, the parts of which are firm enough to be examined in connexion, without any previous coagulation.

The proceeding of Vieussens has only in common with ours, that, in examining the parts of the brain, he scraped: In the rest he was guided by quite other principles; began with the convolutions, and cut them off round the hemispheres, to shew the centrum ovale, which, to this day, is demonstrated and called by his name. He first considered all medullary fibres to originate from the cortical substance of the convolutions, and to be concentrated in the midst of the hemispheres; he then examined the corpus callosum, the fornix, plexus choroides, nates, and testes. In the first thirteen plates he represents only cuts from above downward. At the end he examines the cerebellum and medulla oblongata, so that he represents the connexion between crura and the medulla oblongata in his last plate. Proceeding from above

downward, he speaks of his usual method, (*institutum servando sectionis ordinem*).*

The Historian accuses us of having learned our proceeding from other books: Why has he not learned to consider the cerebral parts in connexion? why has *he* continued to slice and cut the brain like cheese? None of our predecessors has proceeded in the way we do; hence it was impossible to learn our method from them. I have no objection that the brain should be examined in various ways; but one method may be preferable to another, and we think ours the best to show the connexion of the parts, and we think it indispensable for those who examine the brain with physiological and pathological views.

SECTION VII.

A SIXTH consideration concerns the two sorts of substances, of which the nervous system is composed; one greyish and soft, and of unknown organization, the other white, and of fibrous structure. Both are together, and proportionate to each other.

The Historian quotes Vieussens, Haller, Mayer, Reil, Portal, and Cuvier to prove, that the fibrous structure of the brain was known. The reader would be mistaken if he thought, that in our works we have not quoted authors of that kind. We have mentioned the same and others, such as Loewenhock, Stenon, Prochaska, Soemmerring, Sabatier, and others. In a passage of our memoir, p. 248. we say, "*Bonnet ne trouve dans le cerveau que des fibres dont chacune auroit sa fonction particuliere.*"

* Nevrol. univ. p. 87.

We have never thought of being the first who maintain that the brain is fibrous, though we know also that the most erroneous opinions have been entertained with respect to its structure. Our principal ideas are the successive additions, and the aggregation of various parts; the two great sets of fibres, and the unfolding of the convolutions, as I propose to detail in the sequel.

I have already mentioned, that we do not limit our proceeding to the handle of the scalpel, as the conscientious Reviewer, p. 256. and Dissector, p. 150. insinuated. When the Dissector wrote his book, the fibrous appearance could never be displayed by dividing the cerebral mass with a sharp scalpel, p. 126.; as Historian, however, he proves, that many authors, who have only sliced the brain, were acquainted with its fibrous structure. As Author, he speaks of nervous cords, p. 128.; nervous threads, p. 132.; nervous fibriles, p. 123.; nervous fibres as fine as hairs, p. 137.; nervous fibres traversing, p. 128.; innumerable fine fibres *diverging*, p. 138.; and what is more, "when a portion of brown nervous matter, which forms a covering to the convolutions, is exposed to the action of alcohol, or acids, or boiling oil, and is then *torn* asunder, it exhibits a fibrous appearance," p. 127.—As Historian he equally states, that "the apparent fibrousness of the white substance, both in the recent state, and after coagulation with boiling oil, alcohol, acids, &c. has been long known, and no opinion has been more prevalent than that this substance is really fibrous," p. 16.

Now, after that language as Author and Historian, what shall I think of such a man, who, in my second demonstration, before a numerous and respectable audience, came forward and protested against my using the name of fibres, and diverging fibres? who asked me,

like a school-boy, what I call *diverging*? and who, when I requested him to give a name to what he saw, called it "*fibrous appearance*?"

According to our ideas and observations, there is a brown and white matter in the medulla oblongata; and the white goes out of the grey. The Historian replies, p. 34. "that the origin of all, or even of any of the fibres from the grey substance of the medulla oblongata, is a mere assumption."—No such matter or grey substance has been pointed out as attached to the fasciculi, or intermixed with them, p. 35. He doubts, whether the corpora pyramidalia increase during their ascent, p. 76. and therefore, in his book on the brain, has chosen the name of oblong bundles; but he calls upon us, according to our own hypothesis, to point out the grey matter which affords the reinforcing fibres, p. 76.

How shall I prove the existence of brown matter to him, who, in presence of two hundred spectators, declared he saw no brown substance, while all beside declared they did? I know that there are persons who cannot distinguish one colour from another, brown or red, for instance, from green; but the mechanical Dissector having found in the brain so many shades of brown, cannot be excused by that natural defect. The only explanation in his favour may be, that nervous affections are often intermittent. Hence it may be, that just on that day his sight was disturbed, and could not distinguish either fibres or colours. But what astonishes me is, that his affection continues so long, and that he cannot yet see brown matter in the medulla oblongata, and in the pons. As he cannot see it, he adheres more to the literary gospel, which, p. 265. denies the brown matter in the pons, than to his recent quotations in his historical treatise. If he himself has no confidence in

Santorini, why does he represent to his readers that writer as an excellent author? “(which by the by I believe him to be).” The Historian, however, quotes, p. 66. the passage of Santorini, relative to the decussation, where Santorini states, that he employed a long maceration; “for in this way, the fibres being very much washed, and the *intervening cortical or cineritious* matter in great part dissolved, and the filaments of the membranes becoming loose, they are each of them more clearly seen;” and yet, ten pages later, he states, that there is no grey substance to afford the reinforcing fibres. In speaking of the pons, we shall find that the Historian, with respect to Vieussens, commits the same error of which he is here guilty against Santorini. Why does he consider his readers endowed with so little power of comparison?

SECTION VIII.

IN our views, the cerebellum offers the following considerations: It is a particular apparatus, in connexion with, but independent of, the rest of the nervous system as to its existence and functions. In reptiles and fishes it is single and smooth, in birds single and lamellated, in quadrupeds lamellated and augmented by lateral parts. Animals with a single cerebellum have no pons; in quadrupeds the pons is always proportionate to the lateral parts; the cerebellum is in communication with the medulla oblongata by a fasciculus of the corpora restiformia; at the spot of this communication there is greyish matter, the whole of which is called by anatomists the corpus dentatum, or serratum, or rhomboideum, or nucleus, or zig-zag: The brown matter of the cerebellum is proportionate to the white: Finally, the cerebellum

is smaller in young animals and in children than in adults, and most commonly smaller in females than in males.

By the conscientious Reviewer, Dissector, and Historian, only some mechanical appearances are spoken of. The Historian reproaches me for not having shown, in the second demonstration, the set of fibres which bring the cerebellum, especially the corpus dentatum, into communication with the medulla oblongata, nor that set of fibres which we were accustomed to call converging. It is true I did not do so in the second demonstration, but I have done it in other towns as well as in Edinburgh, to a great number of professional gentlemen; and I offer to show the fact to any one who shall procure a fresh brain. For the second demonstration, I trust, I was sufficiently patient with such mechanical dissectors, who tried my temper for nearly five hours in beginning their attacks with a moral question, and quibbling about mere words, such as continuation, fibres, diverging direction, the existence or non-existence of brown matter, and other mechanical definitions; about expressions which they had partly used in their own works, and which they now maintain to have been known 150 years ago. Supposing that I had not shown every thing in one demonstration, it is easily understood, that this must be the case, as it is quite impossible to proceed through the range of demonstration in one brain, particularly if it is turned about and frequently exposed to two hundred spectators. Did I not offer to the mechanical Dissector to repeat the demonstration whenever he might feel inclined, and opportunity occurred? Why has he then rather avoided my presence than contrived to promote mutual information? Why, like the rest of the opposition, does he not make himself acquainted with the real meaning of our investigations? Why does he

turn away his eyes from the facts which I submit to the examination of my auditors?

In our works we speak, with all other anatomists, of greyish substance in the interior of the cerebellum, called corpus dentatum. As this appearance is generally known, I was amazed to read in the literary gospel, p. 269. "Be it known to the reader, that the corpus dentatum, which they have described and represented in their engravings as a great ganglion for the reinforcement of the diverging fibres of the cerebellum, does not contain one particle of brown matter." The mechanical Dissector makes use of the name nucleus, which hitherto was used as synonymous with corpus dentatum, but he means by that expression the nucleus of the nucleus.

The Historian had many words to say about the corpus dentatum, and he complains, that I did not listen to his observations. It may be, that my answers were sometimes different from what they would have been, had his manners and language accorded with the usual rules of decorum and politeness. Our idea is, that the bundle which comes from the corpus restiforme, meets greyish substance, which is in proportion to the cerebellum. The form in which the brown matter appears, is secondary in our views. The corpus dentatum is modified as to size and form in every man. It also presents a modified configuration in each brain, according to a vertical, oblique, horizontal, lateral or mesial section. In the plates of our large work, we have given five different representations of five sections in different directions. We maintain, that the appearances are different, on account both of the sections in different directions and of five different brains. How then could the Historian compare his figure of the corpus dentatum with one of ours, while both cerebella were different in size and form, and the

corpora dentata are not cut in the same direction? The cerebellum of our plate was larger, that of his figure smaller: we have cut more towards the mesial line; he more externally. In addition to which, the interior of the corpus dentatum in our plate xii. and in its diminished copy in my book on Physiognomy, plate iii. fig. 2. contains more white matter than he has represented in the copy which he has taken from our plate. Is this whole proceeding consistent with candour?

SECTION IX.

THE next point to be considered is the pons or annular protuberance. Besides the transverse fibres belonging to the lateral parts of the cerebellum, it contains brown matter and longitudinal threads, viz. the continuation of the pyramidal, oval, and a part of the restiform bodies and new additions.

The conscientious Reviewer states, p. 265. "These infallible anatomists have also described the annular protuberance as another large ganglion, containing much brown matter. This too is incorrect; it is composed chiefly, if not entirely, of white substance." The mechanical Dissector says, p. 140. "The nervous matter of this protuberance is chiefly, if not entirely, of the white kind; the quantity of the brown, I believe, will be found exceedingly small." The Historian affirms, p. 77. that "the annular protuberance, instead of containing a large quantity of grey matter, seems scarcely to contain any of this matter at all."

It is easy to shew the brown colour to every one who has eyes to see. Many anatomists speak of cineritious substance in the pons. Wherever I have demonstrated the

brain, and in Edinburgh also, every other spectator has distinguished two colours, a brown and a white, in the pons; the Anatomist *par excellence* alone cannot see it. Does he not believe in its existence to be consistent with the literary gospel? But how will he reconcile such a state of his vision with his confidence in Vieussens? As Historian he says, p. 14. "That Morgagni *justly styled* Raymond Vieussens, "*Monspeliensis Academiae decus et lumen*," and he himself, p. 82. calls Vieussens an "able anatomist;" but Vieussens has seen and described cineritious substance in the pons. I can only account for his inability to find brown substance in the pons, by his macerating small portions of brain in alcohol or acids. In that way the brown colour may disappear. He therefore will do well to examine a fresh brain. If he then cannot see it, he must find his consolation in other persons who cannot distinguish colours.

The Historian complains, p. 63. that I hesitated to define the boundaries of the corpora pyramidalia. The spectators will recollect that I have answered twice, that we call pyramids what all anatomists call so; that we disapprove of this mechanical name, but make use of it to be understood; that the essential point in our views is the connexion of the cerebral parts with the rest of the nervous system, viz. that in each hemisphere only a part is connected with the opposite side. The spectators will recollect also, that when the Dissector repeated his demand, I repeated the former ideas, made then a longitudinal incision through the pons, and went round to show that mass, in the figure which the Historian has copied from our plate, f, bounded by n-o, which he describes, p. 210. as the line of separation between the posterior set of the diverging fibres and the anterior set, f, or those proceeding from the corpus pyramidale. The

mechanical Dissector was not yet satisfied, but desired me again to mark the boundaries of the pyramids. To procure quiet, I marked them on the bit which was cut transversely, at the lower edge of the pons. The Historian says, p. 64. that I marked "from the forepart of the medulla oblongata to the fourth ventricle:" I do not believe it, since I went round among the spectators, and did not shew the mass from the anterior surface to the fourth ventricle, but only backward to the marked line n-o; and since I spoke distinctly of a posterior set of fibres which do not decussate. Why did the mechanical Dissector not correct me at the moment, as he was so anxious to oppose? In short, the description which I gave in the second demonstration, and what I have shewn to the spectators, and all our works, and all other demonstrations which I have given in Edinburgh, and even what he has copied, p. 210. from our description, prove that we are better acquainted with the structure of this part.

The Historian, after a painful research, proves, that the connexion of the medulla oblongata with the crura cerebri was known to many anatomists before us. Have we ever maintained the contrary? In the description of this part, in the memoir to the French Institute, we say, p. 134. "Pour bien voir ce passage, *connu de la plupart des anatomists*, on fait une incision," &c: we believe only to have given a better description, especially with respect to the longitudinal threads, and to have first shewn the new additions, which the Historian does not yet admit, because he says, p. 84. "Supposing it to be true, *which is far from being proved*, that the longitudinal filaments in the annular protuberance are largest towards the upper part, where they are connected with the crura cerebri, it is in no degree more accurate to describe them as extend-

ing from the pyramidal bodies, and receiving an increase of fibres as they proceed, than it would be to say that they descend from the crura cerebri, and that part of them are prolonged to the corpora pyramidalia, while part of them are lost in the protuberance." At all events, however, this physical appearance, which we have first described and represented, has some interest for a mere mechanical Dissector. Besides, as it is preferable to describe the fifth pair of nerves and others from the medulla oblongata, rather to the tongue and organs of mastication, than from these apparatus to the medulla oblongata; and as in the lower animals nerves exist without brain, and in many quadrupeds a large spinal cord and small brains, we think we can describe the cerebral parts, added to the nervous mass, more properly as beginning with the medulla oblongata. But in the year 1815, when the Dissector wrote professedly on the brain, he did not know this passage of the pyramids through the pons; or if he knew it, why did he terminate the brain proper at the upper edge of the pons, ascribe the mass of the pons to the cerebellum, and the medulla oblongata to the spinal cord?

SECTION X.

ONE of the most important points in our anatomical inquiries concerns the two orders of fibres, viz. diverging and converging, or uniting.

The conscientious Reviewer very modestly decided on this point, stating, p. 261. "Such is the grand system of the diverging and converging fibres of the brain, of which Drs Gall and Spurzheim are the sole inventors and proprietors; a discovery truly, which, at some future time, may throw light on the most obscure operations of the

microcosm. In the mean while it is our painful duty to remark, that the system is a complete fiction from beginning to end. The incorrectness, too, of these gentlemen, on this occasion, admits of no explanation or apology on the score of ignorance: their unceasing professions of the time and labour they have bestowed on the dissection of the brain, entirely preclude this excuse; we must ascribe their inaccuracies solely to intention. It is a wilful misrepresentation in them, therefore, to affirm, that in portions of the brain, *which are composed purely of white nervous matter*, (this phraseology is an invention of the Reviewer,) either diverging or converging fibres can be shewn by the method they have described. They have represented such fibres, it is true, in various plates of the folio engravings; but we can confidently affirm, that no such appearance as they have thought proper to represent between them, is capable of being demonstrated in the human brain by the manipulations which our authors all along profess to practise." (Hey, ho! is it so?)

The mechanical Dissector has not described the two orders of fibres. The Historian, however, is very anxious to prove, that there our ideas are not original. But we positively maintain, that they are not found in the works of any anatomist before us, and that, as the conscientious Reviewer says, we are the sole proprietors. All that has been observed by our predecessors is, that the external part of the crura are connected with diverging fibres, which since Vieussens have been described as descending to and communicating with the medulla oblongata. Even Reil (to whom the learned Historian will not do the injustice to insinuate, that Drs Gall and Spurzheim have borrowed from him their views without acknowledgment, p. 99.) deserves to be mentioned here, only with respect to his essay published in Gren's Jour-

nal for 1795. The description he gives, quoted by the Historian himself, p. 98. is applicable only to the same parts which Vieussens had shown, and which Monro and Vicq d'Azyr had attempted to represent. The passage does not leave the least doubt. It is, "Each crus, *being embraced by the optic nerve*, spreads out like an unfolded fan, almost horizontally, below the great cavity of the brain, towards the inferior and lateral parts, and towards the extremities of the brain."* There is no mention made of the two orders of fibres diverging and converging, none of the two sets of the diverging fibres, not even of the diverging bundles in the great cavities of the brain: After having spoken of the convolutions, I will show, whether Reil, on whom the Historian bestows so much praise, can be considered as entitled to original claims in the two essays inserted in his Archives of Physiology for 1809 and 1812. At all events, the literary gospel, and Anatomist *par excellence*, when he wrote his book, were not acquainted with that structure. Even now the Historian denies evident appearances in the crura and their lateral distributions. He says, p. 103. "The crura cerebri, according to Drs Gall and Spurzheim, contain throughout their whole length a great quantity of grey substance, by which they are continually reinforced with new fibres; whereas the quantity of this substance mingled with them is just perceptible, and no more, and the reinforcement of fibres from it is a mere averment, for which there is no foundation. Nor are there better grounds for the statement, that they receive a still greater

* Jeder Schenkel breitet sich alsdann, nachdem ihn der Sehnerv umfasst hat, als ein entfalteter Fächer fast wagerecht unter der grossen Hirnhöhle gegen die unteren Fläichen, Seitentheile und gegen die Extremitäten des grossen Gehirns aus. *Gren's Journal*, I. p. 102.

increase just where they are embraced by the optic nerve; neither their greatest increase of all, nor the means by which, according to their own principles, it must be accomplished, are susceptible of demonstration."

The mechanical Dissector will excuse me; I never said he could do it, I only say, that I can demonstrate all these statements to be facts to any one who shall procure a fresh brain.

The Historian prefers, p. 105. a singular accusation, in stating, " Their description excludes the posterior lobe of the brain-proper altogether from any connexion with the crura, which is an error of unaccountable magnitude; in so far as the mass of fibres which radiate from the crura into this lobe, is fully as great as that extending into the other parts of the hemispheres, if not greater." It seems the Historian, in writing this, had forgotten the passage, p. 62. where he says, " The second set are distributed on the convolutions of the posterior lobe, and on those which are situated along the whole upper margin of each hemisphere towards the median plane; and their description occupies the paragraphs of the Appendix, from 30—33." Page 7. he tells his readers, that he has inserted verbatim the Appendix, that " it will enable them to perceive, *whether or not* he has, *on all occasions, correctly* interpreted the meaning of the descriptions which are the object of his criticism." I copy these paragraphs verbatim from the Appendix. § 30. Il nous reste à parler de la formation du lobe postérieur et des circonvolutions situées au bord supérieur de chaque hémisphère, vers la ligne médiane du cerveau. § 31. Le faisceau qui sort des corps olivaires et quelques autres faisceaux postérieurs montent, comme les faisceaux des pyramides, entre les fibres transversales de la commissure du cervelet. Dans ce trajet, ils ac-

quierent un renforcement qui est bien moins considerable que celui des pyramides, et ils forment la partie posterieure et interieure des grands faisceaux fibreux (des cuisses) du cerveau." (Thus, we are arrived at the crura.) " Ici ils acquierent leur plus grand accroissement par la masse épaisse de substance grise qui s'y trouve, et qui avec les filets nerveux qu'elle produit, form un ganglion assez dur, applati au milieu et inégal en haut et posterieurement. § 32. Ce ganglion a jusqu'à present, été connu sous le nom de couches optiques ; mais une couche nerveuse du nerf visuel est seulement attachée à la surface posterieure externe de ce ganglion. D'abord ce ganglion n'est nullement en raison directe avec le nerf optique, mais il l'est avec les convolutions qui sortent de ce ganglion. Ensuite en examinant l'intérieur de ce ganglion, on trouve une grande quantité de filets nerveux très fins qui tous vont en montant, et dans une toute autre direction que le nerf optique. Ils se réunissent à leur sortie, au bord superieur du ganglion, en faisceaux divergens. Les anterieurs de ces faisceaux traversent un grand amas de substance grise, et prennent un nouvel accroissement de cet amas, de sorte qu'ils suffisent pour former les circonvolutions *posterieures*, et toutes celles qui sont situées au bord supérieur de chaque hemisphere vers la ligne mediane du cerveau."

Now, if the posterior internal part of the crura enters into the optic thalami, and these form the posterior lobe, I ask every intelligent reader, "whether our description excludes the posterior lobe of the brain-proper altogether from any connexion with the crura?" or whether the interpretation of the Historian is "an error of unaccountable magnitude?"

I can conceive, that an anonymous Reviewer, endowed with his proper modification of consciousness, states what

seems suitable to his purpose ; but it passes my conception, that the Historian could write, p. 109. " These gentlemen have passed over in silence the numerous delicate filaments of white substance, which shoot out from the anterior radiations of the crura into the inner bulbous part of the corpora striata, and are there entirely lost ; an omission which is the more remarkable, as these fibres present another instance of a distribution quite irreconcilable with their system of continued reinforcement."

I only answer, that in our plates v. vi. and xiii. are represented the numerous delicate filaments of white substance, which (to use the Historian's expressions,) " shoot out from the anterior radiations of the crura into the inner, as well as outer, bulbous part of the corpora striata." The outer part is marked L, the inner l, and the large fibrous bundles between them are marked S.

Another singular accusation may be read in the pamphlet, p. 111. It is said, that in the second demonstration I have not allowed to my spectators " a moment's time for close examination." I depend on the veracity of the spectators, whom I purposely requested to leave one bench empty, that I might show every preparation as near as possible. In fact, I dare say I took more trouble in showing the preparations than is commonly the case in anatomical demonstrations, and that, though repeatedly and captiously interrupted, I continued, for near five hours, to go round and between the benches.

This accusation affords me the opportunity of amusing the reader with an anecdote, which will show the zeal of our antagonists in promoting anatomical knowledge. A girl with chronic hydrocephalus and a considerable extension of the head, had died in the clinical ward in the Infirmary of Edinburgh. A friend of mine was so kind as to inform me that the dissection was to be made at half

past twelve o'clock, the 28th of December 1816. As this is one of the cardinal points of our anatomical inquiries, and one that has been the most determinately opposed by the Edinburgh Review, I placed myself, as might be supposed, among the spectators.

Without informing the spectators what was to be done, the dissectors set to work. They employed more than sufficient time to take off the scull-cap; but the spectators, excusing the anxiety of the operators not to spoil their important work, remained quiet. The scull-cap, when taken off, was handed round:—Meanwhile the dura mater was removed, and every spectator, I suppose, expected to see the appearances exhibited, OR AT LEAST TO HEAR THEM MENTIONED; but no such thing. The dissectors in the area surrounded the body, *put their heads together*, so that no one could see what was going on, except themselves. The pupils expressed their disapprobation by hisses. This induced the great dissector to promise that the particulars should be made known. The water was taken out of the ventricles, the cavities were laid open, and the cerebral parts divided into pieces, which at least ought to have been handed round. In vain the spectators repeatedly hissed. The dissectors in the area continued to keep close together round the hydrocephalus, and proceeded silently with the dissection. A gentleman in the area moved sideways, to give me at least a distant view. But he who accuses me of not having given to my spectators a moment's time for examination, placed himself in the opening just before me. The spectators of my second demonstration, however, will recollect, that all his cavilling could not induce me to neglect him in any thing. Though the particular appearances were kept out of view, yet by chance I perceived that the brain had not been absorbed, but that the con-

volutions were shallow and greatly distended. So much for the boasted agency of absorbing vessels!

I have witnessed many morbid demonstrations in various countries, but in no university or college did I ever see a public dissection made with less advantage and less instruction to the pupils. The child was kept in the hospital for many months, and the clinical Professor expressed his desire, that the pupils should derive every possible information from it. To him I give my particular thanks for his kind intention in affording me this opportunity. I regret the more his indisposition, which prevented him from being present at the dissection. I am convinced that he would have gratified me with the inspection of this hydrocephalic head. I consider it in general but justice to state, that neither the professors, nor any of the other gentlemen eminent in medicine, had any share in preventing this case from receiving its proper publicity. *Who was capable of doing so, I leave the conscientious Reviewer and mechanical Dissector to determine.*

The Historian also avers, p. 117. that his figure of the corpus olivare is after nature, and ours imaginary. He cannot have dissected the corpus olivare very often, because he has not yet learned that it varies, like the corpus dentatum, in size and form, in different individuals, and that the form appears different according to the section. His is horizontal, and ours vertical; hence the appearances must be different.

There is still a singular accusation: I am happy that there were so many present who will recollect what happened. Pages 28 and 112, the Historian states, that I denied assertions contained in our works. This, however, I have never done. The first passage of my book was read, when the Dissector intimated, that we main-

tain, that *all the fibres* of the crura originate from the medulla oblongata. He then read, p. 36-37. "I shall now examine the organization of the brain. Immediately before their entrance into the pons Varolii, the pyramids are slightly contracted, but as soon as they enter this mass, they are divided into many bundles, which spring out of the large mass of grey substance contained in the pons Varolii. These longitudinal bundles are covered by a thick layer of transverse cords, which comes from the cerebellum, and which I shall describe hereafter. Some longitudinal bundles are disposed in layers, and others are interwoven with transverse cords. They ascend and are successively enlarged, so as to form, at their exit forward and outward, at least two-thirds of the crura cerebri. Thus, the anterior and external bundles of the crura cerebri are the continuation and gradual completion of the primitive pyramidal bundles."

Immediately after the second demonstration, I caused an anatomical prospectus to be printed to prevent all cavilling suggestions. There, p. 7. I mentioned this peculiar opinion, and ask, "Was he anxious to defend the Edinburgh Review, because, at the same time, he insisted on another suggestion, which he could have learned only from page 258. of the Edinburgh Review, where it seemed suitable to state, that all the diverging fibres take their origin, it seems, in the brown matter of the medulla oblongata?"

When the passage of my book was read, I publicly declared, that I still maintain the same assertions with respect to the successive reinforcement. Thus, I denied not what was in the book, but only his suggestion, that all the diverging fibres of the brain take their origin *in the brown matter of the medulla oblongata*.

The next passage was read, when I examined the structure of the external part of the corpus striatum, and when the Dissector protested against the name fibre; when he maintained, that the brown matter is firmer than the white, and that the former may give to the latter its fibrous appearance. Then he read pages 20-21. of my book, where I speak of the fibrous structure of the white substance. He insisted upon the idea of the Edinburgh Reviewer, p. 256. "We suspect that when our authors are desirous of demonstrating to their less knowing pupils, that the white matter is fibrous, they exhibit some portion of the brain, where, in consequence of the alternations of the two kinds of matter, the white is disposed in threads through the brown. Our readers will perceive, however, that this is quite a different species of fibrousness from that of either kind of matter taken by itself." We maintain, that the white is fibrous whether it is intermixed with brown or not. But how could the Historian relate, p. 112. that "I denied to have ever affirmed, that the white substance, apart from the grey, exhibited a fibrous structure." Is not the whole order of our converging fibres entirely white? A great number of auditors, not only in Edinburgh, but wherever I have demonstrated the brain, will recollect, that I have shewn the fibrous structure of the corpus callosum. It seems the Dissector is accustomed to contradict, and under whatever form he appears, likes to follow his natural inclination.

I leave to those who have seen the demonstration of the brain, to judge whether or not the following remarks of the Historian are correct. Page 134. he says, "that under the denomination of diverging and converging fibres, we have described and represented as demonstrable, and even gone so far as to delineate in our engrav-

ings, parts which have no existence in this organ; and that we have maintained connexions to subsist betwixt all these parts for which there is no foundation in nature, and which they are under the necessity of denying when called upon to display in their public dissections." I, however, have more than once, even in Edinburgh, been told, that in nature the appearance of diverging and converging fibres is more distinctly seen than in our plates. As the Anatomist *par excellence* in many respects differs from other anatomists and physiologists, I may suppose that his eyes are of a peculiar conformation.

SECTION XI.

THE last point of our anatomical considerations concerns the structure of the convolutions. We were the first to teach, that they can be unfolded or distended into two layers of fibres.

The literary gospel states, p. 262. "We affirm it as the result of many experiments, made under every variety of circumstances, that there is no foundation whatever for the supposition, (for supposition at best it is,) that the convolutions consist of two layers contiguous only in the middle." The mechanical Dissector passes over in silence this anatomical point. The Historian, though he has great confidence in the correctness of Reil, and though he has translated a passage from Reil's archives, stating that the medullary laminæ in the middle of the convolutions cohere the most weakly (*die Markplättchen in der Mitte der Windungen hangen am schwaechesten zusammen*) maintains, however, throughout his pamphlet, that the convolutions cannot be unfolded into two layers.

The most curious is the weight he lays on our not being able to demonstrate the existence of a fine nevrilema between the two layers. This remark particularly characterises a mechanical Dissector. We maintain, that the convolutions can be more easily separated in the middle line, and unfolded into two layers; he, from mere fondness of contradiction, does not reflect, that the non-existence of the fine nevrilema is in our favour, because the separation will be still more easy. I will give a few details that the reader may the better understand this point.

When we submitted our memoir to the French Institute, the commissioners related, that we consider each convolution "*comme une espèce de petite bourse ou de canal,*" &c. We replied, that this is not our meaning, but that we admit "*une adhérence de contiguité entretenue peut-etre par du tissu cellulaire, mais non une adhérence de continuité par confusion de substance; une adhérence dans le sens d'agglutination (Anklebung) mais non dans le sens de concretion (Verwachsung).*" *Memoire*, p. 200.

I never speak of this fine nevrilema, and have not done so in any demonstration in Edinburgh; its existence is quite a secondary consideration, the possibility of separating the convolutions into two layers is the leading point. How then could the pamphleteer represent it as the most important matter, and repeat five times, that, if we can unfold the convolutions, we cannot shew the very fine cellular tissue. The mechanical Dissector may amuse himself with its discovery and demonstration; our great pathological point is ascertained, viz. the unfolding of the brain in large hydrocephalic heads.

As nothing is more easily demonstrated in every brain, than the separation of each convolution into two layers,

I will not lose time in detailing unmeaning and secondary protestations. I only mention, that the Historian confounds the bottom with the top of the convolutions. It seems, however, very natural to understand what part of the convolutions we call bottom; because we begin the demonstration of the brain with the medulla oblongata, and consider the successive reinforcement from below upwards. Now it seems natural, that we come first to the bottom of the convolutions, then to their top. It should be the more difficult to misunderstand our meaning, that we always in our demonstrations (and I have done so in Edinburgh) repeat, that the bottom of the convolutions corresponds to the ceiling of the ventricles, particularly to that spot where the diverging and converging fibres cross each other.

The structure of the convolutions is intimately connected with the appearance of large hydrocephalic heads. The cerebral mass is not absorbed, but distended by the water contained in the ventricles. The principal changes take place in the corpus callosum, its appendices, and the convolutions of both hemispheres. The corpus callosum is entire and lifted towards the top of the head, the falx is elongated, the convolutions sometimes quite distended like a thin membrane of cerebral substance, from within white with horizontal fibres, and covered on the external surface with cineritious substance. The distension, however, is not mechanical, but also vital and susceptible of modifications, on account of the continual decomposition and new composition which takes place in the organization in general. At all events, the brain is never annihilated while the mind continues to manifest itself.

The literary gospel states, p. 262. that our conjectures about hydrocephalus internus are quite of a piece with our other discoveries; hence, trash, a complete fiction

from beginning to end, trumpery, quackery. The objections of the conscientious man have the appearance of reasoning; I will therefore answer them as I have done in my Anatomical Prospectus.

1. "Pressing against the convolutions, we presume, would equally succeed, if the brain were made of putty, or tallow, or soft wax." The Historian speaks the same language.

Ans. This is by no means the case: a convolution can be extended only to the double of its vertical depth, and during that proceeding it shows an internal groove.

2. "It is not conceivable, that the secreting vessels should pour out the serous fluid with a force sufficient to account for the distending power in this case."

Ans. This view is too mechanical; has been invented by the conscientious Reviewer, and is now supported by the Historian, p. 158. I say in my Prospectus, "Two things must be considered,—a vital process, and an extension by pressure." The skull, dura mater, and falx, cannot be extended by mechanical force alone, any more than the orbit by a carcinomatous eyeball. This happens by a continual change of matter, during which, according to a general law of nature, the parts which contain, in their new composition are deposited according to the circumference of the contents. Moreover, the hydrocephalic heads are not formed suddenly, and a slight successive pressure would separate parts which a sudden pressure would destroy. Finally, in the distension of any part by dropsy, &c. such as of the eye or skin, we can never account for it by the force with which the secreting vessels pour out the serous fluid. It is the more astonishing that the Reviewer has imagined such a power, and the Historian continues to speak of it, while the third remark refutes their inept suppositions.

3. "It is the very height of improbability, that any such distending power as is here maintained" (suggested by the Reviewer) "should not produce insensibility, or even death in the individual, the instant it began to operate." The Historian, p. 158, expresses the same idea, "that no individual could survive the operation of such a pressure on this organ beyond a few minutes."

Ans. The invention of such a distending power of the secreting vessels shows the mechanical tendency of this changeable person.

4. "It is quite incompatible with the physical properties of the cerebral matter, so far as they are yet known to us, to imagine, that the parts immediately forming the sides of the ventricles can admit of a degree of extension such as this theory supposes, without great and obvious laceration."

Ans. Because it was not known, we looked for an explanation. An extension of the brain takes place, the ventricles are enlarged by the accumulation of water, the convolutions disappear proportionately, the vertical fibres of the convolutions become horizontal, the internal surface remains white, and the external brown. These are facts to be seen in every hydrocephalic head; but nothing can explain them better than the gradual separation of the convolutions from within into two layers.

5. "If there be merely a stretching and unfolding of parts in large hydrocephali, as much cerebral matter, surely, ought to be found distributed through the sides of extended as of the unextended cavities, though somewhat differently disposed; and yet, we believe, there never was an instance of a large hydrocephalus, in which, upon attentive examination, a greater or less deficiency of cerebral matter was not exceedingly obvious.

Ans. So he may say, who has never opened a hydrocephalic head, or, at least, not with the attention which the Reviewer recommends. We have opened such heads, and rely on it, that accurate anatomists in future will find as much cerebral mass in the extended as is commonly found in the unextended state. It rather appears to me extraordinary, that the parts which undergo the changes are sufficient to form the envelope which contains the water.

6. "With respect to the argument deduced from the observation, that persons with hydrocephalus often retain their intellectual faculties, is so manifest a *petitio principii*, as not to require pointing out."

Ans. This is certainly no proof for him, who is not aware of the importance of the brain, who considers its physiology as useless to the medical profession; or for a Reviewer who thinks, that his limbs are fit for voluntary motion without a spinal cord. After his assertion, that "numerous unequivocal instances are on record, and are even occurring every day, in which large portions of the brain, nay, almost the whole, if not actually the whole of this organ, have been completely destroyed by the progress of this very affection; as he holds this to be a fact just as certain, as that there are many persons now alive whose legs have been removed by the knife of the surgeon," it ought not to be difficult for him, to show every day such facts to accurate anatomists. If he can ascertain only one fact, that a hydrocephalic head has continued to manifest the operations of the mind, while the whole brain was completely destroyed and absorbed, I will abandon my investigations into the structure and functions of that organ, and will be satisfied with ignorance. But as long as such a fact is not shewn, I continue to maintain, that the mind cannot manifest its

powers without brain, any more than a limb which has been removed by the knife of the surgeon can exercise voluntary motion.

The Reviewer then concludes his sapient remarks on hydrocephalus, " We have only to add, that we have always been accustomed to consider the changes produced on the cerebral mass in every degree of hydrocephalus, as the effect of an increased and peculiarly regulated absorption; and that we never dreamt of any other agent being concerned in the process, or ever heard of any other explanation of the phenomena being suggested by persons whose opinions have the least weight in physiological matters."

Ans. This is dogmatism in all its glory. In the same manner the whole of modern chemistry might be spurned at, because formerly phlogiston was considered as sufficient to explain the phenomena; and all persons, whose opinions had the least weight in chemistry, were satisfied with this explanation.

We have hitherto seen, that in general the Historian had very little regard for the literary gospel. Not once has he quoted it; on the contrary, he has always proved by quotations from excellent anatomists, that the propositions which the conscientious man denies in the most positive, and not always in the most polite expressions, have been known for centuries. With respect to the existence of brain in hydrocephalic heads, the Historian places the critical Reviewer in a singular situation, and stops him short, by proving that the brain exists, and that Vesalius, Tulpius, Petit, and Morgagni have known it to exist. I now call the attention of the reader to my book on physiognomy, which it was the duty of the conscientious man to review, instead of asserting what suited his purpose. In this very book he will find the same

authors quoted whom the Historian mentions. I even flatter myself, that I have given the history of hydrocephalic heads more complete than the Historian himself. Therefore his conscientiousness forsook him, when he neglected my quotations. The Reviewer and Historian may settle the dispute; we meanwhile continue to maintain our first proposition, that in large hydrocephalic heads the brain always exists.

The Historian speaks of three sorts of large hydrocephalic heads; first, p. 149. of those, as we have described, where the brain begins to increase in its external dimensions, and the convolutions become shorter and shorter, and at last disappear. "In other instances," says he, p. 151. "if the patient does not sink before such extensive changes are accomplished, even the thin remaining layer of white and brown substance forming the vault and sides of the ventricles, gradually disappears, and with this, at last, portions more or less extensive of the parts of the brain situated towards the basis."—We deny any existence of this sort. The thin layer or membrane of the brain never entirely disappears. Morgagni, long ago, has proved how it comes that superficial and inaccurate dissectors have formed such an erroneous opinion; and the Historian might have rectified his error, if he had paid due attention to the details related by Morgagni. (*Epist. xii. de vuln. capitis.*)

Of the third sort, the Pamphleteer speaks as follows: "Sometimes it would appear that the brain may be very greatly enlarged in consequence of effusion into the ventricles, and yet the convolutions not be at all affected. Such a case occurred to Reil; and he mentions expressly that the extension was confined entirely to the ventricles, and that all the convolutions were solid, and not split up, (*gespalten*)."

We have seen such cases, and maintain, that the convolutions never appear split up, and cannot appear so on account of the tissue formed by the diverging and converging fibres at the bottom of the convolutions. The convolutions, wherever, and with whatever depth or height they appear, are solid; they only become shallower by degrees; and the vertical fibres are extended into a horizontal position. The hydrocephalic head of which Cuvier speaks in the report on our memoir, we had shown to him in Paris; the convolutions were thinned, and partly effaced, but, as far as they existed, preserved their internal solidity, as is the case in every other brain.

Thus we admit only one sort of large hydrocephalic heads. The brain is always present. The cavities are distended, the convolutions more or less disappear, and proportionately become shallower; their vertical fibres become horizontal, and sometimes these parts lose their convoluted form, though the substance of the brain suffers no diminution.

SECTION XII.

THE most grave accusation, and which, if true, were indeed formidable, remains to be repelled. At the end the Historian positively states, p. 187. that Reil has been defrauded; and in p. 99. that Reil has the sole merit of having revived the investigation of the fibrous structure of the brain in modern times; that he is the original discoverer of our ideas, and that we have borrowed them from his writings.

How will the conscientious Reviewer here extricate himself? Why did he deny such things as we maintain in our works, since his Historian asserts that Reil has discovered them, and refers to his Archives of Physiology

for the year 1809 and 1812? The Dissector himself, in writing his book on the brain, *forgot* these essays of Reil. But why have we not acknowledged that we owe our anatomical information of the brain to the writings of Reil? The reason is simple; viz. because it is not the case. The proof of this assertion is equally simple: I have only to state the history of our investigations.

While at Vienna, we spoke of the great leading points of our anatomical demonstrations; viz. of the aggregation of various cerebral parts, and their connexion with the medulla oblongata; of the proportion between the grey and white substance; of the diverging and converging fibres; and of unfolding the convolutions.

In the year 1805, the 6th of March, we left Vienna for Berlin, where we repeated our anatomical demonstrations in presence of the medical Professors, and numerous auditors. Outlines of our anatomical and physiological propositions were published, during that spring, by Prof. Bishoff. From Berlin we went to Potsdam, then to Leipzig, where Dr Knoblaach published an account of our doctrines on the brain. Then the usual demonstrations and lectures were delivered in Dresden, and Mr Bloede published outlines of our anatomical and physiological views. From Dresden we went to Halle, where Prof. Reil and Loder, and numerous gentlemen of the profession, honoured us with their presence at the public lectures and demonstrations. With Loder we repeated several times the anatomical demonstrations, and once we dissected with Reil a brain *quietly* in his own room. He was so much pleased with our demonstrations, that he gave to Dr Gall some drawings with which he was formerly occupied, *de structura nervorum et cerebelli*. Thus, I beg to observe, that in the summer of 1805 we demonstrated to Reil the same leading points

in the anatomy of the brain, which we still maintain. We then continued to lecture and to demonstrate the brain, that very same year, in Weimar, Jena, Goettingen, Brownschweig, Hamburgh, Kiel, and Copenhagen. In the year 1806, anatomical demonstrations were made in Bremen, Munster in Westphalia, Amsterdam, Leyden, Frankfurt upon the Main, Mannheim, Stuttgart and Friburg in Brisgaw. In the year 1807, we went to Marburgh, Würtzburgh, Munic, (where we had the pleasure of conversing with Soemmerring,) Augsburgh, Ulm, Zurich, Bern, Bale; and in the autumn of the same year to Paris, where we dissected the brain, first in presence of Cuvier, Fourcroy, Geoffroi de St Hilaire, Dumeril, Dr Démangeon, and others, and successively in many learned societies. Meanwhile numerous publications had appeared in Germany. Dr Démangeon, who had attended the lectures in Hamburgh, published in Paris, 1806, his *Physiologie Intellectuelle*, and mentioned our anatomical views.

In March 1808 we delivered our Memoir to the French Institute. The commissioners declare, at the beginning of their report, that they have hesitated a moment, whether they should examine our paper; because there is a rule, “de ne point émettre avis sur les ouvrages déjà soumis au grand tribunal du public par la voie de l'impression, et l'on pouvoit croire que la doctrine anatomique de Mr Gall a reçu, par l'enseignement oral que ce professeur en a fait dans les principales villes de l'Europe, et par les nombreux extraits que ses disciples en ont repandus, une publicité à peu-près équivalente à celle d'une impression authentique.” They, however, add, that Gall had not given his sanction to any one of the publications, and that this circumstance was one of the motives which induced them to examine our memoir.

After this, Reil published, in his archives, views essentially the same as ours, of the aggregation of cerebral parts, of diverging and converging fibres, and of the possibility of separating the convolutions in the middle line. He does not state, that he was the first who has conceived such general ideas; nor does he mention us as the inventors. He does not, and could not say, that we have learned them from him; he merely describes and represents them in engravings. As we had been in almost every remarkable town, and at all the universities, in Germany, our countrymen knew how to estimate the proceeding of Reil; and it is only the great publicity of our demonstrations, that can excuse Reil for not mentioning them.

It is true, Reil has chosen other names: he calls our apparatus of formation *Hirnschenkel system*, and our apparatus of union *Balken system*; our diverging bundles are his *Stabkranz*. We speak simply of fibres, he of various convexities, obtuse and acute angles of the fibres, of laminae, fossae, and radii of the white substance; of wings, mountains, lobules, teeth, of a comb, and of similar mechanical denominations. These minute descriptions of mechanical forms, and such names, may appear interesting to a mechanical Dissector, who is attentive to every little cul-de-sac, and declares the anatomy of the brain unnecessary to physiological and pathological views. We, on the contrary, think that there would be no end of such mechanical details in comparative anatomy. If, for instance, in the gradation of animals, every new additional part in the cerebellum is to be named, who will learn all the names? and of what use will such a study be? We therefore point out the structure of each part, well aware, however, that each part is modified in the

individuals of different species, nay, in the different individuals of the same species.

This short account is sufficient to prove, that there is no occasion whatever for us to apologize in the least, with respect to the publications of Reil. A few years ago the Historian might have been easily pardoned for his ignorance of historical details; but in the present situation, what his merits are, let others decide.

The learned Historian insinuates, that Reil and Gall had agreed, that the former was to examine the cerebellum, and the latter the brain-proper. But I affirm, that nothing of that kind happened, nor could happen, because our general views of the brain were discovered before we met Reil at Halle, in the year 1805. Reil, with such brains as he operated on, did not succeed by our method, and therefore thought it insufficient, and preferred maceration in alcohol or acids. His words are: "The brain is too pulpy and too deliquescent to be examined in connexion without preparation." He then made frequent use of laceration with the fingers, or of scraping. Thus, the essential difference between Reil's proceeding and ours is, that he prepares the brain artificially, while we prefer a good brain in its fresh state. With this narration I beg the reader to compare the following passage of the candid Historian, where he says, p. 188. "Reil's expectations of assistance from Dr Gall were altogether disappointed, so much so, that he seems not to have considered that person's investigations as worthy of attention; but pronouncing his method inadequate, extended his own inquiries to the department thus fruitlessly assigned to another." This Historian and Critic is told by Reil, that he had tried our method and did not succeed, and hence concludes, that we have defrauded him. A finely contrived story!!!

The Pamphleteer, p. 9. finds it "amusing to hear the committee of the French Institute occasionally named as supporters of our anatomical doctrines." Cuvier, however, was too well acquainted with the German and European literature, to accuse us of plagiarism. He allowed that our method of dissecting the brain is preferable to that commonly used in the schools;—that we are the first who have shewn the swellings in the spinal cord of a calf;—the proportion between the brown and white substance in the brain;—the true origin of the optic and other nerves;—the certainty of the decussation;—the successive reinforcement through the pons, crura, optic thalami, and corpora striata;—the two sorts of fibres in the brain, and the generality of the commissures. As the Report is printed, even translated and inserted in the Edinburgh Medical and Surgical Journal for January 1809, the reader, *in perusing the Report*, may satisfy himself. I also ask the Historian, why he has omitted to tell his readers, that Cuvier, in the Annual Report at the end of 1808, published, that our Memoir was by far the most important which had occupied the attention of the class?

SECTION XIII.

BEFORE I finish with the Historian, I have still to reply to his remarks on our Plates. He relates, p. 2. that he has compared our descriptions and engravings *strictly with nature*; and according to p. 165. he has found, that in our plate iv. which represents the basis of the brain in a female, the medulla oblongata points directly backwards, instead of downwards; and the anterior surface of the annular protuberance downwards, instead of forwards; that the anterior lobes are too

broad, the surface neither concave nor sloping enough, the middle lobes too wide and not pointed enough, and the forms of the convolutions not natural.

Ans.—Who has ever shewn or seen a brain, in which, when taken out of the skull, deprived of dura mater, and placed on its upper surface, the parts of the basis remained in the same position as in the skull? Do not the parts sink more or less, according to the firmness of the brain? I beg the reader to compare with our plate that of Vicq d'Azyr, and see which is the better. I say, the basis represented by Vicq d'Azyr, looks like a soft, collapsed, and flat and deliquescent mass. Indeed, no philosophical mind will, and no mechanical Dissector ought to cavil about minute changes in relative situation of the cerebral parts, when taken out of the head; since these, like all other bodies, must follow the laws of gravity. I also maintain, that a Dissector who adopts one general measurement, and one general form for all brains, and their parts; who does not know that each lobe in every person, as to size and form, is modified, while each, even the minutest part of the brain, as well as of ears and noses, offers modifications, cannot have compared many brains. The important consideration, that each part is modified, is general, and applicable to the parts of every system. It has been well detailed by Dr Barclay with respect to the blood-vessels, in the preface of his *Description of the Arteries*, and will be admitted with respect to the nervous system, by all those who compare the parts in different individuals. The anterior lobes, as they are represented in our plate iv. may be larger than those of the *accurate* Historian, but they are too small for those men to whom the medical school of Edinburgh is indebted for its first celebrity. I also assert, that the females of Edinburgh, who are known for their talents,

have the anterior lobes of their brains larger than those which we have copied.

The remarks of the Historian on our fifth plate can be made only by one who is accustomed to cut the brain mechanically, and who does not consider the parts in connexion, but thinks that all brains, and each part in every brain, are quite the same, without the least modification: I repeat, that we have represented nature, and do affirm, that the general structure of the brain, and its parts, will be found as our plates indicate; but that the modifications of each part are infinite. Such a configuration, however, as the Historian has given of the pons, in his plate i. fig. 2. can only be seen in a putrid brain; or if he gives it as the exact appearance of this part in a fresh brain, he must never have seen the real structure.

As each part in each brain is modified, how can the Dissector maintain, that in plate vi. our representations are not natural? The corpus dentatum, and the arborescent appearance of the cerebellum, seem to him exceedingly incorrect. The former is represented in five different brains and sections, and the latter is shown in seven different brains, partly in the same, partly in different sections; and in each the appearance is modified, for no other reason but because it was so in nature. It was, indeed, more difficult to copy exactly nature, than to make the appearance always the same. I rely on the decision of every anatomist who has had opportunity of comparing brains.

In the viiith, ixth, xth, xith, and xiith plates, the representations of the skull are particularly blamed, and declared fictitious or imaginary, so that they never could have been drawn from nature. In reply, I propose to the Dissector to open the head of a young man, of a very

old person, and of a third, who had been long maniacal, and he may then tell us, whether there is one and the same appearance in the bone. Those who will examine my collection, may convince themselves, that still greater varieties occur in nature than we have represented in our plates.

In plate viii. he finds fault with the outline of the cranium, particularly towards the forepart of the basis; he has never seen an occipital bone of such a form and of such dimensions; such arrangements of lobes and lobules were never observed; the cerebellum is even called a case of monstrosity. Such assertions may be made by a Dissector who never has examined the differences of heads; who thinks, that children of seven years have the full growth of their brains, (the contrary of which, however, any maker of hats might have told him), and that the brains of women and men in general do not show any constant difference. We maintain, that the anterior lobes, their bassilar convolutions, and the cerebella, vary as well as the other parts, and for that reason we have copied them different in size and form, as they occurred.

Plate xvii. is said to be in contradiction to plate xii. The Dissector cannot easily conceive how they may be reconciled. The answer is, that each brain was different, and in the former the bundles were larger, in the latter smaller, and in the latter the bundles are traced to a greater extent towards the convolutions.

In short, he who has not yet observed, that the arrangements, size, and form of the different parts of the brain, present various modifications, instead of speaking of unnatural forms, fictitious appearances, too large or too small, too wide or too narrow, too thick or too thin, too perpendicular or too horizontal, or similar represen-

tations, ought to learn to distinguish the generalities from the particularities, and that one brain is no more the standard of all brains, than the feelings and dispositions of one man are the standard of the whole race.

The conscientious Reviewer complained, p. 154. that he was heartily tired of the mass of nonsense he had been obliged to wade through in my work. I only depend on the constant laws of nature. What has happened, will happen, and every one has the right to observe and to examine for himself. In anatomy, the eyes deserve more confidence than the ears, demonstration than fancy.

I cannot finish this chapter without calling the attention of the reader to a comparison of the statement of the critical Reviewer, the mechanical Dissector, and Historian. To the latter I am under great obligation; and I give him my public thanks for having entirely refuted the conscientious Reviewer, by proving that our anatomical views of the nervous system are not new, and, by detecting the ignorance of that empiric in criticism, has taught him, that not our assertions, but his, are "mere nonsense, amazing absurdities, nay trumpery, and wilful mistatements." The Historian also gives a lesson to the mechanical Dissector, and shows him how improper it is for any one not to quote preceding authors, when he writes professedly on a subject. Supported by the Historian, my labour has become easy.—According to him, the teachers and practitioners in medicine of Edinburgh do not know any thing about the anatomy of the brain, and not one has eyes to see, or even to distinguish brown from white; yet he has not ventured to affirm this of all the medical men of Europe; and as it is proved above that we have not borrowed any thing from Reil, we may continue to speak of our discoveries in the anatomy of the nervous system.

There is another great literary tribunal which has condescended to speak of our doctrines. These quarterly judges, however, do not display great anatomical knowledge. They confine themselves to mere general expressions, and are perfectly willing to give us praise in this respect; to allow us every merit for our method of dissecting the brain; for having shown that the nerves of the body have their origin in the respective parts of it, and not in the brain; and for having stated the morbid phenomena of hydrocephalus much more clearly than has been attempted heretofore. How merciful! Indeed I am obliged to their kind judgment. But as the chief judges of these inferior courts are at variance, we appeal to the great tribunal of the public.

CHAPTER II.

PHYSIOLOGY.

AFTER several indirect attacks in the preceding numbers, the literary Oracle of Edinburgh, No. xlix. p. 227. spoke from his tripod, that "the whole of our doctrines is a piece of thorough quackery from beginning to end." The Quarterly Reviewer (No. xxv. p. 159.) had so little power of discrimination, that he confounded my person with all my countrymen, and accounted for my conduct by my being a German and not an Englishman. I know, however, that he does not possess the characteristic qualities of an Englishman; and the incongruous thoughts

of the Edinburgh Reviewer shew, that he does not belong to the most thinking people of whom he speaks, No. 49. p. 228. Hence, the reviewers themselves serve as proofs, that one individual ought not to be confounded with the whole of his nation.

SECTION I.

THE object of our physiological investigations is the connexion of the manifestations of the mind with the organization. In this respect we maintain, that in this life the mind cannot manifest any power without the instrumentality of brain; and that each sort of manifestations depends on a peculiar part of the brain.

The literary tribunal of Edinburgh does not yet agree with the proposition, that the brain is necessary to the manifestations of the mind. In No. 48. the xth article aspires to prove the contrary. This article looks exceedingly learned, but all the cases, copied from various authors, may be reduced to two classes. The greater number of the facts mentioned prove that the brain may be injured on one side, while the manifestations of the mind continue. This, however, is easily explained, by the cerebral parts being double as well as the eyes, ears, and other senses. Was the Reviewer unacquainted with this circumstance?

Some cases are mentioned, where the whole brain was destroyed, while the mind continued to manifest its powers. Dr Quin's, and especially Sir Everard Home's authority is relied on, p. 447. This gentleman saw a "female child, born hydrocephalic, the head being very large. She lived nearly five months; during this period, nearly 128 ounces of fluid were drawn off from the head at six successiveappings. She was not disordered by the

operations, and, notwithstanding the progress of the disease, continued healthy and strong until within twelve days of her death, when she fell into a wasting. On opening the head, two quarts of a clear pellucid fluid were found within the cranium. The dura mater was complete, the edges of the falx and tentorium in contact with the fluid. The spinal cord was seen at the large hole of the occipital bone, and a little medullary bulb behind the orbits, but that was all that could be found for brain."

There are many cases related in writings, where it is said that there was only water in the cranium, and no brain at all. Sir Everard Home, whose short essay gave to the Reviewer the occasion of writing a long article, seems to have been endowed with the *second sight*, relatively to hydrocephalic heads. It was a great omission, certainly, in the Reviewer, not to copy from Sir Everard's paper that singular case, which never could occur, described as follows: (*Philosophical Transactions for the year 1814. Part II. p. 473.*) "In a boy the enlargement of the head was perceived at three months, and increased for three years, and then appeared to be stationary; and the child till that period was sensible. The upper part of the skull, from that time, began to ossify; and in three years more there was only an irregular space of the os frontis remaining open. The child continued sensible till three years old, and then became gradually less so; did not know what he did; heard sounds, but could not see. At six years old he died. The child was three feet three inches high; the skull twenty-seven inches round; the water contained in the two lateral and third ventricles, was six ale pints and a half in quantity. The cerebrum formed a thin case of medullary substance, surrounding this cavity. The cerebellum was entire." In a note Sir Everard adds, "The lining of the lateral

ventricles was tough; the septum lucidum elongated, so that the corpus callosum was raised up close to the skull; the *falx of the dura mater* being *entirely obliterated*. The water in the third ventricle had split the fornix and septum lucidum into two, and the thin membranes in the lucidum had holes in them, making a communication between the third and lateral ventricles. The substance of the brain surrounding these cavities, as well as the pia mater covering it, had no convolutions; there was a continued smooth surface. On the right side, upon which the child was usually laid, there were no remains of medullary or cortical substance, and there the pia mater and dura mater adhered together; there was no remaining brain between the third ventricle and sella turcica. On the left side of the left hemisphere the medullary and cortical substance was only half an inch thick. The corpora striata and thalami nervorum opticorum were small and tough; the union between the thalami was elongated into a broad flat ligament. The two commissures and iter ad infundibulum had the natural appearance. The olfactory nerves were tough and small; the optic nerves had no medullary pulp; the other nerves going out of the skull had undergone no change."

Why has this infallible Reviewer written so many essays against miracles? Was it this case which induced him to exclaim, p. 448. "This essay we have little hesitation in pronouncing to be one of the most creditable papers which Sir Everard Home has produced. The object of it is quite philosophical, and it is respectably executed." I beg, however, leave to remark, that *such things* as are here stated by Sir Everard, are in absolute contradiction to nature and to reason. Who could see that the two commissures and the iter ad infundibulum had the natural appearance, while there was no remain-

ing brain between the third ventricle and sella turcica, that the pia mater, viz. the blood-vessels of the brain, existed on the right side, while on that side there were no remains of medullary or cortical substance; that the corpus callosum was lifted up, the fornix and septum lucidum split into two, and therefore the communication between the third and lateral ventricles established; that six pints and a half of water were contained in the two lateral and third ventricles; that the cerebrum formed a thin case of medullary substance surrounding this cavity; that the substance of the *brain surrounding* those cavities, as well as the pia mater covering it, had no convolutions; that there was a continued smooth surface; that the lining of the ventricles was tough;—while at the same time there were no remains of medullary or cortical substance on the right side;—that the corpus callosum, the fornix, and the commissures existed without brain on the right side? He who believes in such assertions, places credit in them in the direct ratio of their impossibility; because the existence of lateral ventricles, a thin case of brain, brain half an inch thick, and no brain, are employed to designate the same observation.

If the Edinburgh Reviewer can praise a paper which contains such things, I am proud that our works merited none of his approbation. At all events, “*Judex damnatur cum nocens absolvitur.*” To support my judgment, I say, that the article gives a very imperfect idea of Sir Everard’s paper. Every reader of the article thinks, that the original essay contains the adduced facts, while Sir Everard has not quoted a single author, as if he were the first who had begun to make observations of that kind. It is true, no other can make such observations as the above; but many authors were attentive to the results of injuries of the brain. The Reviewer himself states,

p. 449. that "the greater number of the cases in the paper before us, are so far valuable, only as they serve to confirm what had already perhaps been sufficiently made out by the authors we have just named," (the Reviewer, not Sir Everard Home); viz. "That there is no sort of uniformity either in the kind or the degree of the symptoms which accompany the diseases of the brain." Afterwards, when I speak of our means of discovering the functions of the brain, I will say more of the method employed by Sir Everard Home. Here it is sufficient to have shown, that the Edinburgh Reviewer deserves the application of the law established by himself.

With respect to the non-existence of brain in hydrocephalic heads, Morgagni already has severely blamed his predecessors, especially Duverney. He declares, that in cases perfectly similar he has always found the brain distended into a thin membrane; and he relates, that the same has been observed before him by Tulpius, Vesalius, and several other anatomists. He has also shown, how anatomists, by mere inadvertency, imagine, that the water is contained between brain and skull. The subject is treated at considerable length in my work on physiognomy, p. 147—158.

In addition to the preceding remarks, it may be said, that the literary gospel of Edinburgh does not only believe in the manifestations of the mind without brain, but also in the possibility of exercising voluntary motion of the lower extremities without spinal cord. This curious article, in fact, refers to the case of "a young man who had his spinal cord completely cut across, opposite the tenth dorsal vertebra, by a musket ball, and yet did not suffer the slightest loss of voluntary motion in the lower part of the body." If critical reviewers believe in such things, which are in contradiction to the observa-

tions of all ages and nations, they may, with the same propriety, believe in the stories of giants, of people without teeth, or without neck, in the existence of nations who have lost their tails, and others who still preserve this honourable mark of affinity with the brutes. And we may apply to them their own words: "If they *succeed* in convincing a single individual of common parts and observation that this assertion is truth, they will find little difficulty, we apprehend, in persuading mankind in general, that they hear by their eyes, and see by their ears." NO. 49. p. 247. We think nature is constant in its laws, and never makes an exception. If the spinal cord is necessary to voluntary motion, this latter will never occur without the spinal cord. The time will explode, I trust, such marvellous notions, according to which the manifestations of the mind can appear without brain, and voluntary motion without spinal cord, and able philosophers will explain the large hydrocephalic heads according to sound principles of anatomy and physiology.

Thus we maintain, that there is not one fact well ascertained, that the mind has shown its powers, while the brain, or rather both brains, were annihilated. As to the second part of our proposition, viz. that each species of manifestation of the mind depends on an appropriate part of the brain, I will not quibble long about indirect observations and inductions, but proceed immediately to direct facts and experiments.

SECTION II.

WE endeavour to ascertain *the nature* of the functions of the cerebral parts, by the influence which the size of the organs has on the phenomena of the mind. I beg

to remark, that we do not pretend to distinguish by the size of the organs with what degree of energy the mental powers appear. To do this, we must consider, besides the size of the organs, their internal constitution, their exercise, and the mutual influence of the powers. This distinction is kept in view throughout all my work on physiognomy. In the second edition, which the Reviewer quotes, p. 190, 191. I have detailed our opinion concerning the absolute size of the brain, and conclude, "It is not, however, possible, even in individuals of the same kind, to measure their faculties according to the absolute size of their brain. Hence, it is necessary to look for other means of determining the degree of the faculties of the mind." Pages 215. and 216. I have said, "In order to judge exactly of our proceeding, it must be considered, that we do not endeavour to determine every degree of activity of any cerebral part, but only the nature of its functions; and to this end its size is sufficient." "I admit even the possibility, that in the same individual, the internal constitution of the different parts of the brain may vary, in the same way as the optic nerve may be more irritable than the auditory or olfactory." The critic might also have read, p. 526. "I have often repeated, that in speaking of the actions of men, it is not sufficient to consider the size of the organs of the respective faculties, but that the internal constitution of the cerebral parts, the exercise of their faculties, and their mutual influence, contribute also to their different degrees of activity." Notwithstanding, the conscientious Reviewer tells his readers, that "Gall and Spurzheim, in affirming that the vigour of intellect is always proportional to the size of the head, seem to have been desirous of trying how far their effrontery might be carried." No. 49. p. 247.

The learned critic goes so far as to assert, p. 245. "that there is not the slightest approach to a uniform connexion between the vigour of intellect, or the strength or peculiarity of inclinations in man, and the size of the brain; that intellect of every degree and of every kind, and inclination of every variety, is found combined with brains of all sizes. Page 246. he repeats, "We deny, that there is any constant correspondence, or any connexion whatever, between the dimensions of a man's head and his intellect and inclinations, either in kind or degree.

When I first read the preceding passages, I was giving lectures in Dublin. My auditors at that time will recollect, that, in showing to them a cast, and the picture of a gentleman, I publicly declared, that "if the conscientious person who had written the article on our doctrines in the Edinburgh Review, has such a configuration of head as the cast or the picture, I would give up my farther investigations into the functions of the brain." Since that time I have repeated everywhere the same declaration; and I am convinced that no one, whose head offers such a configuration as that above referred to, could have acted as the Reviewer, without subsequent repentance.

Our numerous observations concerning the influence of the size of the brain on the manifestations of the mind, induce us to maintain, that a too small brain is unfit for the operations of the mind; and that the greater number of idiots from birth have too small brains, and a few of them too large heads, that is, heads distended by water collected in the interior of the brain. We, however, do not say, that all idiots have small heads. Idiotism, in fact, may be observed in heads of every size.

The learned Reviewer replies, p. 246. "We affirm it to be, that idiots in general have *uncommonly* large heads." I should like to know where he has made his observations. On the Continent it is as we state; and I found the same in England, Ireland, and Scotland. Even in Edinburgh nature makes no exception. In the Poor-house near the West Church I saw four ideots; none had a large head, but one had an uncommonly small head. A silly boy with a very small head, is met in the streets of Edinburgh, to the sport of other children. On the other hand, I found several hydrocephalic individuals, who are not ideots. One of them, the most remarkable, lives in Musselburgh. The head of this person, who is 23 years of age, is 39 inches in circumference; but the manifestations of the mind are not suppressed.

Secondly, We maintain, that men of great or universal talents never have small brains; but we do not assert, that large heads are always accompanied with great genius. The explanation of these different propositions is understood, because the size of the brain is a necessary, but not the only condition, to the manifestations of the mind. The internal constitution is as important as its size.

Lastly, We maintain, that in the same individual one part of the brain, being much larger than the others, shows its superior influence on the manifestations of the respective power, in the same way as, in the same person, one muscle, being much larger than the others, shows greater strength of voluntary motion.

These different assertions can be decided by experience alone.

SECTION III.

THE question arises, whether it is possible to distinguish the size of the brain and its parts by the exterior of the head. We affirm that it is so, as far as it is necessary to our purposes.

The Edinburgh Reviewer imagines, that the head must be opened to examine the size of the brain and its parts. If, however, that were the case, only a small number of observations could be made; but as in living persons the size of the brain can be distinguished, observations of this kind may be easily multiplied. It is, however, understood, that the dimensions of the brain are smaller than those of the head; but as there is no empty space between brain and skull, great external differences of size and form in the head, correspond to analogous internal differences in the brain. It is to be observed, that we draw no inference from small insignificant differences of dimension. This explains also, why the teguments and the two tables of the skull, not being exactly parallel, do not prevent our observations in young and adult persons: our inquiries, however, are uncertain in old age; the brain then often diminishes in size, while the external form and size of the head remain the same as they were before. The objection, that the two tables are not parallel, is often repeated, but can be made only by those who have never seen the external marks which we consider as indications of larger cerebral parts.

The conscientious Reviewer states, p. 252. "The difference of the different regions of the brain, whether it be confined to one dimension, or extend to all, is very inconsiderable, seldom, we believe, amounting to half an inch, and never, we are confident, exceeding one inch.

over an extent of six inches, and often it is so small as just to be perceptible and no more."

From this statement I draw the inference, that this learned critic has not compared many heads. Any contractor who furnishes hats to the army could have given him better information. I can assert, that I have skulls in my collection, some of which, in certain dimensions, are the double of others. It is true, there are cases where the difference is scarcely perceptible, but these heads are not the subject of decisive observations.

The conscientious Reviewer was not satisfied with displaying such unusual knowledge, but continued, p. 242. "It is not true, that there are ever such eminences on the surface of the brain, accompanied with projections of the cranium, as Gall and Spurzheim have affirmed;" and p. 253. "We venture to affirm, that such prominences on the head as Gall and Spurzheim have described, indicating certain eminences of the brain within, and uniformly accompanying some peculiarity of intellect or inclinations in the individual, never have been observed; and that all they have been so good as to write on this subject, is a mere fiction. Were it worth our while, we could even undertake to show, without much difficulty, that this piece of invention is inconsistent with itself, in various circumstances, and that it presumes a degree of blindness and ignorance in those to whom it is addressed, which it was really very cruel in Drs Gall and Spurzheim to suppose."

I reply only, that in Edinburgh as well as in other places, in my public lectures, I have shown such prominences of which we speak, on real skulls which I have in my collection. And with regard to the acuteness of the Reviewer in such observations, he will not accuse me of ever placing much reliance on him.

SECTION IV.

EXPERIENCE alone can decide concerning the accuracy or inaccuracy of our observations and inductions. In my work on Physiognomy I have declared, that "we never admit exceptions; that, when an exception occurs, it proves that the truth has not yet been discovered, p. 258.;—that I never advance any thing that cannot be observed by every other person; that I do not listen to any objection founded upon reasoning alone; and that one fact, well observed, is to me more decisive than a thousand metaphysical opinions," p. 270.

The Quarterly Review, however, thought it suitable to tell its readers, "Of course, one instance is very properly considered just as satisfactory an evidence that the conclusion is conformable to fact, as a hundred would be," No. 25. p. 169. "Even admitting this system of Drs Gall and Spurzheim to be ever so plausible as an hypothesis, it cannot possibly derive any sort of evidence from experience. For the same reason, it is equally impossible to contradict it from experience," p. 171. "Even allowing, that the arguments of Drs Gall and Spurzheim, instead of being *sheer nonsense*, had been ever so ingenious and acute, still they could not throw the slightest probability upon the doctrine which they wish to establish, because that doctrine is *matter of fact*, and matter of fact never can be proved by reasoning *a priori*. Whether every protuberance upon the head be, or be not the sign of some particular character of the mind, is clearly a question *of fact*; let it therefore be proved to be a fact, as all other facts are proved: in such a case, the explanation which Drs Gall and Spurzheim propose, would at least have a fair claim to be

heard," p. 177. This is another clear specimen that Reviewers can criticise books without reading them!—From p. 262 to 271. in my book, our proceeding is quite differently described. I will copy only one sentence, p. 264. "It is known that, in general, physical truths improve in proportion as observations are repeated. We continue, therefore, to multiply our observations, and as, in respect to several organs, the number of these observations is immense, we consider the respective organs as established. With regard to them, we must insist on our opinion, so long as from experience we are not convinced of the contrary. Several organs, however, are still only probable, and others merely conjectural, requiring a greater number of observations, in order to be determined with the same degree of certainty, as those which are supported by the most satisfactory proofs."

The conscientious examiner of Edinburgh, with respect to our proceeding, made "some effort, and briefly observed, that not one of our assertions is true, and that not one step of our reasoning is correct," p. 252. "Can it be possible," asks the philosopher, "that the great Drs Gall and Spurzheim have not observed, in the course of their multifarious inquiries into nature, that phenomena may coincide, without being related to each other as cause and effect? Were it established, that all great mathematicians had black eyes, and all poets blue ones, would any sensible man, from this alone, think of ascribing the mathematical talent, in the one case, or the poetical genius in the other, to the colour of the iris?" p. 247.

Had this learned Reviewer also studied Chap. I. of Part III. of my book, he would have seen, that we are aware of the difference between coincidence and the relation of cause and effect to each other, and never lose

sight of it; that we prove our assertions in the same way as any physical truth. If, however, an observer could shew, that only mathematicians have black eyes, and only poets blue ones; that every one who has black eyes, and no one but those, have mathematical talents; or that every one with blue eyes, and only those, are born poets: if he could repeat his observations in various countries; if he could compare the same talents through a series of animals, without finding an exception; if he could support his observations by other means which I have detailed in my book, he might establish a physiognomical sign, and challenge his opponents to shew the contrary. So we do. If, for instance, we speak of a sign of self-esteem, let us see that a man, the most prominent feature of whose character is composed of self-conceit, does not exhibit the sign on his head, and we give up all our observations with respect to this peculiar organ. In the same manner, and by no other means, each organ is to be refuted by one single exception well ascertained.

It cannot be useless to call the attention of the reader to that method which the literary gospel of Edinburgh, NO. 48. Art. x. p. 448. recommends, as follows: "Sir Everard Home's Essay not only possesses a proper method of investigation, but sets an example of it, and is entirely free from the nonsense which is so commonly and so copiously put forth in writings upon similar subjects." Which is then the proper method of investigating the functions of the brain? This the reader does not acquire from the critical Review, but he may learn it from the original paper, inserted in the Philosophical Transactions for the year 1814, Part II. Sir Everard Home tells us, "The various attempts which have been made to procure accurate information respecting the functions that belong to individual portions of the human

brain, having been attended with very little success, it has occurred to me, that were anatomical surgeons to collect in one view all the appearances they had met with in cases of injury to that organ, and the effects that such injuries produced upon its functions, a body of evidence might be formed that would materially advance this highly important investigation." He then informs us, that he has brought together certain observations, "stating them as so many experiments upon the brain, with the conclusions which tend to elucidate this particular injury."

Every one will be anxious to know these observations. We read, "that in the torpid state, commonly attendant upon any violent shake being given to the brain, the senses are so much impaired, that little information can be gained respecting the effects produced upon the internal organs;—that a *coup de soleil* is sometimes accompanied by delirium, loss of speech, and the power of swallowing;—that blood extravasated in the lateral and third ventricles was attended by repeated fits of vomiting and coma;—that coagulable lymph spread over the union of the optic nerves, the pineal gland, and tuberculum annulare, was followed by permanent contraction of the muscles between the occiput and vertebræ of the neck, dilatation of the pupils, and a great degree of deafness;—that the formation of pus under the dura mater covering the right hemisphere, was accompanied by delirium, succeeded by coma;—that a tumour in the substance of the posterior lobe of the brain was attended with derangement of the functions of the stomach and bowels, and with double vision;—and that a deep wound into the right anterior lobe of the brain, attended with inflammation and suppuration, produced no sensation whatever, the senses remaining entire, and the person not knowing

that the head was injured. In a case, also, in which the tuberculum annulare had become so hard as with difficulty to be cut with a knife, a considerable quantity of earthy particles having been intermixed with the medullary substance of the crura and other parts of the cerebellum, and the cerebrum, and upper part of the cerebellum being unusually soft, the effects were, that the boy had been an idiot from birth, never walked, spoke and understood what was said, often went three days without food, and so on."

Sir Everard Home speaks in a manner as if no one before him had made similar observations. His kind Reviewer, however, shews by his numerous quotations, that Sir Everard is mistaken. Indeed, every one who is but half acquainted with the history of the healthy and diseased state of the brain, knows, that many authors have related similar facts. Nay, we learn from them also, that similar injuries of the brain have often been observed without any perceptible derangement of the mind, or any apparent disease of automatic life.

Hence this mode of proceeding is quite unfit for discovering the functions of the brain, and any hope from such a source is in vain. I support my opinion by the fruitless attempt of a great number of authors, and by the unsuccessfulness of Sir Everard Home himself. It is true, he speaks of a body of evidence which might be formed, and of conclusions which tend to elucidate this particular inquiry, but he has not drawn even one inference. In the various pathological affections of the brain, he has observed head-ach, giddiness, faintness, loss of memory, want of sleep, delirium, mania, depression of spirits, melancholy, apoplexy, idiotism, hissing noise in the ears, deafness, blindness, loss of speech, irregular pulse, stupor, the mouth drawn to one side,

numbness of the arms and legs, spasms in the lower extremities, stumbling in walking, pain between the shoulders, nausea, retching, slow action of purgative medicines, vomiting, convulsions, &c. Is Sir Everard Home, perhaps, inclined to draw the inference, that the brain is the organ of these symptoms, or of the states which are opposite to them? This is, I think, sufficient to shew an intelligent reader, that in this way we never shall be able to determine the peculiar functions of the cerebral parts;—that the *Edinburgh Review*, for praising such a paper, deserves no more credit with respect to the physiology than to the anatomy of the brain, and that these critics, as they believe in the existence of cases which are in contradiction to nature and reasoning, have still a great deal to learn before they can become competent judges.

SECTION V.

As to the individual organs of the manifestations of the mind, the literary gospel states only, “To enter on a particular refutation of them, would be to insult the understandings of our readers. Indeed, we will flatter the authors so far as to say, that their observations are of a nature to set criticism entirely at defiance. They are a collection of mere absurdities, without truth, connexion, or consistency; an incoherent rhapsody, which nothing could have induced any man to have presented to the public, under a pretence of instructing them, but absolute insanity, gross ignorance, and the most matchless assurance.”

Such arms, however, will not repel stubborn facts. Our antagonists, it seems, find it more easy to blame than to study, or to deny than to observe. They have

not even considered the meaning of the expressions by which we designate the various powers of the mind. The Quarterly Review, for instance, states that the name *Inhabitiveness*, which I give to the instinct of animals, to live in water or on dry land, in higher or lower regions, and so on; to that instinct, which determines a young duck, as soon as it is hatched, to run towards the water, and the ptarmaghan to dwell at the tops of the mountains, &c. means "a love of dwelling in elevated situations." He explains *Secretiveness* by the love of stealing. The natural history of the two species of rats, the black and the brown, he found very ridiculous; and he thought it sufficient to exclaim, "Credat Judæus Appella!" to change the cerebral organization of these two species of rats. I, however, must continue to say, that the difference of the brains of both species is easily distinguished. My auditors will recollect to have seen it. Thus, I repeat, to incontestable facts alone I shall pay further attention.

The only reasonable difficulty started against the possibility of distinguishing the organs at the lower part of the forehead, and behind the orbits, originates from the frontal sinus, and from the circumstance, that the brain, situate behind the orbits, and between both hemispheres, does not reach the surface of the skull. As, however, I have stated this difficulty, and given our explanation, the Reviewer ought to have copied our answer, instead of saying, "How could these gentlemen think so poorly of the eyesight of their readers, as to imagine, that, by the aid of their beautiful engravings, they could fail to discover, that some of the prominences in the skull which they describe, are said to be caused by elevations and portions of the brain, which are not even in contact with the skull of these parts?" p. 253.

I always show to my auditors the difference between the external bony crest, often erroneously called frontal sinus, and the elevation, which we consider as a greater development of the organ of locality. They will also recollect my demonstrating, that children, and young and adult persons, have no holes between the two tables of the skull at the forehead, and that the real frontal sinus occur only in old persons, or after chronic insanity, in general, when the brain is diminished in size. I will copy only one passage from my book, in opposition to that of the Edinburgh Review. "The cerebral parts, situated behind the orbits, require some exercise on the part of the physiognomist, in order to be exactly determined. Their development is discoverable from the position and configuration of the eyes, and from the circumference of the orbits. It is, therefore, necessary to examine, whether the eye-ball is prominent or hidden in the orbit, or whether it is placed inward or outward. According to the position of the eye-ball we may judge, whether the part of the brain which is situate against a corresponding part of the orbit, is more or less developed.

"It may be questioned, whether all organs reach the surface, so as to enable us to determine the organs of all faculties of the mind by the size and shape of the head? There are, indeed, many convolutions in the middle line of the brain between the two hemispheres; and there are also some others at the basis of the brain, and between the anterior and middle lobes, which, therefore, do not reach the surface of the skull; but it seems to me that a great part at least of every organ lies at the surface, and that if one part of any organ be well developed, the whole participates of this development. The whole cerebellum does not touch the skull, yet it is possible to

determine the size of the cerebellum, according to that part of it which reaches the surface. Accordingly, the cerebral parts, which are, as above noticed, situate in the middle line between the two hemispheres, seem to be proportionate to the superincumbent organs; at least I have always observed a proportion in the vertical direction, between these cerebral parts. In this way, it appears to be possible to determine all the organs, though the whole of their fibres do not terminate at the surface," p. 237, 238.

There remains still an idea to be corrected. In pointing out the functions of the cerebral parts, and in ascertaining, that the size of the organs has some influence on the innate dispositions of the mind, we establish, in a certain degree, a physiognomical doctrine. This has been most erroneously represented by the conscientious Reviewer, in saying, p. 250. "The practical part of their doctrines, as it may be called, the physiognomy, cranio-logy, or cranioscopy, the part which teaches us how to find out, by the shape of the head, whether a man loves his children or kills them; whether he steals or is very benevolent!" We, however, continually maintain, that we never can speak of the actions of man; and after having mentioned the title, *Physiognomical System*, I begin the introduction of my book, "This system is commonly considered as one, according to which it is possible to discover the particular actions of individuals: it is treated as an art of prognostication. Such, however, is not the aim of our inquiries; we never treat of determinate actions; we consider only the faculties man is endowed with, the organic parts by means of which these faculties are manifested, and the general indications which they present."

Thus, the more the reader will compare our works, and the reports given by our antagonists, and their and our opinions with nature, the more he will be enabled to decide of whom it may be said, "Were they even to succeed in shaking off the suspicion of *mala fides*, which we apprehend is inseparably attached to their character, we should not hesitate to say, that we do not know any writers, who, with a conceit so truly ludicrous, and so impudent a contempt for the opinions and labours of others, are so utterly destitute of every qualification necessary for the conduct of a philosophical investigation." *Edinburgh Review*, No. 49. p. 228.

CHAPTER III.

PHILOSOPHY.

THIS chapter may be very short, since in this department our British antagonists confine themselves to general considerations. The logical study of the author in the *Quarterly Review*, No. 25. p. 165. is the most simple: he admits in the mind only one understanding, and in that one he seems defective. "There is," says he, "no more solid reason for dividing understanding into faculties, than for dividing heat or light into faculties." This comparison, however, of understanding with heat and light, is not very apt for simplicity, since neither has been proved to be a single substance. Besides, as one single understanding does not explain the phenomena of the mind, and as all other logicians found it necessary to

adopt several powers, I leave him to make the best use of his one faculty, and proceed to other propositions.

The Edinburgh Review, as to the faculties which we adopt in the human mind, says, p. 243. "The ratiocination of Drs Gall and Spurzheim is of the most difficult species to combat. Perhaps we might content ourselves with saying, that the whole doctrine of the thirty-three faculties to which the argument relates, is downright nonsense, and so put an end to the discussion at once;—but we shall take the liberty of substituting for the names of the thirty-three faculties, two very simple and intelligible terms, viz. *intellect* and *inclination*."

The reasoning, or rather dogmatic decision of a Reviewer, certainly will not repel stubborn facts. I, however, should like to know, why the conscientious Philosopher adopts intellect *and* inclination. May I suppose that he does so, because one or the other alone does not explain the phenomena of the mind? Indeed, there may be strong inclination without intellect. But is inclination always the same? Is, for instance, the inclination of the hen towards the young duck, hatched by her, the same with the inclination of the young duck towards the water? Is the inclination to calumny or respect, to concealment or candour, one and the same? In the same way, is intellect only one? In a boy who can repeat by heart whole pages after having read them once or twice, but cannot compare or distinguish two separate ideas, is the intellect the same as in another who judges with precision of various ideas, but cannot recollect by heart one page? Thus, as we can have one inclination, or one intellect, and not another, philosophers have divided the powers of the mind into different sorts. Now we maintain, that those powers which are adopted by logicians as primitive or special faculties, do not explain

the phenomena of the mind in the state of health and disease. Hence we admit a greater number, and as many as are necessary for the explanation of the manifestations of the mind. Particular and great innate talents, such as for mathematics, or music, or mechanics, and so on, while the other faculties are extremely defective, viz. partial geniuses, who are in every other respect almost idiots, induce us to consider such powers as special. If then we find, by constant observation, that the manifestations of such a power are never separate from the development of a particular part of the brain, we adopt all that is common to the manifestations belonging to one cerebral part as the result of one special power, in the same way as it is acknowledged that all the manifestations of vision belong to one sense. Thus, in the division of the mental operations, we are guided merely by observation and induction. Pride, for instance, cannot be explained by external circumstances alone, nor by intellect or inclination in general; if now its appearance is always connected with a peculiar part of the brain, independently of the other powers of the mind, and of the other cerebral parts, we maintain that it belongs to a special faculty, different from the others. We then observe the different manifestations of this sort, and try to reduce them to one common consideration. Now, whatever speculative reasoning our adversaries may oppose, we insist on our observations, and will yield to facts alone.

Our philosophy of the mind differs from all preceding opinions of the schools. Hitherto the special faculties of the mind were overlooked, and philosophers were satisfied with general or common considerations of the powers, or with the modes of their being affected. Instinct, for instance, in animals is a mere general view, viz. every in-

ternal impulse to act. But the impulse to build, or to sing, or to migrate, or to amass provisions, or to place sentinels, &c. cannot be the same impulse, any more than hearing, seeing, smelling, or tasting, are the same sensation. Hence, the philosophers were satisfied with the general view of instinct, and paid no attention to the special instincts.

An example of a common consideration is perception, that is, perception is common to various powers; but the perception of the size, form, colour, or place of an object, are quite different sorts of perception. In the same way, memory is always a reproduction of the impressions which we have perceived, but there is not one memory for every previous perception. One sort of memory may be very energetic, and another quite defective.

We admit two sources of activity in the mind, an internal and external. To the former belong the instincts of animals, and the propensities and sentiments of man; to the latter, the intellectual operations, as far as we acquire knowledge of the external objects, their qualities and relations. Some powers make man act, others modify, assist and direct the actions; still there are others destined to bring all the other faculties into harmony, and to constitute unity.

One of our ideas, viz. the introduction of consciousness, sometimes active and sometimes passive, in the five senses, puzzled the Edinburgh Reviewer (p. 241.) a good deal. The difference, however, seems to have been observed at all times, since in all languages there are two sorts of signs to express it. In the English we say, I see (passive) and I look at (active); I hear (passive) and I listen (active); I feel (passive) and I touch (active), &c. In other words, consciousness is sometimes involuntary, sometimes voluntary.

These and other considerations are too complex for the simple philosophy of the Reviewers. As our opinions are not attacked in the particulars, there is no occasion for my giving here a more detailed explanation. Those who are desirous of knowing our philosophical propositions, will find them in my work on physiognomy. I have only to add, that if the conscientious Reviewer has found in himself only intellect and inclination, I leave it to others to judge, whether they have found his intellect limited in judgment, and his inclination extensive in malevolence.

CONCLUSION.

CONSIDERING the whole of the preceding statements, I may say, that I have done with those who arrogate the right of thinking and deciding for the rest of mankind; with those "thorough partizans, who are thorough despisers of sincerity;" (*Edin. Review*, NO. 53. p. 14.); who will not allow the least credit to any one that has not their approbation; who anonymously calumniate and detract; who, in doing so, claim the merit of conscientiousness; who disguise, mistate, and misinterpret; who invent ridiculous monstrosities; who, in using the most vulgar language, speak of personal dignity and politeness; with beings who change assertions as it seems convenient; who do not understand the passages which they quote;—who, from different chapters, extract sentences, illustrating different propositions, and represent these their own fictions, as nonsensical and absurd conceptions of the author;—with such writers on the brain, who have nothing in view but minute mechanical differences of size and form, and shades of colour; who, however, cannot see brown substance in the pons Varolii;—who, as if there were not, from ancient times, absurd names enough, invent in

the brain, cul-de-sacs, pits, grooves, mountains, wings, lobules, and so on ;—who never consider the parts in connexion and relation, nay, create artificial separations ;—who are attentive only to the mechanical appearances, and never think of the functions of the parts ;—who believe, that a man can walk, and have voluntary motion of his legs, without spinal cord, can philosophize without brain ;—who can assert, that physiological inquiries of the brain are of no use to the medical profession ; who consider one brain and its parts as the standard of all other brains ; who admit, that the brains of *men* have their full growth at seven years of age, and do not undergo any change afterwards ;—and with such Historians, who affirm from erudite research, and as the result of many experiments, made under a variety of circumstances, that there is no foundation whatever for the supposition, that the convolutions consist of two layers ;—who maintain, that numerous unequivocal instances are on record, and are even occurring every day, in which large portions of the brain, nay, almost the whole, if not actually the whole of this organ, have been completely destroyed by the progress of hydrocephalus ; who hold this to be a fact just as certain as that there are many persons now alive whose legs have been removed by the knife of the surgeon ;—and who at another time prove, that we are not the first who maintain, that the brain exists in hydrocephalic heads, and that Reil could separate the convolutions in the middle line, after we had shown to him that structure four years before ;—who, as author on the brain, did not quote any anatomist to whom the decussation of the pyramids and the communication of the medulla oblongata with the crura cerebri were known ; who ascribes the medulla oblongata to the spinal cord, the mass of the pons to the cerebellum, and termi-

nates the brain at the upper edge of the pons;—who denies the possibility of demonstrating the two sets of fibres, (diverging and converging);—who does not mention the two layers of the convolutions;—and who afterwards, as pamphleteer, asserts, that long ago these things were known,—that especially we have defrauded Reil, who published four years after we had shown him our anatomical discoveries, after we had demonstrated them in different countries, in the Universities of Germany, Denmark, Holland, and in Paris, and after the publication of numerous extracts by our pupils;—who tells his readers, that his pamphlet owes its origin only to his strong anxiety for the progress of medical knowledge, and deep concern for the reputation of a medical school which was indebted to anatomy for its first celebrity throughout Europe, but who makes morbid dissections, even in very rare cases, in the manner I have witnessed and described above;—who in that very pamphlet accuses all anatomists, and almost all medical professors and teachers of Edinburgh, and every one of my auditors, as unfit to distinguish brown and white substance;—who, in his “painful” compilation, forgets the Monros, who deserve to be mentioned as well as Malpighi and Mayer; a neglect the less excusable, that Monro was one of the chief founders of the celebrity of the medical school of Edinburgh.—Certainly, with such critical Reviewers, such would-be Philosophers, such mechanical Dissectors, and such Historians, I have done for ever;—and I may say, with Job, (xiii. 5.) “Oh, that you would altogether hold your peace, and it should be your wisdom!”

THE END.

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