

**On the functions of the cerebellum / by Drs. Gall, Vimont, and Broussais ; translated from the French by George Combe: also answers to the objections urged against phrenology by Drs. Roget, Rudolphi, Prichard, and Tiedemann; by George Combe and A. Combe.**

### **Contributors**

Combe, Andrew, 1797-1847.  
Vimont, Joseph, 1795-1857.  
Broussais, F. J. V. 1772-1838.  
Combe, George, 1788-1858.  
Gall, F. J. 1758-1828.  
Royal College of Physicians of Edinburgh

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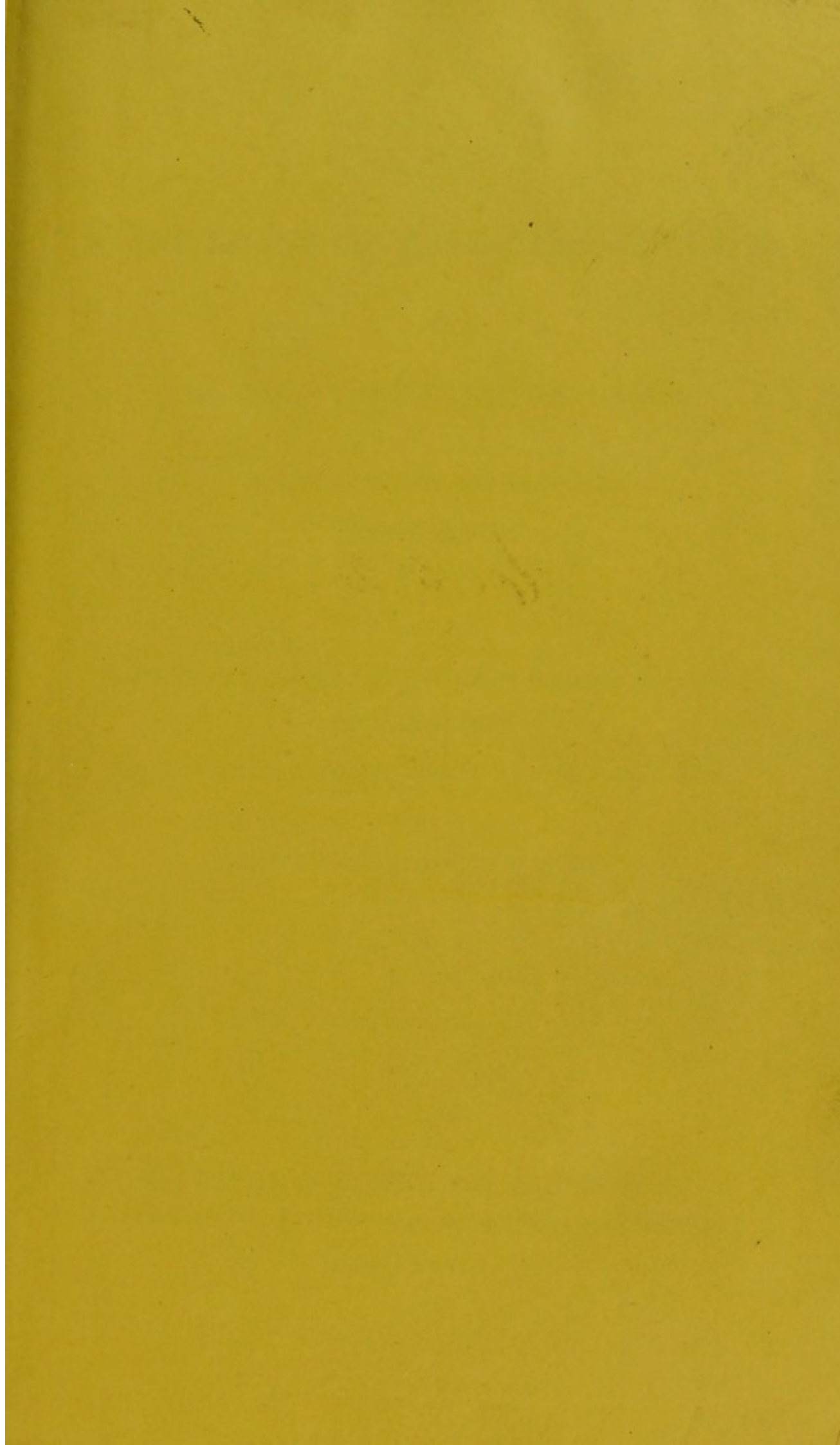




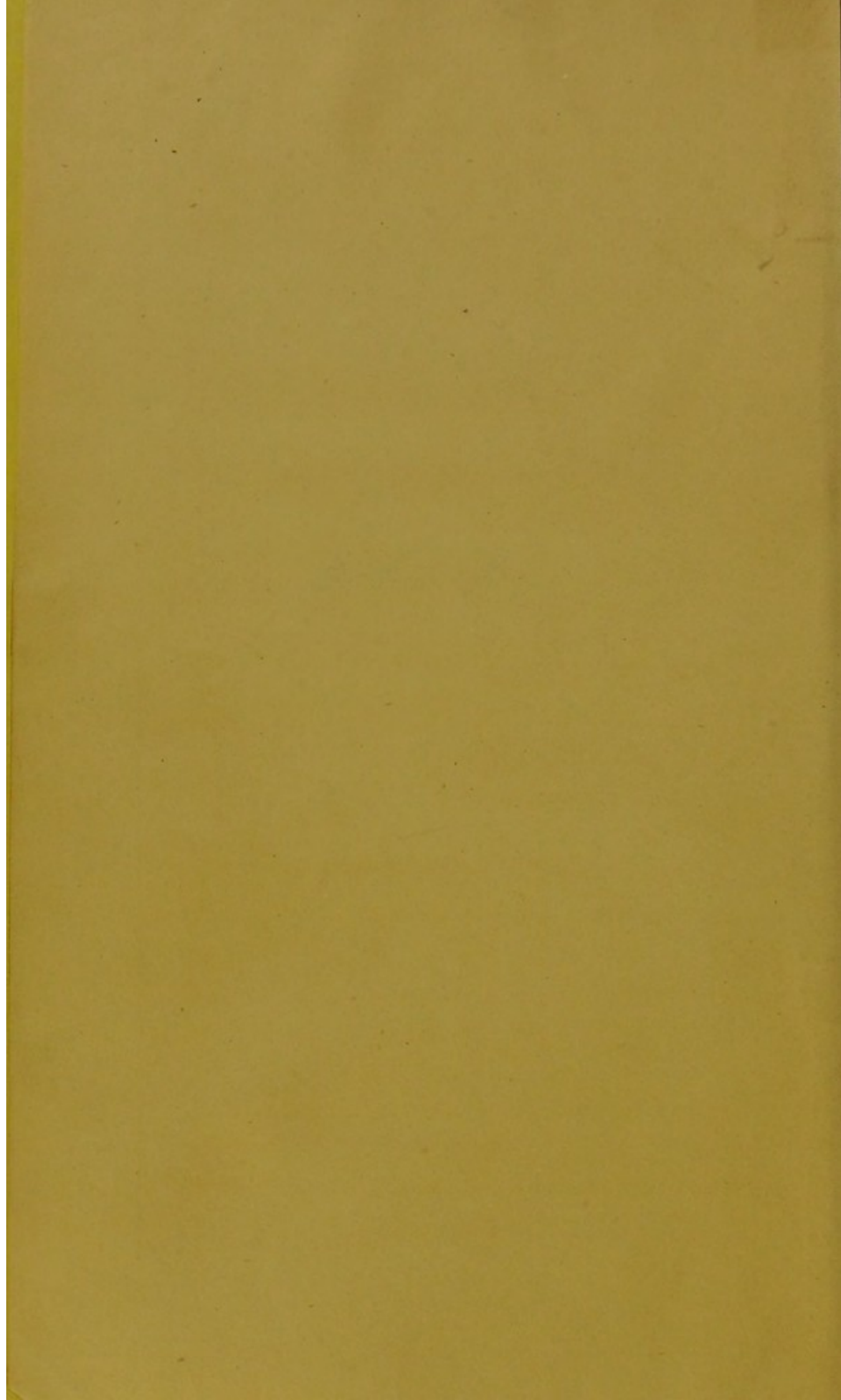
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ON THE  
FUNCTIONS OF THE CEREBELLUM,

BY  
DRS GALL, VIMONT, AND BROUSSAIS,

TRANSLATED FROM THE FRENCH BY  
GEORGE COMBE:

ALSO  
ANSWERS TO THE OBJECTIONS URGED AGAINST  
PHRENOLOGY

BY DRS ROGET, RUDOLPHI, PRICHARD, AND TIEDEMANN;

BY  
GEORGE COMBE AND DR A. COMBE.

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FUNCTIONS OF THE CHURCH

OF THE CHURCH

IN THE

UNITED STATES

OF AMERICA

BY

JOHN W. ALLEN

NEW YORK

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 ERRATUM.

In page 92, line 2d, *for* Fabret *read* Falret

## PREFACE.

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THE instinct of reproduction of the species is one of the most powerful impulses of the human mind, and, according as it is well or ill directed, it becomes the source of much happiness or misery to the individual. Like all other instincts, its intensity and activity are directly dependent on the development and condition of the organs which serve for its manifestation during life. When these are largely developed, and the temperament is vivacious, the sexual feeling is naturally energetic, and then, unless it be controlled and directed by moral and intellectual considerations, it is apt to run into hurtful excess : on the contrary, when the organs are deficient in size, there is a natural feebleness in the power of manifestation, which may lead to bitter disappointment.

As, then, this relation between mental feeling and organization exists, it is not unreasonable to believe that an acquaintance with the mode of action and laws to which the instinct has been subjected by the Crea-



tor, will prove essentially advantageous in a sound regulation of its manifestations. If this view be correct, a proper investigation of the subject is important equally in a physiological and moral point of view.

We are indebted to Dr Gall for the discovery that the cerebellum is the organ of the sexual instinct ; and fortunately the evidence by which this proposition may be proved is easily accessible, and not difficult of application. The phrenologists of this country, however, have hitherto been precluded, by the peculiar nature of the details, from presenting them to the consideration either of physiologists or moralists. They could not be introduced with propriety into works on the general doctrines of Phrenology, because these were intended for the perusal of both sexes, and of persons of all ages and conditions ; yet the non-presentment of them was attended with many disadvantages. Much valuable information was thereby withheld from that class who had an interest to obtain it. Further, not only the opponents of Phrenology, but physiologists of respectable reputation, continued, year after year, in their books and lectures, to represent the experiments of Flourens, Magendie, and others, as conclusive evidence that the sole function of the cerebellum is to regulate the movements of locomotion, and to treat the investigations of Dr Gall on this subject as non-existing, or as a dream ; and, lastly, The reputation of Dr Gall, and the reception of his discoveries concerning the



functions of the brain itself, suffered great injury from this unjust mode of proceeding.

The remedy for these evils appears to me to be to present a translation of Dr Gall's investigations regarding the cerebellum, at full length, but as a separate work; and I have accordingly done this in the present publication. Professional and scientific men will thus enjoy the means of obtaining information that may interest them; while details, which in some respects are not suited for general perusal, will not be obtruded on persons for whom they are not intended.

I have added to Dr Gall's treatise a translation of some observations by Dr Vimont and Professor Broussais, of Paris, on the same subject, and presented also a variety of facts connected with the cerebellum, which have been observed in this country.

To physiologists, and to those who take an interest in the physical and moral improvement of mankind, a knowledge of the laws of organization which influence the mental manifestations in general, is equally important with an exposition of the functions of the cerebellum. I have, therefore, considered this as a fit opportunity for inquiring into the nature of the evidence by which the functions of the brain itself, regarding it as the organ of the mind, may be ascertained. An investigation into this subject, accordingly, forms a continuation of the work, after concluding the evidence relative to the instinct of reproduction. This



inquiry will be found to lead, and I hope on grounds that are at once logical and pertinent, to the conclusion, that the only method by which the functions of that organ can be ascertained, is by comparing the power of manifesting particular mental faculties, with the size and condition of particular portions of the brain.

It is well known, however, that although Drs Gall and Spurzheim, and their followers, have for forty-two years strenuously maintained the superiority of this method of investigation over all others, and have practically followed it, (as they believe, with distinguished advantage), yet that it has been rejected by physiologists in general, who seem desirous of exhausting every possible variety of error before they will adopt it, and who in consequence have failed in discovering either the truth of Phrenology, or the functions of the different parts of the brain.

This general rejection of the method is calculated to create a presumption against its sufficiency ; and young inquirers will naturally desire to learn on what grounds it has been condemned. To satisfy this philosophical curiosity, and to remove the force of this presumption, I have adduced the Objections urged against Phrenology by some of the leading physiologists of the present day,—Doctors Roget, Rudolphi, Prichard, and Tiedemann,—and endeavoured to expose the futility of the arguments, and the unfounded nature of the facts, on



which they have rested their opposition. The Answers to these Objections here presented have, indeed, already appeared chiefly in the pages of the *Phrenological Journal*; but as the attacks against Phrenology are contained in works which enjoy a general and extensive circulation,—as the periodical now mentioned is read almost exclusively by professed phrenologists,—and as the present work is intended for general scientific inquirers, the republication of the Answers in it, will probably prove acceptable to many who desire to be made acquainted with, and to embrace important truths, from whatever quarter they proceed.

The volume concludes with a translation, from the German, of Dr Gall's Defence against an Edict, issued by the Emperor of Germany, in 1802, prohibiting his lectures on the Functions of the Brain, and which order induced him to withdraw altogether from Vienna and the Austrian dominions. This document is contained in a small volume on Dr Gall's doctrines printed at Munich in 1804, which was presented to me by a talented young physician and phrenologist of Vienna, during my visit to that Capital in July 1837. It possesses great historical interest, and as I have not seen it in any other publication, I presume it will be new to most of my readers.

In presenting this work, I have to solicit the indulgence of the reader for its imperfections, on the ground that Physiology is not with me a professional study.

If there had been any immediate prospect of a more competent translator appearing to execute the task, I should not have undertaken it; but as, according to the best of my knowledge, the work, which has been already too long delayed, would otherwise have remained, for some years at least, unaccomplished, I have ventured to prepare this translation according to the best of my ability.

G. C.

EDINBURGH, 1st March 1838.



## INTRODUCTION.

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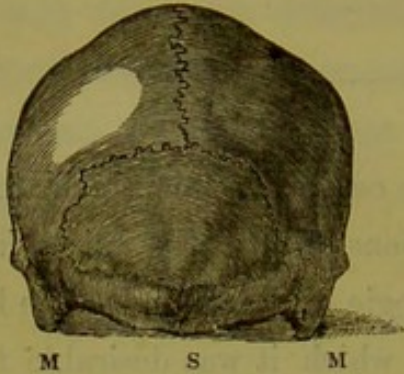
DR GALL'S observations on the cerebellum are elucidated by numerous plates, forming portions of the large folio atlas which accompanies his " Physiologie du Cerveau." The limits in point of expense within which it was desirable to confine this volume, have presented an obstacle to re-engraving these plates; but I have had the less hesitation in omitting them, because the class of readers for whom the work is chiefly intended, will generally be already in possession of preparations and plates of the skull and brain, amply sufficient to illustrate the text; and also because casts of the brain, and of many of the skulls and busts referred to in the subsequent pages, may easily be obtained from Mr Deville of London, Mr Bally of Manchester, and Mr O'Neill of Edinburgh, or inspected in the Phrenological Collections which are now numerous in different parts of the country. I shall therefore present only, *in the first place*, a few practical directions for observing the size of the cerebellum during life, with a view to ascertaining its functions; *secondly*, Such a general outline of the anatomy of the parts as may render the text intelligible to the very few readers who may not have access to the better sources of elucidation before alluded to; and, *thirdly*, A few general remarks on the relation between the structure and functions of the brain, so far as both are known.



I. *Practical Directions for observing the size of the Cerebellum, and ascertaining its Functions.*

The cerebellum, FF of Fig. 7, is situated between the

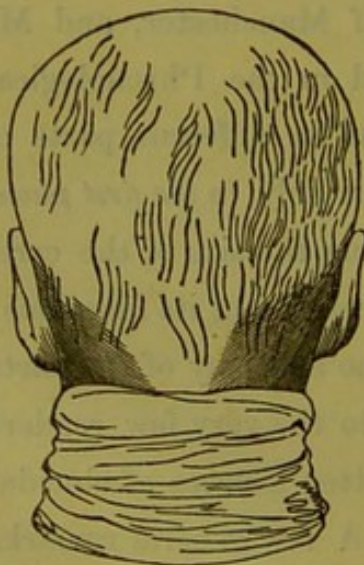
Fig. 1.



mastoid process M M, of Fig. 1, lying immediately behind, and a little below the external opening of the ear, on each side, and the projecting point or process S, in the middle of the transverse ridge of the occipital bone.

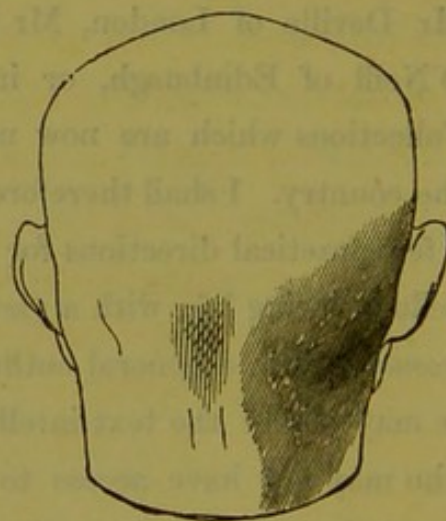
In Fig. 8, No. 48 shews the situation of this process in a section of the skull. The size of the cerebellum is indicated by the extension of the inferior surface of the occipital bone backwards and downwards, or by the thickness of the neck at these parts, between the ears. The difference between a moderate and a large development, will be understood by observing the thickness of the top of the neck in Figs. 2 and 3.

Fig. 2.



Cerebellum moderate.

Fig. 3.



Cerebellum large.

In some individuals, the lobes of the cerebellum descend



or droop, increasing the convexity of the occipital bone, rather than its expansion between the ears. In Fig. 4, O represents a large development downwards, of that part of the base of the occipital bone commonly called the posterior occipital swelling or fossa. In such cases, the projection may be felt during life by the hand, if firmly pressed on the neck.

Fig. 4.

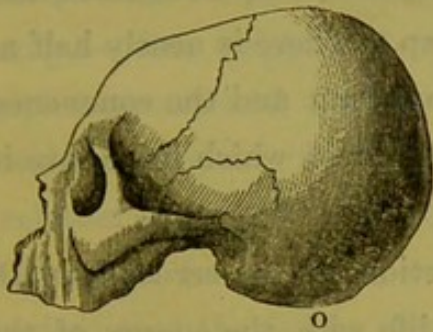
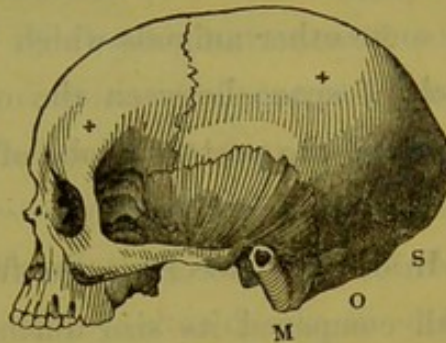


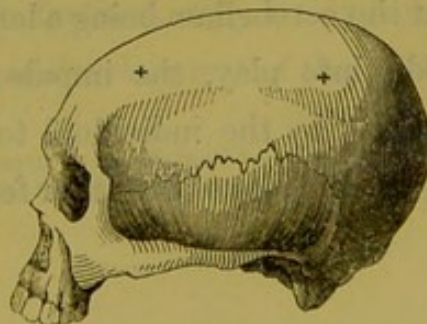
Fig. 5.



In the skull, Fig. 5, the distance between M, the mastoid process, and S, the spine of the transverse ridge of the occipital bone, is large, although the occipital swellings O, do not droop as in the preceding skull. In both Figs. 4. and 5. the cerebellum is large, but smaller in 5. In the former, however, the large size is indicated by the drooping of the bone; in the latter, by the large circumference backwards from ear to ear, or by a thick neck. The external muscles of the neck are attached to the skull in the line of this circumference.

In this skull (Fig. 6.), the cerebellum is small, and it will be seen that the base of the occipital bone extends only a short distance backwards from the mastoid process, while the occipital swelling does not descend as in Fig. 4.

Fig. 6.





Within the skull, and on the same plane with S of Fig. 1, lies the *tentorium*, Fig. 9, T, a strong membrane, which is coextensive with the upper surface of the cerebellum, and separates it from the brain. It is inserted into the inner surface of the occipital bone at 48 of Fig. 8. In some animals which leap, this separation is produced by a thin plate of bone, but Dr Vimont says that this rule is not universal, as the tentorium is membranous in the squirrel, hare, and in some other animals which leap. There is nearly half an inch of space between the cerebellum and the commencement of the posterior lobe of the brain which lies above it, at the point S.

In order to ascertain the functions of the cerebellum, Dr Gall compared its size during life with the energy of the instinct of reproduction. In this investigation, it is necessary to consider the nature of the propensity, and the great size of its organ. The cerebellum, even when moderately developed, is a large organ. It generally comes into action, not, like the others, gradually, and in the course of a series of years, but rapidly and with great vigour at the arrival of puberty. Persons unacquainted with these facts are liable to err in their estimate of the proportion which should exist between the strength of the propensity and the size of the organ. They expect to find a large cerebellum, when a practised phrenologist would anticipate one only of moderate dimensions. To remove this source of error, I observe that, from the cerebellum being a large organ, and from its coming rapidly into play, the impulses which it communicates are often felt by the individual to be superior in intensity and urgency to those of the other feelings which he had previously



experienced; and he concludes that, therefore, his cerebellum must be one of the largest possible dimensions, which may be highly erroneous. Even a moderate-sized cerebellum, and much more so one that is of full size, combined with an active temperament,\* produces feelings of very considerable strength. The objects which excite the instinct (beings of the opposite sex) are frequently presented to the mind, invested with all the attractive influences of youth, grace, beauty,—often heightened too by the highest mental accomplishments, and the most exquisite refinement; and considering the size of the cerebellum, even when moderate, in relation to that of the largest organs of the sentiments, Benevolence, for instance, or Veneration, we need not be surprised that the feeling of sexual love should even, in such cases, be powerfully experienced.

When the cerebellum is really large, and the temperament active, the individual becomes distinguished from his fellows by the predominance of his amorous propensities. In all his vacant moments, his mind dwells on objects related to this faculty, and the gratification of it is the most important object of his thoughts. If his moral and intellectual organs be weak, he will, without scruple, invade the sanctity of unsuspecting innocence and connubial bliss, and become a deceiver, destroyer, and sensual fiend of the most hideous description.

These observations will enable the reader to understand what degrees of intensity in the instinct, may be expected to accompany different degrees of development of the organ.

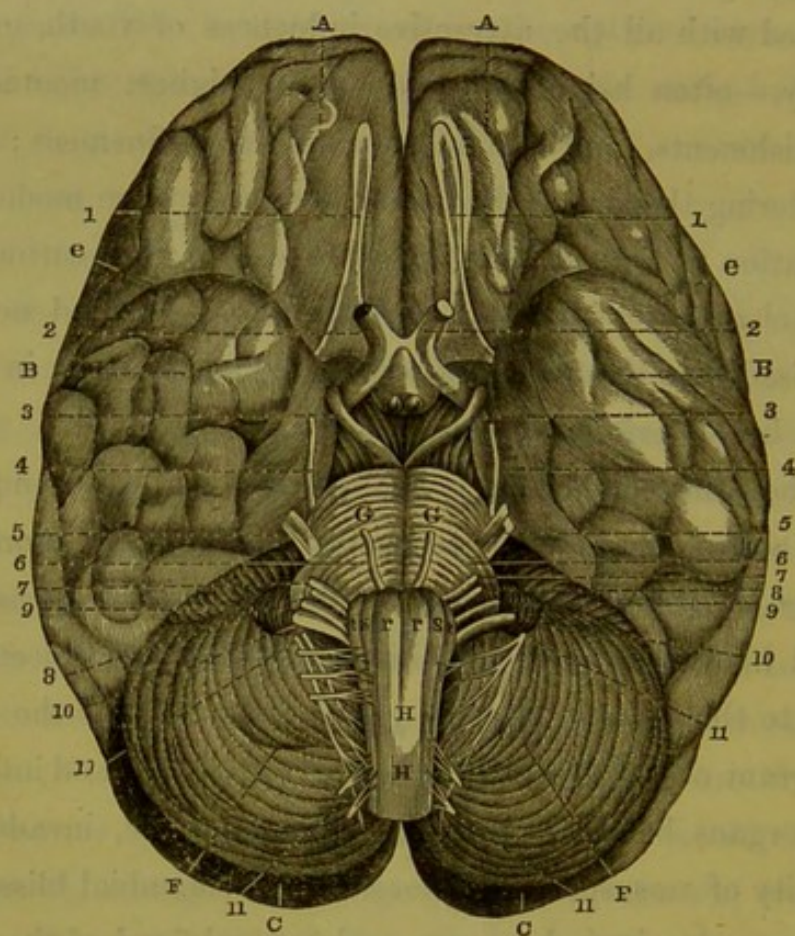
\* The influence of the temperaments is explained in my *System of Phrenology*, p. 43, 4th edition.



## II. General Outline of the Structure of the Brain and Cerebellum.

The following cut represents the base of the brain and the cerebellum :—

Fig. 7. Under Surface of the Brain.



- AC } Are the right and left hemispheres of the brain.  
 AC }  
 FF, The cerebellum.  
 AA, The anterior lobe.  
 e e, The line which denotes the separation between the anterior lobe and the middle lobe.  
 BB, The middle lobe.  
 CC, The posterior lobe.  
 GG, The *Pons Varolii*, which brings the two sides of the cerebellum into communication. It is also named the *Tuber annulare*.  
 HH, The *Medulla oblongata*.



rr, The *Corpora pyramidalia*.

ss, The *Corpora olivaria*.

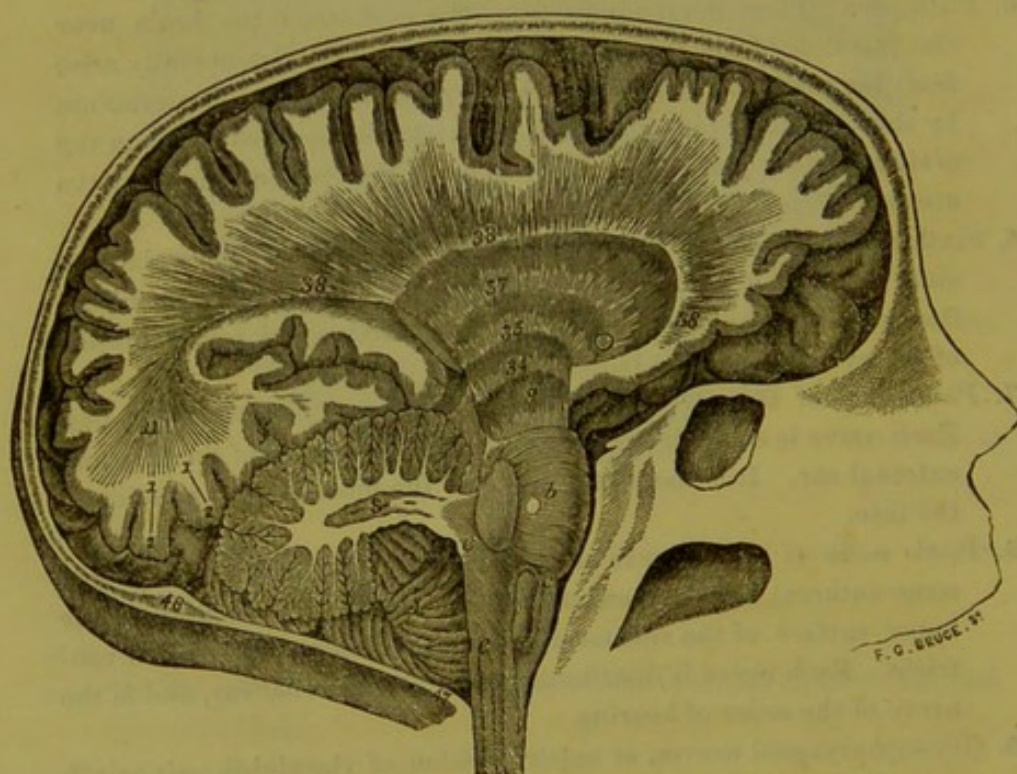
tt, The *Corpora restiformia*.

1. First pair, or olfactory, arise by three origins. These unite and proceed forwards and inwards in a groove in the inferior surface of the anterior lobes of the brain, and form a greyish swelling or ganglion. From this ganglion a great number of filaments proceed through the cribriform plate of the ethmoid bone, and are distributed upon the mucous membrane of the nose. It is the nerve of the sense of smell.
2. Second pair, or optic, arise principally from the anterior *corpora quadrigemina*. Each nerve passes outwards through the optic foramen in the sphenoid bone, and is expanded upon the retina. It is the nerve of the sense of sight.
3. Third pair, or *motores oculorum*, originate from the motor tract of the spinal chord, immediately after they have passed through the *pons Varolii*. Each nerve escapes through the sphenoidal fissure, and supplies five of the muscles within the orbit with motor filaments.
4. Fourth pair, or *trochleares*, originate from the *processus è cerebello ad testes* and *valvula* of Vieussens. Each nerve passes out from the cranium at the sphenoidal fissure, and is entirely distributed upon the superior oblique muscle of the eyeball. It is a motor nerve.
5. Fifth pair. These nerves issue from the surface of the brain, near the junction of the *pons Varolii* and *crus cerebelli*, but actually arise from the restiform bodies. Each nerve escapes from the cranium by three separate openings, and is extensively distributed upon the orbit and other parts of the face. Part of the filaments of this nerve are *sensitive*, and part *motor*.
6. Sixth pair originate from the pyramidal bodies, as they are about to enter the *pons Varolii*. Each nerve escapes through the sphenoidal fissure, and is entirely distributed upon the external rectus muscle of the eye-ball. It is a motor nerve.
7. *Portio dura* of the seventh pair originate from the restiform bodies. Each nerve is extensively distributed in the muscles of the face and external ear. It is the motor nerve of the muscles of expression of the face.
8. *Portio mollis* of the seventh pair, or auditory nerves (eighth pair of some authors), arise principally from a small grey swelling on the upper surface of the restiform bodies at the side of the fourth ventricle. Each nerve is distributed upon the internal ear, and is the nerve of the sense of hearing.
9. Glossopharyngeal nerves, or upper division of the eighth pair (ninth pair of some authors), arise from the restiform bodies near the sulcus which separates them from the olivary, and are distributed upon the pharynx and mucous membrane at the back part of the tongue. It is a sensitive nerve.



10. *Par vagum*, or pneumogastric nerves, or principal division of the eighth pair (tenth pair of some authors) originate in the same line with, and close upon, the glossopharyngeal. These nerves are extensively distributed upon the larynx, pharynx, trachea, œsophagus, heart, lungs, and stomach. Part of the filaments of this nerve are sensitive, and part are motor.
11. Spinal accessory nerves, or lower division of the eighth pair (eleventh pair of some authors), originate from the upper part of the spinal chord, in the same line with the two preceding nerves. They enter the cranium by the foramen magnum, and pass out again from the cranium through the foramen lacerum, along with the other two divisions of the eighth pair. It is principally, if not entirely, a motor nerve.
12. Hypoglossal or ninth pair (twelfth pair of some authors). Each originates from the sulcus between the pyramidal and olivary bodies, and escapes from the base of the cranium through the anterior condyloid foramen, and is distributed upon the muscles of the tongue. It is the motor nerve of the tongue.

Fig. 8.



This figure represents a perpendicular section of the interior of the brain, not far from the mesial line, proceeding



from the surface of the convolutions down to the spinal marrow.

The substance of the brain is composed of a white matter, generally called the medullary portion; and of a grey or *cineritious* matter: the name cineritious being given from the supposed resemblance of the colour to that of ashes. It is sometimes called *cortical*, because it extends over the external surface of the brain as bark covers a tree. It forms the outer part of the brain, it is pulpy in its consistence, and not fibrous. In the cut, Fig. 8, the darkest portions represent the cineritious substance, forming the external surface of the convolutions. It dips down with the convolutions, and forms the dark substance here seen between the folds. It does not blend gradually with the white or medullary matter, but, on the contrary, the line of demarcation is abrupt. It seems to have a greater proportion of blood circulating in it than the medullary.

The white or medullary substance is fibrous, and is represented by the lightest and radiated parts of the cut.

In Figure 8,

*e e*, Is a section of one of the *corpora restiformia*.

*c*, Is a section of one of the *corpora pyramidalia*.

*b*, Is the *pons varolii*.

*g*, Is one of the *crura* of the brain.

34, 35, 37, 38, and 11, Are the cerebral fibres, which, originating in the *medulla oblongata*, as after described, pass under the *pons Varolii*, through the *crura*, and *corpora striata*, and *thalami nervorum opticorum*, and ultimately expand into the convolutions of the brain.

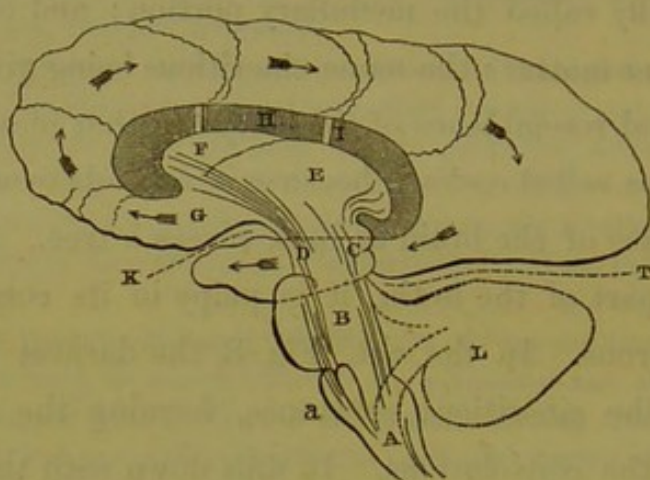
47, 48, Situation of the cerebellum within the skull.

### Fig. 9. Section of the Brain.

This figure is copied with some unimportant additions from Mr Solly's work on the human brain, page 180, and is introduced to shew the situations of the tubercles, and

other parts, which are frequently mentioned in Dr Gall's observations on the cerebellum.

Fig. 9.



- A, Represents the *medulla oblongata*.  
 a, *Corpus pyramidale*.  
 B, Pons Varolii, or tuber annulare.  
 C, Tubercula quadrigemina, with the fibres of the posterior columns passing in front of them.  
 D, *Crus cerebri*, with some of the fibres of the anterior columns. These fibres are more fully shewn in Fig. 8.  
 E, The thalamus nervi optici of one side, or posterior striated body.  
 F, The anterior corpus striatum.  
 G, Substance of the hemisphere springing out from the front of the anterior corpus striatum.  
 H, Space between the corpus striatum and the hemispheres, caused, in this figure, by the introduction of a small piece of wood.  
 I, The two surfaces, being in contact in the natural state.  
 K, Fissura Silvii.  
 L, The cerebellum.  
 T, The tentorium, separating the cerebellum from the brain.

Drs Gall and Spurzheim have the merit of having discovered and first taught the true anatomy of the brain. For many years their representations of the structure of this organ were ridiculed, and the accuracy of them denied with the greatest pertinacity; but they are now very generally admitted to be correct. Some errors may, perhaps, be dis-



covered in them; but their general truth is beyond question.

In surveying the relations of the parts, it is useful to begin with the spinal marrow.

In 1810, Drs Gall and Spurzheim represented the spinal marrow as consisting of a tract for motion, and one for sensation \*; and in 1818, Dr Spurzheim published strong reasons for the inference that certain of the nerves proceeding from it perform the functions of motion, while others communicate sensation.† Several years afterwards, Sir Charles Bell described it as consisting of two halves, a right and left, extending its whole length. According to his first view, he described each lateral portion as consisting of three tracts or columns; the anterior-lateral giving origin to the nerves of voluntary motion; the posterior-lateral giving origin to the nerves of sensation; the middle-lateral to the nerves connected with respiration.‡ The capital, or top of the spinal column, is the *medulla oblongata*. Cruveilhier and Bellingeri deny the accuracy of Sir Charles Bell's opinions on this subject. Bellingeri divides the spinal marrow into three double columns, and assigns motion to the front and back, and the instinctive movements to the middle column. He regards the grey matter of the spinal cord as connected with sensation.§ At all events, Sir Charles Bell's view,

\* Anatomie et Physiologie, &c. p. 67, 4to. Paris, 1810.

† Observations sur la Folie, par G. Spurzheim, p. 26, 27. Paris, 1818.

‡ In his paper read before the Royal Society on 30th April 1835, he appears to have renounced this opinion, and now describes the posterior roots of the spinal nerves as attached to the lateral or middle columns, or middle lateral; p. 231, 3d edition, of "The Nervous System."

§ *De Medulla Spinali*, p. 89, 93, 95, 117.



that motion belongs to the anterior, and sensation to the posterior roots of the spinal nerves, is admitted by Tiedemann, Müller, and the highest physiological authorities, and I here assume it to be correct.

At the upper extremity of the spinal marrow, and in continuation of its anterior, or motory, tract, we meet with the *corpora pyramidalia*.

These bodies consist of medullary fibres, which decussate at their lower extremity (H of Fig. 7).

The fibres of the *corpora pyramidalia* proceed upwards through the *pons Varolii* (GG of Fig. 7).

After escaping above its upper border, the *greatest number* of them pass still upwards, and form the anterior and external bundles of the *crura cerebri* (g Fig. 8), and the exterior part of the *corpora striata*, and ultimately they expand into the inferior, anterior, and exterior convolutions of the *anterior* and *middle lobes* of the brain : Gall, *Phys. du Cerveau*, vol. i. p. 279.

There is an observable proportion between the size of the *corpora pyramidalia* and that of the convolutions now mentioned.

A portion of the fibres of the *corpora pyramidalia* pass into the great ganglion of the middle and posterior lobes, commonly, but erroneously, named the optic *thalami*, and ultimately constitute part of the *posterior lobes* of the brain : Solly on the Brain, p. 233 ; Spurzheim's *Physiog. System*, p. 38.\*

\* The passage of certain fibres from the *corpora pyramidalia* into the *posterior lobes* appears not to be well ascertained, and I am not certain that I correctly interpret these authorities in citing them in support of this opinion. From the influence, however, which the mental organs



Finally, a number of fibres proceed from the lower extremity of the *corpora pyramidalia*, near the point of decussation, accompanied by a number of fibres which originate in the anterior, or motory, tract of the spinal cord, immediately below said point, to the *cerebellum*: Solly on the Brain, p. 155 ; and plate vi. fig. 1.

The fibres of the *corpora pyramidalia* thus constitute the great mass of the anterior lobes ; and enter into the substance of the middle lobes ; into that of the posterior lobes ; and into that of the *cerebellum*.

The *corpora olivaria* (S of Fig. 7.), and *corpora restiformia* (tt of Fig. 7), are placed at the summit of the posterior-lateral columns of the spinal cord, which are devoted to sensation.† The *corpora olivaria* pass upwards into the *pons*

situated in these lobes exercise over various *instinctive* motions of the body, I anticipate that a direct connection will yet be traced between them and the motory tract already ascertained, or that an *additional* tract for manifesting *instinctive* movements will be discovered, with which they and the organs of the other feelings will be found to be connected.

\* “ In the composition of the spinal cord,” says Mr Solly, “ we can observe no line of demarcation by which the tract of sensation may be distinguished from that of motion, but a portion of the cord anterior to the posterior fissure is distinctly ascertained to be appropriated to this function.” “ I shall assume, therefore, that the line of demarcation is about the middle of the lateral aspect of the cord, and that the sensory column, or tract of sensation, consists of two portions, the one posterior to the fissure referred to, and consequently named the posterior column, the other anterior to it, constituting part of the antero-lateral column.”—*Solly on the Human Brain*, p. 225. I recommend to the attention of those readers who regard the want of lines of demarcation between the different cerebral organs as a fatal objection to Phrenology, the preceding observation of Mr Solly, that the same difficulty occurs in distinguishing the tract of sensation from that of motion in the spinal cord. Nevertheless he does not hesitate to state that the essential functions of these different parts of the cords are positively ascertained. His own remark is, that “ it is quite possible that perfect distinctness of parts, as regards their function, without any visible line of separation, may exist.” p. 153.



*Varolii*, and form the posterior and interior parts of the crura; thence they proceed through the great posterior ganglion (*thalami nervorum opticorum*), and then expand partly into the convolutions of the anterior lobe, lying on its superior surface, towards the mesial line, partly into the superior convolutions towards the mesial line of the middle lobe; but chiefly into the convolutions of the posterior lobes: Gall, *Physiologie du Cerveau*, tome i. p. 281.

Sir Charles Bell says that the fibres of the middle lateral columns decussate at the same point as that at which the *corpora pyramidalia* decussate.

The fibres of the *corpora restiformia* ascend and form the chief part of the cerebellum; but a portion of them proceeds still upwards, and enters into the composition of the posterior lobes of the brain.

In the centre of the *crus cerebri*, the fibres of the motory tract (D in Figure 9), are separated from the fibres of the sensory tract to the left of the letter C in Figure 9, by a portion of cineritious substance denominated the *locus niger*.

The two hemispheres of the brain are separated by the falciform process of the *dura mater*, which descends between them to the *corpus callosum* or great commissure. The different parts of the brain are brought into communication with each other by means of the following commissures, which Mr Solly arranges under three heads; the transverse, longitudinal, and oblique.

The transverse commissures, six in number:

1. The great transverse commissure of the hemispheres, or the *corpus callosum*.
2. The pineal commissure.



3. The posterior commissure, or commissure of the posterior cerebral ganglia.
4. The soft commissure, or commissure also of the posterior cerebral ganglia. \*
5. The anterior commissure, or commissure of the *corpus striatum*, or anterior cerebral ganglia.
6. The commissure of the cerebellum, or *pons Varolii*.

The longitudinal commissures, two in number :

1. The superior longitudinal commissure.
2. The inferior longitudinal commissure, or fornix. It connects the parts of the same hemisphere.

The oblique commissure is single. It consists of,

1. The inter-cerebral commissure, or *processus è cerebello ad testes*, with the valve of Vieussens.—Solly on the Human Brain, p. 194.

### *Of the Cerebellum.*

The cerebellum consists of three portions, a central and two lateral.

The cerebellum proceeds, in the first instance, from the *corpora restiformia*. The fibres of these bodies proceed upwards, enter into the *corpus dentatum* of the cerebellum, and finally expand into its laminæ or folds.—Gall, *lib. cit.* p. 250, 251.\*

\* The *corpora restiformia*, says Mr Solly, or the *processus è cerebello ad medullam oblongatam*, “are not, as they have been usually described, bodies which are formed solely by the *posterior* columns; nor are they bodies which consist of fibres from the posterior columns, to which some fibres from the anterior columns are added, the additional fibres lying perfectly parallel to those of the posterior columns; but they are bodies

Certain fibres, already described, arising from the summit of the anterior column of the spinal marrow, and from the lower extremity of the corpora pyramidalia or motory tract, proceed upwards and laterally, and enter the cerebellum. Mr Solly (p. 57) has the merit of having first clearly demonstrated the course of these fibres, although Drs Gall and Spurzheim have alluded to their existence.

The *pons Varolii* is the great commissure uniting the two lateral portions of the cerebellum.—Gall, *lib. cit.* p. 258.

#### *Of the Corpora Quadrigemina.*

Certain fibres originating in the corpora olivaria, are said by Tiedemann\* to form the corpora quadrigemina (Fig. 9, C). Reil says that some of the fibres of the corpora pyramidalia go to them.

The superior pair of the corpora quadrigemina or tubercles, are regarded by Dr Gall as ganglions, which give origin to the optic nerves (*lib. cit.* p. 121). The functions of the inferior pair are not ascertained. They are placed on the upper part of the medulla oblongata.

A broad band of medullary substance, "thick laterally, but extremely thin in the centre," passes from the cerebellum upwards and forwards to the tubercles, commonly called the *processus è cerebello ad testes*, and the *valvula of Vieussens*.—Solly, *lib. cit.* p. 178.

which consist of fibres that interlace in rather an intricate manner, the interlacing fibres consisting of some from the antero-lateral, and some from the posterior columns."—P. 158.

\* On the Anatomy of Foetal Brain, p. 182-3; *Bennett's translation.*



*Relation between the Structure and the Functions of the Brain.*

The convolutions of the brain appear to stand in a relation to the spinal marrow analogous to that which the superficial expansions of the nerves of the external senses, of motion, and sensation, on their respective organs, bear to it.

The convolutions of the *anterior* lobes of the brain \* manifest the intellectual faculties.†

The Intellectual Faculties enable man to perceive objects that exist, their qualities and relations; and when acting together, they constitute WILL.

The convolutions which manifest these faculties spring from the *corpora pyramidalia*, which are now generally considered to be the top of the motory tract of the spinal marrow. Here, then, is a direct relation between the convolutions which *manifest* WILL, and the motory tract which *executes* WILL; an arrangement that appears to accord with the best established principles of Physiology.

The convolutions of the *middle* and those of the *posterior* lobes of the brain, manifest the Propensities and Sentiments, or Feelings.

These convolutions spring chiefly from the *corpora olivaria*, but partly also from the *corpora restiformia*. These bodies constitute the top of the *sensory tract* of the spinal cord.

\* Precisely speaking, it is only the anterior, inferior, and lateral convolutions of the anterior lobe which manifest intellect. The superior convolutions of this lobe manifest sentiments or feelings.

† The evidence of the functions of the convolutions is stated in the different works on Phrenology.



The cerebellum springs from the *corpora restiformia*, which, as has just been mentioned, is viewed as a portion of the top of the sensory tract of the spinal marrow.

The function of the cerebellum is to manifest the instinct of reproduction, which also is one of the *feelings* of the mind.

But, in the next place, certain fibres proceeding from the *corpora pyramidalia*, or motory tract, enter into—

The middle lobes,

The posterior lobes, and

The cerebellum.

While, therefore, the convolutions of the anterior lobes \* are formed of fibres connected with the motory tract, the convolutions composing the posterior and middle lobes, and the cerebellum, are composed of fibres connected with *both* the motory and sensory tracts.

The middle and posterior lobes, and the cerebellum, manifest a variety of different feelings, each particular feeling being connected with a particular part of these masses, as is explained in the works on Phrenology.

Each of these feelings acts upon, and manifests itself by means of the muscular system. Thus fear, rage, affection, or any other feeling, communicates great energy of action to the muscles of voluntary motion.

Each of the feelings instinctively impresses motions on the muscular system peculiar to itself, and expressive of its distinctive character, which motions are named by phrenologists its natural language. Thus, the organ of Self-Esteem, when predominantly large, produces an instinctive tendency

\* Excepting always those on the upper surface which manifest feelings, and which derive their origin from the sensory tract as before described.



to carry the head and body reclining backwards. The organ of Firmness, when predominantly large, produces the tendency to support the body in a stiffly erect position, as if the spinal cord were supported by a rod of iron running along its whole length. The cerebellum also impresses peculiar motions on the muscular system expressive of its character. These motions, as I have said, are instinctive; that is to say, they are the instantaneous and direct results of the activity of the several feelings, and not the consequences of intellectual perception and will.

The arrangement of structure by which each of these organs of feeling is supplied with fibres in direct connection with the motory tract, is in harmony with this influence of the emotions over the motions of the body. But we should expect a *separate* tract for *instinctive* motion, which is clearly distinguishable from voluntary motion, and also that the organs of the *feelings* should be connected directly with it. There is still much obscurity in the views of physiologists concerning the connection of the middle and posterior lobes with the motory tract.

Again, it is certain that the mental *emotions* exercise a powerful influence over the organic functions; when agreeable, they stimulate them to healthy action, and when painful, they depress their energies, and produce liability to disease. Reciprocally, when the organic functions, such as digestion, respiration, and secretion are disordered, an irritable and distressing state of the mental feelings is induced.

The intimate relations between the convolutions of the brain devoted to the mental emotions and the sensory tract of the spinal cord, is in harmony with these facts.

The habit of contending with *intellectual* difficulties, if



unconnected with feeling, does not injure the organic functions so severely as do strong and painful emotions; but it weakens the locomotive powers. Sedulous students of abstruse problems, acquire a great aversion to locomotion.

These facts correspond with the arrangements of structure by which the convolutions of the anterior lobes devoted to intellect, spring from the motory tract, and are less intimately connected with the sensory tract of the spinal marrow.

The convolutions of the anterior lobes bear an analogy to the peripheral expansion of a simple nerve of motion; while the convolutions of the middle and posterior lobes, and the cerebellum, bear an analogy to the peripheral expansion of the *combined* nerves of sensation and motion.

The functions of the convolutions of the brain, and of the laminae of the cerebellum, being to manifest respectively thought and mental feeling, they do not produce what, for distinction's sake, may be called *bodily pain*.

These convolutions and the cerebellum, although situated, when man is in the erect position, *above* the spinal cord, nevertheless stand in the same relation to it, as do the peripheral expansions of the cerebral nerves; that is to say, they are composed of the *distal* ends of the fibres, which can be traced inwards to the spinal cord.

If an experimenter were to separate the *motory* branch of the fifth nerve (arising in the *crus cerebri*,\* the motory tract), and expanding on the head and face,—from the *sensory* branch of the same nerve (arising from the posterior

\* Mr Solly describes this branch as arising from the "inter-cerebellar commissure, very close to the cerebellum,"—*Lib. cit.* p. 249; but he adds, that its origin from these fibres proves they "must be a portion of the motor tract."—P. 251.



columns of the spinal cord, about an inch and a half below the pons Varolii, the sensory tract), along its whole course; and if he were to destroy the former, or motory branch, at its periphery, the animal would experience no *pain*, because this is a nerve of motion; and no convulsions would ensue until the lesions reached close upon the motory tract itself;\* because the influence of irritation on nerves of motion is propagated only *outwards* from the spinal cord; and in the case here supposed, the mutilations and irritations would proceed *inwards* from the *distal* extremity of the nerve *towards* the spinal marrow. The whole portions of the nerve lying *outwards* from the point of irritation, as this point by successive mutilations approached the spinal marrow, would be necessarily destroyed, and therefore could produce no movements.

When Magendie, Flourens, and other physiologists, cut away the convolutions of the anterior lobes, they performed an experiment analogous to this which I have now described. They commenced at the distal extremity of the fibres of the convolutions which are connected with the motory tract, and they destroyed them in proportion as they carried their ablations towards the spinal cord. Whatever the functions of these convolutions might be, the power of manifesting them must obviously have ceased by their destruction; and as it was known that the convolutions of the brain do not manifest ordinary sensation, they were not authorized, by the

\* I here suppose the *whole* filaments to be *destroyed*, commencing from the periphery. If the nerve were merely *irritated* at any part of its course, the muscles which derive their filaments from the part of the nerve *below* the point irritated would be thrown into contraction, while those deriving their filaments from points above the part irritated would remain quiescent.



analogy of the nerves, to expect either pain or convulsions to be excited until they arrived at the motory tract itself; which accordingly was actually the case. When Magendie cut the corpora striata and tubercles, the animals "rolled," "went forward," "extended," and "bent their heads and extremities."

Again, if the nerves of motion and sensation ramified on the hand were destroyed in combination, commencing from the surface of the skin at the extremity of the fingers, and proceeding upwards to the spinal cord, pain would be felt, because a nerve of sensation had been destroyed, and such nerves propagate their impressions *inward* from their peripheral expansions *towards* the spinal cord and brain; but, for the reason before stated, there would be no convulsions, until the motory tract of the spinal cord itself was reached and irritated.

The experiments of Magendie and Flourens, in cutting away the convolutions of the middle and posterior lobes of the brain, were analogous to this supposed proceeding. They removed parts which manifest mental emotions, but do not produce pain; and the organs being destroyed, no emotions and no pain were manifested. These organs are connected by some fibres with the motory tract, but these fibres were cut away from their distal extremities, and no effect on motion was produced until the motory tract itself was assailed; all which facts accord with the views of the structure and functions of the brain and spinal marrow now presented.

These experimenters held Dr Gall's discovery of the functions of the *convolutions* of the brain in too much contempt to allow themselves to see these plain connections and results.



Indeed they did not proceed as if it were possible that his ideas *could* be true. Still their experiments, however little calculated to throw light on the functions of the convolutions, produced phenomena which harmonized with the functions ascribed to these parts by Dr Gall. Cuvier reports, that when Flourens destroyed the hemispheres, the animals so mutilated became "quite drowsy;" they "had no *will* of their own;" and "made no *spontaneous* motion." Further, "he pricked the hemispheres without producing either contraction of the muscles or any apparent pain to the animal." In short, Flourens found that, by cutting downwards from the surface of the convolutions of the brain, he did not cause either muscular contraction, or excite pain, until he arrived "at the top of the *medulla oblongata*, at the spot where the quadrigeminal bodies are attached to it," but that, by irritating the motory and sensory tracts at this point, he produced both pain and convulsions. Magendie produced motion by irritating the corpora striata.

These facts appear to shew that the proper or peculiar functions of the sensory and motory tracts commence at these parts; and they accord with Dr Gall's views of the structure and functions of the convolutions.

In irritating the brain from below *upwards*, Flourens found that, after he had passed the point at which the tubercles are attached, his operations produced neither pain nor convulsions.

This fact also accords with the views of the structure and functions now presented. If the convolutions had been organs of motion or of ordinary sensation, convulsions and pain should have followed by irritating their fibres at the *end next the spinal marrow*; but their functions are to mani-



fest intellectual perception and mental emotions, and Flourens does not report that *these* powers were not disturbed by his irritations. There are no muscles which receive nerves of motion, nor mucous surfaces which receive nerves of sensation, *above* the corpora striata and corpora quadrigemina in the brain. The fibres, therefore, which ultimately constitute the convolutions, although proceeding from the motory and sensory tracts, may reasonably be presumed to perform functions distinct from motion and sensation. Our view is, that they are the organs of mental faculties which *use* muscular motion and sensation as their instruments of manifestation; and the relations of the convolutions to the two tracts in question accord with this idea.

Finally, it will be observed that the fibres of the cerebellum spring from the sensory tract, and *also from the motory tract*, and that it is situated *close* to the commencement of these tracts, assuming the commencement to be at the tubercles, as asserted by Flourens.

The results observed by Flourens in his experiments on the cerebellum, as reported by Cuvier, were these: "During the ablation of the first slices of the cerebellum, only a little weakness and a want of harmony in the movements occur. At the removal of the *middle slices*, an almost *general agitation* is the result. The animal, continuing to hear and to see, executes only abrupt and disorderly movements. Its faculties of flying, walking, standing up, &c., are lost by degrees. When the cerebellum is *removed*, the faculty of performing regulated movements has entirely disappeared."

Although muscular motion is excited, according to Magendie, by irritating the *corpora striata*, and, according to Flourens, by irritating the tubercles, the precise points at



which this influence of irritation commences is not well ascertained. In cutting away the hemispheres of the brain, the entire convolutions appear to have been removed without muscular action having been induced; whereas it is said, that movements were manifested on removing the first slices of the cerebellum. These facts, if they were not liable to great complication and uncertainty from the deep injuries in which they involved the nervous system in general, would appear to shew that the surface of the laminæ of the cerebellum is more directly related to the motory tract than is the surface of the convolutions of the brain. The motions, however, described by Flourens as accompanying the mutilations, may have been caused by intense pain, arising from irritation communicated through the cerebellum to the *sensory* tract, or by irritation communicated through the same medium to the *motory* tract, in consequence of the very close approximation of the cerebellum to these tracts. Whether the difference of the effects produced by injuring the convolutions, and by injuring the laminæ of the cerebellum, may have arisen from the closer approximation of the cerebellum than of the convolutions, to both the motory and sensory tracts, it is difficult to determine; but not one of these results excludes the fact demonstrated by Dr Gall in the following pages, that the cerebellum manifests the instinct of reproduction.

Farther, there is no specification of the *depth* to which either the *first* slices, or the *second* slices, in these experiments extended, while the last slices extirpated the cerebellum entirely, or cut down directly on the sensory and motory tracts. If we consider the connection and close approximation of the cerebellum to the motory and sensory tracts (see Figs. 8 and



9), we shall have no difficulty in discovering how very applicable the following words of Cuvier are to the phenomena which ensued upon these operations. "After all," says he, "it must be observed, that in too deeply extirpating the *tubercles*, we interfere with the *medulla oblongata*, and then violent convulsions, which last long, make their appearance." He might have said, with equal reason, that when we too deeply extirpate the cerebellum, we interfere with the *medulla oblongata*; and we need not be surprised that an animal which has suffered this mutilation should appear as "in a state of apparent drunkenness," unable to regulate its movements.

Dr Vimont (p. 130 of this work) considers that the *processus vermicularis*, or middle portion of the cerebellum, does not perform the same functions with the two lateral masses; and he states that he had found it "always large in the climbing animals, such as the cat, the squirrel, and the marten; and in those whose footsteps are very sure, or which are able to walk easily on sloping ground, or on steep places." Mr Solly, on the other hand, observes on this point as follows. "Reil held, with Gall and Spurzheim, and I have no doubt he was correct in his opinion, that the 'cerebellum is not composed of elementary portions essentially different, but is one homogeneous mass.' As I understand the cerebellum, it is one instrument for the production of power, and not many instruments united together. It consists, like the cerebrum, of two hemispheres united anteriorly by a commissure called the pons Varolii; and by Reil, it is described as being united posteriorly by another commissure, which, projecting on its upper and under surface, forms what have been called, from their appearance, the *processus vermiformis*



*superior and inferior.* I confess this portion of the cerebellum does not appear to me to be an apparatus of union, but rather a centre of power placed in the mesial line, and connected laterally with the two hemispheres, perfectly analogous to the cerebellum of the bird and hare; Reil himself stating that, in the brain of the hare, there is little more than a vermiform process. Indeed, a mere section of it ought to be sufficient to convince us that it is no true commissure."—*Lib. cit.* p. 196. I leave the reader to form his own judgment whether the whole cerebellum performs a single function; or if, as Dr Vimont thinks, the middle portion is connected with motion, and the two lateral portions with the instinct of reproduction.

Professor Broussais (p. 134 of this work) says, "I ascribe the direction of the (voluntary) movements, the acts, the aptitudes, which have a reference to generation, to the influence of the cerebellum. But I maintain that these cannot be accomplished without the permission, nay, without the assistance, of the brain; and that it is in this sense alone that the cerebellum can be considered as the regulator of muscular action."

These remarks agree with the views of the structure before presented. The cerebellum is composed of fibres connected with the motory tract, and of fibres connected with the sensory tract. It is an organ of feeling, but it also influences voluntary motion by instinctive impulses, as the other feelings do. It is obvious that it may accomplish this effect by means of the fibres before described, which connect it directly with the motory tract of the spinal cord.

Dr Broussais continues, "The brain always remains master of the movements of the cerebellum tending towards the



act of generation, so as to be able to assist them; and it effectually exerts this power, in a variety of circumstances, as every one knows." The view of the structure before presented, which regards the anterior lobe as the organ of will, and as composed of fibres, connected directly with the motory tract, accords with these remarks of Broussais. The *will*, by means of the powerful and direct influence which the anterior lobe exerts over the motory tract, "remains master" of all the voluntary movements.

It is now generally admitted by physiologists, that the optic nerves terminate in the *corpora quadrigemina*, which are, in fact, their proper ganglia. The functions of the retina and optic nerve are to receive and transmit the impressions of light to the brain. It, therefore, is an organ of sensation. These impressions, however, instinctively produce a great variety of muscular movements, especially in the eye itself and its integuments.

The *corpora quadrigemina*, which are its ganglia, are derived from the *corpora restiformia*, or sensory tract.\* But Reil says, that these *corpora* also derive fibres from the *corpora pyramidalia*, or motory tract. This structure corresponds with the idea that the optic nerves are organs of sensation, but that they also influence motion.

Farther, the eyes are *moved* by the third pair of nerves, or COMMON OCULO-MUSCULAR, and by the fourth pair, or INNER OCULO-MUSCULAR. Mr Solly describes the third pair as connected by one portion "with the motor tract in its passage through the pons Varolii. The other portion is

\* There is still some obscurity in the works of different anatomists on this point; which arises probably from a want of precision in their views of the exact limits of the olivary and restiform bodies.



partly lost in the locus niger, and partly mingled with those fibres which the intercerebral commissure, or processus è cerebello, sends through at this point." (P. 246.) The *fourth* pair "arise from the intercerebral commissure, close to the optic tubercles; the nerves proceed, in fact, from those fibres which descend to the centre of the crus cerebri, so that we at once observe an immediate connection between the fourth pair and the third, and, as we shall presently see, with the motor root of the fifth also." P. 247.

This structure, which derives the nerves that move the eyes from the motory tract, is calculated to render them subject to the will, while their origin also from "the intercerebral commissure, close to the *optic tubercles*," is in harmony with their subserviency to the instinctive impulses of the organ of vision. Many motions of the eyes are instinctive, and many voluntary. The circumstance of the eyes being provided with two nerves of motion, and one of them, the third pair, having a double root, strengthens the conjecture that there may be two motory tracts, one for voluntary and another for instinctive motion. Some of Mr Grainger's observations seem to favour this idea.

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To those readers, who have not studied the evidence which has convinced me that the convolutions of the anterior lobes manifest intellect and will, and that the convolutions of the middle and posterior lobes, and also the cerebellum, manifest feelings or emotions, the foregoing views will probably appear to be mere fanciful theories; but I respectfully beg leave to doubt of their competency to form a sound

judgment on the subject, until they shall have investigated the evidence. I trust that, to phrenologists, they will appear more consistent with the rules of sound philosophy. I do not present these hints as ascertained science, but as an exposition, in the first place, of the harmony that reigns between the best established views of the structure of the brain, and the functions ascribed by phrenologists to this organ; and, secondly, of the truth of their assertion, that no well established anatomical or physiological facts have yet been brought to light that are inconsistent with their views. Every reader will judge for himself, how far I have been successful.

I have some hope that the mode of viewing the relations between the structure of the brain and its functions, which I have here attempted to exhibit, may be useful in leading more accomplished inquirers into the right road of investigation.

G. C.



DR GALL

ON THE

FUNCTIONS OF THE CEREBELLUM.\*

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THE first and most universal of all the Commandments was, "Increase and multiply." Why, then, make use of circumlocution in treating of the most indispensable and most powerful of all the instincts? Let us rather consider it with the attention which its high importance demands. Let us trace it in its regular and irregular action. Although the subject has attracted the attention of many naturalists, and been treated of a thousand times, it still furnishes results equally new and important for the physiologist, the physician, the teacher, and the moralist.

In treating of the natural history of the instinct of reproduction, I shall prove, that it neither originates, nor has its seat, in the external organs, but that these parts are subordinate to a superior power situated in the brain; and consequently that it is in the brain that we must look for the causes which influence this instinct in health and in disease; that by the brain the phenomena connected with it are explicable; that the brain regulates every circumstance related to it; and that we must act on the brain, if we wish to modify its different manifestations.

\* Instinct de la Génération, de la Reproduction; Instinct de la Propagation; Instinct Venerien. Zeugungstrieb; Fortpflanzungstrieb; Geschlechtstrieb.



*The Instinct of Reproduction is a Function of the Brain,  
and does not belong to the External Organs.*

Naturalists often attribute to executive instruments acts which originate in directing organs;—for example, the hut of the beaver is ascribed to its tail; the intelligence of the elephant to its trunk; painting and sculpture, and all the intellectual faculties of man, to the external senses, and particularly to the hands. In like manner, it has been customary to impute to the influence of the external organs of this instinct on the brain, or even to these organs exclusively, the phenomena which should properly have been ascribed to the brain, and to its influence on them. As the seat of the propensities and instincts was placed by these philosophers in the ganglions, and in the plexus of the abdomen and thorax, it was naturally conceived that the instinct of reproduction could be located nowhere except in the external instruments which alone were observed to act a part in the accomplishment of its function.

There was, however, a means by which this error might have been discovered. From time to time, children of two, three, four, or five years of age, have appeared, in whom the external organs were still undeveloped, and in whom there existed no seminal secretion, but who were nevertheless urged by an imperious propensity towards the opposite sex, and gave themselves up blindly to pleasure. Old persons, of either sex, were also met with, in whom the pretended exciting cause was nearly destroyed by age, but who nevertheless were tormented by desires which their external organs no longer enabled them to gratify. Men who had suffered castration and eunuchs have been known to be strongly excited by sexual appetite. Physiologists had even recorded cases in which females, deprived by primitive malformation of the *uterus*, had nevertheless experienced the solicitations of the propensity.



In all times philosophers have been much engaged in observing the changes produced by the age of puberty on youth. Remarking the development and growing activity of the external organs, the illusive idea was entertained that the moral and intellectual faculties, which at this period began to manifest themselves with increased energy, were subjected to the influence of these organs. They entirely lost sight of the brain, the different parts of which acquire, at the same moment, an augmentation both of development and vigour. This last is the sole cause why the boy and girl cease to be children; why their moral and intellectual faculties then manifest themselves with greater energy, and why this activity indicates itself by the sparkling of their eyes, and by a new expression pervading all their features.

The difference of the formation of the brain in the two sexes is the cause why the young man and the marriageable girl still preserve their distinctive characters, notwithstanding that the same change takes place in the external instruments in each at puberty. Imperfection in the condition of the brain also, is the cause why certain cretins and idiots continue not the less imbecile after puberty, although their external organs of reproduction are not only completely developed, but energetically exercised. The same circumstance is the cause why sometimes children of three or five years of age, having these organs developed, and feeling the impulses of the instinct, continue nevertheless to be children in every other respect.

Should not all these phenomena have corrected the views of physiologists, and led them to search for the causes of the moral and intellectual changes which occur at this period of life, elsewhere than in the external organs of sex?

I shall cite several passages from the work of M. Georget, in which he speaks of the sympathies of the cerebral organs, vol. ii. p. 159, et subsequ. The reader will easily judge who has first originated these ideas.

“Before entering into the detail of facts,” says he, “let me observe, that if the question be concerning a propensity,



a desire, a passion, we must look for its immediate seat in the brain; for this organ alone is the instrument of the intellectual and affective faculties, at least in man and the larger animals: Love—the desire of union between the sexes—then, is situated in the brain. But an excitant, a want which shall awaken by sympathetic influence this desire or propensity, may exist elsewhere; and then, sometimes the desire shall exist first in order, solely by the excitement of the brain, and shall give rise to the want; at other times, on the contrary, the latter shall be the stimulant to the former. These are precisely the relations between the external organs of sex and the brain.

“ I. *Action of the Brain on the External Organs; sexual desires arising directly from the action of the Brain.*

“ I do not advance any paradox when I maintain, that the most numerous and powerful causes of sexual desires spring from the action of the brain. The following propositions afford incontestible evidence of the fact.

“ 1. Sexual desire is a cerebral phenomenon.

“ 2. The genital functions are for the most part subject to the will. I do not believe that the propensity is ever so irresistible, that the individual cannot withstand its indulgence at least for the moment. Besides, it follows the law common to it and the other feelings, that they acquire a degree of power and influence over reason, in proportion to their energy.

“ 3. The sexual desires, and the action of the related external organs, present in their development, progress, and termination, phenomena analogous to those exhibited by the other propensities, whose functions are under the immediate direction of the brain. Thus, these desires and this action, actually null at birth and during the earlier years of existence, begin to manifest themselves more or less rapidly, more or less strongly, in those years which precede puberty. From this epoch (which arrives in general between the ages



of twelve and sixteen, sometimes earlier, and sometimes later, according to the dispositions of individuals, to sex, and climate),—to the age of forty-five or fifty, they are in their highest degree of energy. Thereafter, they proceed in a decreasing ratio; and finish by becoming feeble and extinct in proportion as the brain loses its force, and simultaneously with the other passions.

“ 4. The causes which most frequently awaken sexual desires, and excite the external organs, are, lascivious ideas, conversations, and books; gay affections; assemblies of the two sexes, such as plays, societies, balls, &c.; the sight of persons of the opposite sex; contact, &c. It is certain that, in the great majority of cases, the idea or thought of the gratification desired, precedes the excitement of the external organs. It is then that those sensations are experienced which indicate a strong impulse sent forth from the centre of sensation. On the contrary, the causes which produce forgetfulness or inactivity in this function, are, solitude, or a limited society; the absence of the opposite sex, and of circumstances calculated to excite voluptuous ideas, such as romances, books and pictures descriptive of love; the constant occupation of the mind on objects which engage its attention; muscular fatigue; distressing moral emotions; *ennui*, disgust, possession of the same object, &c.

“ 5. During sleep, the brain has so great an influence over the external organs, that voluptuous dreams are sufficient to excite emission; ‘and if the direct excitement of the external organs,’ says Cabanis, ‘is often the true source of those voluptuous images which are formed in the brain during sleep, it is no less true that very frequently the existence of these images in the brain are the sole causes of the excitement of the external organs.’\* ‘During sleep the imagination has an increased power over certain organs,—those of sex, for example, because in this state, owing to the brain not receiving external impressions, the internal impressions are more lively and powerful.†’

\* *Rapports*, etc. tome i. p. 540.

† *Id.* tome ii. p. 184.



“ 6. Thought and imagination very obviously exercise an influence over sexual power,—over the promptitude and vivacity of enjoyment. The enjoyment is more or less lively, prompt, easy, nay, even more or less possible, according to the state of excitement or indifference, of desire or disgust, in the mind. Examples crowd on us in support of this proposition.

“ 7. Slight excitement of the brain, produced by spirituous liquors, and coffee, wakens the propensity to love: drunkenness extinguishes it. Between these two states, there often exists another, very remarkable, in which there are violent desires without actual appetite; an excitement purely cerebral combined with the most perfect tranquillity, nay absolute inaction, of the external organs.

“ 8. The effects of pretended charms, &c. cast over the bridal bed in times of ignorance and superstition are known; also the success *des noueurs et de dénoueurs d'aiguillettes*, &c., of men who pretend to tie and untie the scrotum.

“ 9. Medical practitioners have observed sexual phenomena in affections of the brain. Thus erection in men has been observed by Bichat as the consequence of cerebral commotions: it is not rare in low typhus. M. Serres has seen it in two cases of affection of the cerebellum, one of which was an effusion of blood, and the other an inflammation. It is known that men who are hanged often exhibit this phenomenon; and that some individuals have been so depraved as to seek to procure for themselves enjoyment by this odious method.

“ 10. Loss of blood—enfeeblement of the propensities and passions by macerations—fasting—vegetable diet—the use of water exclusively for drink—diminish, and at last produce oblivion of, carnal desires, particularly in individuals who are not naturally excited by a high degree of them.

“ 11. Dr Gall places the seat of physical love in the cerebellum. This is, perhaps, the point of his doctrine (concerning the plurality of the cerebral organs and their sepa-



rate functions), on which he has collected the greatest number of proofs. The observation of M. Serres is here of some weight. M. Larrey cites the case of a soldier who, after having received a cut from a sabre in the neck, never again experienced any sexual desire. Hippocrates assures us, that the Scythians produced impotency by cutting the veins behind the ears. This tradition, which, no doubt, is false as to the fact itself, nevertheless shews that they had observed some connection to exist between the neck and the organs of reproduction. Ferrand (*de la Maladie d'Amour ou Mélancolie érotique*), assures us, that physicians have accomplished good by the application of leeches to the neck, behind the ears, in patients attacked by this disease. Finally, I have seen in the Salpêtrière, one of those women of ardent temperament, whose only alienation consisted in the most imperious sexual desires. Before her entry into the hospital, she had several times supported, nay provoked, the embraces of ten, twelve, or fifteen men in one day. During her stay in the hospital, she frequently experienced severe pain in the neck, during which she was seized with violent sexual desires, which she satisfied by masturbation. She gave herself up to this practice ten or twelve times a day; it relieved her feelings, produced no accident, and immediately removed the pain in her neck."

"12. The following fact is both important and well calculated to throw light on the question in hand. It is observed that, in man, (for in woman the point cannot be ascertained), the diminution and extinction of the power of external indulgence constantly precedes the diminution and extinction of the internal desires. We see this occur naturally in old men, and accidentally in libertines, whose external organs are no longer capable of excitement, in consequence of having been over-excited. Those old men who do not pretend to ascribe to prudence what results from weakness, acknowledge, without difficulty, this fact; and many persons have remarked the melancholy and miserable condition of those unfortunate wretches, whose external organs, with-



ed and inert, can no longer respond to the action of the brain, yet who still feel sexual desires, so much the more pressing as it is impossible to satisfy them.

“ II. *Action of the External Organs on the Brain : Sexual desires excited by the action of these organs.*

“ The state of erection generally excites sexual desires, either by the sensation agreeable or disagreeable which accompanies it, or by recalling to the mind the idea of the enjoyment of which this state is a condition. Sometimes, as we have just seen, it arises from the influence of the brain, but at other times it is provoked by circumstances purely local, such as pressure, or rubbing on the mucous membrane of the penis or vagina. At the same time it is well worthy of remark, that the sensual act thus excited is far less ardently desired than when the brain is the first source of it, unless the want have the power of wakening the desire in its full energy.

“ 2d, The greater number of diseases which affect the external organs of reproduction have no influence on the brain as excitants of sexual feeling. Even when they cause erection, as may be observed in certain inflammatory affections of the urethra, in the irritation of the neck of the bladder consequent on the use of cantharides,—they are felt to be painful rather than calculated to excite desire.

“ 3d, The action of heat on the external organs favours erection, while cold produces the contrary effect.

“ 4th, Those physiologists who are unwilling to acknowledge any influence of the brain over the external organs, and refer all the phenomena to these organs exclusively, rely much on the results of castration. It is indeed certain, that individuals who have been castrated at an early age present very remarkable appearances. It is not, however, absolutely true, that these persons are entirely destitute of sexual desires ;—there are numerous examples to the contrary. Eunuchs do not present the characteristics of viri-



lity : they retain the feminine complexion. Like women, their voice is infantine, and hair does not develope itself on their face or thorax. Their brains remain destitute of moral and intellectual energy ; and like other feeble beings, the eunuchs are false and vindictive, cheats and dissemblers. NARSES is perhaps the only exception to be found in this unfortunate class ; by his exploits, his courage, and his force of character, he shewed himself to be the equal of a great captain. Eunuchs, as well as castrated animals, become rapidly fat. These last, like the former, either do not acquire, or they lose the peculiarities of their sex. Moreover, some animals (but only the males) undergo remarkable changes in the season of love. Thus the substance of the horn of the stag grows from one branch ; certain birds are decked by a tuft, and their voice recovers the power of singing.

“ One fact cannot destroy another fact ; and although the testicles exert an influence, no matter how, on the brain, it is equally certain that this organ is the seat of sexual desire, and most frequently becomes the exciting cause which calls into action the desire of union between the sexes. In like manner, the muscles and the integrity of the spinal chord are essential to the exercise of the voluntary movements, although the source of these movements, the point from which they depart, resides in the centre of volition. I shall not here enter into any explanation, because I acknowledge that my reflections have not led me to any satisfactory conclusion regarding the nature of the relations subsisting between the testicles and the brain.

“ In woman also, the attempt has been made to find a part of the sexual apparatus corresponding to the testicle, and to represent it as the seat of the desire of sexual union, and most authors have declared themselves in favour of the uterus. They go so far as to place in this organ all the erotic or pretended erotic diseases ; whence the names of *furor uterinus*, hysteria, and *suffocatio matricis*, by which they designate them. Not only do no facts render this opinion



probable, but many evidently prove the contrary of it. Thus, women have been observed, who, although destitute of the uterus, were nevertheless extremely addicted to sexual pleasures. The clitoris has been regarded by some as charged with this function. Moschio, Albucasis, and Ferrand, recommend the excision of it in certain amative affections. Professor Dubois affirms that he succeeded in this operation in the case of a young nymphomaniac. Nevertheless, no one follows the practice of these observers, probably because no advantage is expected to arise from it.

“ It follows from what has been said, that every morbid exaltation of the sexual appetite is a true monomania, the seat of which can exist no where else than in the brain; and that the cause of it, like the cause of this appetite itself, must most frequently be sought for in this organ. I am fully convinced that in this case the external organs are excited only secondarily, and are only the associates of the brain. Let it be observed, 1st, That this disease occurs precisely by the force of circumstances which we have considered to be excitants of the brain. Thus, it is in large towns,—in public life,—among those classes who frequent plays, balls, societies, and read romances, and who have no occupations which distract them from these objects, or fatigue them,—that nymphomania appears. It is scarcely known in the country and in small towns; where, however, the desire of sexual union is felt as well as in these other circumstances. 2d, That masturbation which extinguishes—satisfies the want—is here rarely of any avail, because the patient is tormented by particular desires, to possess such or such an object. 3d, Neither are men whose imaginations, in general, are very little occupied with these ideas, subject to this affection, although, according to my opinion, they experience stronger impulses of desire than women. We shall return to this subject in the part of this work which treats of Pathology. Authors, in general, confound with this disease another affection which they place equally in the genital organs, in the uterus, although, in the majority



of instances, it has no connection with it, and which only occasionally recognises this organ as its origin,—I mean pretended hysteria. One remarkable fact ought to have struck those physicians, and to have enlightened them concerning the seat of these maladies: it is this,—that affections of the organs of reproduction,—of the uterus, vagina, penis, testicle, or ovaries,—whether acute or chronic, such as cancers, syphilis, gonorrhœa, hydrocele, sarcocele, dropsy of the ovaries, and uterine tumours of all sorts,—remain more completely local than any other diseases: they frequently manifest their presence only by local inconveniences, or even are discovered only after death, having been altogether unknown during life.

“ M. Esquirol knew a lady aged fifty years, in whom the *menses* had ceased to appear for a year, but in whom the discharge reappeared and lasted several years, in consequence of a lively passion of love which disturbed her tranquillity.”

M. Georget had previously proved, in his first volume, p. 387, that sexual pleasure is a sensation or perception of the brain; that all the phenomena which relate to it are equally cerebral, and that the sperm appears to be destined exclusively to excite the vitality of the germ.

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Every thing, then, confirms the proposition, apparently so paradoxical, that all the phenomena related to the functions of the organs of reproduction, have their origin in the brain.

Let us now inquire what part of the brain presides over the sexual instinct.



*The Cerebellum is the Organ of the Instinct of Reproduction.*

Nobody will maintain that the propensity to reproduce, or the instinct of generation, is an artificial product of the will, of the understanding, of education, or of the influence of external circumstances. I shall not, therefore, waste words in proving that this instinct is a primitive power, a special propensity existing independently of others. Neither shall I treat of its natural history, which, on the one hand, is sufficiently well known, and on the other would lead me into inexhaustible details. I proceed, then, at once to the merits of the question, and I shall begin by communicating to my readers the circumstances which first led me to form the idea now advanced, which is equally new in physiology, and strange to public opinion.

*History of the Discovery that the Cerebellum is the Organ of the instinct of Reproduction.*

A young widow, a short time after the death of her husband, was attacked by melancholy and violent convulsions. These affections were preceded by a very disagreeable tension and feeling of heat in the nape of her neck. A few moments afterwards, she fell to the ground in a state of rigidity, to such an extent that at last the nape of her neck and vertebral column were strongly drawn backwards. The crisis never failed to terminate by an evacuation, which was accompanied by a convulsive voluptuousness, and a real ecstasy; after which she continued free from attacks for some time.

I frequently supported her during the crisis with the palm of my hand on the nape of her neck, and felt at that spot a great heat. I remarked also an arched protuberance of considerable size in the same place. Afterwards this lady acknowledged to me, that from the close of childhood,



she had felt it impossible to resist the impulses of an imperious necessity ; and that in these moments, when her feelings were the most urgent, the tension and sensation of heat in the nape of the neck were the most disagreeable.

These circumstances awakened my attention. I recollected having remarked similar symptoms in like cases, and I soon learned that persons of a very ardent temperament feel tension and heat in the nape of the neck on particular occasions, especially after excessive and inconsiderate emissions, or after a prolonged continence.

Apollonius of Rhodes,\* in speaking of the passionate love of Medea, says, "The fire which devours her attacks all her nerves, and makes itself felt even behind the head, in that spot where pain is most poignant, when an extreme fervour seizes on all the senses."

Van der Haar had already called the attention of physicians to the connection which subsists between certain diseased affections of the organs of reproduction and convulsive movements, and tremulousness of the muscles accompanied by pain and sensations of heat in the nape of the neck.

Tissot has recorded observations of the same kind. A watchmaker of seventeen years of age, lost consciousness and experienced convulsive movements of the head, which was violently drawn backwards, and his neck swelled every time when he provoked emission by onanism. During the time of these attacks, which became habitual to him, he felt violent pains in all the posterior region of the neck. They produced, at last, extreme feebleness in the extensor muscles of the neck. In another subject, the same causes produced a complete paralysis of these muscles, in such a manner that the patient allowed his head to rest habitually on his breast.

Prepared as I was by my previous discoveries, the idea could not escape me, that a connection might exist between

\* Histoire abrégée de la Littérature Grecque, de M. Schœll, vol. i. p. 99.



the functions of physical love and those parts of the brain situated at the nape of the neck. In a short time, I collected a prodigious number of facts in support of the idea.

In the first place, however, I shall explain the form and situation of the part of the brain named the Cerebellum.

In Plate iv. 1, the inferior surface of the two lobes of the cerebellum are represented. Plate xv. 1, 1, represents its superior surface; Plate viii. its lateral aspect; Plates v, vi, x, xi, xii, xiii, xvii, represent preparations calculated to shew its internal structure.\*

The size of the cerebellum differs considerably in different individuals, even of the same age. In adults its breadth is from four to five inches, its thickness from twenty to twenty-five lines, and its length from two to three inches and some lines.

The cerebellum is placed in the great occipital swellings of the skull. In front, its lobes touch the petrous portions of the temporal bones, and posteriorly, the transverse portion of the spinous process of the occipital bone (*spina cruciata*). Laterally, it extends to the point of the inferior angle of the parietal bones; it occupies the whole of the posterior region of the skull from the broad part of the mastoid process, on the one side, to the middle of the *spina cruciata* (where there is generally a projection outwards), and onwards to the broad portion of the mastoid process on the other side. In the general sense, its breadth occupies the whole space of the occipital *fossæ*, from the one mastoid process to the other.

As the bones of the cranium are very thin where the cerebellum touches them, their external prominences corres-

\* These, and all the other numbers in this section, refer to the Plates in the large Atlas which accompanies Dr Gall's great work. It is not reckoned necessary to incur the expense of reproducing them here, because this translation being intended for professional readers, they have ample opportunities of studying the structure of the cerebellum in other works necessarily in their hands.



pond exactly with their internal cavities, and these cavities present the precise forms of the cerebellum. Consequently, the skull in this region will be narrower, broader, flatter, rounder, higher, or lower, according as the cerebellum extends more or less in different directions. The reader may compare Plates viii, x, xi, xii.

Some men of science pretend that it is impossible to judge of the size of the cerebellum during life in the human species, because, say they, from the projection which is felt almost immediately above the hollow of the neck, to the occipital hole, it is impossible to feel the skull. But it is precisely in this place that the two lobes of the cerebellum separate, and leave between them a space in which the lower portion of the vertical part of the occipital spine is placed, in consequence of which the form of the cerebellum is not concealed. About half an inch towards the side, the posterior and inferior portion of the occipital bone forms an arch towards the posterior edge of the mastoid bone. The more this prominence is arched, the more it descends towards the nape of the neck, the more it enlarges itself towards the ears, the greater is the size of the cerebellum. In this case, the nape of the neck is large and thick, the neck rounded, and large and thick behind the ears. When, on the contrary, the cerebellum is little developed, these parts are flat, narrow, depressed. The neck, although thick where it leaves the trunk of the body, will be narrow between the one mastoid process and the other.

These observations will enable the reader to judge of the greater or less degree of development of the cerebellum.

I shall consider the organ and functions in the natural state of health; in the state of health modified by artificial means; and in the state of disease.



*Proofs drawn from the state of Health that the Cerebellum  
is the Organ of the instinct of Reproduction.*

1. Nothing resembling a cerebellum can be distinguished in those animals which propagate without conjunction of the sexes. In all creatures, on the other hand, which copulate, we find a part of the brain placed immediately above the spinal marrow, and occupying the situation which the cerebellum holds in the more perfect animals. If it were possible to prove that this conformation holds good without exception in the smallest animals, whether terrestrial or aquatic, this fact alone would be sufficient to establish that the cerebellum is the organ of the instinct of propagation.

Scarcely had the facts before detailed suggested the ideas now explained concerning the functions of the cerebellum, before a difficulty struck me. Plants propagate; therefore, cerebral matter is not necessary for the accomplishment of the act.

But it is necessary to distinguish, in regard to propagation, what part of the process belongs purely to organic, and what to animal life.

In organic life, fecundation and development take place without consciousness, and without participation of the individual. Animal life transforms this unconscious function into one accompanied by consciousness; in animals it becomes a want, a highly active propensity, the satisfaction of which is accompanied with pleasure.

In the more perfect animals, we find these two functions combined. The organic function is in relation with the animal function, but in such a manner that, in the act of propagation, each of them acts a part which may be independent of that performed by the other.

The experiments of some naturalists appear to prove, that it is possible to communicate fecundation to the eggs of females of certain species, by means of the seminal fluid taken from the male, in the same manner as the germ of



plants with pollen taken from the stamina. Conceptions which have taken place during swooning, drunkenness, or other influences which abolished consciousness, are not perhaps events absolutely rare. The organic instruments of reproduction possess intrinsic life, and enter into activity independently of consciousness in the animal. Coition accomplished by violence, or the embraces of a man who excites disgust or even horror in the woman, are followed by conception. I am acquainted with women who, in no period of their lives, have felt the slightest inclination for men,—who could not comprehend how a man could inspire a woman with any feelings different from those excited in her by a child or another woman,—who never yielded to the embraces of their husbands but from a sense of duty,—and who, although they have borne several children, have never experienced the least sensation of pleasure.

Every one knows that there is no proportion between fecundity and the inclination to sexual embraces. How frequently do the wishes of the tenderest spouses fail in being fulfilled! It appears even, that, in certain cases, too ardent a flame may present obstacles to fecundation. It is usual to abate the ardour of mares by beating them, by exposing them to fatigue, or by throwing cold water on them, before leading them to the stallion. I have repeatedly had hybrid female birds, which solicited with ardour the love of all the males, which built their nests with an indefatigable activity, laid eggs, covered them with an exemplary perseverance, and which, when they saw their hopes deceived, gave themselves up to profound sadness. The sexual ardour of the mule is well known, equally violent and steril.

As, then, organic fecundation is absolutely independent of animal coition, the existence of fecundation without the intervention of the brain, cannot be alleged to prove that the *instinct* (which always implies consciousness) of reproduction can be accomplished without cerebral assistance.

2. In the description of the cerebellum,\* I have shewn

\* Tome I. de mon grand Ouvrage, p. 149 in 4to, et p. 176 in fol.



that, in mammiferous animals, it is composed of a middle or fundamental part, and of two lateral portions or lobes.

In oviparous animals and insects, fishes, and amphibious creatures, the fundamental portion constitutes the whole of the cerebellum.

Birds, Pl. i. figs. 2, 5, 7, 6, have only this integral part of the cerebellum, which I call the fundamental portion, and which others have called the vermicular process, *Processus vermiformis*.

In all the mammiferous animals, on the contrary, the two lateral portions exist; they, however, like the fundamental part, have different forms in different species. Pl. iii. fig. 1, and Pl. iv. fig. 1, represent the lower surface, and Pl. xvi. the upper surface of the cerebellum.

This essential difference in the cerebellum in those species of animals which differ essentially in their mode of propagation, creates a presumption in favour of the existence of an intimate connection between the cerebellum and the act. But I proceed to adduce more rigorous proofs.

3. The successive stages of increase and decrease in the manifestation of the sexual instinct, bear a direct relation to the increase and decrease of the cerebellum.

In the new-born infant, the cerebellum is the least developed of all the cerebral parts. Its proportion to the brain is different in every individual; it is sometimes nearly as one to nine, sometimes as one to twenty, and sometimes still less. In the adult, on the contrary, it is in relation to the brain as one to five, at least as one to seven. The nervous fibres of the cerebellum are, of the whole encephalon, those which are latest of presenting a distinct appearance. The cerebellum attains its highest degree of development, at ages varying from eighteen to twenty-six, in different individuals.

The sexual instinct developes itself in a corresponding order of progression. It glides imperceptibly into the mind of the young of either sex; the eyes become more brilliant; the look more expressive; the gait acquires an air of increased



pretension; they are liable to be seized with an inexplicable infantine melancholy; they feel a want for which they cannot account; they experience confused desires; until at last the presence of a beloved object solves the enigma, and spreads a vivid pleasure over the whole soul.

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MM. Scemmering, Ackerman, the brothers Wenzels, and others, maintain that, at the age of two or three years at the most, the cerebellum, as well as the brain, have acquired their full development.

Mons. Scemmering, whom the others have copied, is mistaken. He rests his assertion on the fact, that he had found the brain of a boy of two years, as large as that of an adult, which he had opened at the same time. He supposed that all human subjects have nearly the same quantity of brain; and, according to this hypothesis, his conclusion would be just. But as the weight of the cerebral mass differs in adults, so that it is sometimes two pounds, sometimes two pounds and a half, three pounds, and sometimes still larger, it may easily happen, that a boy of two or three years of age, so favourably organized as to manifest great moral and intellectual faculties, may possess a mass of brain larger than that of an adult whose faculties are all very moderate.

In our numerous dissections of the brain, we have always been very attentive to the cerebellum. But with the exception of some extraordinary cases, we have never observed the same proportion between the cerebellum and the brain in subjects under sixteen or twenty years of age, as in adults. It is easy to establish the truth of this remark, by comparing the skulls of young boys and young girls with those of men and women. What a difference already between the skull of a boy of ten or twelve years of age, Pl. xxxvii. and that of a new-born infant, Pl. xli.! In the new-born infant, Pl. xli., the whole base of the cranium is still contracted into a truncated cone; the mastoid processes still closely approach towards each other;



the occipital swellings are not yet prominent—they are actually flat. In the second year, the mastoid processes are more widely separated from each other; the occipital *fossæ* have become deeper, and resemble more the segment of a sphere. All these changes are still more observable in the skull of the boy of ten or twelve years of age. The imperfect development of the cerebellum is seen also in the cranium of the young girl of six years, Plate xxxviii. In turning the base of the cranium of a subject below puberty towards us, we see at once that the distance from the one mastoid process to the other (which distance determines the size of the cerebellum), is much less than that from the one parietal bone to the other. In the adult, on the contrary, these two distances are in general very nearly the same. Accordingly, these facts have at last led M. Chaussier to admit, that the cerebellum does not develop itself till towards the age of adolescence.

At the approach of old age, the nervous turgescence of the cerebellum diminishes, and in the same proportion the man and woman become, by little and little, strangers to their destination, till at last they respectively become isolated and without an object.

M. Rudolphi assures us, that he has never found the cerebellum less in aged subjects than in adults.

As in advanced life the whole nervous mass experiences diminution, why should the cerebellum form an exception to this rule? If we compare the occipital *fossæ* in adults and in very old subjects, then, unless all the bones of the skull have already become thin by the effects of decrepitude, we shall find the occipital *fossæ* less transparent in the latter than in the former; and this, because a deposition of bony matter has taken place on their internal surface, in proportion to the shrinking of the cerebellum. I have, in my collection, skulls in which the cerebellum has shrunk to the dimensions almost of that of a new-born infant, and in which the space which it occupied has been much contracted, not only by the thickening of the walls of the occipital



*fossæ*, but also by that of the petrous portion of the temporal bone.

4. Nature does not follow a uniform rule in the development of the cerebellum. We know the influence which climate exercises over the period at which the two sexes become capable of the act of reproduction. But there are great differences, in this respect, between different individuals in the same place and in the same family. One boy will shew an interest in the opposite sex even at the age of three or four,—in another the instinct will lie dormant till the fourteenth or fifteenth year.

The same differences appear on the approach of age. In one man, the masculine power ceases to exist at forty, in another it manifests itself still at the age of seventy or eighty.

It is not a rare occurrence that this instinct manifests itself, in the most decided manner, even before the ordinary period. At Paris, I saw a boy of five years of age who, in regard to his bodily powers, seemed to be sixteen; his sexual organs were fully developed; he had a strong beard—a rough and masculine voice—in a word, he presented all the indications of complete virility. He had already satisfied his desires with women. I did not allow myself to be deceived by these external indications. I did not ascribe the precocious manifestation of the sexual instinct to the premature development of the external organs; because, a short time previously, I had seen a girl of nine years of age who appeared to be a completely developed woman. She manifested only the indifference of a child when her parents exhibited her to me, and she had never shewn the slightest interest in any object related to physical love. In Buffon and other authors, we find similar examples of large children, who presented all the marks of puberty, without any one having observed in their conduct indications of their being alive to the instinct of reproduction. In the girl before mentioned, the cerebellum was developed only to a very insignificant extent; but the case is quite the contrary in



the boy of five years. The nape of his neck was large, rounded, and strong, although the rest of his head had scarcely attained to the dimensions common at his age. Moreover, he was a child in every other respect.

We found the cerebellum quite as much developed in a boy of ten years, who was detained in the house of correction at Leipzig for having violated a young girl. At Paris, I have seen the boy of a mulatto, under three years of age, who assaulted not only little girls but women, and attacked them with audacity and determination to satisfy his desires. He experienced in his external organs, which were not prematurely developed, but which presented only the dimensions proportionate to his age, more than momentary erections. As he was surrounded by girls who permitted him to satisfy his desires as an amusement to themselves, he died of consumption before having attained the end of his fourth year. His cerebellum was developed in an extraordinary degree, while the remaining parts of his head were not larger than is usual at his age. He was, in every other respect, only an ill-brought-up and spoiled child.

It is truly astonishing that physicians and naturalists have never sought for the seat of the propensity to sexual love in other quarters than in the organs of sex. Every day we see young boys and young girls of three, four, or five years of age, give themselves up headlong to onanism, without shedding any fluid, and without the development of their external organs giving occasion to suspect in them the tendency to any such vice. We deceive ourselves greatly in believing that we can prevent children thus organized from contracting pernicious habits merely by protecting them from external seduction. This precaution is applicable to ordinary children, but there are some who are seduced by their own nature. Who seduces the young apes? Moreover, some old men present phenomena resembling those which occur in children; after their external organs have already become paralyzed, they are devoured by vehement internal desires.

The facts which I have just cited, of children whose ex-



ternal organs were, or were not, developed, and who not only experienced the impulse of the instinct, but who possessed also the power of coition, and in whom the cerebellum alone had acquired a premature development, prove to demonstration that the seat of the propensity must be sought for, not in the genital parts, but in the cerebellum.

5. The energy of the instinct of reproduction bears a direct proportion, in adults, to the development of the cerebellum.

There are men and women who perform the act of cohabitation only as an act of duty. Coition inspires them with repugnance and disgust. Those who attach a great importance to it are in their eyes sensual beings, debasing themselves below the brutes. We do not discover in these individuals the least difference in the external organs which should distinguish them from other men and women, but they uniformly exhibit a feeble development of the cerebellum.

By the kindness of Baron Larrey, I have seen a soldier in whom the antipathy to women had degenerated into a true mania. The appearance of a woman excited in him violent convulsions, and threw him almost into a state of fury. Dr Spurzheim has seen a similar example in England. In both of these subjects the development of the cerebellum had absolutely stopped short.\* A physician of Vienna, distinguished for his great talents, shewed a marked antipathy for women, a singularity which we attributed at the time to his love of solitude. Some years afterwards he died of pulmonary consumption. In his skull, in other respects very large, Pl. xlv. 1, 1, 1, 1, the space for the cerebellum is very small. The distance from the one mastoid process to the other is scarcely three inches; the occipital swellings, instead of being convex, are in some parts quite flat, in others

\* It has been objected that no organ can produce an effect opposite to that of its function. But is not the stomach the organ of the appetite for food, and does it not happen that, in consequence of disease in this organ, we are seized with disgust for every species of aliment?



actually depressed, and on this account present even an unequal surface.

In a French Abbé, resident at Vienna, we admired an exemplary continence, and a manner singularly reserved towards women, so much the more that he was passionately fond of dress, and passed the day in going from one party to another. He died, and his skull is one of those which I preserve as an example of the very small development of the cerebellum. The occipital swellings are so flat, that the skull appears as if the inferior portion of the occipital bone had been cut off. Pl. *xlvi*. 1, 1.

The lady of whom I have spoken above, page 17, has a large and beautiful head like a man, and she possesses distinguished talents; but the nape of her neck has very little breadth under the ears, which indicates a small development of the cerebellum. I have hitherto found this conformation in all persons in whom the instinct of reproduction was naturally feeble.

Wherever a similar organization has existed, the individuals have been little devoted to the fair sex. The portraits of Charles XII. Pl. *lxi*. fig. 1. of Newton, and of Kant, shew, and certainly without any intention on the part of the artists, that the necks of these distinguished men were small, and consequently their cerebella little developed. Is it astonishing that Saint Thomas à Kempis, in whose portrait the same characteristics appear, is represented as furnished with a fire-brand, in order to drive away from him a young woman full of beauty and attractions? These are the men whom nature devotes to celibacy.

To form an idea of the difference which exists between a small and a considerable development of the cerebellum, the reader may compare the skull, Pl. *viii*. with those of which I have just spoken, and again with those in which this organ has attained to an extraordinary degree of development. Plates *xxxix*. and *xl*.

In Vienna, a devout and superstitious female fortune-teller, although already advanced in life, always maintained



two lovers. In her small spherical skull, Pl. L. fig. 1, the occipital swellings are very large, rounded, and prominent towards the base. The same conformation appears in the large skull, Pl. xxvii. of a woman who was confined in the House of Correction at Grætz, in Styria, and who previously followed the armies as a prostitute. A teacher of languages, who in every other respect led a very regular life, assured me that he could never satisfy himself in his enjoyments with women; and in his skull, Pl. xxxix. fig. 1, the lower region is very large, and it projects so much from above downwards, that its descent below the external opening of the ear exceeds by more than an inch that in the skulls of the chaste French Abbé, and the young physician before mentioned. The same conformation is found in a celebrated physician, who in a very short time had sent three young and robust wives to the grave, and who, at nearly fifty years of age, kept four vigorous girls in his house. And what is very remarkable, there never existed the least spark of jealousy between these women, probably from a cause which they mentioned to me, that they were all four more than satisfied with enjoyment.

The reader is requested to compare the portraits of Charles XII. of Newton and Kant, also with those of Piron and of Mirabeau, Pl. lxi. fig. 2, both of whom were much addicted to sensuality; of Nicholas Chorier, author of a work entitled, "*Meursii elegantiae Latini Sermonis*," translated into French under the title of the *Academie des Dames*; of Arétin, equally voluptuous and caustic; of Francis I, who used to say that "a Court without women is like a year without a spring, and a spring without roses." The ancient sculptors gave this large and strong nape of a neck to Epicurus, certainly without intending, when they did so, to ascribe to him the strength of Hercules. This observation may be verified every day in society.

When the cerebellum acquires an excessive development, to such an extent that the nape of the neck forms a kind of projecting pouch from above downwards, the instinct also



will be found to act with the force of a deranged impulse. When motives of a higher order, and great moral and intellectual qualities, do not sustain individuals thus organized, that extent of sexual enjoyment which nature intends, does not suffice to satisfy their desires : it appears to them insipid ; and they burn with a fire resembling that which consumes individuals in large societies of the same sex secluded from the other.

I have had opportunities of observing several men and women, who were the slaves of this propensity in a depraved condition. The large and arched nape of the neck is conspicuous, particularly in the women. Almost all the women abandoned to this propensity have at the same time a robust and masculine constitution. The men, on the contrary, have an effeminate body ; their limbs are rounded, fat, mottled, and small, and their breasts very conspicuous. The ancient historians describe Nero as delivered up to the most intense voluptuousness, and say " that his propensities were painted on his countenance ; that he had small eyes, sunk in fat, A THICK NECK, a large belly, and thin legs ; and that his fair hair, and his delicate rather than majestic face, caused him be recognised at once as effeminate." I have, however, met with some exceptions to this rule.

We saw in a House of Correction in Holland, some men already advanced in life, who, with an organization otherwise favourable, had been addicted to similar disorders. They acknowledged their incapacity to resist the impetuosity of their propensity, and had already several times relapsed into the same vice. They were convinced that their conduct had been blameable and unbecoming ; but they begged not to be set at liberty, because if they were set free, they would relapse into crime. We made the individuals who accompanied us remark the excessive development of the cerebellum in these men.

I beg to be allowed here to make one observation. An individual who lives in a state little accordant with his dispositions, finds himself in contradiction with his external



condition and with himself. Is it prudent, or is it just to impose by surprise on young persons in whose hearts probably a smothered flame glows, the nature of which they do not understand, vows which must during their whole lives place them in contradiction with themselves? Should not the different conditions in society be the results of the differences of organization in those who devote themselves to them? If you wish to have vestal virgins and cenobites, choose those individuals who have been eunuchs from their mother's womb.

What I have described as appearing in the human species, is found equally in the lower animals. There are dogs, horses, &c. which never copulate; there are females which, although their sexual organs present all the indications of heat, never permit the approach of a male. The cause of this anomaly is always found in the defective development of the cerebellum. Near Berlin we visited a stable in which we saw five bulls. In one of them, the nape of the neck was not nearly so broad and strong as in the other four; and we declared that this bull should not be nearly so powerful a male as the others. "You are right," said M. de Beyme, formerly Minister of State, to us: "We fatten him because he is not useful at the head of the herd."

Bulls, stallions, and rams, on the contrary, are the more ardent, the more the nape of their neck is broad, round, and vigorous. At Vienna, all the fanciers of pigeons know that the male pigeons which have the strongest napes, are those which pursue the females with the greatest ardour, and people avail themselves of this circumstance to seduce the females of a neighbour, and make them come to their pigeon-houses. They deprive the most ardent male of his female; he then makes excursions into the other pigeon-houses, carries off their females, and forces them to accompany him home. Immediately the male of the seduced female follows her; and in this manner successive seductions take place, till the robbed proprietor terminates the sport by putting



the ravisher to death. Rats, mice, the mole, and the guinea-pig, have the cerebellum very large, and it is in consequence of this organization that the instinct of reproduction is very active in them.

In animals that are abundantly fed, in canaries, and especially in pigeons and ducks, of either sex, in dogs, mares, stallions, and apes, the same depravity is often observed which I have just described as occurring in the human species.

All these are so many proofs that the instinct of reproduction bears a direct relation to the degree of development of the cerebellum, and consequently, that this part should be considered as the organ of this propensity.

6. The difference observable between the two sexes in regard to the degree in which they manifest this instinct, depends also on a difference in the degree of development of the cerebellum.

It is a question, whether in man, and also in the lower animals, the instinct of propagation has a greater degree of activity in the male or in the female?

I admit that exceptions, in regard to particular individuals, exist, but I maintain that men, in general, feel this propensity much more strongly than women; and the following observations establish this proposition.

Among the lower animals, the females of several species, such as bitches, mares, cows, are limited, in the manifestation of this propensity, to certain seasons and periods, while the males are disposed to love all the year. The habitual condition, then, of these females is not sufficient to maintain the instinct of reproduction in constant activity. An external influence is necessary to excite their organization before they suffer or desire the approach of the male.

Even in those animals which live in a state of marriage for life, such as the greater number of birds, of martens, and of foxes, the males are more ardent and more addicted to infidelities than the females. Hence arise, among these animals, perpetual jealousies and combats. It may be re-



marked, that in certain species the females are very jealous, while in others they shew no sign of jealousy.

In our own species also, man is more strongly excited to the pleasures of love than woman. Hippocrates even recorded this truth. Nature has subjected the female to the inconveniences of pregnancy, the pains of childbirth, and the duty of suckling and rearing children, and she has adapted the entire organization of woman to accomplish this high and important object. On the other hand, the part which man performs in regard to reproduction is limited to the mere act of fecundation. The pleasures of love are strongly desired by the young man, by the adult, and often also by the man advanced in life. All our social institutions testify the abuses in which our sex indulges, from its superiority in physical force, and they bear the stamp of our jealous propensity to sexual pleasure.

Let us now inquire whether these phenomena can be explained by his organization.

It has hitherto been supposed, that because man is the stronger of the sexes, his propensities should, from this cause alone, be the more violent; but in the section on the organ of the mind, I have proved, that absolutely there is no direct proportion between the intensity of the propensities and the activity of the faculties, on the one hand, and the physical strength of the constitution, taken collectively, on the other. Small animals are, generally, more ardent in love than the large. Who does not know that often large men and large women are very dull, and small and feeble individuals very ardent in the enjoyment of voluptuousness?

Difference of education is equally incapable of explaining the facts. In the first place, its influence does not extend so far as to destroy the natural dispositions. In the second place, we should never forget that education itself, public institutions, and laws, are the results of our organization: *We* do not produce them,—it is the Author of our being who produces them *through* us. Finally, in the case of the lower animals, how can the difference in the manifestation



of the instinct of reproduction in the two sexes, be the consequence of education?

No; the case is quite different. An eternal law of nature must be founded on quite a different basis. The cerebellum is, in general, perceptibly larger in males than in females. In the greater number of instances, when we place the male brain and the female brain of the human species, and also of the lower animals, side by side, the brain of the male is distinguished by a larger cerebellum. The best method for rendering this difference perceptible is, to place the brains in water, that they may preserve their forms, and not become flattened by their own weight.

This observation has been confirmed in all the animals which I have had it in my power to observe, from the shrew-mouse to the elephant.

It almost appears that this difference is more remarkable in the human than in the other species of animals. Accordingly, there is no creature with whom man should be tempted to barter, when it is considered, that he is in a condition to enjoy, during the whole period of his adult life, and at all seasons; that his enjoyment is the most complete, &c. The reader is requested to compare the cerebellum in women, Pl. iv. x. xiii. with the cerebellum in men, Pl. v. viii. xi. xii. xv.

It may be objected, that man having, in general, a larger mass of brain than woman, should also have a larger cerebellum.

But I have already shewn that there is no fixed proportion between the different parts of the brain. The cerebellum is independent of the brain, and forms an organ by itself. Sometimes there exists in the same subject a large brain and an extremely small cerebellum; in other instances the cerebellum is very largely developed, and the brain is very small. The young physician of whom I have spoken before, who shunned women, had a head of the largest size, and one of the largest brains, yet his cerebellum was very small. The female fortune-teller, on the other hand, had



scarcely one-half of the quantity of brain possessed by the young physician, but her cerebellum was much larger. In treating of the organ of the love of children, I shall shew that certain portions of the brain are more largely developed in women than in men. Consequently this objection falls to the ground.

To be able to make successful observations on this organ in the lower animals, it is necessary to know exactly the situation of the cerebellum in the skull. In many animals, particularly in those species which never walk erect, the cerebellum holds a horizontal position, and almost entirely behind the brain. See Plate xxxiii., the brains of the kangaroo, fig. 3, and of the tiger, fig. 5. In these animals, the degree of its development is recognised by that of the posterior part of the cranium, above and on the two sides of the occipital hole.

In others, the cerebellum is, nearly as in man, covered, either entirely or in part, by the posterior lobes of the brain; and it is only by inspecting the base of the cranium that we can successfully ascertain its greater or less development. See Pl. xxxiii. fig. 4, the brain of the lion; and Pl. xxxiv., that of the ape, fig. 1; and of the ourang-outang, fig. 2 and fig. 3.

In birds, the cerebellum, consisting solely of the vermicular process, or the fundamental portion, extends from the posterior middle region of the two hemispheres of the brain, to the occipital hole, Pl. i. fig. 11, 5, 7, 6; but it fills only the middle portion of the occipital bone, for the lateral parts contain the organs of hearing. This middle portion is constantly larger and more rounded in male than in female birds. The Pl. lvii. represents, fig. 1, the skull of a cock; fig. 2, that of a hen; fig. 4, that of a female turkey; and fig. 5, that of a turkey-cock. Compare, Pl. lviii., the skull of the male field-rat with that of the female, fig. 1; the skull of the male cat, fig. 4, with that of the female cat, fig. 3; and the skull of the male dog, fig. 6, with that of the female, fig. 7.



Also, in Pl. lix., the skull of the male calf, fig. 2, with that of the female calf, fig. i.\*

Thus, then, the conformation of the cerebellum in the two sexes accords perfectly with the physiological fact, that the instinct of reproduction is more powerful in males, in general, than in females; and this accordance is another proof that the cerebellum is the organ of this propensity.

7. The kind of caresses which certain animals practise should have long ago arrested the attention of naturalists. Sometimes the male, sometimes the female, is in the custom of exciting the nape of the neck of the objects of their desires. Long before the act itself, the male cat bites, amorously, the nape of the neck of the female, and sometimes he continues this play during an entire day. I have often seen bitches, in heat, bestow blows with their nose on the napes of dogs, not very ardent, to excite them to copulation. The male duck, before proceeding to the act of fecundation, mounts tranquilly on the female, and passes his bill three or four times across the nape of her neck; and it is only then that the female squats, and that the copulation takes place. In spring, I have had occasion to observe at my ease, the loves of sparrows of a cottage placed in my garden. The male, in uttering cries expressive of the ardour which he felt, performed sundry leaps round the female, his head strongly drawn backwards, and his wings extended. The female appeared to pursue the male, and leapt often upon him, giving him blows on the nape with her bill. After this prelude, the two retired in all haste to a tree, where they completed the act.

The proofs which I have already adduced are sufficient to establish, that the cerebellum is the organ of the instinct of reproduction. I have still more evidence, however; but I shall reserve it for a later portion of this work.

\* In all the figures of Plates lvii. and lviii. the occipital swellings are marked I I.



*General Observations on the Organ of Propagation, and  
on the Instinct itself in a state of health.*

The Greeks, Arabs, and some modern pedagogues, have regarded the cerebellum as the seat of memory. Willis deduced the talent for music from the softness of its structure; and Malacarne wished to determine the degree of power possessed by the intellectual faculties by the number of its laminae. Mons. Portal believes that the cerebellum secretes the animal spirits, or that it is intended to execute the functions of the brain when the latter is attacked by disease. Reil considers it as a voltaic pile. Several physiologists regard it as the source of organic life. These hypotheses do not rest on any facts.

There is no definite proportion between the intensity of the instinct of propagation and fecundity; and, in like manner, there is no determinate proportion between the development of the cerebellum and that of the sexual organs.

It is indubitable that the organ of this instinct enables every animal to distinguish between the male and the female of its own species. We are tempted, indeed, in certain cases, to admit that it establishes, in nature, a general peace between all that is male and all that is female. It is known that many male animals, particularly apes, dogs, stallions, and parrots, lay aside their habitual mischievousness, and forget even their anger, in the presence of women. I have seen the most furious bulls, whom dogs and men had in vain attempted to subdue, give way before a female servant who came up with a scourge in her hand. On the other hand, I had much difficulty one day in saving a lady, with whom I was walking in a meadow, from the fury of a cow. This cow bore an indomitable hatred against women.

Perhaps some of my readers may be of opinion that we cannot admit an organ of the instinct of reproduction in the brain, because, in a number of animals, the activity of this instinct is limited to certain periods, and that, in their case,



it appears sometimes not to exist at all, and at other times to assume a complete ascendancy over the rest. But, in many cases, this objection would be applicable also to the existence of the external organs of sex; and, besides, there are many other instincts which are dormant in certain seasons and alive in others, the organs of which, as I shall immediately prove, exist all the while in the brain.

This phenomenon may be explained even in favour of the idea that the cerebellum is the organ of the instinct of reproduction. I have collected many heads of birds, at the beginning of spring, the season of their most ardent loves; I have collected others at the commencement of winter, a period when all the feelings related to propagation are exhausted. In the heads collected in spring, the cerebellum was larger and more turgescient; in the skulls, the elevation which corresponds to it, is manifestly larger and more rounded, than in those collected in winter.

In spring, all the parts situated about the nape of the neck receive an abundant influx of blood, and experience a peculiar nervous turgescence. The throat is developed, and, in general, these parts are, during the whole season of love, in a state of irritation. In the female chamois, a tumour is formed at this time, which suppurates or oozes, during all the season of rutting, a liquor of a very strong odour. All this proves that the testicles and the ovaries are not the only parts which diminish during the period of repose of the sexual instinct, and which in the season of love recover a greater degree of fulness.

From all these phenomena, I draw the conclusion, that a reciprocal action exists between the cerebellum and the parts which immediately surround it, as well as between it and the sexual organs themselves. In treating of the state of disease, I shall bring forward still more convincing proofs of this assertion.

Another objection stated in the *Dictionnaire des Sciences Médicales*, t. xxxvii. p. 267, is the following: "Why, then, if the protuberance related to such or such a particular sen-



timent or passion remaining the same, are there such continual changes in the corresponding sensitive affection or disposition? If physical love, or the sexual appetite, has its organic seat behind the head (*derrière la tête*), why are the intermission, the changes, the degrees of strength or weakness of the passion, always proportionate to the state of irritation or of action of a particular centre of sensibility, or of a particular nervous apparatus, whose influence is sufficiently well known? If it be the predominance of such a centre, and its actual excitation by the seminal fluid, impressing and irritating it, that determine appetite and produce the passion of physical love, why should it not be *there* also that this love should have its proper organ or peculiar seat?"

When Mons. Delpit printed this article in 1819, my treatise "On the Instinct of Propagation" had been previously published in the year 1818, and my ideas were known by my public lectures, and by the works of my scholars. Why does not Mons. Delpit undertake to state all my proofs, and to invalidate or refute them, one after the other? These gentlemen pretend to destroy the most extensive evidence of every kind by a single remark, however much it may be uttered at a venture. That may well be called (to use an expression of Cabanis relative to the faculties of women), skimming lightly over the surfaces of things. The editors of a dictionary undertake the obligation of transmitting to posterity the existing knowledge of their own age. But all the writers in the Dictionary of Medical Sciences, who have spoken of the anatomy or physiology of the brain, have indulged themselves, and often in bad faith, in the most puerile inaccuracies. In the first place, why does Mons. Delpit say that the sexual appetite has its organic seat *behind the head* (*derrière la tête*)? Why does he not say distinctly, as I do, that it has its seat *in the cerebellum*? Is there not a little malice here? In the second place, if Mons. Delpit and his committee had devoted due attention to their subject, they would have found, in almost all the treatises on this instinct,



that its intermissions, variations, and degrees of energy and weakness, are not by any means proportionate to the irritation of the sexual organs. They would have found that this instinct is sometimes very energetic, before the least irritation exists in the external organs; that, too often, it is also very active, when these organs have ceased to answer to any sexual stimulus; that it exists sometimes in castrated subjects, even in individuals mutilated in this way by an imperfection in their original organization; and they would have found, moreover, that it is much more the action of the brain than the excitement of the external organs by the seminal fluid, which gives rise to the sexual appetite. Finally, they would have felt, that, in order to take a part, with any hope of success, in discussions on important questions in physiology, it is necessary to have at least some idea of the difference between a directing or regulating organ and its instruments of execution.

*Proofs drawn from the state of Disease, in favour of the proposition that the Cerebellum is the Organ of the Instinct of Propagation.*

*Influence of Castration on the Cerebellum.*

Castration may take place in early youth, or in adult age.

In the former case, the influence of the operation on the whole constitution, on the instincts, the propensities, and the intellectual faculties, is more general and better marked than in the latter case. Every male animal whatever which has undergone it, as well as man, acquires female forms. In man, the beard does not grow, the throat is not developed, and from this cause the individual never acquires the voice of the male, &c. Every one is acquainted with these phenomena; but no attention has been paid to the most essential fact of all—the defective development of the cerebellum.

The cerebellum is arrested in its development, and does



not attain nearly to those dimensions which it would have acquired if castration had not taken place. If we examine the part occupied by the cerebellum, in the skulls of men and animals who have been castrated young, it appears as if shrivelled; it is not so broad and deep; even the bones of the skull immediately contiguous to it are thicker, less transparent, and more rugged, than in non-castrated subjects. The skull of the castrated cat, Pl. lviii. fig. 5, may be compared with that of the entire cat, fig. 2. The skulls of castrated rabbits, sheep, and horses, may be compared with those of the entire animals of these species. The difference strikes one at the first view, and becomes still more perceptible when we measure the cavities which inclose the cerebellum, the occipital *fossæ*, in all their dimensions.

It is from this defect in the development of the cerebellum, and not at all from the inferior prominence of the muscles (as I have proved in the section on the influence of the brain on the form of the skull), that all castrated animals have the nape of the neck thinner and narrower than entire animals, such as the ram, the bull, &c. This difference is well marked even in the cock and capon, although in this species of animals the cerebellum is placed in the middle of the posterior portion of the cranium. See Pl. lvii., the skull of the capon, fig. 3, and that of the cock, fig. 1.

This imperfect development of the cerebellum is also the sole cause why, in some cases, the instinct of propagation does not manifest itself, or manifests itself only in a very imperfect manner. If Boileau had not been deprived of his virility by the blow which a turkey-cock struck him with its beak in his infancy, he certainly would not have poured out his caustic bile on the fair sex; and the cruelty with which, according to report, eunuchs treat women, would take at least a different direction, if the development of their cerebella were not prevented.

When castration takes place after the completion of growth, or at least at a period when the cerebellum is already to a great extent developed, it does not prevent the



manifestation of the instinct of propagation, nor destroy the power of exercising copulation. This is a certain proof that the instinct depends on other conditions than the existence of the genital organs and seminal fluid.

Some physiologists, who derive the instinct from an irritating quality in the prolific fluid, maintain that in castrated persons the semen remains in the blood, and that this explains all the phenomena which eunuchs present.

This explanation assumes, that in the blood a true spermatogenic liquor may exist, which has not been secreted by the testicles, nor received into the seminal vessels, and absorbed from them. But this hypothesis is in contradiction with the principles of anatomy and physiology. According to the views of these physiologists, the seminal fluid should exist equally in the aliments; and, moreover, why does it not exist in the blood of animals which have been castrated in a very early age, in the blood of female animals, and of women?

To escape from these difficulties, they have recourse to the liquor of the prostate gland; it is this which, if we are to believe them, produces, in eunuchs, not only the instinct itself, but which gives them the capacity also to carry it into practical effect. But it is well known that the secretion of this liquor takes place in decrepit subjects, and that it does not at all exist in children, who nevertheless have frequent erections.

It appears, however, that the effects of castration are not the same in every subject. In some, the hairs of the beard fall off, never to reappear; the throat, which was already developed, contracts again, and the voice becomes once more that of a boy who has not attained to puberty. In this case, and I am tempted to say in every case, the influence of the operation ends by manifesting itself on the cerebellum. It does not always diminish to the extent of becoming equally small as if its development had been arrested in infancy; but it shrinks and flattens considerably; the occipital swellings, also, which were previously rounded, become flat, and the space between the two mastoid processes becomes



narrower. These changes are followed by a greater calm in the temperament, and at last by impotency.

Thus, then, observations made on eunuchs, prove that the instinct of reproduction does not depend on the genital organs, but on the cerebellum.

All these facts, however, do not hinder M. Richerand from saying: "Cranioscopy makes the cerebellum the organ of physical love; that is to say, it lodges there the reproductive faculty. In vain is it objected that the cerebellum in eunuchs is as large as it is in other men; that the amputation of the genital organs, performed early, extinguishes amorous desires, without preventing, nevertheless, the cerebellum from growing; that it is very difficult, not to say impossible, to judge, in an entire head,—one not stript of the flesh, of the greater or less projection of the inferior occipital swellings corresponding to the cerebellum; that wounds of this region, as well as those of the spinal marrow, must diminish the generative faculty in the same manner as they weaken all the other faculties; that the physicians of the German Bishop, who was seized with amorous insanity, and whose case he relates in his lectures,—cured him by means of castration, and not by inflicting a wound in the nape of his neck; that it was not in consequence of a wound in the cerebellum, that the Scythians, mentioned by Hippocrates in his immortal work *De Aëre, Locis, et Aquis*, became incapable of generation; that, in general, the cerebellum is larger in animals than in men, although the greater number of the former are deprived of the power of making love at all seasons, and are less salacious. M. Gall ascribes no weight to all these observations, and pursues his course without deigning to reply to them.

"Tel un ânon broute l'herbe naissante,  
Malgré les cris du maître et des servantes." \*

After what has been said, I content myself with remark-

\* "Such an ass plucks the springing grass, regardless of the cries of the master and of the servants."—*Des Erreurs Populaires relatives à la Médecine*, seconde édition, p. 265.



ing, that it is absolutely false that the cerebellum is larger in animals in general than in man; the ox, the horse, the ass, the pig, and an infinity of others, have it obviously smaller. It is only the elephant, and the great aquatic mammiferous creatures, who have it larger.

*Influence of Unilateral Castration on the Cerebellum.*

Of all the facts which go to prove that the cerebellum is the organ of physical love, the effect produced by the removal of a single testicle is undoubtedly the most decisive.

On every occasion when one testicle only has been removed from any animal, of whatever species, the lobe of the cerebellum, on the opposite side, visibly decays, or is altered in some way in its substance.

M. Dannechy has communicated to me the following fact, which he observed himself in the hospital of the School of Medicine, in presence of M. Patrix, assistant surgeon of the establishment, and of several pupils. It is recorded under the No. 108 (15th July 1817), in the volume of the Pathological Observations made in the hospital. In the dissection of Jean-Michel Brigaud, who died the 14th of July 1817, after having, on 30th December 1815, undergone an operation for diseased testicle of the right side, the following remarks were made. "The brain and the cerebellum were covered with a light layer of a white transparent albuminous substance. The left lobe of the cerebellum was much softer and flatter than the right lobe. Its convolutions or folds appeared also more sunk or obliterated on the same side. Each of these lobes having been opened at exactly six lines of distance from the lateral portion corresponding to the *medulla oblongata*, we were surprised to see how much greater the proportion of the white matter and of the grey matter was in the right lobe; the difference was estimated at more than a third. The interior development of the cranium also corresponded to this difference."

Baron Larrey sent to me a soldier who, in undergoing an



operation for hernia, had lost the right testicle. Several years afterwards his right eye became weak; he began to squint with the diseased eye, and could scarcely any longer distinguish objects with this eye. I examined the nape of his neck in presence of the two physicians who had brought him, and I found the occipital swelling of the left side much less prominent than that of the right side. The difference was so perceptible, that the two physicians were struck with it at first sight.

I have caused several rabbits to be castrated, some on the right side and others on the left. Having had them killed six or eight months afterwards, I have found, without exception, the lobe of the cerebellum of the side opposite to that on which the castration had taken place smaller, and the occipital swelling flatter than on the other side.

It is true that I am acquainted with a man who lost a testicle four years ago, and at present no difference is discernible between his occipital swellings; but this fact proves nothing against the propositions which I have before advanced, because the change may exist in the interior, without having yet become perceptible on the exterior of his skull.

*Influence of Lesion of the Sexual Organs on the Cerebellum.*

From an early period, hunters have observed that injuries of the testicles in roebucks and stags have a remarkable influence on the horns. The animal no longer casts them, and he becomes deformed in a variety of ways; fungous excrescences grow out from them in the form of cauliflowers. We saw, at Marbourg, a considerable collection of horns thus deformed. Every one knows the difference between the horns of oxen and those of bulls.

According to what laws do these phenomena take place? This question may give rise to widely different opinions. In consequence of the views before mentioned, when I treat-



ed of the influence of the brain on the skull in the state of disease, I maintain that, in all these instances, a change first takes place in the cerebellum,—that there is a diminution of the nervous fulness in this part. I have proved in the same section, by adducing numerous facts, that the bony substance of the skull becomes either more dense or thicker, whenever the brain begins to diminish, whether in consequence of a long cerebral disease, or in consequence of old age. It is only in this manner that we can conceive the excessive growth of the horn, after injury of the testicles, to take place as a consequence of the diminution of the cerebellum produced by these lesions.

Another observation, made also on stags, appears to confirm the reciprocal influence of the cerebellum and the horns. When the horn is cut close to the crown, immediately before the rutting season, the copulation of the stag becomes unfruitful. However, it is narrated that a fallow deer, which was abundantly fed in a park, proved an exception to this rule.

I proceed now to facts which I have observed in man himself, and which leave no room for doubt concerning the injurious influence of lesions of the testicles on the cerebellum.

A man of thirty had the left testicle slightly crushed when he was about the age of twenty-four; by little and little this testicle decayed, and assumed the consistence of a little ball of cotton. After having attended my lectures, he was curious to know if this accident had exercised any influence on his cerebellum. He soon perceived that his right occipital swelling was much less rounded than the left. I have myself carefully examined this subject, and I have found the facts to be completely confirmed, that the left testicle was wasted, and the right side of the cerebellum decayed.

A short time afterwards, a domestic servant came to me, and complained that he had been for some time past much more indifferent towards women than usual, and he ascribed



this altered state of feeling to a blow which he had received in the testicles some years before. In him also I found the left testicle almost entirely wasted. I applied my hand to the nape of his neck, and I felt that the right occipital swelling was entirely flat; the left, on the contrary, was considerably elevated. By chance, one of my pupils came to my house, and I made him enter without saying a word. After he had examined the nape of the neck of the servant, he asked him whether one of his testicles was not smaller than the other,—a question which excited great astonishment in the man. Here, then, are two cases of wasting of the lobe of the cerebellum, where there had been injury of one testicle. I shall now advert to some observations, which authorize us to conclude, that the medullary fibres ascending from the organs of generation, along the spinal marrow to the cerebellum, cross or interlace with the medullary fibres of this part, just as a great part of the medulla oblongata interlaces or crosses with a great part of the brain.\*

Baron Larrey sent to me a soldier, aged thirty, who, nine years previously, had bruised his testicles against the pommel of the saddle, in consequence of which they swelled, and became very painful. A short time afterwards they began to waste away, so that at last there remained only two little membranous balls. The occipital swellings are narrow, sunken, and unequal; he does not feel the least irritability in his feeble penis, and women are to him, now, quite objects of indifference, although formerly he was much addicted to them.

The following facts, recorded in the volumes of *Pathological Anatomy* in the Hôtel Dieu, have been communicated to me by M. Rousseau, charged with the editing of this collection.

A patient named Florat, about sixty years of age, died on the 19th of March 1818, of a cyst, with inflammation and suppuration of the prostate gland. The left testicle

\* In opposition to what we have said in vol. i.



was a little less in volume than the right; the left epididymis was swelled, hardened, and contained a little pus; the right lobe of the cerebellum was less than the left.

A woman died of acute peritonitis, which had been preceded by dropsy in the right ovary, and afterwards by abdominal dropsy. Punctures had been repeatedly made. In the post-mortem examination, the ovary was found converted into three large cysts containing serous matter; the left lobe of the cerebellum was perceptibly wasted.

In another woman, who died on the 11th November 1818, there was found, on dissection, in the right ovary, a cyst containing about a tea-spoonful of limpid serosity; the left ovary was sound. Nothing remarkable appeared in the brain, but the left lobe of the cerebellum was about a third less than the right.

A young man, of twenty-one years of age, tall, of an arid constitution (constitution sèche), entered the Hospital of La Charité, in the first half of the month of August 1822. He had a dull air, scarcely spoke, and complained of a deep-seated and continued pain in the posterior part of the head, on the right side. He had prominent eyes, and the skin of the face, as well as that of the body, was sallow, dry, and earthy in its appearance. He remained on his back in bed, and moved himself with difficulty, although he was not paralytic either in feeling or motion, but sensibility was very dull in him. From the time when he became sick he had had no erection. The right testicle was of an ordinary size. The left side of the scrotum was more voluminous. There was a *hydrocele*, which rendered it impossible to appreciate the size of the testicle on this side.

This patient died on the 6th, and was opened on the 7th of September. The right lobe of the cerebellum was found to be larger than the other, and a well marked prominence was visible on its superior surface. This prominence was cut down upon from the side, and a reddish tumour was found, fleshy in its appearance, in the middle of the medullary substance, which presented the following changes.



It was larger than that on the opposite side; its colour was of a clear yellow; it had a considerable degree of consistence, for a layer of some length was lifted without breaking. The same could not be done with that of the left lobe. I have said a layer, because the portion of medullary substance which immediately surrounded the tumour had the appearance of a membrane half a line in thickness. At the first, I believed that it was a cyst, but almost every where it was confounded with the surrounding medullary substance; in some points only could it be separated. His penis was considerable in length, wrinkled, and the glans uncovered.

The right testicle, as I have said, was of the ordinary size. On the left side, there was a hydrocele of middling extent, and the testicle of this side was softer, and a little smaller than the other.

These observations confirm the opinion, that, in the state of health, also, the cerebellum should diminish in mammiferous animals and birds, every time when, after the season of heat, the testicles diminish.

*Influence of Injuries of the Cerebellum on the Genital Organs.*

Hippocrates says of the Scythians, that, "when they were sick, they opened a vein behind each ear; when the blood had flowed from them, they felt themselves overcome by feebleness, and went to sleep. On waking, some found themselves cured; but this was not the case with all of them. For my own part, I consider that this method of cure was very hurtful to them; because, behind the ears, there are veins, the section of which produces impotency; and it is precisely in those, I believe, that they bleed themselves, for, when they approached their wives, they found themselves incapable of executing the act of copulation." \*

\* *Hipp. de Aëre, Locis, et Aquis*, No. 20. Edit. a Foës.



Aleméon also regarded the seminal fluid as a constituent portion of the brain. This opinion appears to have been pretty generally adopted in ancient times.

In the "*Memoires de Brantome*," containing the lives of the illustrious men and great captains of his age,\* we find the following passage :—" M. de Burie died without descendants, and never had any. His wife, who was *naïve* and free, said, that it was no fault of his or hers, for they had both done their duty to produce them ; but that, formerly, her husband had received in the wars a blow with a sword, or the but-end of a musket, on the nape of the neck, which prevented him from sending home the semen, so much so, that the real stuff could neither pass nor flow into her womb, but only a little clear watery fluid, which was not sufficient for impregnation or conception."

At Vienna, I was consulted by two officers, who had become impotent in consequence of blows from fire-arms, which had grazed the napes of their necks. One of them recovered by degrees the generative faculty, married, and was the father of several children. At this time, I had not directed my attention to the changes which the nape of the neck itself undergoes after such injuries.

At Berlin, Dr Formey spoke to us of a man who, in consequence of a wound in the nape of the neck, had at first irregular erections, after which he fell into a state of complete impotency. He recovered his virility, however, at the end of six months. In this case, the inflammation caused by the wound had first produced irritation in the cerebellum ; this irritation was followed by feebleness, as all inflammations and over-irritations generally are ; whence arose, at first impotency, then return of strength, and with it return of virility.

A coachman, who previously had been a hero in the fields of love, struck the nape of his neck against a joist. After this accident, his wife complained to me that the powers of

\* Edition in 12mo, London, 1739, Tom. ii. p. 182.



her husband diminished daily, and that he had at last become completely incapable. Baron Larrey has found the remark which Hippocrates made on the Scythians confirmed by the Egyptians. They frequently apply cupping glasses in the nape, and numerous observations have convinced the Baron that these bleedings, often repeated, considerably impair the power of the soldiers in reference to the act of copulation.

This *savant* has had the kindness to communicate to me the following facts, relative to the influence of lesions of the nape of the neck on the instinct of propagation.

Before his departure for Spain, he enabled me to see a soldier of the Imperial Guard, paid off on account of a general feebleness in all his organs, particularly marked in the non-action of his generative parts. This subject had been wounded in the nape of the neck, by a fragment, at the taking of Alexandria, at the descent of the French army into Egypt. He was then eighteen years of age. After a succession of serious accidents, which accompanied this wound, and for which he remained in the hospital for the space of three or four months, his genital organs fell into a state of decay, and he lost, along with their physical functions, the power of erection in the penis, and the desire to see women. This young man has remained without a beard, thin, discoloured, weak, languishing, and speaking like a woman. He had arrived at the age of thirty-two, when he was paid off, and he did not appear to be more than eighteen.

Another Imperial Guardsman, of the corps *des chasseurs à cheval*, presented himself to Baron Larrey, with a cicatrice which cut transversely the occipital swelling, the consequence of a blow with a sabre received at the battle of Wagram. Before this event, the *chasseur* had frequented women as much as his comrades did; after it, he was deprived not only of the power of erection, but he lost all desire.

*Auguste François, Maréchal-des-logis*, of the horse-artillery of the Guard, received, at the battle of Benevento, a blow from a musket-ball, which traversed, from the one side to



the other, the attachments of the extensor muscles of the head, grazing the inferior occipital swellings, which were very prominent in this individual, and which were denuded of the aponeurotic attachments. Baron Larrey divided the two openings made by the ball, and extracted a portion of the patient's shirt, which had remained in the course of the wound, which was dressed with emollients. The patient experienced at first severe pains in the occiput, heaviness, and a numbness in the lower extremities. Sight and hearing diminished to such a degree that he could scarcely distinguish large objects and hear the sharpest sounds. The testicles diminished, and fell into a state of atrophy; the penis also withered, and remained without action. In the mean time, the wounds cleansed themselves; the local concomitants disappeared, and the patient found himself healed before the fiftieth day.

*René Bigot, chasseur à cheval*, of a strong constitution, and passionately addicted to women, received, at the same battle of Benevento, a blow from a sabre, which cut the skin and all the convex or projecting portion of the occipital bone, down to the *dura mater*, a part of which also had been cut. The surgeons saw the right lobe of the cerebellum through the opening of the *dura mater*. The slightest touches on this organ caused vertigo, syncopes, and convulsive movements, without being accompanied by the slightest indications of pain. Baron Larrey detached the osseous portion of the occipital bone, which had continued to adhere to the flap, which he applied gently over the excavation of the cranium, having taken care to make an incision at the base of this flap to favour the escape of fluids. The portion corresponding to the opening of the *dura mater* did not adhere, on account of an oozing which never ceased to take place from the interior of the cranium, where, otherwise, there was no effusion. These fluids escaped, at every dressing, by little bubbles, which produced a slight hissing, attributed to the external air, which entered and came out by the same opening.



After a few days, the patient lost the senses of sight and hearing on the right side. He experienced, at the same time, acute pains along the course of the dorsal spine, and a sort of tingling in the testicles ; which diminished perceptibly, and were reduced, particularly on the left side, to the size of a field-bean, in less than fifteen days. Immediately afterwards, he lost the recollection of the pleasures which he had enjoyed with a great number of women.

He supported the journey from Benevente to Valladolid very well. Farther, the wound was in a favourable condition, and, except that the functions of hearing, seeing, and generation, seemed obliterated for ever, it gave hopes of healing, when symptoms of inflammation appeared and increased, in spite of the means which Baron Larrey used to prevent them. The pains in the head and spine caused the patient to utter distressing cries. He lay constantly in bed on the right side. The least motion produced convulsions, and when he raised himself up to take soup or medicine, he fell down in fearful fainting fits.

Baron Larrey applied a blister to the head ; he prescribed cooling drinks, and every other suitable remedy : but the disease increased more and more, and the patient died, in a state of tetanus, on the 7th of February 1809, thirty-eight days after the accident, which happened on the 29th December 1808.

The opening of the body revealed the following appearances. There was a great loss of substance in the occiput. The opening already mentioned in that part of the *dura mater* corresponding to the right lobe of the cerebellum, which was sunk or flat, was of a yellowish colour, without supuration or effusion ; the medulla oblongata and spinal marrow were of a dirty white, of a firmer consistence than in the natural state, and reduced one-fourth part in volume. The nerves which proceeded from them appeared equally wasted.

Batiste Vallet, forty years of age, of an athletic constitution, a soldier in the 5th Regiment of Infantry of the Guard,



entered the Hospital du Gros-Caillou on the 28th of August 1821, affected with complete hemiplegia of the whole of the right side of the body. The superior extremity was destitute of the powers of motion and sensation. The inferior was still in some degree serviceable in walking, although in a very miserable manner. The cause of this hemiplegia was discovered to be a fall from a height of five feet, about a month previously, on the left lateral and anterior portions of the head. The headach, the first consequence of the fall, had assumed, in a few days, such a degree of intensity, that Vallet had been forced to enter the hospital at that time, and had been placed in the fever ward. Foot-baths, bleeding, six leeches on the temples, a blister to the right arm, had been the means of cure resorted to. Moreover, the patient had gone out fifteen days after his admission, in the hope that walking, exercise, and a purer air might be favourable to him. This hope having been deceived—for, to the hemiplegia, which had become more and more decided, there was added a remarkable disturbance and a weakness in the vision of the right eye—paralysis of the tongue began to appear, and a considerable difficulty in pronunciation. Vallet then returned, as we have mentioned, on the 28th of August, and was placed in the ward for the wounded.

Baron Larrey, after an attentive examination of this subject, recognised an unnatural protuberance in the left lateral and superior region of the skull. Some questions put to the patient, relative to the causes of these phenomena, led Vallet to speak of his fall. From that time Baron Larrey recognised easily that all the symptoms which the patient had presented, and still presented, were owing to the concussion of the brain and the alteration of the bones of the skull, of the meninges, and without doubt of the whole encephalic mass. He resolved, accordingly, to attack the evil at its source. After having depleted the cerebral vessels by means of a bleeding at the jugular vein, he shaved the whole head, and applied a large blister on the whole left side of it. Four days afterwards, Vallet began to walk



better, the fingers had recovered some of their movements, the fore-arm began to bend on the arm. A second and a third blister applied after an interval of eight days, helped on this sensible amendment; some calomel pills, a portion of valerian for the night, frictions with camphorated camomile oil on the paralyzed limbs, were the other means employed. On the 17th of September, a first moxa was applied on the lateral and superior parts of the neck; and some days afterwards two issues were established by means of potash, one on each side of the base of the skull, between the mastoid processes and the superior occipital swellings. The movements of the extremities were then half re-established; the patient could rise, walk easily, and take all the exercise necessary to maintain this improvement. His speech still continued a little embarrassed; the words in which the letter *r* occurred were those which he pronounced with greatest success, and which he chose, as it were, instinctively. We had previously observed this selection of words, and this greater facility in pronouncing them in several other patients labouring under cerebral affections of this nature. Several other moxas, to the number of ten or twelve, were applied successively, and at suitable distances of time, on the line of the branches of the facial nerve, and of the cervical pair which go to form the right brachial plexus, to complete the cure. It took place, indeed, towards the beginning of November, but it was not yet sufficiently established to remove all apprehensions of the return of the hemiplegia. Nevertheless, Vallet, who was of an obstinate disposition, and in whom, besides, the intellectual faculties were little developed, satisfied with his present condition, was unwilling to endure the application of more moxas, and gave himself up to intemperance. Some days afterwards he was punished for his headstrong folly, and for his inattention to the regimen prescribed to him. The paralysis returned, accompanied by embarrassment of the gastric functions. His head was shaved of new, and a large blister was applied; a potion with castor-oil was ad-



ministered ; a scruple of the acetate of ammonia was added in an antispasmodic potion ; new moxas were applied to the base of the skull, on the side opposite to that of the paralysis, and on the cervical pair of nerves of the same side. This relapse was successfully met by these means ; and strength having completely returned in the right extremities, the patient was finally discharged on the 19th of October, perfectly cured.

Four months afterwards, we learned that Vallet, after having been again for some time in the hospital, in the fever ward, for a catarrhal affection, with headach and general prostration of strength, had died there on the 7th of February. The next day the body was opened.

The skull having been sawed circularly, and lifted off with precaution, we observed that the left side of this arch was not so deep as the right. The surface of the *dura mater*, lightly injected, and of a yellowish tint, presented the traces of a chronic inflammation. The arachnoid coat was sound. Being lifted up by means of a pipe, it was clearly seen that this membrane was in no degree inflamed ; a fact which could not have been believed without this precaution, for at the first view a very pretty injection was perceptible, a very decided inflammation, but both of which were situated in the *pia mater*, and not on the arachnoid membrane, which was, indeed, scarcely perceptible from the red tint shining through its transparencies. All the principal vessels were distended and gorged with black blood. In lifting up the brain from the base of the cranium, the left optic nerve was seen to be larger and denser than the right ; it was also of a reddish colour. A *steatomatous* tumour, in a state of suppuration, surrounded the basilar artery at its bifurcation, and rendered its diameter less. This tubercle had even contracted a point of adhesion to the *pons Varolii*. All the medullary substance of the brain, especially the left hemisphere, was gorged with blood, and of a more firm consistence, and a greyer appearance than natural. The right hemisphere approached more to this state by its softness



and whiteness. Even in the annular protuberance, the left side was also denser than the right, and it did not yield but with difficulty to strong degrees of pressure. The convolutions of the brain also, were less deep and narrower on the left side.

When the examination of the brain was completed, and attention was directed to the base of the skull, we were struck with the extreme difference which the middle *fossæ* presented. The right was nearly one-half larger than the left. The differences in the *anterior fossæ* were less remarkable. As to the *posterior fossæ*, those which contain the cerebellum, their smallness and their want of depth excited astonishment. They led us to observe the cerebellum and the genital organs, to see what relations existed between them. The two lobes of the cerebellum, of an equal form, were in fact one-half less in volume than what is generally found in a subject of the same size, and the testicles, reduced to the dimensions of a kidney-bean, possessed scarcely any consistence. The penis was only six lines in length. (The head of Vallet, which is at present in the course of preparation, will be transmitted to Dr Gall.)

The thoracic cavity also presented interesting phenomena, and analogous to those of the bones of the skull. The right cavity was much less than the left. The lung on this side was very small, adhering on the two sides, and in a state of recent inflammation. The liver, very voluminous, mounted up into this cavity, and must have contributed to compress the lung on this side. The left lung, on the other hand, was very ample, as well as the cavity which contained it. The heart was of an ordinary size; the auricles were gorged with blood. The abdomen presented no appearances of alteration.

Baron Larrey ascribed the osseous reduction which we have remarked, to the energetic action of the revulsive topical remedies applied, and the employment of which was continued as long as possible,—seeing that, at the time of Vallet's entry to the hospital, this left portion of the cranium



presented a manifest prominence. Besides, this statement has been discussed and proved by this accomplished practitioner in similar circumstances, and in other cerebral affections,—epilepsy, for example.

Dr Thouvenelle communicated to me a case of a similar injury, and which had the same results.

I again remark, that in Bigot it was chiefly the left testicle which wasted away so much as to be reduced to the dimensions of a field-bean, while it was the left hemisphere of the cerebellum which had been wounded. Baron Larrey shewed me similar examples in the Hospital of the Guards, and gave me two skulls in which the marks of sabre cuts on the occipital fossæ proved the injury of the cerebellum. In all these subjects, there had been wasting and softening of the testicles.

I shall report another very remarkable example, although in this instance I cannot decide whether the testicles or the cerebellum suffered first. A boy of thirteen years of age had given himself up for some time to onanism with so much determination, that all the means used to correct him were unsuccessful. At last he was attacked by an obstinate incontinence of urine and vomiting. From the commencement of his attack, he crawled along with difficulty, and at the end of a few months he was paralyzed in the lower extremities. His pupil lost the power of contraction; for a considerable time, however, he saw a little towards the internal angle of the eye, but in the end his sight entirely disappeared. Convulsions came on about the eyes and mouth. The paralysis then became complete. His lungs even ceased to act, and he died of suffocation. On dissection, I found more than a pound of limpid water in the cerebral cavities. Both lobes of the cerebellum were filled with pus in the interior; however, the suppuration had made much greater ravages in the left lobe than in the right. The commissure of the cerebellum (*pons Varolii*) was singularly diminished by decay (*atrophie*), and of a yellowish colour. Both testicles were very small; the one on the right side had almost entirely



disappeared, and was of a very soft consistence. This, then, is another coincidence between lesions of the two opposite sides.

There existed in the family of this young man a hereditary predisposition to water in the brain. I am persuaded that the water found in the cerebral cavities was not connected with the onanism, except in so far as its presence had increased the irritability and the sensibility of the whole nervous system.

The suppuration of the cerebellum must also have been long established, for the pus had more the appearance of a putridity than of pus of good quality. It is then a question to be resolved, whether it was not the diseased condition of the cerebellum which led this young man into the practice of onanism, who in other respects was rational in his conduct and very well brought up. Or must we admit, on the contrary, that it was onanism which caused the disease of the cerebellum? Whichever it was, the case proves the reciprocal influence which the organs of generation and the cerebellum exercise over each other.

From all that has been said, it follows that diseases and lesions of the cerebellum have an influence on the genital organs, as well established as is the influence of diseases and injuries of these organs on the cerebellum. It appears even that the life of the cerebellum is more independent than that of the genital organs.

*Influence of Diseases of the Cerebellum on the Genital Organs, and especially on the Instinct of Propagation.  
Erotic Mania.*

I shall treat of this subject in a twofold point of view. It is my intention, 1st, To state some additional proofs in favour of the immediate connection which exists between the cerebellum and the sexual organs, as well as the instinct of



propagation ; and, 2dly, To expound the true cause and origin of erotic mania.

There are certain diseases of the genital organs which, although they occasion a violent irritation in these organs, exercise no influence on the instinct of propagation. The most violent inflammations of these parts, arising from external causes, are never accompanied either by mania or by amorous desires. When an irritating eruption occurs in these organs, either in man or woman, it produces in them, indeed, a burning and painful heat, and an unsupportable itching, but it never excites desires. I have seen, for example, in diseases of the abdomen, of the liver, of the kidneys, the patients suffer, for weeks together, violent erections, which, far from exciting the least sexual desire, tormented them.

M. Foderé relates that, in consequence of the sting of an insect, a man had violent erections without the least voluptuous sensation.\*

None of these observations can be new to experienced physicians. How, then, does it happen that almost all of them maintain, with Cabanis, that the sexual organs are frequently the seat of mental alienation ? †

A young man, of a robust and plethoric constitution, had arrived in Vienna, where, from want of opportunities, he had lived for some time in a state of greater continence than usual, and he fell suddenly into an erotic mania. He had long-continued erections, and his testicles were swelled and painful. What could be more natural, according to the received ideas in medicine, than to seek for the cause of his malady in the inflammation of the external organs. Accordingly, his physicians employed every means to subdue the local inflammation, and to remove the over-irritation of these organs. But, notwithstanding all these means, the patient continued in the same state during three weeks. When I was called in, I directed the attention of my col-

\* Sur le Délire, T. i. p. 316.

† Sur le Rapport du Physique et du Moral, etc. T. i. p. 107.



leagues to the inflammation of the brain, and especially of the cerebellum. We concerted our plan of cure in reference to this idea ; and in a few days the inflammation and swelling of the sexual organs, and also the mania, had disappeared.

It may be admitted, without any hesitation, that when a diseased condition of the generative organs is accompanied by mania, these parts are disordered only secondarily, or by sympathy, and that the seat of the malady is in the brain ; or, if the mania be erotic, in the cerebellum. Before proving this assertion by a faithful picture of erotic mania, I shall make some observations on the origin of this disease.

A young man, well educated and full of talent, who, from his infancy, felt himself strongly drawn towards erotic ideas, overcame them to a certain extent by means of his tendency to devotion, which was equally strong. When his social relations would have permitted him to give himself up without constraint to the pleasures of love, he was not long in perceiving, with a feeling of alarm, that often it became extremely difficult for him to avert his attention from the voluptuous images which haunted him, and to direct his mind to the pressing and important duties of his situation. His whole being was absorbed by sensuality. Not to become a victim completely to these feelings, he was under the necessity of occupying himself assiduously with scientific pursuits, or of finding out for himself some agreeable new occupation. His cerebellum was of an extraordinary size.

A very talented lady was equally tormented from her infancy by the most irregular desires. A very careful education which she had received, was alone capable of saving her from the most inconsiderate conduct, to which she was impelled by the violence of her temperament. When, at a later age, she found herself abandoned to her own propensities, she tried every means to gratify her burning desires, but gratification appeared only to excite them. Often she saw herself on the point of falling into mania. Reduced to despair, she abandoned her home, left the town, and took



refuge with her mother in an isolated country house, where want of objects, the strictest severity of manners, and the cares of a garden, prevented the explosion of the evil. After having again lived for some time in a large town, she felt herself threatened with a relapse, and she took refuge a second time with her mother. At her return, she came to me in Paris, and complained that she was a woman driven to despair. Every where, said she, I see only the most lubricious images; the demon of luxury pursues me every where, without ceasing, at table, even in my sleep; I am disgusted with myself,—yes, I feel that I can no longer escape from either insanity or death.

I stated to her, shortly, the natural history of the instinct of reproduction, and called her attention to the shape of the nape of her neck. Although her head is very large, the diameter of the nape of her neck exceeds the distance from the one ear to the other. She understood the cause of her condition. I advised her to continue her journey to join her mother,—to vary her occupations, in order to diminish the activity of her cerebellum,—frequently to apply leeches to the nape, to moderate the state of irritation of this part,—to avoid heating food and exciting liquors, &c.

A man had lived for several years in a well-assorted marriage, of which several children were born, and he had acquired by his activity a handsome fortune. When he had retired from business, and led an idle life, his innate predominating propensity gained, by insensible degrees, the ascendancy. He abandoned himself to such an extent to his desires, that, while yet in possession of reason, he looked on every woman as a victim destined to satisfy his sensuality. Whenever he saw from the window any woman, of whatever condition, he announced, with all haste, and with accents of joy, to his wife and daughters, the happiness which awaited him. At last, this partial insanity degenerated into general mania, and a short time afterwards he died in the Hospital for the Insane at Vienna. His skull proves that his



cerebellum had attained to very considerable dimensions. Pl. xl. 1, 1, 1, 1.

M. Pinel reports a similar example: "A man," says he, "had filled with praise a public situation till his 50th year. An immoderate desire for sexual pleasures then appeared in him; his look is vivid and animated; he frequents the haunts of debauchery, delivers himself up to every excess, and returns again into the society of his friends, and paints to them the charms of a pure and spotless love. His aberration increases by degrees; it becomes necessary to shut him up. Solitude exalts his heated imagination; he paints in colours of fire the pleasures which he had tasted with those whom he styled celestial beauties; he falls into ecstacy in speaking of their graces and their virtues; he wishes to erect a temple to love, and believes himself raised to the rank of the gods. These were the preludes of a violent fury, accompanied by delirium."\*

We see by these examples, that persons whose cerebellum has acquired an extraordinary development, have a natural predisposition towards erotic mania; but these cases also shew, that the extreme activity of this organ does not really produce it, except when the individuals in whom this disposition exists give themselves up exclusively to the enjoyments of physical love. They prove, moreover, that frequent enjoyment is not a remedy for this species of insanity.

In the hospitals, we have constantly found the cerebellum to be very largely developed in all the subjects who were attacked by erotic mania, and in all those who, being attacked by a general mania, gave themselves up irresistibly to onanism.

M. Esquirol shewed us the cast of a woman who had been attacked by erotic mania. The very prominent occipital swellings shewed a cerebellum developed in an extraordinary degree.

\* De l'Aliénation Mentale, deuxième édition, p. 15, 16, § 13.



The same rule, however, holds good in regard to this organ as in regard to all other parts. Not only an excessive activity, arising from original constitution, may degenerate into mania,—that is, into such a vivid activity that it is no longer subject to the direction of the will; but other causes also may exalt the action of the cerebellum to that point at which it becomes erotic mania, even in individuals who, according to the ordinary course of nature, were not predisposed to it.

Do we not see, in acute diseases, individuals who never previously shewed any tendency to become insane versifiers or quarrellers, compose verses and seek disputes with all the world? These cases, it is true, are very rare, and the history of the previous life of the patient, joined to the conformation of cerebellum, will always afford an explanation of the phenomenon, and enable the physician to determine whether, in his treatment for a cure, to pay most attention to the accidental causes or to the natural dispositions; to general or to partial derangements.

I present the following picture which M. Pinel draws of erotic mania, as well because it confirms what I have hitherto said regarding this disease, as because M. Pinel himself, while he appears to attribute this species of insanity only to a kind of effervescence which takes place in the sexual organs, is constrained to revert to a particular disposition.

“ It is in either sex a physical effervescence of the generative organs, with the most lascivious gestures, and the most obscene speech. It is so much related to the internal disposition that it lasts only during the disease. I have seen persons the most commendable for the purity of their morals, experience, during a determinate period of their maniacal condition, this unfortunate desire for debauched women, and then return, after their convalescence, to their primitive character of reserve and of extreme decency. I have seen this affection develope itself in extreme cases in the following manner: First, There was senseless gaiety, an animated



look, voluptuous research in the toilette, unquiet curiosity, trembling in the hands, dull pains in the womb, burning heat in the interior of the bosom, extreme mobility in the eyes, impatience: the attack is then at its height; unmeaning language mingled with salacious expressions and obscene remarks, vociferations, lascivious gestures and movements of the body, all the unbridled impetuosity and the illusions of an erotic delirium appear. This impetuous passion gives way to a necessary depression, and a melancholy repose succeeds, or rather a state of lassitude; emaciation is then extreme, and this internal fury brings after it exhaustion, and *dementia*. *L'embonpoint* is re-established by degrees. The disease becomes sometimes periodical, and life is passed in alternations of erotic derangement, and of the most stupid apathy.”\*

The description which we have just read, leads us naturally to that state of irritation of the cerebellum which produces the diseases known under the name of satyriasis, of priapism, and of nymphomania. However different the disordered ideas and sentiments which accompany these diseases may be, they all proceed from, or revolve round the instinct of propagation; and consequently this species of mental alienation should be arranged in the class of erotic mania. As the source of this disease has been sought for exclusively in the sexual organs, no other remedy except castration has been in general pointed out. By this mutilation, the activity of the cerebellum is weakened; and on this account the evil sometimes abates after the operation. But assuredly, this means has not always a useful effect; for in the great public hospitals for the insane, as Cabanis has already observed, we frequently see miserable creatures pull away their own testicles, without any effect on the state of the brain following the act. I am quite willing that, for want of a better curative process, we continue, in these cases, to cut stallions; but when, in our own species, there is still

\* De l'Aliénation Mentale, deuxième édition, p. 67, § 78.



a possibility of a cure, it may certainly be accomplished with less inconvenience by a method of treatment in which attention shall be directed solely to the condition of the brain, and above all to that of the cerebellum.

A great deal is said about a species of mania produced either by too great continence, or by excessive emissions of the seminal fluid.

Too severe a continence, supposing it really to exist, may undoubtedly occasion inflammatory diseases, and especially an inflammation and over-irritation of the cerebellum and of the whole brain, and in consequence, produce acute erotic mania, or a general mania.

Cabanis imputes the evils which result from too great a continence, sometimes to the organs of generation, sometimes to an unhealthy state of the brain. If he had known the reciprocal influence of the cerebellum on the organs of generation, and of these on the cerebellum and the brain, he would not certainly have hesitated between these two opinions. The extraordinary condition of the intellect, the singular affections and propensities, and the *bizareries* which occur in mobile and delicate young girls and young men, when their new desires are too long restrained, should have directed him to the truth. And when he remarks that nothing is more common than to see women, in the access of hysteria, acquire an elevation of ideas, a degree of penetration and of eloquence, which did not naturally belong to them, should he not, as a good logician, have decided for the brain?

He even cites from Buffon the celebrated history of a curate, who, in consequence of a rigorous chastity, had fallen into a hysterical delirium approaching to mania. During the whole continuance of the delirium, the patient displayed sundry talents which had not been cultivated in him. He made verses and music, and, what is still more remarkable, he drew with great correctness and truth, the objects which presented themselves to his eyes, without having ever previously touched a pencil. Nature cured him by very simple



means. But although he continued to be a man of intellect, he saw disappear with his disease, a great portion of the wonderful powers which it had called forth into action. Who will venture to ascribe to the genital organs, poetry, music, and the art of design?

The maniac mentioned by Mons. Pinel appears to have been in the same situation. "Sometimes," says he, "an opposite excess, that is to say, propensities strongly excited and not satisfied, may also produce a complete derangement of reason. A tender melancholy, and vague inquietude, the object of which was neither concealed nor misunderstood, were observable in a young woman of twenty-one years of age, endowed with a strong constitution and a lively sensibility. Every thing concurred to excite her imagination: constant reading of the most amorous romances, a kind of passion for all the productions of art in the erotic department; habitual association with young people of both sexes, of whom the males delighted her by their personal attractions and all the seductions of gallantry, and the females by dangerous examples and indiscreet confidences. The most refined coquetry was then elevated into a principle, and became a serious occupation. Her pride, flattered by the smallest assiduities, led her to regard them as certain triumphs, which she never ceased to speak of, or to make the subjects of her internal reveries, until a new adventure superseded the first. A *faux-pas* appeared inevitable, or at least very much to be feared, and her parents hastened to conclude a marriage founded upon some considerations of convenience. The husband selected was of a mature age; and in spite of the advantages of his stature and of a strong complexion, was less fitted to satisfy than to excite her desires. The melancholy of the young lady degenerated into a sombre jealousy; and she ascribed to infidelities what was only the effect of debility in the organs. A kind of wasting succeeded; her features altered, an interminable babble ensued, with the most complete disorder of ideas, the prelude, or rather the manifest sign, of an established mania." \*

\* Sur l'Aliénation mentale, deuxième édition, p. 47 et 48, § 53.



It is, however, I believe, very rare that continence is carried so far as to produce such effects. Nature has so many means of diminishing too great an abundance of the seminal fluid, even without the assistance of the individual, that this species of derangement is very seldom to be feared. Believe in Him who tries the reins and the heart—" *It is not good for man to be alone.*" Often an extreme continence is hinted at as the cause of a complaint, when its true source should be sought only in secret debauch. A bishop who edified his diocese by an exemplary life, fell, towards his sixtieth year, into a state of melancholy, accompanied by manifest weakness of intellect. Every one deplored the unmerited misfortune of so holy a man. He placed his confidence in me. I often went out with him, sometimes walking, and sometimes in a carriage. Every time he saw a young woman, he sighed deeply; every time when he saw a happy couple, he pressed my hand with warmth, and cried, "How great is their felicity!" I anticipated the confessions which he might probably have to make to me; I spoke to him, in the tone of friendship, of the happiness which spouses enjoy in a well-assorted marriage, and of the intention of the Creator, inscribed on all his works. The veil dropt, and exposed the *man* to view; the pious bishop acknowledged to me that he was one of the number of those who sin seven times a day.

Acquainted as I am with human weakness, I am much more disposed to attribute erotic mania to excess of indulgence, than to too severe a continence. Excess produces such an irritability and excitability of the cerebellum, that it is no longer in the power of the individual to arrest the torrent of lubricious ideas and voluptuous images which come pouring upon him. But as the primitive cause acts with a greater degree of violence in proportion as the other faculties are feeble, this kind of mania degenerates speedily into *dementia*, and into general weakness of the whole body. Here, again, I cite a case reported by Mons. Pinel in support of my assertion.



“ A young man,” says he, “ of a strong constitution, the son of a rich father, had attained his full growth towards the eighteenth year of his age; and it was at this period of extreme effervescence in his senses that he began to give himself up to his propensities with all the impetuosity of an ardent temperament, fostered by the facilities which a daily assemblage of female workers in a great manufactory afforded. He then addicted himself to pleasure without restraint and without measure, frequently at various hours of the day and of the night. At the age of twenty, he added other excesses, not less destructive, those of intemperance, and frequent visits to places of debauchery. Venereal diseases, alternately cured and contracted anew, were joined to exhaustion, and combined with other affections of the skin. Commercial pursuits then rendered it necessary for him to undertake frequent journeys in a post-chaise, by day, by night, and at all seasons of the year. Courses of mercury were alternately begun, suspended, and renewed, without order and without rule. From that time the most marked symptoms presented themselves of a profound hypochondria: laborious and imperfect digestion; disagreeable flatulence; acid eructations; alternate costiveness and relaxation of the bowels; severe periodical pains from the colic; alarms without cause; extreme pusillanimity; disgust of life; and several attempts at suicide. A blind and puerile credulity in the virtue of medicaments, and an entire confidence in every species of empiricism, were joined, at twenty-five years of age, to a complete incapacity for the pleasures which he had abused, and to a constantly increasing decay of reason.” \*

All the examples hitherto adduced prove that no kind of erotic mania can have its seat in the genital organs themselves; and that we must look for the cause of this alienation in the same place where that of all other mental derangements is found. And as it is the cerebellum which is the

\* Sur l'Aliénation mentale, p. 46 et 47, § 57.



organ of the instinct of reproduction, it is on it that the over-excitements and derangements of this propensity must depend.

I shall subsequently explain, in treating of the other fundamental faculties, how it happens that erotic mania is accompanied sometimes by devotional feelings, sometimes by pride, sometimes by some other sentiment in a state of exaltation.\* Here I continue to add proofs of my assertion, that the cerebellum is the organ of the instinct of propagation, and I find new examples in the different manners in which it manifests itself in the state of idiocy.

*Observations on the Activity or Inactivity of the Instinct of Propagation in Idiocy.*

The prejudice is generally entertained, that idiots, imbecile persons, and cretins, are very lascivious, and a prey to all the effects of a lubricious temperament. Assuming the case to be so, I ask, whether the genital organs of these poor creatures have a particular conformation? If they have reached to a more exuberant development, or if they secrete a more exciting seminal fluid? If they are able to fill their imaginations with more vivid lubricious images? None of these assertions can be maintained.

Besides, it is far from being the case that the instinct of propagation is manifested in a state of great activity in all idiots and in all cretins. I have examined a great number of these individuals, and the following is the result of my researches.

Whether the genital organs be large or small, they never exercise a determinate influence on this instinct.

The propensity remains inactive in all cases in which the cerebellum has attained only to a small degree of development. The savage of Aveyron, who resides in the Deaf

\* This explanation is given in Dr Gall's work "Sur les fonctions du cerveau," of which this treatise on the cerebellum forms only one section.



and Dumb Institution at Paris, at sixteen years of age, had not manifested the least inclination for women; and his cerebellum was very feebly developed. At Salzburg, Professor Hartenkeil shewed me a cretin above twenty years of age, who had never manifested, in any way, the instinct of propagation, although he was well made, and enjoyed perfect health. In him, also, the cerebellum was very small. In another ward, on the contrary, the same professor shewed me a woman so very ill-formed, that, instead of walking, she trailed herself on the ground. This unhappy being experienced a kind of rapture every time she saw a man. Scarcely had I approached her, when she crawled into her bed, and invited me, by the most lascivious gestures, to follow her there; she even threw off all her clothes to give me a more cordial reception. Her intellectual faculties are considerably inferior to those of the lower animals, but her cerebellum is very much developed. Accordingly, all her movements, even in solitude, are directed to satisfying her lubricity. I could relate a great number of similar cases, all of which confirm my opinion; but I shall limit myself to the recital of a few, less in order to support my doctrine, than to offer a moral observation to the reader.

At Munich, we saw a boy of fifteen years, who, from his seventh year, had desired to abuse his sister, and had attempted, but unsuccessfully, to strangle her, because she resisted his propensity. His idiocy was not complete. He spoke a little, recognised persons, and felt pleasure, like a dog, in looking at the passengers through a window. His cerebellum was excessively developed, and it was necessary to keep girls and women strictly removed from him. At Paris, M. Savary, then Minister of Police, and M. de Bourienne, took me to see a boy, about sixteen years of age, who absolutely refused to learn any thing, and whose society became extremely pernicious to his fellow scholars, not only on account of his want of capacity for instruction, but also on account of unnatural desires. I drew the attention of these gentlemen to the small development of his forehead,



which explained the invincible indifference which he testified for every kind of instruction. I led them to observe, at the same time, the great prominence of his occipital swellings, and his large and thick nape of the neck, which accounted for his unbridled desires. The idiocy of this subject was still less complete than that of the young man of Munich, of whom I have just spoken. This leads me to offer another observation.

In several hospitals for the insane, and in some houses of correction, we have met with subjects who were said to have become insane in consequence of excessively frequent emissions of the seminal fluid, or who were devoted to punishment for having given themselves up to onanism.

I am very far from denying the pernicious influence which onanism exercises on the manifestation of the intellectual faculties, and several passages of my writings sufficiently prove this fact. But in this case, another cause is generally to blame. Nature had bestowed defective intellectual faculties on all the subjects of this description whom I have had occasion to observe. In them, the anterior part of the skull was narrow and very little elevated; or else they were more or less hydrocephalic. The posterior parts of the head, on the other hand, their nape and cerebellum, had acquired a degree of development out of all just proportion to that of those parts of the brain which manifest the intellectual powers. A human being thus organised is in the condition of a lascivious animal; he is an ape in heat. The organ of the instinct of propagation governs him imperiously, because there is no other organ to balance its activity. Neither decency, morals, nor religion, can act on such an individual; punishment cannot frighten him,—in short, no motive can be found capable of conferring self-constraint on a being debased below the brutes, and who has no will. The philosophical observer here recognises the feebleness of the understanding, as the cause why the individual is abandoned to a brutal sensuality, while the vulgar erroneously regard it as the consequence of that abandonment.



*The Instinct of Propagation survives the destruction of the Genital Organs, and exists in the absence of these parts.*

A married woman, who died of a cancer of the womb, had acted as a prostitute during her long malady. A short time before her death, she insisted on the approaches of her husband. A lady was the victim for more than two years of an extremely painful disease; she had become a perfect skeleton; abundant and fetid discharges, severe dysury, &c. announced her approaching dissolution. Notwithstanding of all this, and although her condition rendered all matrimonial approaches absolutely impossible, she desired, with a voluptuous ardour, the embraces of a husband whom she loved. After her death, which happened a few days afterwards, I found the vagina, the womb, and the bladder, partly dissolved in an ichorous and fetid liquid, and partly so much eaten away, that the membranes and fibres tore on the slightest touch.

At Vienna, I attended the wife of a manufacturer, who lived on the very best terms with her husband, both physically and morally, although she had never had any menstrual discharge. As she never became pregnant, and wished to have children, her conformation was examined with care, and after repeated intervals. Neither the midwives nor the accoucheur could ever discover a womb.

Mons. Richerand also cites several examples of women who had the same vice of conformation, and who, nevertheless, had an inclination for the pleasures of love. He asks, "Does the womb impress on the sex all its distinctive modifications, and should we say with Van Helmont, 'PROPTER SOLUM UTERUM MULIER EST QUOD EST,' '*in consequence of the womb alone woman is what she is?*'" Although this organ reacts on the whole female system in a very evident manner, and appears to subject to its empire almost all the actions and affections of woman, we are of opinion that it is



far from being the sole cause of the characteristics which distinguish her; seeing that these are perceptible from the earliest periods of life, when the uterine system is yet far removed from activity. A very curious observation,\* recorded by Professor Caillot in the second volume of the *Memoires of the Society of Paris*, proves better than all the arguments which can be accumulated, to what point the characteristics of the sex are independent of the influence of the womb. A woman was born, grew up, and was educated, with all the external indications of her sex. At the age of twenty or twenty-one, she wished to obey the instinct which she experienced: Vain desires! superfluous efforts! she had nothing beyond the vulva properly constituted. A little canal, the orifice of which did not exceed two lines or two lines and a half in diameter, held the place of the vagina, and terminated in a cul-de-sac, one inch in depth. The most minute examinations made by introducing a sound into the urinary bladder, and the fore-finger into the rectum, did not succeed in discovering a womb. The finger introduced into the intestine felt distinctly the convexity of the sound placed in the bladder, in such a manner as rendered it evident that no organ analogous to the womb separated the base of this organ from the anterior wall of the rectum. The young woman had never been subject to the periodical evacuation; no hæmorrhage supplied its place; she had experienced none of the inconveniences which attend the non-appearance of the menses: on the contrary, she enjoyed most excellent health. She was deficient in none of the characteristics of her sex, except that her womb was very little developed. Having arrived at the age of twenty-six or twenty-seven, she has become subject to frequent discharges of blood from the urinary organs."†

\* In the works of La Métrie, *Système d'Epicure*, § 14, a similar and not a less interesting observation will be found.

† *Nouveaux Elémens de Physiologie*, septième édition, t. ii. p. 392, § cxcv.



I have already spoken of the instinct of propagation remaining after the testicles have been removed, and when the functions of the womb had completely ceased.

*Particular Diseases of the Cerebellum.*

I have had an opportunity of observing a disease of the cerebellum altogether particular.

At Vienna, Count Philip H——, upwards of forty years of age, complained for several months of hæmorrhoidal pains: besides these, he felt continual nausea, a very disagreeable pressure on the nape of the neck, and a tendency to fall forwards, as if he saw a precipice at his feet. Several physicians ascribed all these symptoms to the hæmorrhoids: for my part, I inferred from them the existence of some organic lesion in the brain. Some months afterwards the patient died, and we found on the *tentorium* a fleshy mass of two inches in diameter, which had compressed the cerebellum. Afterwards, I read in the works of Hahnemann the description of the same symptoms: the examination of the dead body presented the cerebellum in a state of complete suppuration. At this time, I had not yet directed my attention to the influence of the cerebellum on the instinct of propagation, and on the sexual organs.

*Observations by M. Serres.*

In order to complete the proofs of the influence of the cerebellum on the sexual organs, I shall copy the researches on organic diseases of the cerebellum, printed in the *Journal of Experimental and Pathological Physiology*, by F. Magendie, Member of the Institute, 1822, Nos. 2 and 3, by Mons. B. A. Serres, Chevalier of the Legion of Honour, one of the physicians of the Hospital de la Pitié, head of the anatomical works of the Hospital, &c.

In this memoir, Mons. Serres proposes to fix the attention of observers on apoplexies of the cerebellum. As all



his observations are pathological proofs of the relation which exists between the cerebellum and the instinct of propagation, they should properly constitute a part of my treatise on the animal function of the cerebellum.

*First Observation by M. Serres.*

In the month of April 1814, a man, about thirty-two years of age, was brought during the night to the Hôtel Dieu, by the National Guard of Paris. Those who brought the patient informed us that he had been found on the quay with prostitutes, one of whom, who was present at his reception into the hospital, declared that it was in the act of coition, and after having drunk a good deal, that he fell into the condition in which we saw him.

His face was extremely red, the temperature of his head and neck was very high, his pulse was very strong and quick—ninety pulsations in a minute. His respiration was irregular and slow, nine, ten, or eleven inspirations per minute. He was in a state of profound somnolency, he gave no signs of consciousness, and, from time to time, was seized with convulsive movements, and with a tetanic rigidity, which lasted three or four minutes. In putting the patient to bed, we observed that he was in a state of erection; and the organs of generation were in a state of heat, which formed a contrast to the cold that pervaded the extremities. Thirty leeches were applied to the jugular veins, and he was bled largely from the foot, which produced no effect. He died the tenth hour after his admission to the hospital. The rigidity of the penis had ceased four hours before his death.

The first idea which presented itself to us, at the sight of the unusual erection with which the patient had been affected, was, that he had made use of some preparation of cantharides, which, as every one knows, generally produces a considerable degree of irritation in the genital organs. This opinion pre-occupied our minds in opening his body,



and it appeared to us so much the more probable, because these organs were still very much swelled and red, and that the *glans penis* presented violet spots, as if gangrenous. Under this idea, we instituted some experiments on the liquids contained in the stomach and intestines, which yielded no satisfactory result. We then proceeded to open the skull, and the encephalic mass being exposed, all the pupils present, as well as myself, were struck with the high irritation of which the cerebellum had specially been the seat. The hemispheres of the brain presented no trace of organic change, the anterior pair of the quadrigeminal tubercles were very slightly inflamed, the posterior were so to a much greater extent; the medullary mass designated by the name of the *processus cerebelli ad testes* was of a vinous red; the laminae of the cerebellum which cover it, and which, by their junction, form the superior vermicular process, had the same appearance, and presented three or four small cavities similar to those which are represented fig. 3, Nos. 2, 3, 4; in the *corpus dentatum* of the *processus cerebelli ad testes* a more considerable cavity was found, the extent of which would have contained a musket-ball of the ordinary size; the hemispheres of the cerebellum were very much injected, but in a less degree than the superior vermicular process; the *corpus dentatum* of the hemispheres of the cerebellum, and the bundles known by the name of the *crura* of the same organ, preserved the appearances of a very considerable irritation.

It may easily be believed that this organic lesion did not strike us exclusively on account of its situation. The opinion of Gall on the relations which connect the genital organs with the cerebellum was too widely known not to present itself naturally to the mind; and it appeared to us that, in this case, the rigidity of the penis, and the inflammation with which the sexual organs had been affected during the short period of the disease, were connected with the irritation of which the cerebellum had been the seat. However, such an extraordinary pathological relation could



not be established by one single fact. Before admitting this opinion, it was necessary to see whether it was verified in analogous cases. With this view I consulted the numerous observations which I had collected on the subject of apoplexy, and which I used in the composition of the memoir before quoted. Among the number, there were two individuals whose cases had presented inflammations of the cerebellum, and who, during the continuance of the apoplexy, had presented well marked erections and abundant emissions. One of them had been communicated to me by my colleague Dr Lafore, then house-surgeon of the Hôtel-Dieu. I was in possession only of these three cases when I published my first work on the organic disease of the *encephalon*, and their singularity was the cause of the reserve which I then practised in making them known. New facts have occurred to confirm the first positions, and not only are we now able to regard as certain the existence of apoplexies of the cerebellum, but we are authorized to hope that they can be distinguished, in the generality of cases, from those which affect the other parts of the *encephalon*.

*Second Observation by M. Serres.*

Thomas Marie-Anne, day-labourer, aged 55, of a sanguine temperament, robust, and much addicted to sexual pleasure, passed a part of the day of the 19th April 1818, in a tavern. In the night, somnolency, heaviness, momentary agitations, and loss of consciousness occurred. The patient did not answer the questions put to him, and had erections during part of the night.

He entered the hospital on the morning of the 20th of April, and presented the following symptoms. His face was red and swelled, he had a slight stupor, from which it was possible to rouse him by excitement; his respiration was short, with sighing at intervals, thirty or forty inspirations per minute; pulse full, strong, frequent, eighty pulsations per minute; hemiplegia on the left side of the body, chiefly



in the leg ; agitation of the right side. (Bleeding from the right jugular vein, purgative *lavement*, barley, *arnica* for drink ; an antispasmodic potion.)

His consciousness having returned after the bleeding, the patient was surprised to find himself in the hospital. In the evening there was a strong paroxysm, his face was much injected, swelled, considerable heat in the neck, respiration slow, eleven or twelve inspirations per minute ; pulse hard, more frequent than in the morning, ninety-four beats per minute ; profound somnolency, insensibility to all kinds of excitement, very violent satyriasis, tumefaction and redness of the scrotum, immobility of the left side, convulsive movement in the right ; abundant ejaculation at the end of the paroxysm. Bled—mustard sinapisms to the legs. The state unaltered after the bleeding ; rattling in the throat during the night. On the morning of the 21st, face violet, swelled, lips and nose cold, respiration very rare and very short, pulse small, irregular, and very quick ; distortion of the mouth, swelling and vivid redness of the genital organs, abundant ejaculation,—death at nine o'clock.

On opening the body, and lifting up the skull-cap, a quantity of black blood flowed out. All the sinuses of the *dura mater*, as well as the confluent of the sinus, were filled with black clots ; the vessels of the *dura mater* were swelled in their whole extent ; the substance of the brain presented nothing particular, but the cerebellum was redder than in the natural state. When a part of the superior vermicular process was cut into, the action of the air speedily gave it a colour of bright red, the whole white substance of this part had changed colour ; one would have said that it had been macerated in blood. Being immersed in water, it lost its colour, but never regained its natural tint. Some portions of the *corpus dentatum* of the processes appeared to be corroded, but there was no distinct cavity.\* The process was divided in the direction from before backward ; we saw

\* The word in the French is *foyer*, which generally means a cavity containing fluid or broken down pulp.



black thick blood enclosed in the fourth ventricle. This liquid had penetrated forward into the fissure of Sylvius; backward into the *calamus scriptorius*.

On immersing the cerebellum in water, we observed that this blood came from a cavity situated in the central part of the right hemisphere of the cerebellum. Its size, transversely, was two centimetres and a millimetre; and from before backwards, one centimetre and three millimetres; from above downwards, it might be about one centimetre three or four millimetres; its edges were broken and unequal, especially on the external side; it occupied the centre of the grey mass which is met with at the middle of the hemisphere of the cerebellum; all the white substance was injected, especially as we approached nearer to the walls of the cavity; the colouring formed kinds of zones concentric with the cavity; the annular protuberance towards the origin of the fifth pair was inflamed, as was also the olivary eminence and the commencement of the spinal marrow. The lungs were gorged with blood, as also the right cavities of the heart and the *venæ cavæ ascendens et descendens*; these parts presented, besides, no other organic alteration; the intestinal canal presented no particular appearance.

*Third Observation reported by M. Mance, formerly  
House-Surgeon of the second class of La Pitié, and  
Dissector for the Amphitheatre of the Hospitals.*

Nicolas Bourgoïn, aged 46, was brought to the hospital on the 17th May 1818. We could procure no information concerning his previous condition; in truth, he presented all the symptoms of violent cerebral apoplexy. Respiration, above all, was greatly altered, and presented, besides, peculiar general spasmodic movements, almost convulsive, and occurring at intervals. One symptom on which M. Serres fixed our attention was, that the somnolence continued uninterrupted during these movements. (Bleeding at the arm, twenty leeches on the jugular veins, at noon a purgative



*larcement*, vegetable lemonade, an antispasmodic potion.) Considerable remission of the symptoms after the leeches.

At seven in the evening, a very violent paroxysm, short, unequal, and rare respiration; pulse strong, hard, and quick; erection; tension and swelling of the genital organs; abundant emission of semen during the night, and apparently at the commencement of the paroxysm.

On the 18th the apoplectic symptoms were more intense than on the previous evening. We communicated to M. Serres the satyriasis which we had observed, and from this symptom he told us that it was probable that the cerebellum was the principal seat of the disease; the spasmodic movements had ceased, and nothing remained except a slight degree of locked-jaw; the respiration was always short and unequal; the pulse sharp, frequent, intermittent, every seven or eight pulsations. The somnolency was profound, the pupil contracted, and scarcely moving on presenting to it alternately light and darkness; the limbs were stretched and stiff; a great resistance was found when we attempted to bend them. The insensibility became general. The satyriasis no longer existed, although the genital organs, the penis, and the scrotum, were very red, swelled, and of a high temperature. (Mustard plasters to the feet; twenty leeches to the jugulars; antispasmodic potion.) In the evening the convulsive movements reappeared; the satyriasis was very violent; rattling in the throat appeared at six o'clock, and the patient sunk at eleven.

*Opening of the Body.*—The examination of the body took place at the anatomical lecture. The head was violet, chiefly at the ears; heat was still preserved twenty-seven hours after death: the limbs were rigid and extended; the genital organs were swelled; some brown spots were observed on the scrotum. M. Serres related, on this occasion, a similar case which had occurred some time before at the amphitheatre. A body brought from the hospital of Bicêtre had struck us, at the distribution of the subjects, on account of the enormous enlargement of the penis. M.



Serres having caused it to be brought to his lecture, remarked to us, that, according to the connection which he had observed between the irritation of the genital organs and that of the cerebellum, it was probable that we should meet with traces of it on the latter organ. The examination of the encephalon had confirmed this prognosis, and had given rise to a long discussion between M. Serres and M. Lallement, present Professor to the Faculty of Medicine of Montpellier. In recalling this circumstance, M. Serres made us remark that the present case presented a very interesting opportunity of verifying this fact; so much the more, as the knowledge which we possessed of the history of the disease did not allow us to doubt of the coincidence of an acute affection of the head with a great irritation of the genital organs. The skull being opened, our attention was immediately directed towards the cerebellum, the size of which was greater than usual. The *pia mater* and the arachnoid coat being removed, this organ appeared to be of a very lively red; some drops of blood were effused between the furrows of its superior surface. Being cut into, to about four lines of its depth, all the medullary substance was of a lively red. We discovered three little cavities formed in the thickness even of the organ. The first, situated behind, at some lines from the superior vermicular process, was unequal, had its greatest diameter transverse, and contained little clots of blood. The second, farther forward, approached nearer to the mesial line, and was double in extent to the former; it was also unequal, more extended from the front backwards than transversely, and was filled by a clot of blood. The third was situated on the anterior and superior part of the cerebellum. Besides, the whole substance of the superior vermicular process presented, at intervals, little clefts, which might be viewed as the rudiment of several small cavities. The *corpora quadrigemina*, especially the posterior pair, were inflamed, as was also the large bundle known by the name of *Processus cerebelli ad testes*.

The lungs, as well as the right cavities of the heart, were



gorged with black and coagulated blood. Dr Edwards and Dr Lisfranc were present at this examination.

*Observation recorded by M. Dubois, Elève Interne of the first class of the Hospital of La Pitié.*

A man named Gambar, fifty-two years of age, by profession a public writer, was brought to the hospital of La Pitié on the 5th of March 1819. At his arrival, he was labouring under a complete loss of consciousness and insensibility of the skin. His face was very red and injected. Death soon occurred.

The skin was cold at the extremities, and covered with a viscous sweat; deglutition was difficult, noisy, and laborious; the pulse was strong, marked, and full; the eyes were red and injected like the rest of the face. There was a complete loss of power in the limbs when lifted up,—they fell down by their own weight; we pinched the skin without causing pain; there were some involuntary stools. The limbs of the left side, opposite to the one that was paralyzed, were contracted; the fore-arm, strongly bent, could not be extended except with difficulty; the left side of the face was seized with spasmodic contractions, similar to those excited by electricity in the limbs of an animal newly dead. The penis was in a state of constant turgescence, and, in certain moments, in complete erection. We observed on the surface of the gland some violet spots, which looked as if gangrene was to be apprehended. The attendants informed us that the patient had always lain on his back in bed, his head strongly drawn backwards.

The examination of his body was made by M. Serres himself, in his anatomical course. He had prognosticated a lesion of the cerebellum, in consequence of the affection of the genital organs. There was found in the brain an effusion into the thick part of the *corpus striatum* of the left side or left hemisphere. The effusion was at least three inches long and one inch broad. The blood was recently effused, and a



deepish red clot ; a more liquid blood appeared at the surface ; the portion of the brain which formed the sides of the sac in which the blood was contained, was diffuent in consequence of its maceration in the blood. The under portion was injected with blood. We saw on the upper part, and on the sides, several yellowish hard spots, true cicatrices of the brain. This organ, besides, was much injected. The cerebellum, and especially the superior vermicular process, presented numerous little effusions of the size of a grain of hempseed ; but in the posterior portion of the hemisphere to the right of the cerebellum, we saw a clot of blood of the size of a musket-ball. All the rest presented nearly the same organization which we saw in the effusions of the brain.

Mons. Serres drew this conclusion, that the priapism had been produced by the lesion of the cerebellum, which has a known connection with the genital organs, as Dr Gall has in fact established ; and he thinks that, if these lesions have not yet been observed by other physicians, it has been from their want of attention to the subject.

*Continuation of Researches on Organic Diseases of the  
Cerebellum by Mons. Serres, Physician to the Hospi-  
tal of La Pitié, &c.*

The preceding article has made known to us the pathological relation which exists between the cerebellum and the organs of generation in man. The knowledge of this relation has been furnished to us by examination of patients during life, and by organic alterations after death. To trace up symptoms to their seats, and to establish their connection with the organic alterations which the dead body presents to us, is the course which, in my opinion, true observers in medicine ought to follow, to raise this science to that degree of certainty which it is capable of attaining.

Every one knows that diseases of the encephalon are



those which offer the least probability for a certain diagnosis. It has been seen, however, that not only have we precisely indicated, during life, the seat of cerebellar apoplexies, but that the opening of the dead bodies has answered to our expectation in confirming our prediction. Not wishing to exercise any undue influence on the minds of practitioners, I have allowed the pupils, who had collected the observations at my clinical lectures, to speak; I have added nothing to their reflections; I have retrenched nothing: I shall act in the same manner in regard to a new case which has been communicated to me by Dr Falret, already known in science by his interesting researches on suicidal melancholy.

*Case observed and reported by Dr Falret.*

“ I have just read, with the greatest interest, your first memoir on cerebellar apoplexies, inserted in Mons. Magendie's Journal. I regret that I do not find in it a notice of a case of apoplexy which I had occasion to observe in our division of the Hospital of La Pitié.

“ In 1820, I paid the tribute of my applause to the precision of your diagnosis in organic diseases of the encephalon, in an article on the annual medico-chirurgical report of the hospitals, which, owing to circumstances not depending on my will, has not been printed. I reported on this occasion the conversation which we had in one of the wards of our division, on the subject of an apoplectic patient of about fifty years of age, who, the evening before, had entered the Hospital.

Could you indicate, said you to me, the precise seat which the effusion should occupy in this patient? No! I replied; and it would be rash, in my opinion, to pronounce decidedly. Well, you added, I am this rash physician. I think that the effusion is in the cerebellum; I am warranted in this conclusion by the erection of the penis, a circumstance which, in fact, had strongly arrested both your attention and mine.



This proof did not appear to me at first to be insusceptible of an answer; but as you announced at the same time that the patient had not more than thirty-six hours to live, in spite of all the efforts of medicine, I waited with impatience for the examination of the body; and, to my great astonishment, I saw your diagnosis perfectly justified.

“ I shall not report here all the apoplectic symptoms which appeared in this patient; they were the same as those which occur in cerebral apoplexy, with the addition that the penis was enlarged, extended, red—in a word, in a state of erection. The following is, shortly, what we found on opening the body.

“ Considerable development of the genital organs, brown spots on the gland and on the scrotum, the limbs rigid and in a state of extension. The integuments of the skull presented nothing remarkable. The sinus was gorged with blood; about three ounces of it flowed out on opening the head. The vessels of the pia mater were enlarged; the grey substance of the brain was reddish, and injected; but no trace of effusion, recent or old, was to be found in this organ. The case was otherwise in the cerebellum. Independently of the injection of this organ, we observed in the central portion of the superior vermicular process a large cavity, extending itself from right to left towards the hemispheres of the cerebellum; a clot of blood filled it. The *processus cerebelli ad testes* was very red; the fourth ventricle contained blood. The lungs were gorged with blood. The organs in the abdomen presented no remarkable appearance.”

Although these observations are, in some degree, repetitions of each other, and all contain the same confirmation of the relation between acute irritation of the cerebellum and that of the genital organs, I shall report still a few others which I have collected myself, persuaded that we cannot accumulate too great a number of particular facts, when our object is to prove the existence of a disease unobserved, yet serious in its character.



*Apoplexy in the Cerebellum.*

John-Baptist-Francis Girardin, aged 42, potter, of a strong constitution, with a short thick neck, large face, greatly addicted to eating and drinking, much devoted to women, was habitually subject to a hemorrhoidal flux, which had been suppressed for some months. In January 1819, the patient had contracted gonorrhœa, during the course of which he had vivid erections, which had rendered the application of leeches to the perineum, and the use of hip-baths, necessary. He was scarcely cured when he returned to his old habits. He had passed the night of the 18th and 19th January in a brothel; and having fallen into a violent fit of rage, in consequence of the representations which were made to him on the impropriety of his conduct, he left his home on the morning of the 19th. He was brought back in the evening without consciousness, his face swelled and red, and his clothes covered with food and wine which he had vomited. An attempt was made to cause him to vomit by employing ipecacuan, but without success. He was brought into my division on the 20th of March, in the morning. After having told me the facts now mentioned, his wife took me aside, and stated confidentially, that, during the whole night, he had been in a state of erection, and that she presumed he had contracted a new venereal disease.

I immediately examined his genital organs, and found that, in fact, the penis was still swelled and red. On examining the shirt which had just been taken off him, we observed that there had been a large emission during the night. In other respects, the patient was in a state of stupor; the face was livid and swelled; the neck was swelled; the hand, applied to the occipital region, felt a burning heat; the respiration was short and slow; every two or three minutes there was a very long inspiration; the pulse was strong, full, hard, and frequent; the eye was injected, the pupil contracted; when the upper and lower extremities were



excited, the patient drew them back, and appeared to have the sensation of pain.

I ordered a copious bleeding at the jugular vein; I employed *revulsives* on the lower limbs, and applied compresses dipped in iced water to the genital organs; I administered internally lemonade in great quantity, and after the bleeding, I ordered twenty leeches to be applied to the hemorrhoidal vessels.

The respiration became more free after the bleeding and the application of the leeches; the pulse fell; there were nine, ten, or eleven pulsations, at least, in the minute. The genital organs, nevertheless, continued still swelled and red, but the erection of the penis no longer appeared. The stupor was diminished, and the patient appeared even to have regained his consciousness.

In the evening, a violent paroxysm having come on, the surgeon of the guard was called, who observed the erection of the penis, and believed that he saw the patient carry his hand to it. He applied ice to the penis; the erection appeared to be subdued for some time, but during the night it returned with a new intensity. The respiration became very difficult; the noise which his snoring occasioned, greatly disturbed the neighbouring patients; he passed the night in this condition.

On the morning of the 21st the erection had ceased; the respiration was short; the lungs seemed only partially to dilate; the pulse was sharp and very rapid; the face was livid and cold; the temperature of the extremities was below the ordinary standard; the eye was dull; the pupil was dilated and motionless when the ball of the eye was rubbed, and when the eyelids were alternately opened and shut. The patient appeared to be paralyzed on the right side; every thing announced a speedy approach of death. He expired, indeed, five hours after the visit.

*Examination of the body.*—The integuments of the cranium being removed, we observed that the external protuberance of the occiput projected very considerably. We



were equally struck by the shortness of his neck, and with the volume of the muscular layers in this region. On opening the skull, the sinuses were gorged with blood, especially the *Torcular Herophili*. The vessels of the pia mater on the brain and cerebellum were much injected; the choroid plexus inclosed several small watery cysts, which are designated under the name of *hydatids*. After having exposed the cerebellum, we perceived the substance corroded in its anterior part; on the borders of this erosion there were some bloody clots, which indicated the presence of a hollow cavity in this part; in fact, on thrusting the finger into this rent, we penetrated into a cavern extending about four lines to the right, towards the middle portion of the hemisphere of the same side; on the left, the cavity, less deep, had extended more towards the front in the centre of the hemisphere; internally, the cavity had been bounded by the *tonsillaire lobule*, which formed its bottom; in this manner, it had not penetrated into the interior of the fourth ventricle.

The white substance of the cerebellum was irritated and red all round the cavity; there were even some traces of capillary vessels filled with blood, which gave it a marble aspect.

The lungs were gorged with blood, as were also the right cavities of the heart; the *vena cava ascendens*, the internal and external jugular veins, and all the sinuses of the brain, as we have already observed, were full of blood.

The stomach, and the commencement of the small intestines, were inflamed; some of the mesenteric ganglions were gorged here and there. The bladder was full of urine. The swelled genital organs presented some livid stains towards the middle part of the scrotum; the laminous tissue of the cavernous bodies of the canal of the urethra and of the gland, were gorged with blood.

The volume of the cerebellum was more considerable than in the normal state; the vertebral arteries were of a larger calibre than usual. This increase of calibre was found also



in all the arteries of the cerebellum. It may even be presumed, that the relative increase of the cerebellum proceeded from this dilatation of the arterial system.

*Apoplexy of the Cerebellum without Erection of the Penis.*

After these precise observations, I was almost authorised to conclude, that the pathognomical symptom of apoplexies of the cerebellum was erection of the penis during the continuance of the disease. I have indeed put this question to myself: Is every apoplexy of the cerebellum accompanied by unusual erection? This conclusion might be deduced from the foregoing facts. The following case will afford us an exception, dependent probably on the place which the effusion occupied in the depth of the cerebellum.

Jean-Charles Montagnon, washerman, 50 years of age, melancholy from his early youth, had been subject, from the age of 40, to very troublesome giddiness in the head, for which an issue was applied to his left arm. He also fell into the custom of having himself bled every two months, to replace a hemorrhoidal flux which he had had from thirty to forty years. These precautions, and a well-regulated regimen, made him enjoy pretty good health till the 12th of August 1821. On this day he heated himself much in a forced walk, and lay down in the sun, where he slept about two hours. On wakening, he felt himself benumbed; the right leg appeared to him to be so heavy, that it felt (as he said to his sons on returning home), as if it were composed of lead. He was dejected all the evening; eat and drank very little, and went to bed afflicted by the most melancholy ideas.

During the night, the patient, (having risen to satisfy some wants of nature), was seized with a violent giddiness, followed by a fall, in which the anterior part of his head struck against a night-stool placed near his bed. He was lifted up deprived of consciousness. He was in a state of profound



stupor and insensibility. He was made to swallow liquorice water mixed with an infusion of linden, which produced an abundant vomiting of greenish matter.

After having vomited, he felt himself still more overcome; he said that he was undone, and relapsed into the stupor, from which the vomiting had for a moment relieved him. The physician who was called in, on the morning of the 13th, ordered him to be bled in the arm, which produced no change in his condition. It was then resolved on to carry him to an hospital, and he entered La Pitié on the 13th, at two o'clock in the afternoon.

The young surgeon in waiting was called, and found him in the following state: His face very red, and depressed; respiration very slow: he had been threatened with suffocation in the coach which had brought him: the pulse hard, strong, and quick, intermitting at every three or four beats: paralysis of the right side: slight movement in the arm when it was excited: immobility and rigidity of the leg: the tongue yellow: the eye fixed: the pupil contracted, and very sensible to the action of light. Mustard blisters were applied to the thighs: veal water containing tartar emetic was administered, which he swallowed with difficulty, on account of the contraction of the muscles of the jaw. Antispasmodic potion.

I saw the patient in the evening. He was already dying; he was pale; the lips violet; the face entirely changed; respiration quick, intermitting; it had not been possible to cause him to swallow his potion. He died during the night, after having had some convulsive movements of the face.

*Examination of the body.*—From the report made to me by the *élève de garde*, who had seen the patient, I believed that he had died of cerebral apoplexy, and that we should meet with an apoplectic cavity in the left hemisphere of the brain, the paralysis having been on the right side. In consequence, I directed all my attention to this part of the encephalon; but having found it untouched, I continued my researches with all the interest which a well-established case



of paralysis, coinciding with perfect integrity of the hemispheres of the brain, could inspire. I made the cerebellum be removed with care, although I had no suspicion that this organ could be the seat of the disease; but in turning it up, and taking it out of the skull, the house-surgeon's finger who made the dissection, penetrated into a cavern situated at the base of the left hemisphere of this organ. This sac contained about half an ounce of blood; the borders of it were unequal, but the cerebellum had not been entirely torn, for there was no effusion of blood into the occipital cavities of the skull. It extended a little outwards and backwards, towards the thin lobule of *Malacarne*. Internally, it had not extended into the fourth ventricle, although its extremity was directed towards the external portion of the rhomboidal body, (*corpus striatum* of the cerebellum). The matter which lay around it was inflamed only to the extent of one or two lines. As to the rest, the substance of the cerebellum was not inflamed, although the vessels of the pia mater were more distended than in the ordinary condition.

The lungs and the right cavities of the heart were gorged with black, thick, and coagulated blood.

Although this observation does not coincide with the foregoing, it deserves the greatest attention. I made inquiries at his relations, to learn if erection had taken place after the attack. The answer was in the negative. It was added even that the patient had always led a regular life. During his stay in the hospital, no sign of swelling or erection of the penis was observed. It is, therefore, to be presumed, that this symptom did not appear in this patient. It now remains to be discovered, whether the absence of erection was owing to the different situation which the cavity in this case occupied in the cerebellum. I think that it was so; but only after additional observations shall have been made, will it be possible to draw this conclusion with certainty. For the present, we must limit ourselves to calling the attention of practitioners to the point.



*Apoplexy of the Cerebellum having its seat in the Lobule Tonsillaire.*

Among the great number of human brains which I have prepared for my large work on the Comparative Anatomy of the Encephalon, I have met with numerous examples of organic lesions, old or recent; one of them was situated in the *lobule tonsillaire* of the cerebellum. The dead body had been brought from the hospital of St Anthony, to the amphitheatre of the hospitals, and was very robust. I could obtain no account of the disease. I regret this the more, because I have established that man alone possessed this *lobule*; that among apes, seals, and the *cetacea*, whose heads approach nearest to that of man, no part similar to it is met with. In this dead body, this *lobule* had been destroyed almost entirely by the cavity, and blood was effused into the fourth ventricle. The genital organs were largely developed, but were not pathologically swelled.

*Apoplexy of the Cerebellum in Woman.*

It is unnecessary to observe, that the symptoms drawn from the erection of the penis can present itself only in man. Woman being also exposed to this severe malady, we should endeavour to recognise it in her by other indications; perhaps some may be found in the state of the clitoris, in the tendency to nymphomania. Only one of my observations relates to this subject.

*Chronic Affection of the Cerebellum.*

Marie-Jeanne-Josephine Dubourg, thirty-three years of age, sempstress, was very early addicted to sexual pleasure. Although she had given herself up, in a brothel, to every excess of debauchery, she had not menstruated till her



twentieth year, probably on account of these very excesses. Up to her thirtieth year, according to her own account, she had been fatigued, but never satisfied, with sexual pleasure; and in spite of the efforts of her relations to withdraw her from the scandalous course of life which she led, she felt herself, she said, irresistibly drawn to it. This state continued till her 29th year. At this age, cohabitation with men being incapable of assuaging her desires, she gave herself passionately up to excesses of masturbation, and fell into a habitual drowsiness at the end of these practices, which was attributed to the spirituous liquors which she equally abused.

Ashamed of this condition, she submitted to pretty violent treatment. Her *clitoris* was burned, in the hope of causing her erotic furors to cease; these being regarded as the cause of a pulmonary consumption with which she was afflicted. She left the Hôtel-Dieu without having experienced the relief which she expected, and resumed her former habits.

She experienced, besides, very intense headach, and, at the age of thirty-two, became completely imbecile. She died at La Pitié, which she had entered to go to the Hospital of Salpêtrière. She sunk under pulmonary consumption.

On opening her body, we met with an induration of the superior and inferior vermicular process; in some places there were little ulcerations which would have held an ordinary pea. The borders were hard, yellowish, the bottom was covered by a thick membrane; there was a yellowish serosity in the little cavity; besides, on the anterior portion of the superior vermicular process, all the part of the cerebellum was softened, and of a yellowish white; the substance of the cerebellum had degenerated into the state in which it is found in the human embryo of the second month, and at the commencement of the third. All round the vermicular processes, the cerebellum was inflamed and harder



than in the natural state. The arteries of the cerebellum were more developed than usual.

I had caused the body to be injected, in order to examine these last arteries properly: I directed my attention equally to those of the pelvis, and we found all the divisions of the hypogastric artery enlarged in volume; the uterine, the vaginal, the vesical, the hemorrhoidal, were all prodigiously dilated. Was there any relation between these developments of the arteries of the pelvis and the nymphomania? I think that there was; and I found this idea on the anatomical inspection of the same arteries in consumptive patients. Every one knows that phthisical patients are disposed to coition and to masturbation in a disordered manner. In about sixty bodies which I have examined with this object in view, I have constantly found the divisions of the hypogastric artery greatly augmented in volume. Whether this dilatation of the arterial system be an effect or a cause, it appears to me to be equally important to establish the observation. I have also observed the cerebellum, but have found nothing remarkable either in its volume or in its weight.

*Chronic Inflammation of the Cerebellum in an Ape.*

I am indebted to the kindness of the Professors of the *Jardin du Roi* for the advantage of having been able to examine the encephalon of a great number of species of mammiferous animals, which I should not have been able to procure by means of my private correspondents. In an ape which died in November 1821, I met with the mesial part of the superior vermicular process, and the internal part of the right hemisphere of the same organ, softened, and yellowish, resembling a kind of *boullie*.

The ape had died of pulmonary consumption. It had not suffered from paralysis, but for several months it had been sad, taciturn, and without appetite.



*Apoplexy of the Cerebellum in Women ; an observation  
communicated by Dr Fabret.*

“ Marie-Jeanne-Renouville, fifty-three years of age, of a sanguine temperament, of a small structure, the daughter of a man who had died of apoplexy, had always enjoyed good health. On the 2d of September 1817, she was found dead in her bed. Nothing on the evening before had indicated her approaching death. The opening of her body took place twenty-four hours after death. An enormous effusion of blood was found in the skull, which appeared to have commenced in the fourth ventricle, and to have torn the cerebellum to such a degree that it formed a cavity the walls of which were only three lines in thickness, and which was filled with a clot of blood of the size of a hen's egg. The heart was very large, the left ventricle was more than an inch in thickness. There were some ossifications at the arterial valves. The other *viscera* appeared to be in a healthy condition.”

Cabanis knew three men who, in the prime of life, had become suddenly impotent. Although they were in other respects in good health, they became sombre and chagrined, and their intellect appeared immediately to become weaker from day to day. It is very probable that the impotency of the organs of generation had been preceded by disease of the brain, particularly and primarily of the cerebellum.

*Conclusion.*

These numerous physiological and pathological facts observed in man and the lower animals, not only prove that the cerebellum is the organ of the instinct of reproduction, but they serve also to explain the following phenomena :—

1. How irritants applied to the nape of the neck, such as blisters, setons, frictions with volatile and spirituous sub-



stances, often produce a violent irritation in the genital organs, excite the menstrual discharge when it has been suppressed, remove complaints caused by its suppression, and cure impotency arising from debilitating causes, much better than all the means which are usually made to act on the sexual organs.

2. Why, on the contrary, cupping-glasses, leeches, cold lotions, and embrocations, applied to the nape, frequently cure erotic mania, especially when it has appeared suddenly, and constitute excellent remedies against priapism, satyriasis, nymphomania, and nocturnal pollutions, always assuming, however, that these last are not a consequence of exhaustion.

3. Why hanged men have violent erections and abundant emissions of seminal fluid.\* If it be true that the same symptoms manifest themselves in furious madness, frequent bleedings, &c., in the nape, might perhaps produce beneficial effects in this disease also.

4. Why, in some injuries of the brain, the wounded direct their hands first to the organs of sex, and then to the head.

5. Why, in the case of inflammation of the genital organs, there is always great danger when delirium and inflammation of the parotid glands is combined with it, or when, in delirium, the patients often direct their hands to the sexual organs.

6. Why the disease terminates almost always in death, when in these cases there is delirium, disordered and convulsive movements, and prostration of strength; symptoms which are usually explained by a *typhus* fever, while they proceed from an inflammation of the brain.

\* Archenholz reports, in his *British Annals* (*Brittische Annalen*), that, in England, debauchees have pushed refinement so far, as to invent a machine, by the aid of which their mistresses hang them for some instants before receiving them in their arms.—*Gall's foot-note.*

This machine is located by Mr Archenholz in England, apparently for the same reason which induced the King of Denmark to send Hamlet to this country—the assumption that the English are mad. The idea is unworthy of Gall.—*Transl.*



7. Why, in men who have died of apoplexy produced by the efforts of a voluptuous coition, we almost always find blood effused in the cerebellum. Very lately I have had a fresh opportunity of confirming this observation.

8. Why excessively ardent amorous desires are frequently the precursors of apoplexy.

9. Why a very ardent copula, too frequently repeated, is capable of producing mental alienation. Forestus, lib. x., observ. 25, reports an example of this occurrence.

10. Why, in the Turkish and Persian soldiers, who have made an excessive use of opium, erections continue a long time even after death.

#### *Philosophical Observation.*

Before closing these considerations, I direct the reader's attention to the different degrees of manifestation of which the instinct of reproduction is susceptible. In infancy, and even in some individuals who have attained full growth, we find it a nullity; these persons are absolutely indifferent about women. It begins by manifesting itself under the form of interest, of liking, of inclination; by and bye it acquires the form of desire, of passion, and it may terminate by degenerating into the most disordered lubricity and true erotic mania. All these different degrees, then, are only modifications of one and the same fundamental quality. This observation confirms what I have elsewhere said (in the *Physiologie du Cerveau*) concerning the gradation of the same propensity, and it will be of great use in our subsequent enquiries.



## REMARKS BY DR GALL

ON THE

*Report made to the Academy of Sciences on the Experiments relative to the Functions of the Nervous System, by Baron Cuvier, Perpetual Secretary to the Academy.*

M. Flourens has lately made experiments, by mutilation, on the nervous system, particularly on the brain and the cerebellum. He believes that, by this method, he has established that the cerebellum is the regulator of the movements of locomotion. As the public may be induced to conclude from them that the cerebellum is not the organ of the instinct of generation, I have judged it necessary to institute a critical analysis of these experiments.

I have not seen the memoir of M. Flourens; I am acquainted only with the report of Baron Cuvier: hence it is this report which must serve for the basis of my reflections.

Baron Cuvier commences by clearing up, in a very judicious manner, the new language adopted by M. Flourens. I see with pleasure that he has endeavoured to avoid the confusion of the terms irritability and sensibility, and of sensibility and conductor of irritation, &c. &c.

“ In order, then,” says M. Cuvier, “ to express, in general language, the true questions which M. Flourens has proposed, and which are not perhaps sufficiently clearly pointed out in the title of his memoir, we shall say that he has endeavoured to ascertain by experiments,

“ 1. From what points of the nervous system artificial irritation may depart, so as to reach the muscles.

“ 2. To what points of this system the impression must be propagated to produce sensation.

“ 3. From what points voluntary irritation descends, and



what parts of the system must remain entire to produce it regularly.

" We shall add, that, in this first part, he has considered these questions only in so far as they relate to vertebrated animals, and to their nervous system of animal life; that is to say, to the brain, the spinal marrow, and to the nerves which proceed from these parts.

" To resolve these questions, the author commences with the nerves, and repeating, in regard to them, the experiments already known, he establishes the two general effects of their irritation, such as we have just announced them: he shews, in a precise manner, that, in order to give rise to contraction, there must be a free and continuous communication of the nerve with the muscle; and that, for sensation, a free and continuous communication with the encephalon is necessary: and he concludes that neither contraction nor sensation belongs to the nerve; that these two effects are distinct; that they may be produced independently of each other; and that these propositions hold true, at whatever place, and at whatever branch of the nerve, the communication is intercepted.

" Employing the same method for the spinal marrow, he arrives at similar results. When it is irritated at any point, it occasions contractions in all the muscles whose nerves take their origin from below that point, if the communications have remained free. It occasions no contractions, if the communications have been cut off. The case is exactly the reverse in regard to sensation; and, as in the nerves, the empire of the will has need of the same freedom of communication as sensation, the muscles below the point of interception no longer obey the will of the animal, and it no longer feels them. Finally, if the spinal cord be intercepted in two different places, and if the interval between them be irritated, the muscles which receive their nerves from this interval alone experience contractions, but the animal does not command them, and receives from them no sensations.

" We shall not report all the combinations with which M. Flourens has varied his experiments in regard to this



point; it is sufficient to say that they all lead to the result which we have just stated.

“ The author concludes from them that sensation and contraction belong no more to the spinal cord than to the nerves; and this conclusion is certain in regard to entire animals. It would be a great object to discover whether it holds equally good in the case of animals which have lost their encephalon, and which, in certain classes, appear far from losing instantly all their animal functions; but this is a topic to which we shall have occasion to recur in a subsequent part of this report, even in reference to the warm-blooded animals.

“ M. Flourens concludes, farther, from one part of these experiments, that it is by the communication established among all the nerves, by means of the spinal marrow, that, what he calls the dispersion or the generalization of the irritations, or, in other words, the general sympathies, are established; but he has not sufficiently developed this proposition to enable us to appreciate the reasonings on which he supports it.”

These experiments and results being known, I may dispense with offering any observation on them.

Baron Cuvier continues :—

“ He arrives at last at the encephalon, and it is in this central part of the system that new light may be expected from experiments better directed than those of the prior physiologists.

“ Indeed, although Haller and his school had made many experiments on the brain to discover its vital properties, and any thing that might be peculiar in the functions of the various parts of which this complicated organ is composed, it may be said that these experiments did not yield sufficiently rigorous results; because, on the one hand, at that period, neither the connection of the different parts of the encephalon, nor the directions and communications of their medullary fibres, were sufficiently known; and, on the other, because they were not sufficiently isolated in the experi-



ments. When the brain was compressed, for example, it was not certainly known on what point of the interior the compression had been most strongly applied: when an instrument was inserted, the depth to which it had penetrated, and the organ into which it had been introduced, were not sufficiently examined. M. Flourens states, and with some reason, this reproach against the experiments of Haller, of Zinn, and of Lorry; and he has endeavoured to avoid similar errors by operating chiefly by the way of ablation; that is to say, by removing, in all instances in which it was possible to do so, the part whose special function he wished to discover."

The objections which Baron Cuvier adduces against the methods followed by Haller, Zinn, and Lorry, are very forcible; but there is every reason to fear that *ablation* is the very reverse of a good method of discovering the *special* functions of any part of the brain. M. Cuvier distinguishes very clearly the vital properties of the brain from its special functions. We shall see by-and-bye whether the function which M. Flourens believes himself authorised to ascribe to the cerebellum, be a special function, or rather a vital property common to the spinal marrow and the medulla oblongata. It is necessary, in the first place, to *know* the special functions which make part of animal life, before being able to establish them by ablation of their organs; because, in this case, the animal can no longer manifest either the presence or absence of the functions. But, up to the present moment, these special functions have *not been known*. Suppose that M. Flourens wished to verify, by the ablation of the cerebellum, the point, whether this part be, or be not, the organ of the instinct of generation, how will he make the animal live long enough to enable him to tell that it either possesses or has lost this instinct? By the ablation of the hemispheres, sight and hearing are lost;—should we be authorised to infer from this fact, that sight and hearing are the special functions, or all the special func-



tions of the hemispheres? If we did not know beforehand that the animals possess memory, how should we judge that the same operation has destroyed it? How can we make an animal thus mutilated exhibit proofs of special functions of which we have no conception? And how can we execute the ablation of a single organ, purely and simply from the brain? Do we know it where it begins and ends, and how it is bounded?

Finally, how can we remove one part, without involving the neighbouring portions? How cut out the cerebellum, especially in mammiferous animals, without injuring the *medulla oblongata*, and all the parts with which it is in communication, its interlaced commissure in the *pons Varolii*, with the nervous bundles of the *medulla oblongata*, the *tubercula quadrigemina*, &c.?<sup>\*</sup> And as the effects of the lesion are transmitted, to what part shall we ascribe the symptoms themselves?

Farther, We should never lose sight of the fact, that the same part may possess its own general vital function, and its particular animal function. If it were true that the lesion of the tubercles in birds always occasions convulsions, it is not the less certain that the tubercles are devoted to vision. In like manner, the cerebellum may actually participate in the vital functions of the *medulla oblongata* and spinal marrow, and nevertheless manifest also a particular animal function. Baron Cuvier admits that the brain is necessary not only for vision, hearing, and converting the irritations of the other nerves into sensation, but also that it is the seat of memory and judgment. Which of these are the general vital functions, and the particular, special, animal functions?

Thus, all the experiments by mutilation or by ablation confirm the views which I have stated in the section on

<sup>\*</sup> See Mr Solly's work on the human brain, p. 483. He there describes his plate vi., in which he delineates fibres proceeding from the anterior column of the spinal marrow, and ascending to the cerebellum, and which thus connect the motor tract with this organ.—TRANSL.



the mutilations of the brain considered as a means of discovering the fundamental qualities and faculties of the mind, and the seats of their organs,\* viz. that at most it is possible to obtain by these methods only some results, almost always very doubtful, in regard to the phenomena of irritation, of sensibility, of locomotive movements, and of the functions of certain viscera, but that we shall never procure the least information concerning the special functions of the cerebellum, or of the particular parts of the brain.

But Baron Cuvier continues :

“ In order to render the facts which M. Flourens has obtained more clearly intelligible, we shall recapitulate, in a few words, the *ensemble* and the mutual relations of the parts of which we are treating.

“ It is now known, and particularly by the late investigations of Messrs Gall and Spurzheim, that the spinal marrow is a mass of medullary matter, white externally, grey internally, divided longitudinally, from above downwards, into anterior and posterior columns, the *fasciculi* of which communicate with each other by means of transverse medullary fibres; that it swells out at particular distances; that it gives off at each swelling one pair of nerves; that the *medulla oblongata* is the upper part of the spinal marrow enclosed within the skull, which also gives off several pairs of nerves; that the fibres of communication between the two columns cross at the *medulla oblongata*, so that those of the right side proceed upwards into the left side, and *vice versa*; that these *fasciculi*, after having been enlarged, in the mammiferæ, a first time, by a mixture of grey matter, and having formed the prominence known by the name of the *pons Varolii*, separate and take the name of *crura cerebri*, still continuing to give off nerves; that they are again enlarged, by a new mixture of grey matter, in order to form

\* Vol. iii. p. 155, of the 8vo edition of his work, “ *Sur les Fonctions du Cerveau.*”



the masses commonly called the *optic thalami*; and a third time, to form those named *corpora striata*;—that from the whole external border of these last enlargements, arises a lamina, more or less thick, more or less convoluted externally, according to the species, covered entirely on the outer surface with grey matter, forming what is called the *hemispheres*. This lamina, after having been reflected upon itself, in the middle of the convolutions, is united to that on the opposite side by one or more commissures or fasciculi, of transverse fibres, the largest of which, existing only in the *mammiferæ*, is named the *corpus callosum*. It is also known that, on the *crura cerebri*, behind the *optic thalami*, there are one or two pairs of smaller swellings; known, when there are two pairs, as in the *mammiferæ*, by the name of *corpora quadrigemina*, and from the first of which the optic nerves appear to arise; that the olfactory nerve is the only one which does not evidently take its rise from the spinal marrow, or from its columns; finally, that the cerebellum, a single mass, white internally, and cineritious externally, like the hemispheres, but often more divided by external folds, is placed transversely behind the *corpora quadrigemina*, and over the *medulla oblongata*, to which it is united by transverse bundles which are called the *crura cerebelli*, and which are inserted into the cerebellum at the sides of the *pons Varolii*."

The reader who will compare the report on our Memoir, presented to the Institute in 1808, with these passages, will easily discover certain points in the anatomy of the brain, which were then doubted or denied, but which are now admitted and adopted. Such a change of opinion does justice to our anatomical discoveries, and does honour to the reporter. I am fully convinced, that as soon as this distinguished naturalist shall have studied my physiological discoveries, his ideas will accord with mine.

When Baron Cuvier speaks of the cerebellum as a single mass, he can mean only the cerebellum of fishes, reptiles



and birds. The part which, in birds, constitutes the whole cerebellum, forms only the middle or fundamental portion in mammiferous animals. At each side of it, in the latter, there exists a lobe more or less complicated, but always symmetrical with its counterpart on the opposite side. Consequently, the cerebellum of the mammiferæ belongs, like the brain, to the class of double organs.

“ It was in these masses, so different and so complicated, that the place of departure of irritation and the arrival of sensation, were to be looked for. Their respective co-operation in voluntary actions was also the point to be ascertained; and this is what M. Flourens has particularly endeavoured to do.

“ In the first place, he examined how far up he could proceed in order to excite powerful irritation in the muscular system; and he has found a point where these irritations produced no effect. Commencing, then, at the opposite surface of the brain, he irritated it downwards, more and more deeply, so long as he produced no effect on the muscles; and when action did begin, he found himself at the same spot where action had ceased, as he proceeded upwards. This is also the place where sensation, produced by irritation of the nerves, ceases; above that point, pricking and wounds produce no pain.

“ Thus, M. Flourens has pricked the *hemispheres* without producing either contraction of the muscles, or any apparent pain to the animal. He removed them by successive slices, and he performed the same operation on the *cerebellum*. He has at the same time removed the hemispheres and the cerebellum: the animal remained impassive. The *corpora striata*, the *thalami nervorum opticorum*, were attacked and removed without any other effect. Even contraction of the iris did not take place, neither was it paralyzed.”

Dr Spurzheim and I, seeing so much uncertainty and contradiction in the experiments cited by Duverney, Gautier,



Kaw-Boerhaave, &c. resolved to institute similar experiments, the results of which are already stated in my vol. ii. p. 83, and in Dr Spurzheim's *Phrenologie*, p. 8. If we remove from hens or pigeons the greater part of the two hemispheres of the brain, and the great commissure, as far as the two ventricles, the animals give distinct indications that they see and hear; but none of them, so far as we observed, took the food which was presented to them. Nevertheless, if food was put into their throats, they swallowed it. Rabbits mutilated in the same manner have run, seen, heard, and even taken food spontaneously. A pretty considerable lesion of the cerebellum alone, is not more injurious to the functions of the senses and to life, than the destruction of the superior portions of the brain itself; but if these lesions penetrate into the great apparatus of increase (the *corpora striata* or the *thalami nervorum opticorum*), or, more deeply still, into the great commissure of the cerebellum (the *pons Varolii*), they induce convulsions and death.

We have drawn from these experiments the conclusion that the whole brain is not necessary to the functions of the senses; but they do not in the least enable us to decide whether a portion of the brain, and *what portion* of this organ, is indispensable to the execution of the functions of the senses *with consciousness*.

Here, then, are results quite different from those obtained by M. Flourens. Indeed, I cannot comprehend how the whole brain, the whole cerebellum, together with the *corpora striata* and the *thalami nervorum opticorum*, can be removed without affecting the medulla oblongata, &c. and exciting by that means convulsions and death; not to mention the consequences of considerable hæmorrhage which is inevitable.

“ But when he irritated the quadrigeminal tubercles, shivering and convulsions commenced; and this shivering, and these convulsions, increased as he penetrated deeper into the medulla oblongata. The irritation of these tubercles,



as well as that of the optic nerve, produced violent and prolonged contractions of the iris."

In reading this passage, I maintained beforehand, that the lesion of the tubercles would not produce convulsions in any degree more constantly than the lesion of the hemispheres; because these tubercles are ganglions planted on the upper extremity of the *medulla oblongata*, and not at all a continuation of it. Messrs Fodéra, Fossati, and myself, have injured and cut away the anterior part of these tubercles, but with the precaution not to touch the *medulla oblongata*, which lies below them. The rabbits did not experience the least convulsion; but immediately when we penetrated deeper, the animal was seized with powerful and instantaneous convulsions.

"These experiments accord with those of Lorry, printed in the third volume *des savans étrangers*."

"Neither the irritations of the brain," says this physician, "nor those of the *corpus callosum* itself, produce convulsions. It may even be removed with impunity. The only part, among those contained in the brain, which has been found uniformly and universally capable of exciting convulsions, is the *medulla oblongata*. It alone produces them to the exclusion of all the other parts."

"They contradict the experiments of Haller, and of Zinn, in so far as the cerebellum is concerned; but after what M. Flourens has seen and exhibited to us, it is clear that these physiologists had touched the *medulla oblongata*, without perceiving that they had done so.

"In his language, M. Flourens concludes 'that the *medulla oblongata* and the tubercles are irritable;' which, in our language, means that they are conductors of irritation, like the spinal marrow and the nerves; but that neither the brain nor the cerebellum possess this property. The author concludes also, that these tubercles are the continuation



and superior termination of the spinal marrow and *medulla oblongata*; and this conclusion is quite conform to what their anatomical relations and connections indicated.

“ Wounds of the brain and of the cerebellum no more produce pain than they do convulsions; and in ordinary language, it would be inferred from this, that the brain and cerebellum are insensible. But M. Flourens, on the contrary, says, that they are the sensible portions of the nervous system; which simply signifies that it is to them that the impressions received by the organs possessing sensibility must arrive, in order that the animal may experience a sensation.

“ M. Flourens appears to us to have sufficiently established this proposition in so far as relates to the senses of sight and hearing. When the lobe of the brain on one side is removed from an animal, it does not afterwards see with the eye of the opposite side, even although the iris of this eye preserve its mobility; when both lobes are removed, it becomes blind, and it does not hear.”

M. Fodera had the kindness to repeat these experiments in presence of Messrs Dannecey, Fossati, Londe, Georget, and myself. The following is the result of our observations on pigeons and rabbits.

The two first rabbits from which the central and superior parts of the cerebellum were removed, died in three or four minutes. The hæmorrhage, as well as the convulsions, were considerable. The section was made from below upwards. In one of them, the *medulla oblongata* was reached by the instrument, and tetanic convulsions were the results.

The same operation was performed on a third rabbit. It had an astonished look; started, ran almost constantly backward; drew its head several times backwards, leaped into the air in starting; twice it leaped to the height of a foot and a half. When it was pricked, it drew back its limb rapidly. We destroyed, by little and little, the whole



of the centre and a portion of the hemispheres of the *cerebellum*. The symptoms increased with the destruction.

The first pigeon whose cerebellum we destroyed, like the preceding rabbit, drew its head strongly backwards, going sometimes forward, and sometimes backwards.

After we had removed the two hemispheres of the brain, from a second pigeon which was quite young, it drew its head backwards, and walked forwards and backwards.

The third pigeon, after the same operation, appeared at once as if it had been rendered unconscious and made an end of, by tetanic contractions and strong convulsions.

In a fourth rabbit, we destroyed the superior portion of the ventricles, except the *thalami nervorum opti-  
corum* and *corpora striata*. The animal appeared for some time as if destroyed; then it uttered very painful cries, and ran forcibly forwards. We then destroyed the *corpus callosum*. The animal was tranquil; it went forward when excited. We destroyed the *thalami nervorum opti-  
corum* and the *corpora striata*, the animal kept its feet and walked forwards.

Here, then, are results very different from those obtained by M. Flourens; and they will always be found to differ according to the irritability and the age of the creature, and according as the instrument is blunt or sharp, so that it pulls more than it cuts.

In general, it is not possible to perform exactly the same operation, two or three times in succession, and to obtain always the same results. Several other experimenters have sacrificed great numbers of pigeons, of rabbits, of dogs, &c. and always with the same uncertainty and inconstancy in the phenomena. Nothing is more common than convulsions, as the direct consequences of injuries of the cerebellum, both in birds and in the *mammiferæ*. It would, indeed, be much more reasonable to say of the cerebellum, than of the quadrigeminal tubercles, that its anatomical relations and connections with the spinal marrow and *medulla oblongata*, explain convulsions. These experiments, then, do not at all



contradict, at least in many instances, those of Haller, of Zinn, &c.

“But we do not find that he has established this point with equal success in regard to the other senses. In the first place, he has not made, and, indeed, could not make, any experiments regarding the senses of smell and taste; and even as to touch itself, his experiments do not appear to us to be conclusive. It is true that the animal thus mutilated appears as if drowsy; it has no longer any will of its own, and it does not execute any spontaneous movement; but when it is struck or irritated, it performs the motions of an animal that rouses itself from sleep. In whatever position it is placed, it recovers its equilibrium; if it is laid on its back, it rises; it walks if it be pushed. When it is a frog, it leaps if it be touched; when a bird, it flies if it be thrown into the air; it struggles when annoyed; if water be poured into its beak, it swallows it.

“Certainly it will be difficult to believe that all these actions take place without being excited by any sensation; and it is very clear that they are not the effects of reason. The animal escapes without an object; it has no longer any memory, it will run repeatedly against the same obstacles; but this proves, at the most, that the animal is in a state of somnolency, or that it acts like a man asleep; these, indeed, are the expressions of M. Flourens himself. But we are also far from believing that a man asleep, who moves in his sleep, and who, in this state, knows how to take a more convenient position, is absolutely destitute of sensation. And although the perception of the sensations was not distinct, and although he has not preserved any recollection of them, it does not necessarily follow that he has had none. Hence, instead of saying, as the author does, that the cerebral lobes are the sole organs of sensation, we should confine ourselves to the facts observed, and limit ourselves to stating that these lobes are the sole receptacle where the sensations of sight and hearing can be consummated, and become perceptible to the animal. If we wished



still to add to this appropriation, we should say that they are also the parts where all the sensations take a distinct form, and leave durable impressions and recollections; in short, that they are the seat of memory, in virtue of which property they furnish the animal with the materials of its judgments. This conclusion, reduced to these correct terms, would become so much the more probable that (besides the presumption in its favour, arising from the structure of the lobes, and their connections with the rest of the system), comparative anatomy furnishes another confirmation of it, in the constant proportion between the volume of these lobes and the degree of intelligence of animals."

As to the opinion of Baron Cuvier, I request the reader to re-peruse what I have said in vol. ii. p. 85, and the following pages, where I state the reasons which appear to me to shew that other nervous systems, entirely independent of the brain, may also produce sensations and spontaneous movements.

Baron Cuvier appears to admit memory alone to have its seat in the brain, and he explains judgment by the animal acting on the materials which it furnishes. Has he considered the different kinds of memory? Will he explain by memory also the various, opposite, and multiplied instincts, propensities, and intellectual talents, which exist in the different species of animals? Does depth of judgment bear a uniform proportion to the strength of memory? No! As long as physiologists shall be content to travel in the beaten tract of the old philosophers, all their researches into the functions of the different parts of the brain will remain destitute of right direction and of practical results.\*

\* The works of Dr John Abercrombie afford a striking confirmation of the truth of this remark. In one set of books he treats of the moral and intellectual faculties, without bestowing any consideration on the influence of the different portions and different conditions of the brain on their manifestations; and in other volumes, he records the pathological effects of diseases and injuries of the brain, without reference to any known functions of the same parts in a state of health. In short, he publishes a pathology of the brain without reference to its physiology; and a philosophy of the mind without regard to its organs. The public



"After the effects of ablation of the brain properly so called, M. Flourens examines those of the extirpation of the quadrigeminal tubercles. The removal of one of them, after a convulsive movement which speedily ceases, produces, as its permanent effect, blindness of the opposite eye, and an involuntary whirling round; that of the two tubercles render the blindness complete, and the whirling more violent and of longer duration. Meanwhile, the animal preserves all its faculties, and the iris remains contractile. The deep extirpation of the tubercle, or the section of the optic nerve, paralyzes only the iris; from which fact M. Flourens concludes that the ablation of the tubercle produces no other effect than would ensue from the section of the nerve; that this tubercle serves only as a conductor of vision, and that the cerebral lobe alone is the limit of sensation, and the place where it is consummated by being converted into perception.

"He calls attention, besides, to the circumstance, that, in pushing the extirpation of the tubercles to too great a depth, we come to interfere with the *medulla oblongata*, and that then violent convulsions are produced, which last for a long time."

M. Flourens was very near acknowledging that the tubercles are not always irritable. When he says that the animal preserves all its faculties,—of what faculties does he mean to speak?

"That which appears to us to be most curious and new in these experiments of M. Flourens, concerns the functions of the cerebellum.

"During the removal of the first layers, only a little feebleness and a want of harmony in the movements appear.

will judge whether this mode of proceeding be more philosophical, and more conducive to the advancement of science, than that of Dr Gall, whose discoveries, principles, and facts, Dr Abercrombie treats as not existing.—TRANSL.



At removal of the middle layers, an almost general agitation occurred; the animal, while it continued to hear and to see, executed only abrupt and irregular movements. Its powers of flying, walking, and of standing up, were lost by degrees. When the cerebellum was removed, the faculty of executing regulated movements entirely disappeared. Laid on its back, the animal no longer rose up; still it saw the blow that was aimed at it, it heard noises, it endeavoured to avoid danger, and made numerous efforts to this effect without success; in a word, it preserved the faculty of sensation, and that of will; but it had lost the power of causing its muscles to obey its will: it succeeded with difficulty in standing on its legs, supporting itself by means of its wings and its tail.

“By depriving it of its brain, it had been brought into a state of somnolency: by destroying its cerebellum, it had been rendered as if it were drunk.

“It is a surprising thing, says M. Flourens, to see a pigeon, in proportion as he loses his cerebellum, losing gradually the power of flying, then that of walking, and at last that of standing up; this last even disappearing only by degrees. The animal begins by not being able to stand perpendicularly on its legs; then its feet are not sufficient to support it. At last, every fixed position becomes impossible to it; it makes incredible efforts to attain some such position, without success; and yet when, exhausted by fatigue, it appeared as if it wished to take some repose, its senses were so susceptible, that the slightest gesture made it resume its contortions, without the least convulsive action occurring so long as the medulla oblongata and tubercles were untouched.

“We do not remember that any physiologist has made known facts similar to these singular phenomena. The experiments on the cerebellum of quadrupeds, and particularly on that of adults, are very difficult, on account of the great masses of bone which it is necessary to remove, and the large vessels which must be opened. Besides, the greater number of experiments operated according to some system known beforehand, and saw a little too clearly that which



they wished to see; and certainly no one had suspected that the cerebellum was in any manner the balancer, the regulator of the locomotive movements of the animal. This discovery, if experiments repeated with all due precautions shall establish its generality, cannot fail to confer the greatest honour on the young observer whose work we have analyzed.

"The Academy will now be able to judge, as well as ourselves, that, independently of the superfluous mutations of language, and of known facts which the author was obliged to reproduce to give connection to his work, this memoir presents, on many old facts, more precise details than any which we possessed before, and that it contains others equally new and precious for science."

As the experiments of M. Flourens appear to me to be more particularly bold and uncertain in regard to the functions of the cerebellum, I shall make a few more observations on them than on the preceding subjects.

It was said on page 102, that M. Flourens removed the cerebellum by slices; that he removed it entirely, and that the animal remained impassive. Here it is said, that, during the ablation of the first slices, a little feebleness and a want of harmony among the movements appeared; that, on removal of the middle slices, an almost general agitation was manifested, and that the animal, while it continued to see and to hear, executed only abrupt and irregular movements.

Why is the reporter afraid to give to this want of harmony—this nearly general agitation—and these abrupt and irregular movements—their proper name, that of convulsive motions? He is afraid apparently to do so, because then the results obtained by M. Flourens would coincide with those reached by so many other experimenters, and they would lose the merit of novelty.

It is true that, if the different cerebral parts existed separately in the skull without any connection between them;



if they could be removed without injuring their integuments, and without causing any hemorrhage, we might expect, by their entire ablation, to arrive at some certain results ; but how can the reporter be so inconsiderate as to ascribe certain effects to the ablation exclusively, when the part removed is intimately connected by the base, by the middle, and by the top, with the *medulla oblongata* and spinal marrow, with the annular protuberance or *pons Varolii* ; with the *crura* of the brain, and with the quadrigeminal tubercles ;—when the part is one the ablation or mutilation of which is impossible, without implicating in the most forcible manner the organs appropriated to voluntary motion and the functions of several of the senses ; when it is one whose removal is unavoidably attended by a constantly increasing loss of blood ? How can physiologists base their propositions on experiments thus entangled, thus complicated, thus disagreeing both in their nature and their effects ; which are contradicted by the experiments of other practised investigators, and which produce different phenomena every time they are made, whether on another animal or by different physiologists ?

Is it surprising that the animal lost successively the power of flying, of standing up, of executing regulated movements, of raising itself from its back, when it was ceasing, by successive stages, to live ? But I shall be told, that the animal saw, heard, and made many efforts to escape from danger ; that it had, therefore, the faculty of will, and consequently that the irregular movements cannot legitimately be ascribed to impending death. Go to the bedside of your patients, and you will see some of them who have no longer power to move an arm, to shut their eyes, to put out their tongue, and who nevertheless see, hear, and feel, and who, internally, make many vain efforts to give signs of life, without there being the least reason to ascribe these phenomena to the partial death of the cerebellum.

The pigeon whose cerebellum we had destroyed, nevertheless walked both backwards and forwards. To do this, were not regular locomotive movements necessary ? And



even to support itself painfully on its wings and tail, as occurred in the experiments of M. Flourens, regulated movements were still indispensable.

Thus, every thing combines to prove, that the notion that the cerebellum is the balancer and regulator of locomotive movements, is much more a singular idea than a true discovery.

“The integrity of the cerebral lobes,” continues the Report, “is necessary to the exercise of vision and hearing; when they are removed, the will no longer manifests itself by spontaneous acts. Nevertheless, when the animal is directly excited, it executes regular locomotive movements, as if it endeavoured instantaneously to put an end to its pain and discomfort; but these movements do not accomplish this object, very probably because its memory, which has disappeared with the lobes, which were its seat, no longer furnishes a basis or elements for its judgments. These movements, for the same reason, have no sequel, because the impression which has caused them leaves neither memory nor durable will. The integrity of the cerebellum is necessary to the regularity of these locomotive movements. Let the brain be entire, the animal will see, hear, and experience very obvious and energetic volitions; but if its cerebellum be removed, it will never command the equilibrium necessary for its locomotion. Moreover, irritability subsists for a long time in the parts, without the brain or the cerebellum being necessary to it. Every irritation of a nerve, excites contractions in the muscles on which it is ramified; and every irritation of the spinal marrow, causes contractions in the parts situated beneath the irritated point. It is altogether in the top of the *medulla oblongata*, at the spot where the quadrigeminal tubercles are attached to it, that this faculty ceases of receiving and propagating, on the one part irritation, and on the other pain. It is at this place, at least, that sensations must arrive in order to be perceived; and it is from it, also, that the commands of the will must depart. Thus, the con-



tinuity of the nervous organ, from this point to all parts of the system, is necessary to the execution of voluntary movements, and to the perception of impressions, whether internal or external.

"All these conclusions are not identical with those of the author, and especially they are not expressed in the same terms; but they are those which have appeared to us to result most rigorously from the facts which he has well established. They will, without doubt, suffice to enable you to judge of the importance of these facts, to engage you to testify your approbation to the author, and to invite him to continue to communicate to you the further progress which he may make in so very interesting a labour."\*

In the experiments which we made on rabbits, Dr Spurzheim had removed a great portion of the hemispheres, and the animals continued to see and to hear; and we had, therefore, previously established it as a matter of fact, that the totality or the integrity of the hemispheres is not necessary for the exercise of the functions of the senses. Without tormenting the poor creatures, we had previously proved that disease of one of the anterior tubercles induces disease or blindness in the eye of the opposite side. See my large work, tom. i. p. 113, et suiv. in 8vo, and p. 81 in folio.

Without inflicting these torments, Scëmmering had previously said: "A patient has been seen, in whom a wound had penetrated so deep as the *corpus callosum*, and who, whenever pus accumulated in it, lost the use of the eye on the side opposite to that of the wound, but who recovered his vision the moment the pus was evacuated. The sensation of sight, therefore, takes place in the brain." Passage cited tome ii. p. 70.

Finally, the pretended results of the experiments of M. Flourens, very rarely agree with the pathological facts observed in injuries of the cerebellum. I have mentioned, on

\* The Report is subscribed "Portal, The Count Berthollet, Pinel, Dumeril,—The Baron Cuvier Reporter."



pages 48 and 122, the case of René Bigot, in whom, in consequence of a blow from a sabre, the right lobe of the cerebellum was seen across the opening of the dura mater. *The slightest touch on this organ caused giddiness, faintings, and convulsive movements*, without producing the slightest sensation of pain. The same patient lost the senses of sight and hearing on the right side. On dissection, the right lobe of the cerebellum was found sunk, of a yellowish colour; the *medulla oblongata* and spinal marrow were of a dirty white, of a finer consistence than in the natural state, and reduced one-fourth in their volume. The nerves which went off from them appeared to be equally atrophied. The analogous condition of the *medulla oblongata* and spinal marrow with that of the cerebellum, proves that the cerebellum is closely connected with these two parts, and that the lesion of the one influences the other. How, then, can we isolate the cerebellum, and ascribe to it effects which arise quite as much from the injury of the parts with which it is connected?

The blindness, or alterations of sight, which so frequently supervene after diseases or injuries of the cerebellum, are explicable by the connexion and proximity of the cerebellum to the tubercles. The same connection explains also the vivacity of look, the fire, or languor, or abasement of the eyes in concupiscence, or after the satisfaction of amorous desires.

The patient Auguste Francois, mentioned on page 48, felt severe pains in the occiput, heaviness, numbedness of the lower extremities, and so great a weakness in the senses of sight and hearing, that he could scarcely see large objects or hear the acutest sounds.

Baron Boyer, in his *Traité des Maladies Chirurgicales*, tom. v. p. 78, cites an observation of Lapeyronie, and another fact reported by Petit of Namur, which appear to prove that the lesion of the cerebellum, by whatever cause produced, communicates to the body an extraordinary acuteness of feeling. In the observation of Lapeyronie, the patient was a man whose intellect was not at all disturbed, and in whom



physical sensibility was very great. On opening the head, it was observed that the cerebellum presented only a mass of tubercles filled with pus. The observation of Petit is still more precise. A soldier received a musket shot; the ball had traversed the left portion of the cerebellum, and penetrated into the posterior lobe of the left hemisphere of the brain. During the forty-three hours that he lived, his judgment was sometimes sound; he then answered the questions which were put to him, but most frequently he was delirious. He was always in a state of agitation, turning in his bed from side to side, and moving without ceasing his arms and legs. Sensation was so acute in his whole body, that, at the slightest touch, he withdrew the part on which the fingers had been laid, as if it had been cut or burned. This is a new proof of how much injuries of the cerebellum exert a direct influence on the *medulla oblongata* and spinal marrow, and on all the nerves which spring from them. In consequence of the phenomena which this wound had presented, Petit made experiments on living dogs, and although in the greater number of them, the lesion of the cerebellum was accompanied by an increase of sensibility on all parts of the body, he avows that, nevertheless, they only left him in doubt. This is another evidence how much injuries inflicted with an object in view are variable and uncertain in their effects, and how much the effects must be extended and multiplied when the lesion of the cerebellum is very considerable, or has lasted for a sufficient length of time to transmit its irritation to all the neighbouring parts with which it is connected, to the *medulla oblongata*, the spinal marrow, the quadrigeminal tubercles, the auditory nerves, the *pons Varolii*, the *crura cerebri*, the *optic thalami*, the *corpora striata*, and by these to the entire nervous mass of the hemispheres.

In like manner, irritations of any portion whatever of the hemispheres, or of the entire hemispheres, may be transmitted successively to the lower parts with which they are



continuous. There is no physician or physiologist who is not able to cite a great number of facts in support of this assertion. How often do lesions or diseases of the brain or of its integuments, contusions, commotions, compressions, effusions, excrescences, cancers, hydatids, inflammations, and suppurating cavities, the seat of which, according to appearances, is confined entirely to the hemispheres, produce symptoms of a most general and alarming nature; the loss of the external senses, of the powers of sensation and locomotion, excruciating pains, partial or general convulsions, &c. &c.

The reader will now be able to judge what degree of confidence the prudent physiologist can allow to pretended results obtained by means of mutilations and violent ablations of the cerebral masses, especially when these experiments are performed with the view of ascertaining the special functions of the organ thus irritated, and thus intimately connected with so many other organs.\*

\* These observations were published by Dr Gall in 1823, and were obviously written before Sir Charles Bell's discoveries relative to the functions of the spinal marrow were known by him. Dr Gall's arguments acquire additional weight from these discoveries, and they are now beginning to be appreciated and to produce effect.

When Cuvier's report was first published, it was hailed by the anti-phrenological press as recording splendid additions to physiological science. The Edinburgh Medical and Surgical Journal for January 1824, thus spoke of it: "Dr Gall," says the journalist, "incensed to find his organs of Love, Philoprogenitiveness, and many other propensities and noble faculties, all snatched from his hands to make up one poor paltry machine, for regulating the baser bodily motions, and another equally contemptible for conveying to the mind the impressions of sense, has vehemently resisted such an appropriation, and endeavoured to obstruct his adversaries' progress." After stating that Dr Gall repeated Flourens' experiments on the cerebellum, and obtained from them different results, it is added, that "he, Dr Gall, should not venture to uphold his *few, meagre, imperfect*, and most *prejudiced* experiments, before the *careful* and *varied* researches of Flourens and Rolando, and still more the deliberate approval of the illustrious Cuvier, and his no less distinguished coadjutors;" and the journalist thinks "it is no small proof of Flourens' accuracy, that so *acute and captious* a controversialist as Dr



Except in regard to the influence which injuries of the cerebellum exercise on the *medulla oblongata* and spinal marrow, there does not exist, either in the state of health or of disease, any relation or any proportion between the cerebellum and the regularity of locomotive movements. Animals deprived of the cerebellum still enjoy regular locomotive powers. The cerebellum of fishes, of reptiles, and of birds, is more simple than that of mammiferæ. Do the latter enjoy more regular, more prompt powers of locomotive action than the former? What relation is discoverable between the successive stages of development of the cerebellum, from the moment of birth to the twentieth or thirtieth year, and the regularity of the locomotive movements? Are the

Gall has been reduced to *such sorry subterfuges*;" and that Gall's objections "are *very odd ones*, for such a *fanciful and reckless theorist*." \*

Few physiologists of reputation will now assert that Flourens' experiments have satisfactorily established the functions of the cerebellum, and although many continue to reject Dr Gall's views, they no longer pretend that their truth is necessarily excluded by the results of these mutilations. Not one of them would now write in the style of this extract from the Edinburgh Medical and Surgical Journal. The inadequacy of these experiments to establish the propositions deduced from them by Cuvier, is so obvious, that his adoption of them, and the exaggerated importance at first assigned to them, by the medical press both of France and England, can be accounted for only by the then nearly universal jealousy of Dr Gall. The whole history of the reception of his discovery shews, that no hypotheses were too absurd, and no assertions relative to the structure, weight, physiology, and pathology of the brain were too glaringly unfounded and contradictory, not to be received with nearly unanimous approval and belief by the physiologists of the day, provided only that they possessed the merit of being adduced in opposition to Phrenology. This spirit still exists, to a considerable extent, in both countries, but every year it is diminishing. In proportion as Dr Gall's real merits shall become known, a juster judgment will be formed of the honesty, magnanimity, and knowledge of his detractors.—TRANSLATOR.

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\* We recommend to the reader who wishes to form a correct judgment whether the phrenologists or their opponents were most free from prejudice on this question, to compare the Review from which these passages are extracted with a Review of Flourens' Experiments, which appeared in the Phrenological Journal, vol. i. p. 455; in September 1824.



movements of the man between twenty and thirty years of age, more regular than those of the boy and girl from five to fifteen? Has it ever been proved by observation that individuals possessing a large cerebellum, manifest more regular movements than persons in whom it is small? The cerebellum is smaller in women, in general, than in men; yet do women walk and dance with less regularity, less art, and less grace than men? Do castration and lesions of the testicles, which exert so powerful an influence on the cerebellum, produce derangement in the regularity of locomotive movements? And what connection can be discovered between the regularity of these movements and the influence of lesions or diseases of the cerebellum on the genital organs? What have the painful tensions and heat at the nape of the neck, and the apoplexies of the cerebellum, so frequently associated with the sexual functions, in common with the regularity of locomotive movements? None of these questions is solved by the hypothesis of M. Flourens. They all contradict it, whenever the cerebellum is recognised as the organ of the propensity to union between the sexes.

Why admit an organ for the *regularity* of locomotive movements separate from the organ of these movements themselves? Would it not be absurd to admit, besides the organs of sight and hearing, separate organs for the regularity or irregularity of these two functions? Especially, as the irritations, the lesions, and the disturbances of the organs of voluntary motion, the *medulla oblongata*, spinal marrow, &c. are sufficient to explain the irregularities of the locomotive movements which occurred in these cases.

To give a rational foundation to the notion that the cerebellum regulates the locomotive movements, it would be necessary to shew that some proportion exists between the cerebellum and the power, quickness, and regularity of these movements. No part of comparative anatomy supports this idea. But this proportion does exist universally with regard to the *medulla oblongata*, the spinal marrow, and the



nerves which derive their origin from them. The power, agility, and regularity of the movements of the tiger, horse, caïman, boa, &c. are proportionate to the size of these organs in these different creatures, but not at all to that of the cerebellum, which is smaller in them than in man.

Let us add, that the phenomena produced by these lesions vary with the different ages of the animals, and in different individuals; that the conclusions founded on them cannot legitimately be transferred from the fish to the reptile, from the reptile to the bird, from the bird to the mammifer, and from the mammifer to man; that, consequently, they will never furnish certain and useful positions either for the physiology or pathology of man; and, lastly, that all these cruel mutilations possess no other merit than that of a vain curiosity.



CASES OF INJURIES OF THE CEREBELLUM REPORTED  
BY BARON LARREY.

IN the third volume of Baron Larrey's *Mémoires de Chirurgie Militaire et Campagnes*, p. 262, the following cases are reported, which have also been shortly noticed by Dr Gall, on pages 47 and 48 of this publication.

François (Auguste) Maréchal-des-logis, of the Horse-Guard Artillery, received, at the battle of Benevento, a wound from a musket ball, which traversed from side to side the insertions of the extensor muscles of the head, *grazing upon the two inferior occipital swellings* (corresponding to the lobes of the cerebellum), which being very prominent in this individual, were denuded or stripped of their tendinous attachments. I dilated the two openings made by the ball, and extracted a portion of his shirt which had remained in the wound, and then dressed it with emollients.

The patient at first suffered severe pain in the occiput, with heaviness and numbness in the lower extremities; his sight and hearing were impaired to such a degree, that he could scarcely distinguish large objects, or hear the sharpest sounds. *The testicles diminished in size, and fell into a state of atrophy or wasting. The penis was also reduced in size, and remained without action*: however, the wound healed, and all the symptoms disappeared before the fiftieth day.

Bigot (Réné), chasseur à cheval, or light horseman, of a strong constitution, and very fond of women, received, at the same battle, a cut from a sword, which divided the skin and all the convex or projecting portion of the occipital bone through to the dura mater, of which a very small part



was touched. We saw the right lobe of the cerebellum through the opening of the dura mater. The slightest pressure upon this organ caused giddiness, fainting, and convulsive movements, without the least sign of pain. I detached the portion of bone adhering to the flap, and gently applied it to the excavation of the cranium, with the precaution of making an incision at the base of the flap to allow an outlet to fluids. The part corresponding to the opening of the dura mater did not adhere, on account of the continual oozing from the interior of the cranium, where, however, there was no extravasation. From the first day, the patient lost the sight and hearing of the right side. He experienced at the same time acute pain in the course of the dorsal spine, and a kind of tingling in the testicles; these diminished sensibly, and in fifteen days were reduced to the size of a bean. Soon after he lost all idea or recollection of his enjoyments with the other sex. He supported the journey from Benevente to Valladolid very well. His wound was in good condition, and excepting that the functions of sight, hearing, and generation, appeared for ever gone, he gave us hopes of cure. Inflammation, however, gradually increased, convulsions, pain of head and spine, and, lastly, tetanus appeared, and caused his death on the thirty-ninth day, 7th February 1807. On dissection, we found great loss of substance at the occiput; the opening of the dura mater, corresponding to the right lobe of the cerebellum which was shrunk, was of a yellow colour, without suppuration or effusion. The medullæ oblongata and spinalis were of dull white, of firmer consistence, and reduced one-fourth of their natural size. The nerves which arise from these parts were likewise wasted.

In vol. ii. p. 150, Baron Larrey relates as follows:—Pierre Soult, 22d chasseurs, received from a Mameluke, at the battle of Salehyeh, a cut from a sabre, which, after dividing the skin and external *protuberance of the occipital bone*, divided the extensor muscles of the head as far down as the



sixth vertebra, of which the spinous process was cut. An enormous flap was the consequence, which hung upon his shoulders. His chin fell down upon the breast. It was soon united, and the man cured. Larrey adds in a note—"I have since had occasion to see this soldier, who declares that he has been deprived of his generative powers ever since that wound."



OBSERVATIONS BY J. VIMONT, M.D., OF THE FACULTY OF PARIS, ON THE ORGAN OF THE PROPENSITY OF REPRODUCTION. Extracted from his "*Traité de Phrénologie Humaine et Comparée*," vol. ii. p. 233. 4to. Bailliére. Paris and London, 1835.\*

*In the first volume of his "Traité de Phrénologie," Dr Vimont describes the anatomy of the skull, brain, and cerebellum, in man, and in the chief species of the vertebrated animals. I select only the following remarks from this portion of his work.*

"The tentorium of the cerebellum," says Professor Richerand,† "which divides the interior of the cranium into two parts of unequal capacity, is composed of bone in some animals, which advance by leaps and precipitous movements; in the cat for example, a creature which is able to perform leaps from a frightful altitude without being stunned."

This assertion of M. Richerand appears to me to be entirely gratuitous. Several animals, accustomed to make great leaps, the squirrel for instance, have not an osseous tentorium; the hare and the warren rabbit are in a similar predicament. The same organization is discovered in apes, distinguished by their leaps and gambols. Besides, this plate of bone is met with in animals whose motions are slow and heavy; such are the raccoon, the badger, &c. I may add, that it has happened to me to meet with the tentorium of the cerebellum extremely small in the skulls of dogs,

\* In this extract, all the passages printed in italics are remarks by the translator; the remaining passages are translations from Dr Vimont's work. His report of Rolando's experiments is a little abridged, but not otherwise altered.

† *Physiologie*, vol. ii. p. 116, 5th edition.



which were reported to be excellent leapers and runners ; and although this plate did not completely separate the brain from the cerebellum, these animals, during life, had presented no indication of cerebral commotion. Plate vi. fig. 1, represents the vertical section of the skull of a young dog, which was an excellent leaper. It will be seen that the osseous tentorium is very little developed. Tome i. p. 53.

*In his second volume, p. 230, Dr Vimont proceeds to discuss the organ of the instinct of Reproduction. After some preliminary remarks, he proceeds as follows :*

What part of the cerebro-nervous system, says Dr Vimont, is connected with the propensity of propagation ? Multiplied observations appear to prove, that the nervous mass which we have described in our first volume under the name of the cerebellum, is the seat of the propensity to reproduction. The following are the leading facts on which phrenologists rest this assertion.

1st, The cerebellum does not acquire its full development till the period when the sexual desires are manifested.

2dly, The energy of the propensity bears a relation to the development of this organ. The facts accumulated in support of this proposition are exceedingly numerous.

3dly, Apoplexies of the cerebellum have been accompanied by erotic phenomena carried to the highest degree.

4thly, Complete castration produces a remarkable diminution in the cerebellum, and, after a sufficient lapse of time, causes a diminution of the volume of the nape of the neck. From this arises the difference which exists between the nape of the bull and that of the ox.

5thly, Unilateral castration causes a diminution of the lobe of the cerebellum, lying on the side opposite to that on which the operation was performed.

In the memoir which I presented to the Institute in 1827, I said that I had not observed any diminution in one of the



lobes of the cerebellum of four rabbits, which I had castrated on one side, and preserved alive during eight months. In four other rabbits which I have since fed during *eighteen months*, I have found, after death, a perceptible diminution in the opposite lobe of the cerebellum.

To the facts now mentioned, I add the following, which have been observed by myself.

In all the animals which multiply rapidly, and which propagate several times a year, the cerebellum is in general very largely developed. The reader will obtain evidence of this fact by examining the cerebellum of the cat, Pl. lxxv. fig. 4; of the hare, id. plate, fig. 8; of the squirrel, pl. xxix. fig. 4; of the guinea pig, id. pl. fig. 1; of the mole, id. pl. fig. 12.\* It may also be asserted, that in the *Rodentia*, which is precisely that class of animals that multiplies most rapidly, the cerebellum is found largely developed, regard being always had to the size of the brain. Perhaps it will be objected, that birds which have only the middle lobe of the cerebellum, present a great activity of the organ of reproduction. Generally speaking, this assertion is far from being true. A very considerable number of species of birds do not experience the impulses of love except once or twice in the year. In our climate, there is only a very small number of birds which form an exception to this rule, and they belong to the domestic species, which are well fed and protected from the inclemency of the seasons. It is quite true that a single cock in a court-yard is sufficient to serve several females. This fact, which at first sight would appear to be in opposition to the general law of nature that the energy of an organ coincides with its volume, may be very easily explained. We shall see (as I have already mentioned in the course of this work), that if the comparison between the size of the cerebral organs and the energy of their functions

\* These numbers refer to the plates in the large folio Atlas which accompanies Dr Vimont's work. In accuracy of delineation, and beauty of execution, it is unrivalled in the department of science which it is intended to illustrate.



be made *between individuals of the same species*, nature will give her evidence in favour of this rule.

Nobody can deny that the animal functions are performed with more activity in birds than in quadrupeds. It is not surprising, therefore, that the cock should recruit his strength more quickly, on the one hand, by the nature of his own organization, and, on the other, by the greater abundance and more nutritive quality of the food which he finds in the court-yard, than in the wild state. Farther, I do not think that, consistently with reason, we can compare, in all its particulars, the act of reproduction in man and in the cock. The losses which the one experiences, and which must be a great cause of exhaustion, bear no resemblance to those sustained by the other. This explains the more frequent repetition of the act by the cock.

I have examined and compared four brains, of which two belonged to cocks excessively ardent in love, and two to two others which were very little so. In the former two, the cerebellum was very much developed. Fig. 1, Pl. lxxi. represents one of these brains. The cerebellum of one of the two latter cocks presented a difference in size amounting to more than one-third, compared with the cerebellum represented in the plate. The cerebellum of the fourth was nearly as large as that in one of the ardent cocks, but presented a singular softness. The remainder of the encephalon, the quadrigeminal tubercles excepted, was also very soft.

The carnivorous animals, which present a great development of the cerebellum, such as dogs and cats, are very ardent in love.\* Among more than two hundred skulls of cats in my collection, I have examined the brains of upwards of thirty, and I have constantly found in all the very ardent females, a voluminous cerebellum.

Who has not been wakened by the cries which these ani-

\* In the dog, the cerebellum is partly covered by the hemispheres of the brain; in the cat, on the contrary, it is entirely uncovered. See the plates in which their brains are represented.



mals utter in the season of their loves? I have frequently observed female cats come roaming into a garden which was overlooked by my windows, and there utter cries which never failed to attract several males whom I saw on the tops of the walls. In an instant afterwards, the most infernal caterwaulings began, as if half a dozen of children had been receiving a flogging.

The experiments which have been performed by many physicians on the cerebellum of certain animals are well known. Professor Rolando of Turin was the first who made them in considerable numbers. His observations on this subject are contained in a work published at Sassari in 1809, entitled, *Saggio sulla vera struttura del cervollo dell'uomo e degli animali, e sopra le funzioni del sistema nervoso*. The following are the principal results obtained by the experiments of this expert anatomist. They were performed on the cerebellum of mammiferæ, of birds, of reptiles, and of fishes.

*The general import of the passage cited by Dr Vimont from Rolando's work may be thus abridged.*

Rolando made an opening with the trepan on one side into the cerebellum of several pigs, and of one sheep, and cut out all of the organ which he could reach. Scarcely had the lesion extended beyond the side trepanned, when the animal was struck with hemiplegia, and perished amidst convulsive spasms and hemorrhage.

It is very difficult, he continues, to penetrate into the cerebellum of quadrupeds, without depriving them instantly of life. The animal which appeared to him best suited to these experiments was the kid. Having made an opening with the trepan, he cut in different directions, with a sharp scalpel, the cerebellum of one of these animals, after which it could not support itself on its legs, it appeared as if it had become paralytic. It lived twenty-four hours in this state, and died in convulsions. Dissection then shewed a great quantity of blood coagulated on the fourth ventricle,



which was apparently the principal cause of the spasms and death. He adds, that, in a great variety of experiments, the diminution of motion was always in direct relation to the injury inflicted on the cerebellum. Sometimes the animal was entirely paralyzed; sometimes it was so only on one side; at other times the anterior and posterior extremities only were deprived of motion, according as the cerebellum was destroyed in whole or in part.

He performed the same experiments with the same results on birds. He remarks that the animals never fell into a state of fainting or insensibility. They kept their eyes open, and looked at objects, but in vain tried to execute the least movement by means of the muscles of voluntary motion. Sometimes, however, they made slight movements with their wings and their inferior extremities; but these appeared to result solely from the inherent irritability of the muscular fibres, or occurred when some small part of the cerebellum had been left unremoved, so that it could still perform part of its functions. When he destroyed the cerebellum suddenly, or removed it entirely, the animal was constantly seized with complete paralysis; but when the lesion was only slight, it recovered the power of motion in a few hours.

Experiments on the cerebellum of reptiles and fishes gave similar results, which are detailed at full length by Dr Vimont.

Professor Rolando concludes, "I have made innumerable experiments to discover the effects of lesions inflicted on the quadrigeminal tubercles, and on the parts adjoining to the *optic thalami*, but I have seldom obtained corresponding results. This is not surprising when we consider the peculiar interlacing of the numerous fibres which occur in these parts. It is extremely difficult to know what are the bundles of fibres which have been injured in these operations; and, besides, we cannot draw clear and precise conclusions where a difference exists in the results."—*Lib. cit.*



*Dr Vimont, after pointing out the contradictory results obtained by Magendie and Flourens, from their mutilations of the brain and cerebellum of animals, and expressing his own opinion against the sufficiency of mutilations to reveal the functions of the brain, concludes his remarks as follows :*

My numerous anatomical observations on the cerebellum of vertebrated animals, lead me to believe that it is not a simple organ. Its size, its form, and its composition, as I have shewn in my first volume, differ much in the different classes of animals. We know that it is generally little developed in reptiles ; that in birds it is composed of half rings, of which the largest occupies the middle portion ; that in quadrupeds, in the *quadrumani*, and in man, it is composed of two lateral masses, consisting of different plates, in the middle between which a swelling is seen called the *processus vermicularis*. In studying the cerebellum, I have been struck with the size of this part in some species ; for example, the cat, Pl. lxxv. fig. 4 ; the squirrel, Pl. lxxiv. fig. 4. In the former, this middle portion is seen to be composed of two masses quite distinct, Pl. lxxix. fig. 2 ; the one superior, *c*, and the other inferior, *c* 2 of the same figure. I do not believe that this middle portion of the cerebellum performs the same functions with the two lateral masses. One very certain fact is, that I have always found it very much developed in climbing animals, such as the cat, the squirrel, the marten ; and in those whose footsteps are very sure, or which are able to walk easily on sloping ground or on steep places. I have found the *processus vermicularis* very largely developed in the following animals : The wild goat (*chamois*), the goat (*chevre*), mule, ass, horse, and sheep. I shall not, therefore, be surprised if some relation shall be found to exist between the development of this part of the cerebro-spinal nervous system and the agility and security of the footsteps of animals. Some facts in pathological anatomy will perhaps one day throw light on this question, either in man or the inferior animals.—P. 242.



OBSERVATIONS BY DR F. J. V. BROUSSAIS, MEMBER OF THE  
INSTITUTE OF FRANCE, PROFESSOR TO THE FACULTY OF MEDICINE  
AT PARIS, &c. &c., ON THE ORGAN AND PROPENSITY OF  
AMATIVENESS. Extracted from his *Cours de Phrénologie*.  
Baillière, Paris and London. 1836. 8vo. P. 164.

Professor Broussais, after describing the cerebellum and the functions ascribed to it by Dr Gall, whose opinions he adopts, as being supported by his own observations, proceeds, on page 164 of his *Cours de Phrénologie*, as follows.

The *vivisectors*, or those who practise experiments on living animals, have considered the cerebellum as the regulator of muscular movements. They found on the assertion, that when the cerebellum is injured by cutting slices out of it, the muscular movements become disordered, the animal can no longer direct them according to its will, or according to the end which it appears to have in view; but similar results are obtained when sections are cut away from the base of the brain, towards the quadrigeminal tubercles for example; in a word, at the points where the principal nerves of muscular movement unite.

I acknowledge that I do not comprehend the meaning of the function ascribed to the cerebellum expressed in these terms—the *regulator of muscular movements*. In aid of what faculty does the cerebellum regulate these movements? Is it for the intellect, for the Will? This is not proved. The will regulates motion in the infant as well as in the adult, in the eunuch and in the entire man; and yet the cerebellum differs much in these different cases. I know that it may be alleged that it executes both functions; that, in point of fact, it diminishes in volume when it no longer excites to generation, but that it preserves sufficient dimen-



sions to regulate movements. I do not deny it to exert an influence on the voluntary movements, as will immediately be seen; I mean to prove only that this is not its sole function, and that it exerts a considerable influence on the act of generation.

Does it regulate muscular movements in reference to the fulfilment of its own functions? I believe that it does, under certain conditions. This kind of regulating power, then, would be nothing more than a prerogative belonging to it in relation to the generative functions; on this point I shall enter into some explanations. I have observed in some animals—in fowls, for example—that a blow inflicted on the nape of the neck, makes the animal draw back; makes it walk backwards sometimes for several seconds. Some diseases of the cerebellum produce the tendency to fall backwards. A young man, in whom this organ was affected, in consequence of solitary erotic excesses, felt himself inclined to draw backwards, and occasionally fell backwards. But do these facts prove that the cerebellum is the regulator of the muscles in all cases? They would rather lead us to think that this organ specially exerts an influence on the extensor muscles of the head, on those of the spine, those of the pelvis, and of the organs contained in it, which are seen to enter into great activity in the act of copulation; in short, on the muscles of which the organ makes the greatest use in executing its generative function. I can positively affirm, that the capacity for regulating the muscular movements, or manual dexterity, or dexterity of any kind, are not at all in relation to the cerebellum. I have positive evidence that men possessing a large cerebellum can be extremely awkward.

There is another organ, that of Constructiveness, which appears to exert a much greater influence over the regularity of voluntary movements, and to which manual dexterity appears to belong. We shall study it in a subsequent part of our course. The organ of Resistance may be mentioned also as conducing to this effect.



Let us, however, consider a little more at length the relations of the cerebellum with the muscular apparatus of locomotion.

The brain influences all the muscles. This is a fact proved in the most positive manner by pathology and pathological anatomy. An effusion, a rupture which takes place at the converging points of the fibres of one of the lobes of the cerebellum, produces as complete a hemiplegia as that which results from effusions occurring in the *corpora striata* and *optic thalami*, which belong to the brain; whence it evidently follows, that all the locomotive muscles are in communication, by their nerves, with the cerebellum. We have no reason, therefore, to be surprised, that the excitement of the cerebellum convulses them, and that the compression of the same organ paralyses them. Moreover, all the muscles assist more or less in the accomplishment of the act of generation, although some contribute to it more powerfully than others. It is necessary, therefore, that all of them without exception should be in connection with the brain.

Let us still add a few remarks, which may assist in throwing light on the question.

We assert that, in the normal state, the cerebellum alone is not competent to move the muscles. No fact is more certain; for if the brain do not act, or if it act incompletely, the movements necessary for the accomplishment of generation do not take place. It follows that the cerebellum alone is not sufficient to execute voluntary movements, and that to accomplish this object the intervention of the brain is necessary. It may be granted, that the cerebellum excites them and regulates them when the brain acts in concert with it, and the will assists. That it induces the brain to regulate them, with a view to the fulfilment of its functions, is a fact which may be observed in many of the domestic animals, both birds and quadrupeds, in which the movements connected with generation commence whenever the individuals of the other sex are perceived to be in a determinate attitude. Every one knows that even the pre-



sence of the female is not necessary to give rise to these movements, for they commence in the dog whenever we raise it up in a certain manner. It is clear that if his brain did not act, the cerebellum alone would not execute these movements. But if the brain assist, the cerebellum will not fail to regulate them after its own manner, under the conditions just mentioned; or rather to cause them to be regulated by the Will, on which it acts as an instinct. Let us add that, although the Will may direct these movements in man, we must acknowledge that they are first reduced to a determinate order by the cerebellum, because it is this organ alone which excites them in the lower animals, who cannot be taught by education, and in whom they cease to appear after castration.

I ascribe, then, the direction of the movements, the acts, the aptitudes, which have a reference to generation, to the influence of the cerebellum. But I maintain that these cannot be accomplished without the permission, nay, without the assistance, of the brain; and that it is in this sense alone that the cerebellum can be considered as the regulator of muscular action. Thus the brain always remains master of the movements of the cerebellum tending towards the act of generation, so as to be able to arrest them; and it effectually exerts this power, in a variety of circumstances, as every one knows.

You are aware that Gall has availed himself of a passage in an ancient Greek author to support his opinion on the cerebellum. But that passage is of no consideration in the present state of the science. We have much better methods of arriving at the truth. Gall has the honour of having established the proposition, as a positive fact, that the cerebellum is the primary organ of generation. In demonstrating this point, he proceeded by the method of direct or empirical observation,—that is to say, by establishing, by numerous facts, that persons in whom the posterior and inferior part of the head is much developed, and who have the cerebellum large, are more inclined to the ge-



nerative act, than those whose heads present a different development.\*

These observations of Dr Gall have been subsequently confirmed by all inquirers who have studied the subject with due attention, and especially with impartiality; and phrenologists possess considerable collections of skulls and casts which support them. The evidence is so conclusive, that the generative function is ascribed to the cerebellum, but without being able to affirm that it executes no other functions.†

Nevertheless, some individuals, who are opposed to Phrenology, maintain that the generative propensity has been observed very powerful in persons who had scarcely any cerebellum, or whose cerebellum had been destroyed, or in whom only the rudiments of it existed.‡ I do not know to what extent such alleged facts merits our confidence. For my own part, I declare that they will inspire me with none, until they shall have been verified by phrenologists.§ It is necessary to be on one's guard against facts which are attested only by the adversaries of a science, because it is well known to what extent the spirit of speculation may lead to falsehood in assertion. We exhibit collections of positive facts, and we daily repeat our observations. If some exceptions exist, we do not deny them, but set them down as points to be explained. It is not sufficient to shew us merely single cases; our opponents must make collections

\* It will be observed that the question here does not relate to any action exerted by the cerebellum on the muscles, but to an impulse communicated to the genital organs.

† I have stated in most of my works, the opinion that the cerebellum also exercises an influence on all the viscera. It is known that, when over-irritated, it excites vomiting, &c.

‡ It has not been said that these individuals were incapable of regulating their muscular movements.

§ See reasons assigned for this hesitation in a subsequent page of this work, in the section entitled "On the nature of the evidence by which the functions of different parts of the brain may be established,"—

TRANSLATOR.



in contradiction to ours, and the histories of the individuals must be completely authentic. This has not been done, and we are justified in doubting the truth of these assertions. I defy those who advance them to produce proofs,—I shall not say *superior* to those which we exhibit, but at all *equal* to them. For my own part, whenever, since I knew the system of Gall, I have been consulted by individuals who complained of the inactivity or infidelity of the generative organs, I have immediately directed my attention to the cerebellum, and I have always found it very depressed. When children have been presented to me, who, before the age of puberty, have manifested an extraordinary propensity towards the sexual act, and who have divined the process supplementary to it, I have always found the cerebellum very largely developed. This has never failed. I defy the opponents to produce pathological facts which can be weighed in the balance against those which I possess of this description.

Dr Gall had also remarked that diseases which irritate the cerebellum, maintain the organs of generation in a state of morbid excitement. Nothing is more true. M. Serres has made the same observation; and several other observers, worthy of confidence, have reported analogous facts. Other contradictory facts have been cited. It has been said that inactivity of the genital organs has coincided with other diseases of the cerebellum, such as scirrhus tumours and tubercles. Who does not know that chronic diseases cause organs to lose the power of exercising their functions? Even a smattering of medical knowledge is sufficient to be convinced of this truth. Thus, a disease of the cerebellum, which at first presented the characteristics of inflammation, and was attended by excitement of the muscles and of the functions of generation, may terminate in degeneration of the parts, which shall produce inertness in the organ, and destroy this excitement. The objections, then, are not of much weight. Besides, without forcing facts, without straining our ingenuity, without hunting for sophisms,—who does



not know that a moderate excitement of the brain exalts the movements of the moral and intellectual faculties, and that a more powerful excitement, which induces congestion, paralyzes them? Why will it not be allowed that the cerebellum, when excited to a certain extent, may increase the activity of the genital organs, and that, at a later stage, when congested to excess, it may paralyze them. This should be the case; and, indeed, cannot be otherwise. But the adversaries of a doctrine take advantage of all facts which appear to them to be capable of shaking its credit.

It has been said, also, that extraordinary and morbid excitement of the genital organs has taken place in coincidence, or in connection, with diseases of the spinal marrow. I can easily conceive this to be the case; because the nerves which communicate feeling and activity to the generative organs do not proceed directly from the cerebellum to them; they descend through the spinal marrow. Why, then, if the spinal marrow be irritated, should these nerves not excite the genital organs? This is very conceivable, and we see proofs of it in the interesting work of M. Olivier d'Angers, as well as in the experiments of Dr Ségalas, who has produced emissions in guinea pigs, by irritating the spinal marrow in the lumbar region. A nervous trunk, when irritated, produces excitement in all the parts on which the nervous filaments proceeding from it are ramified.

Thus it appears that all these objections are of very little importance, and can do nothing to invalidate the results of direct observations, which are constantly repeated.

Dr Gall has been so bold as to hazard the statement that the development of the genital organs exercises no influence on their activity; that they may be inert although very much developed, if the cerebellum be very little so. At first I refused to admit the correctness of this fact; but experience has proved to me that it is true. I have ascertained the inertness of the genital organs in man, although very considerably developed; but the cerebellum was depressed. I have ascertained also the contrary, in opposite



conditions; in such a manner that I can no longer doubt. I assure you that it has not been from rashness, nor without reflection and numerous observations, that I have ventured to take up the defence of Phrenology. I have multiplied observations, as far as it has been possible for me to do so, before entering the lists of its defenders.

We must, however, consider the manner in which the cerebellum acts in executing the function of generation, which appears to be its principal function. Perhaps Gall has scarcely entered into sufficient details on this subject. I shall attempt, so far as my limited ability will permit, to supply this defect.

The cerebellum certainly does not form the *ideas* connected with generation: these belong to their proper organs (in the anterior lobe of the brain). This fact is very observable in the lower animals. Many of them, whose ideas are very obtuse, nevertheless present a very powerful instinct of generation. Erotic ideas, then, must not be ascribed to the cerebellum; it is by the manner in which the latter excites the brain that it calls up these ideas in man; it keeps them up, because a relation has gradually become established between it and ideas of that kind. When the perceptions of the senses have arrived at the intellect, these perceptions become associated with the cerebellum in proportion as this organ is increased in growth, in the same manner as other ideas associate themselves with other feelings.\*

\* This portion of the text will probably appear obscure to readers who are not acquainted with Phrenology. It may be thus elucidated. According to Phrenology, the Intellectual faculties alone form ideas, and they are manifested by the anterior lobe of the brain. The *feelings* are connected with the posterior and middle lobes; and the intensity of each feeling bears a proportion (other conditions being equal), to the size of its own special organ, and not to the size of the intellectual organs. Hence, if lascivious objects be presented to the eyes, or addressed to the ears of two different individuals, they may see them, and hear them, and understand them, with equal precision, by means of their intellectual organs, if these be equal; but if, in the one, the cerebellum be extremely deficient, the objects will excite in him a very feeble sexual emotion; while, if the cerebellum be very large in the other, they will make a deep



In a former lecture I have given some explanations on this point. Do not, therefore, regard the cerebellum as the immediate seat of sexual ideas, but as the organ which excites these ideas, and as a means of keeping them up and of recalling them.

The cerebellum appears to be the primitive organ which excites the organs of generation to secretion and erection at the development of puberty. The cerebellum begins to grow large before the organs of generation develop their powers. Thus, it is the cerebellum which puts them in action, which impels them to develop themselves, and which, in short, excites them to the secretion of a prolific fluid and erection. But, at the same time, it excites the encephalic apparatus. It acts, then, in two directions; for the ideas relative to generation assume an entirely different colour, when the cerebellum is developed, from that which they

impression, and call forth a lively interest in him. In this case, the ideas or conceptions of the objects (being equally clear in both individuals) depend on one set of organs (the intellectual), which they possess in an equal degree; while the emotion (differing so widely in intensity) depends on another organ (the cerebellum), in the size of which they differ; in other words, it is not the cerebellum, but the anterior lobe, which forms ideas. I have found these views confirmed by many facts. For example: those individuals in society whose delicacy is most sensitive, when allusions are made to the sexual feeling, are *not* those who have a small cerebellum, and in whom the feeling itself is most weak. Such persons, although their intellect be good, are dull in apprehending these allusions when veiled; and when the expressions used are too distinct to be misunderstood, they appear to them to partake as much of the character of impertinencies as of indelicacies. On the contrary, when the cerebellum is large, and the corresponding feeling is strong, in combination with large organs of the moral sentiments, which furnish purifying and controlling motives, the persons thus constituted are *extremely sensitive* to indelicate allusions. They *divine them*, even when shrouded in the most studied ambiguity; they *understand their nature*, and could enjoy them, but that their moral feelings condemn them; and from this conflict and condemnation arises the *sensitiveness* of their minds in regard to such topics. When the cerebellum is very large, and the moral organs are deficient, there is a want of delicacy corresponding to the deficiency of the purifying and controlling powers. The individual is then very much in the condition of the lower animals in regard to this feeling.—TRANSLATOR.



previously possessed. It excites the sentiments and intellect. Moreover, the cerebellum receives excitement from the genital organs depending on the double cause which I have just mentioned, secretion and erection. There is a reciprocity of action between them. Thus the sexual excitement may begin by the imagination, or by the perception of the object, or originate in the organs of generation themselves. I must avoid entering into details on this point. In the last case, as soon as the organs are excited, they communicate a stimulus either to the cerebellum, which reacts on the brain, or to the brain, which in its turn excites the cerebellum.

The cerebellum is maintained in its normal degree of development by the continuance of the generative action. If the genital organs, the organ of secretion in particular, which is the foundation of this function, disappear, the cerebellum diminishes. Castration proves this fact: the cerebellum becomes depressed, the lower part of the head shrinks; while the other parts preserve nearly their original dimensions. When the bull is converted into an ox, the nape of the neck is observed sensibly to shrink. Nevertheless, this shrinking does not proceed so far as it does when castration has been performed before the development of the organs of generation and of the cerebellum; and the muscular system preserves a greater volume and more energy in animals which have been castrated after the evolution of the genital organs, than in those who have been mutilated before it. This is a fact which has been often observed, and which is now turned to account. If one wishes to have a strong horse, for example, it is usual not to subject him to castration until after he has been completely developed. In this case, the falling in of the nape of the neck never goes so far as if the operation had been performed before the evolution of his generative system, although it does take place to a perceptible extent. Hence it is clear that the cerebellum preserves its normal volume as long as the genital organs preserve their action; and that, when this action



is withdrawn, the cerebellum, and the muscles also, losing their activity, experience a diminution in volume.

There is another important fact, which has been turned to advantage on both sides of this argument. When castration takes place in man, after the complete development of the genital organs, he continues to form erotic ideas, while he entertains none, if the operation has been performed before puberty. Every one knows that in those countries in which their manners tolerate this species of mutilation, certain eunuchs continue to feel an inclination for the other sex, when they have suffered castration only after complete development. Those who cultivate Roman literature will recollect certain verses of Juvenal, too coarse to be quoted, in which he castigates the lubricity of the Roman ladies. He mentions that they did not mutilate the young men chosen for their baths, and with whom they intended to abuse themselves, until after the complete development of puberty. At this epoch, the cerebellum having acquired its full development, had modified the other organs of the encephalon in such a manner that erotic ideas did not disappear completely, and that erection could still take place without secretion.

Another physiological and pathological observation appears to me to merit attention. When erotic excitement has been long abused, it establishes in the nerves of the whole generative system, in those of the neighbouring organs, and even in all the lumbar and femoral muscles, a mode of action, accompanied by a kind of sensation which tends to pass from pleasure into pain, and which, for want of a better phrase, may be named *une volupté douloureuse*. This perversion of sensibility does not cease to make progress, and ends in a considerable diminution of the muscular vigour of these regions. The species of semi-paraplegia which results from it is generally incurable.

I shall now exhibit some specimens of the cerebellum. In this head (shewing a cast to his audience) there is an enormous development of it. The distance between the



two ears is prodigious, even disgusting and repulsive, from the resemblance which it bears to a brute. This man was guilty of several crimes against chastity in England, each more abominable than another, which forced him to go into exile. You will observe also that those parts (at the sides of the head) where the selfish propensities are situated, surpass in development all the other regions of the encephalon, and greatly exceed the intellectual organs in size. No controlling power opposed the action of the organs which produced his crimes. You have here (shewing other casts) examples of the cerebellum much developed in men who possessed distinguished intellectual faculties with elevated sentiments, and their high reputation proves that the generative instinct never led them to reprehensible actions. Here again is the head of a man who was passionately devoted to natural history, and curious in collections. He was very fond of women. You observe that the cerebellum is very much developed; but the higher faculties are very powerful, they were cultivated, and the honourable recollections which the *savant* has left behind him, shew that the organ of generation did not reign despotically over his conduct.

The same observation applies to the head of Gall,\* and to many others which it is unnecessary to present.

Here, again, is the head of Pigault-Lebrun, whose romances partake largely of the erotic quality, but, after all, are not gross. You see in this other head, the perceptive organs well developed, much Self-Esteem and Love of Approbation, in a word, organs capable of guiding the action of the cerebellum. I wish you always to study the organs in this manner, I mean in their mutual relations. I could greatly multiply these observations; but as we are obliged to take our examples from persons who are well known, to

\* The cerebellum was largely developed in Gall, and although the feeling did not subjugate his intellect, it produced irregularities of conduct which were reprehensible.—TRANSL.



inspire the greater confidence, one feels a delicacy in bringing too many forward.

I shall now shew you several examples of an opposite description. This head is that of a mathematician who felt aversion to the sex, and was never married. He has the reputation of having remained in the virgin state. Here is another whose condition was precisely the same. You may easily conclude that their actions were dictated by those regions, which you perceive to be predominant, the anterior and superior, and not by the posterior region, the depression of which is strikingly conspicuous.

I here shew you the head of Bontillier, a brigand. He was abandoned from the first to all sorts of vices, and at last degraded by the crime of parricide. You observe the enormous development of the posterior and lateral regions, and no adequate powers of control in the anterior lobe. He was besides uneducated. There is always a deficiency in the development of the controlling organs in those men who have been guilty of disgraceful crimes in relation to the cerebellum.

*Auxiliary Faculties.*—After these details, I shall notice the organs which favour the action of the cerebellum, and those which tend to repress it. I shall follow this course in examining all the other organs, wherever they admit of my doing so. The organs which favour the action of the cerebellum are those of the soft affections, of friendship and attachment, and especially those of the love of children, Imitation, the sentiment of Mirthfulness, Ideality or Imagination, a considerable activity of the perceptive organs, particularly of music and of benevolence. All these, to use a vulgar expression, lend strength to temptation.

*Antagonist Faculties.*—The feelings and intellectual faculties which act in a contrary direction to the cerebellum, are rage, hatred, cunning, and circumspection; because these last organs force men to reflect, and during reflection, the activity of the instinct of propagation diminishes. The organ of Acquisitiveness may be added; almost all misers are



indifferent to sexual pleasure. This is a remarkable fact. One of the greatest enemies of this function is shame, combined with a want of self-confidence, especially when these two are remarkably strong. Want of self-confidence produces much relative impotency; that is to say, impotency which exists only in certain circumstances, and which may occur with a sufficiently vigorous generative power. I assure you, without hesitation, that I have been much assisted by the system of Dr Gall in my diagnosis of Anaphrodisia (the want of the generative power). I easily recognise the individuals in whom the defect of power is relative merely, caused by the want of courage and self-confidence, combined with a large development of the organ of credulity (Wonder). Such are the men who are made to believe that their scrotum is tied (*qu'ils ont l'aiguillette nouée*), words which may be paraphrased by the expression, that they labour under *relative impotency*. The opposite conditions are not less striking for the observer; for the men who have abundance of self-conceit always act up to their means, whatever these may be. It is, therefore, not without cause, that coxcombs, presumptuous persons, and even fools, are renowned for their success with the fair sex. It is curious to trace the relations of these vulgar opinions to the anatomy and physiology of the brain.

On the other hand, and in regard to the influence of the intellect, it is not the less certain, that the generative power is enfeebled by the sustained exercise of reflection, by abstract researches into causation, and by meditation; by the excessive study of mathematics, and by all those kinds of labour which tend to call the nervous energies towards the organs of thought (situated in the anterior lobe). An excessive development of the organs of Order and Weight are not favourable to the functions of the cerebellum. Men extremely formal, regulated, and methodical, experience in these dispositions a kind of counterpoise, which preserves them from excesses to which, otherwise, this organ would prompt them. At the same time, you



will bear in mind, that the organ is sometimes so predominant that it overcomes all these obstacles; but unless it is so to a great extent, it may be guided by these other powers. The effect of these influences may be observed in both sexes, but more particularly in women, for whom, in general, celibacy is less distressing than for men. In a word, all the feelings which tend to egotism (according to Dr Sarlandière, who admits a group of this nature), all the faculties which dispose to reflection and meditation, furnish checks against the abuses of the organ of generation; while gaiety, dissipation, pride, presumption, imagination, and intellectual idleness, all contribute to increase its activity.

An extremely large development of the cerebellum tends to produce excesses, of which the following are the consequences: These excesses induce exhaustion of the nervous energy more efficaciously than the extraordinary activity of any other propensity, because the act of generation partakes to some extent of the nature of a convulsion. It weakens prodigiously the locomotive power, and at the same time the vigour of the intellectual faculties, in such a manner that it is one of those functions, the excess of which is the most prejudicial to the individual. A multitude of diseases are the results of its abuse, and especially convulsive affections, disturbances of circulation, and derangement of digestion. Its moral effects are disorder in affairs; for such excesses induce stupidity, or at least a deplorable indolence.

These consequences should not be lost sight of in education. Those young persons who feel themselves in danger of being overcome by this propensity, should betake themselves, in due time, to the corrective influences which we have pointed out, if they do not wish to expose themselves to moral and physical degradation, and abridge the course of their existence.

*Defect of Development.*—The effects of deficient development of the cerebellum may be mentioned. This defect weakens the benevolent affections: we have already remark-



ed that the malevolent affections tend to depress the generative functions. Defect of the cerebellum permits the egotistical sentiments to assume the ascendancy. The best judges of human nature desire always to see this propensity rather a little too vigorous than too weak. It then exercises a favourable influence on the benevolent affections. This fact is certain. Eunuchs afford evidence of it. The selfishness of these beings is proverbial. In our day, they do not act important parts in public affairs; but history presents us with examples of eunuchs who were ministers of state, generals of armies, in a word, distinguished personages. Almost always, egotism, coldness of heart, defect of benevolence, and jealousy, have characterized them.\*

\* The doctrine in the text appears to be rather vaguely expressed. The organ of Amativeness never produces, as its proper function, any other feeling than that of love between the sexes, and it is not, therefore, a generous or moral propensity. Its direct object is the gratification of the being who experiences its impulses. It exercises, however, an extraordinary influence in exciting to activity all the other organs with which it is combined; but there is no preference, in this respect, in favour of the moral organs. If it be large, and be combined with large moral organs, it will stimulate them into activity towards the opposite sex, and a young man will then become chivalrously kind, good, and brave, in every thing regarding women. Its influence will be the same in the case of the opposite sex. They, under the influence of a similar combination, will be kind and generous to men. The extraordinary interest which some women feel in boys, out of all reasonable proportion to their love for girls, arises from the same combination.

But if the same degree of the organ of Amativeness be combined with deficient moral, and large animal organs, it will stimulate the latter, and the unhappy possessor of this combination will become only the more sensual, brutal, selfish, and ferocious, the more he is governed by his cerebellum. This power of the cerebellum to excite the organs of the animal propensities, where these predominate, explains the atrocious murders which some men commit on the victims of their sexual passion, not only in the fury of storming besieged towns, but in cool blood. Many years ago, I saw a man and woman walking on the brink of the precipitous rock which forms the western front of Salisbury Crags, near Edinburgh. A few minutes afterwards I heard an alarm, and saw the body of the woman lying at the foot of the sloping bank below the rock,



*Depraved manifestations* of this propensity have much less reference to the predominance of the cerebellum than has been generally believed. This species of corruption is owing rather to the deficiency of the moral and intellectual faculties. It is favoured by bad education, by example, by the sequestration of the sexes. Certain other propensities may contribute to it, such as the love of children, for these affections are nearly related. The propensity, in some persons, to enjoy their own sex may also, according to some late observations, occasionally bear reference to the individual himself possessing several feminine qualities. It is suspected, for example, that the taste which prompts a man to substitute one of his own sex for a woman, proceeds from the predominance of some organs peculiar to woman, particularly in the passive object. In like manner, the analogous vice in females, supposes a woman who possesses masculine qualities by the configuration of her brain. These ideas are not my own. They were communicated to me by a phrenologist, an accurate observer, who will publish them by and bye. I am far from appropriating them. They are very interesting; because it is repugnant to reason to ascribe to the generative propensity, which has such a determinate and direct object, as the approach of the two sexes, disgraceful vices, that degrade men without realizing the object intended by nature.

The generative instinct acquires vigour by exercise, if the act has not been carried so far as to exhaust the strength and deteriorate the organs. It is weakened by inactivity. This fact is easily proved by observing Cenobites and all persons who live in celibacy. After having painfully resisted the propensity in youth, both sexes at last become entirely masters of it, to such a degree as scarcely to feel its existence at an age at which those who have

dashed to pieces. Her paramour had pushed her over the precipice, to avoid the consequences of his illicit love. He escaped down the slanting side of the hill, and was never afterwards heard of.—TRANSL.



been in the practice of exercising it moderately, still enjoy decided powers of reproduction.

These remarks accord perfectly with the previous observations which I made relative to castration. We state that the generative organ long maintains its activity in the male sex, when it is properly exercised, provided no organic affection exist in the *viscera*; while it decays in the opposite conditions.

I have treated of this function at considerable length, because it is very important, and phrenologists, in general ascribe to it a high rank. It is, indeed, the most indispensable of all, for it leads to the preservation of the species. P. 187.



ADDITIONAL CASES ILLUSTRATIVE OF THE FUNCTIONS  
OF THE CEREBELLUM, collected by the TRANSLATOR.

*Case in which the Sexual Propensity was experienced in  
absence of the External Organs.*

In addition to the cases cited on pages 69 and 70, the following is reported by Montaigne: "I have lately seen," says he, "a herdsman in Medoc of about thirty years of age, who has no sign of any genital parts; he has three holes by which he incessantly voids his water; he is bearded, has desire, and covets the society of women."—*Montaigne's Essays*, b. ii. ch. 30; *Cotton's Translation*, vol. ii. p. 612.

*Case of J—— S——, a Child of three years and two  
months old.*

On 10th December 1822, Mr James Deville of London sent me a cast of the head of J—— S——, of which duplicates are to be found in most of the Phrenological Collections. It exhibits a large development of the posterior region of the skull, and in particular, the circumference from the one mastoid process to the other, by the crucial spine of the occipital bone, is very great for a child of that age, indicating a large cerebellum. Mr Deville wrote as follows:

"I took the cast of the head of J—— S—— myself in May last (1822). He was then four feet seven and a-half inches high, and three years and two months old. He is a native of Sussex, and his age was authenticated by the minister of the parish where he lived, which certificate was shewn with him. His body and arms shewed great muscular strength, but his legs appeared weak. He also shewed great weakness when walking. At three years one month



old, he could lift two half hundred-weights, one in each hand, easily off the ground. His voice was very powerful, the strongest and loudest I ever heard; his intellect weak, and very childish in all his actions, preferring childish toys and amusements before youthful or manly. Upon a question to his father as to his amateness, he at first stated that he never shewed any symptoms of it; but on farther question he stated, *that he had erections during sleep, with emissions.*"

*Cases of large development of the Cerebellum accompanied by very powerful manifestations of the propensity of Amateness.*

The Phrenological Society possesses the cast of the head of an individual marked K——, in whom the cerebellum is very largely developed, in combination with large organs of the moral sentiments. When the cast was taken, the individual was living with his fifth wife, and in two cases less than six weeks, and in no instance more than four months, intervened between the death of one of his wives, and his marriage with the next. The great development of the moral organs accounts for his preferring to gratify Amateness in wedlock rather than by illicit indulgence.

The Rev. Dr I——, a clergyman of the Church of Scotland, who died some years ago, presented the same combination of a very large cerebellum with large moral and intellectual organs. He was repeatedly married, and the extremely short intervals which elapsed between the death of one wife and his union with another, brought some degree of scandal on his character. He stated confidentially to a highly respected member of the church, that the urgency of nature compelled him to resort to these precipitate alliances as the least of two evils.

The cast of the head of Dr Gall himself presents a large



development of the cerebellum, and he was considerably addicted to the indulgence of this propensity.

The Phrenological Society possesses the cast of the head of a criminal named Mitchell, who was executed for murdering a young woman whom he had seduced. The cerebellum, and also the organs of Destructiveness, are enormously developed, while the organs of the moral and intellectual faculties are very deficient.

The following case is reported in the *Phrenological Journal*, vol. v. p. 636.

*" To the Editor of the Phrenological Journal.*

"SIR,—Though I am by no means so full a believer in the details of the doctrines of Gall and Spurzheim as to entitle me to the appellation of phrenologist; yet as I deem it the duty of every man to contribute all he can, however small that may be, towards the establishment of truth, I beg to communicate to you the following case, which, if you think worthy of a place, you may insert in your *Journal*.

"The case to which I refer occurred in the Royal Infirmary here to-day. During the examination of the morbid appearances in the body of a woman who died yesterday morning, some eminent antiphrenological gentlemen were engaged (as has been the practice for some time past) in weighing the cerebrum and cerebellum, and in ascertaining their relative proportions; and the circumstance in this case to which I am now to call your attention is the correspondence of an immense cerebellum with a full manifestation, during life, of the functions which phrenologists ascribe to that part of the cerebral mass; and I doubt not you will consider the case the more valuable, as I go on the weights which I saw taken by these opponents to your doctrines.

"The following circumstances, by patient examination, I ascertained this afternoon at the house of the father of the woman, who resides in 31 Richmond Place.

"Till the age of fifteen she displayed no other disposi-



tions than those of an amiable, good-tempered girl ; however, she had not been much under the eye of her stepmother for two years previous to this age, from whom I got the following information :

“ When nearly sixteen years of age, she became servant in the family of Sir James Ferguson, and the first report of her conduct, made by the housekeeper to her stepmother, was, ‘ that she was in every respect a good servant, but that she seemed too fond of the society of the men-servants in the hall ;’ and before the end of a year she was dismissed the house in consequence of becoming pregnant, being not yet seventeen years of age.

“ Between this period and her twenty-first year, little that is precise is known of her conduct by my informant. At this time she was again admitted into Sir James’s establishment, at the recommendation of the housekeeper, who considered her ‘ a weak but good-hearted girl, who would be steadier as she grew older.’ Unfortunately, however, the disposition previously complained of was again manifested ; and, becoming a second time pregnant, was again dismissed the house.

“ She subsequently became servant to Mrs Johnston in Newhaven ; but of her conduct while here her friends know nothing, being determined to take no farther notice of her, except that during this time she had a third child. So convinced was her father of the indomitable nature of this propensity in her, that when requested to admit her into his house, he refused, declaring, that ‘ taking her in would be an inlet to a’ wickedness,’ and often wished ‘ she had died when she was born.’ He, and all his other children, I ought probably to mention, are decent and well-behaved people ; and from the accounts I received, never shewed this woman, in their conduct, any bad example.

“ Leaving Newhaven, she entered the service of a gentleman in Broughton Street, where for a time she appeared a mild good servant ; but at length her habits formerly complained of were discovered, and from this house she was



driven away in consequence of a fourth pregnancy. Soon after delivery she was admitted into this hospital, where she died, now in her thirty-second year.

“ The whole brain in this case weighed considerably less than the average weight of the brains of females. The cerebrum weighed only 2 lb. 10 oz. some grains, while the weight of the cerebellum without medulla oblongata or pons, was pronounced, with great surprise, by these gentlemen, to be no less than about  $5\frac{1}{2}$  oz., a size which they stated was not only proportionally but absolutely greater than any they had yet met with in the course of their investigations either in man or woman ; which coincidence, by the bye, we expected from the uniformly candid conduct of Professor C. would have been mentioned by him, as on a former occasion, to the students present.

“ The above are only a few of the prominent circumstances of this poor woman’s history, hurriedly drawn up while fresh on my memory, which, should they be considered insufficient to establish the coincidence in question, may be easily increased by examining any of the servants who lived with her in the families mentioned.

“ The forehead of this woman appeared, while she lived, low and sloping, while the greater proportion of cerebral matter seemed to be situated above and behind her ears.

“ Wishing all prosperity to the phrenologists, so far as their doctrines are founded on truth, I am, Sir, your most obedient servant,

W. G. D.

“ EDINBURGH, 3d May 1828.”

In the Phrenological Journal, vol. viii. p. 407, a case is reported in which the cast of the head of an uneducated man was sent to Mr James Simpson for a phrenological report on the dispositions which it indicated. Mr Simpson returned a written statement of his inferences, in which, *inter alia*, he says: “ The *animal* endowment is excessive ; and although the *intellectual* is very considerable, the *moral* is sadly deficient. The *Amativeness* is very great, and it is



scarcely to be expected that it has been restrained from coarse and selfish indulgence." The gentleman who sent the cast furnished remarks on Mr Simpson's inferences, in which, among other things, he says; "With regard to his amative propensity, every body acquainted with him knows that it is very great; he is, in fact, the slave of that feeling, and never speaks of a woman except in an animal point of view," p. 411.

It is an opinion very generally entertained, that the ardour of the sexual propensity bears a proportion to the temperature of the climate in which the individuals live, and not to the size of any particular organ with which the feeling is connected. Facts disprove this idea. Six real skulls, and also casts of six other skulls, all of Esquimaux, were presented to the Phrenological Society, on which Mr Robert Cox published a very able report in the *Phrenological Journal*, vol. viii. p. 289. I make the following selections from his remarks.

"The Esquimaux constitute the population of the frozen wilds of North America and Greenland. Previously to the recent expeditions of Ross, Parry, Franklin, Lyon, and Beechey, little was known respecting their dispositions and habits; and even yet, our information on many important points is rather superficial. Enough, however, has been recorded, to enable us to form a tolerably correct estimate of the general character of the race; and as the Phrenological Society has the fortune to possess twelve specimens of the Esquimaux skull, brought from the shores of Baffin's Bay, and other arctic parts of America,\* we have been enabled

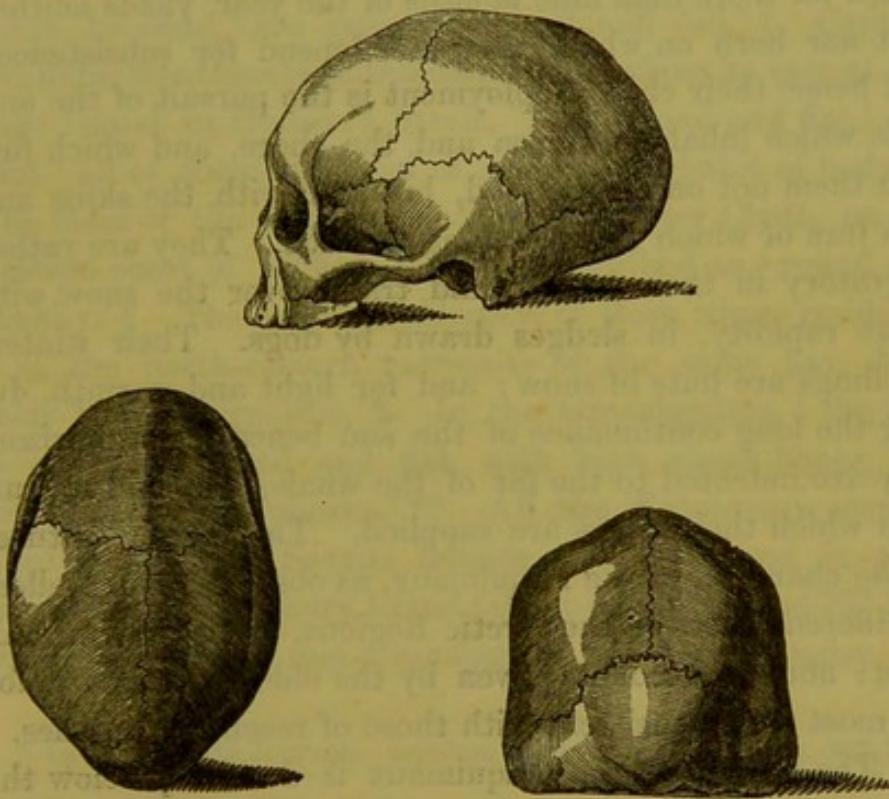
\* "Six of these are real skulls, and six casts. No. 1. was presented by Thomas Buchanan, Esq. of Hull; No. 2. by Thomas Turnbull, Esq. surgeon, Galashiels, who found it, in 1825, at Disco, an island on the eastern coast of Baffin's Bay; No. 3. (which was found in the snow by Captain Parry) by James Wardrop, Esq. of London; and No. 4. (from Baffin's Bay (Lat. 74° 20' N.) by James Hay, Esq. of Leith. No. 5. was procured at Hopedale, Labrador (Lat. 55° 31' N.) by Mr R. Morrison, and was presented by Sir G. S. Mackenzie. No. 6. was brought from



to obtain a pretty accurate idea of the cerebral configuration which prevails in those regions of the world.

“A striking uniformity of general appearance is presented by the skulls of the Esquimaux.\* They are long, rather narrow in proportion, and frequently of respectable magnitude. The coronal region is narrow, and slopes rapidly towards the sides of the head. The forehead also is narrow; the occiput protuberant.”

ESQUIMAUX.



Icy Cape, near Behring's Strait, by Mr Collie, surgeon of H. M. S. Blossom, who added it to the Society's collection. Four of the *casts* were presented by the Phrenological Society of London; but we have no information regarding the exact places where the originals were procured. Surgeons of whale-ships would confer a benefit on Phrenology, by bringing to this country such Esquimaux skulls as may fall in their way. Indeed, the Phrenological Society look upon crania from every part of the world as valuable acquisitions to their already extensive collection. Any from the South Sea Islands would be especially acceptable.

\* “This is remarked also by Blumenbach, respecting four Esquimaux skulls in his possession, to which more particular allusion will afterwards be made.”—*Decas Quarta*, p. 12.



“ The dimensions and development (published in the *Journal*, vol. iv. p. 365) of four skulls found by Mr Collie, surgeon of the *Blossom*, at St Lawrence Island, near Behring's Strait, closely resemble those above noted. The inhabitants of that island, as Mr Collie informs us, ‘ are evidently an Esquimaux race.’

“ The life which is led by the Esquimaux, when contemplated by persons accustomed to the comforts of civilized society, appears full of privation and hardship. The ground, frozen for more than nine months of the year, yields neither root nor herb on which they can depend for subsistence ; and hence their chief employment is the pursuit of the animals which inhabit the sea and the shore, and which furnish them not only with food, but also with the skins and rich furs of which their clothing is made. They are rather migratory in their habits, and travel over the snow with great rapidity, in sledges drawn by dogs. Their winter-dwellings are huts of snow ; and for light and warmth, during the long continuance of the sun beneath the horizon, they are indebted to the fat of the whale, seal, and walrus, with which their lamps are supplied. The leading features of the character of the Esquimaux, as observed by travellers in different parts of the Arctic Regions, are in general uniform ; and the accounts given by the older writers are, for the most part, consistent with those of recent authorities.

“ The stature of the Esquimaux is decidedly below the European standard. The tallest whom Captain Lyon ever saw, was five feet nine inches and three quarters in height, and the shortest only four feet ten inches. The highest woman was five feet six inches, while the smallest was four feet eight inches only ; between these, of course, there were intermediate sizes, all, however, inclining to the lowest scale.\* Their temperament, so far as it may be inferred from the published descriptions of their personal appearance,

\* Lyon's Private Journal. London, 1824, p. 307.



is decidedly lymphatic. Captain Lyon calls them a 'phlegmatic people,'\* and informs us, that 'even in the young and strong men, the muscles are not clearly defined, but are smoothly covered as in the limbs of women,' and that 'the skin in both sexes appeared to be, and was, quite smooth.'† The younger individuals among the Esquimaux described by Captain Parry, 'were all plump, but none of them corpulent; the women inclined most to this last extreme; and their flesh was, even in the youngest individuals, quite loose, and without firmness.' The faces of the Esquimaux, he adds, 'are generally round and full.'‡ Captain Franklin mentions that the Esquimaux whom he met at Savage Island, in Hudson's Strait, 'were broad and flat,' and that 'all of them appeared of a plethoric habit of body.'§ The faces of the natives seen at the River Clyde, on the western coast of Baffin's Bay, are described as 'round and chubby.'|| The *Arctic Highlanders* of Ross, whose residence is at the north-eastern extremity of the same bay, have their bodies corpulent.'¶ In the Greenlanders, 'the face is commonly broad and flat, with high cheek-bones, but round and plump cheeks.'\*\* All the Esquimaux seen by Captain Lyon at Savage Islands, 'were fat and in good case.'†† At Salisbury Island (about 63° 30' N. and 77° W.) the same voyager met a number of 'boisterous, noisy, fat

\* Lyon's Private Journal. London, 1824, p. 353. † Ib. 307-309.

‡ Parry's Second Voyage, 4to, p. 492-3.

§ Narrative of a Journey to the Shores of the Polar Sea, in the years 1819-22, p. 18.

|| Parry's First Voyage, 4to, p. 282.

¶ Ross's Voyage, 4to, London, 1819, p. 125.

\*\* Crantz's History of Greenland, translated from the High Dutch. London, 1767. Vol. i. p. 132. Crantz was sent from Denmark to collect information as to Greenland and its inhabitants. He resided about thirteen months in that country, in the years 1761-2. See also Egede's Description of Greenland, p. 118. (London, 1745.)

†† Private Journal, p. 40.



fellows.\* In Beechey's 'Narrative of a Voyage to the Pacific and Behring's Straits,' engravings are given of a number of the natives of the north-western angle of the American Continent—all indicating a highly lymphatic temperament. The same peculiarity distinguishes six Esquimaux delineated in the 7th plate of Captain Lyon's 'Brief Narrative;' and the plates which illustrate the publications of Ross and Parry, almost without a single exception represent the natives with that rotundity of visage which is generally the sign of a lymphatic constitution."

Mr Cox remarks, that the organ of Philoprogenitiveness (No. 2.) is very large in those skulls, and he adduces the strongest testimony of several travellers to the great warmth of their affection for their children. He then proceeds to observe, that

"Not much inferior in size to Philoprogenitiveness in the heads of the Esquimaux is the organ of the Sexual Propensity; and the strength of the feeling corresponds. Connubial fidelity is almost entirely unknown amongst them. 'It may be safely affirmed,' says Parry, 'that in no country is prostitution carried to greater lengths than among these people.'† Captain Lyon mentions that 'the women are treated well; are rarely, if ever, beaten; are never compelled to work; and are always allowed an equal authority in household affairs with the men. Though a phlegmatic people,' he adds, 'the Esquimaux may be said to treat them with fondness; and young couples are frequently seen rubbing noses, their favourite mark of affection, with an air of tenderness. Yet even those men and women who seem most fond of each other, have no scruples on the score of mutual infidelity, and the husband is willingly a pander to his own shame. A woman details her intrigues to her husband with the most perfect unconcern, and will also answer to any charge of the kind made before a numerous assemblage

\* Lyon's Brief Narrative of an unsuccessful attempt to reach Repulse Bay, &c. p. 129.

† Vol. v. p. 273. See also p. 300.



of people. Husbands prostitute wives, brothers sisters, and parents daughters, without shewing the least signs of shame. It is considered extremely friendly for two men to exchange wives for a day or two, and the request is sometimes made by the women themselves.\* Crantz informs us that the Greenlanders are inclined to licentiousness, and adds,—‘I have been assured that they can read the language of the ogling eye, unattended with the least concomitant mien or motion, better than the adepts in Turkey.’† These facts shew that the amative feeling manifests itself in every part of the world with an energy not materially affected by climate and temperature.”

*Cases in which the Cerebellum being small, the propensity of Amativeness was feebly manifested.*

On page 24, Dr Gall refers to the case of a French Abbé in whom the organ was small, and who manifested the propensity in a low degree. A fuller description of this case is given in the Phrenological Journal, vol. vii. p. 29, which was furnished by the late Mr A. A. Royer of the Jardin de Plantes at Paris.

“*The Abbé Lactoture’s skull.*—This Abbé lived at Vienna, where he took refuge during the French Revolution. He was much in the best female society. He was greatly liked by them, solely on account of the little polite attentions which he paid them, and of his natural talent for ladies’ works. He did not push his gallantry far, and very pretty women sometimes amused themselves by encouraging him; but on such occasions he shunned their society, and appeared to suspect their designs. He confessed that he was a stranger to the attractions of love, and was born with a natural chastity. Indeed, he was never known to have formed an intimacy with any woman, although he greatly

\* Lyon’s Private Journal, pp. 353–4. See also pp. 137, 168.

† History of Greenland, i. 192. A similar account is given by Egede, p. 139.



desired to please the fair sex, and dressed himself with much care. It was said he was of neither sex; an imputation which did not offend him, but made him laugh. Gall showed the head in relation to the organ of Vanity, and the smallness of the cerebellum, the organ of Physical Love. The organ of Benevolence is considerably developed, as is that of Mechanism. Attachment in friendship is very large, and, in combination with that of Vanity, explains the Abbé's desire of society."

The case of Lacenaire, who was executed at Paris in 1835, is reported in the *Phrenological Journal*, vol. x. p. 397.

"In most criminals the organ of Amativeness is largely developed, and abuse of the sexual feeling is generally one of the causes of their dissipation, and one of their incentives to crime. In Lacenaire the organ is, however, of very moderate size, and from the same authority\* it appears that he 'had very little inclination for women,' and that his dissipation consisted in drinking to excess."

The late Dr John Macintosh mentioned to me, that he had attended a woman of middle age in Leith, who was reported to him as having led a most dissipated life, associating, frequently in a state of intoxication, with the most abandoned characters. She died, and he examined her body. He was struck with the small size of the cerebellum, which was considerably under the average dimensions of the organ in women. At this time he believed that the woman had been a common prostitute, and thought that he had found a fact hostile to Phrenology. He resolved to ascertain her history as correctly as possible, and went to her brother, who was a respectable man, and who had been greatly distressed by her conduct. The brother assured Dr Macintosh that prostitution was almost the only

\* *Gazette des Hopitaux*, 1 Mars 1836.



vice of which his sister was entirely free, and that it had often been remarked as singular, that even when under the influence of intoxication, and living among the most abandoned women, she repelled the advances of men. Dr Macintosh returned and prosecuted his examination of the body, and found the hymen healthy and unbroken.

*Cases of Insanity connected with Development of the Cerebellum disproportionately large in reference to the organs of the Moral and Intellectual Faculties.*

It is a principle of Phrenology, that each organ tends to activity with a degree of intensity proportionate, other conditions being equal, to its size. Hence, if in any individual one particular organ, or any group of organs, greatly exceed all the other organs in point of size, there will be in him a predisposition to excessive manifestation of the corresponding mental faculties; and this excessive mental energy will be prone to degenerate into insanity, especially when any exciting cause is applied. "The influence of predominant development in giving a predisposition to disease in the organ, is very manifest on comparing a number of monomaniacs, or patients deranged on one point, with each other; for, as a general rule, the deranged faculty, or feeling, will be found to correspond with the most highly developed organ; and no one who has made accurate observation can have failed to notice the coincidence. Even in general mania, I have almost invariably found the mental disorder taking its character from the functions of the predominant organs. As illustrations of this principle, Dr Gall was in the habit of shewing the skull of a man in whom the cerebellum was enormously large, and the chief feature of whose alienation was to believe himself to be the husband of six wives; and he also possessed the skull of a woman in whom the organ of Philoprogenitiveness was extremely large, and who, in her ravings, fancied herself pregnant with five children. Dr Spurzheim mentions having seen many simi-



lar cases; and I have myself observed several, both in the Salpêtrière of Paris, and in the hospitals of this country; and, even in acute cerebral disease, I have, in several instances, seen the consequent mental affection consist in an exaggeration of the character indicated by the predominant organs. But, although this is the general rule, it must never be forgotten that a small organ *may* be in a state of morbid activity when a larger one is sound; and that a large organ may be in a state of atony, and its function be altogether in abeyance; as happens daily with the brain, considered as a whole, in the opposite states of delirium and dementia.”\*

The following case will illustrate these remarks. The subject of it was Robert Dean, a young man who was executed for murdering a child in 1818. A cast of his head was procured by Mr Donkin of London, after death, and is now to be found in the collection of the Phrenological Society. It indicates a large development of the cerebellum, and also a large development of Destructiveness. These were the sources of his crime. In him, however, there is also a large development of Philoprogenitiveness, giving the love of children—of Adhesiveness, producing attachment—and of Benevolence and Veneration. On the other hand, the intellectual organs, particularly those of reflection, are disproportionately small; and the controlling organs of Firmness and Cautiousness are also small. The facts of the case are recorded as follows.

#### *Case of Robert Dean.*

(*Extracted from the Caledonian Mercury of 18th October 1818.*)

“ On Friday evening last, a dreadful and most unaccountable murder was committed in Thames Street, Kent Road, London, by a young man named Dean, an engraver. The

\* Observations on Mental Derangement, &c.; by Andrew Combe, M. D. p. 106.



victim was a girl under five years of age, named Mary Albert, in the family of whose parents Dean was very intimate, and he had always *shewed the greatest fondness* for the infant. He had taken the little girl in his arms to a neighbouring shop, and bought her some apples; and soon after the infant was found with its throat dreadfully cut, and died in about half an hour. Dean absconded; but surrendered himself on Tuesday morning. He was brought by Myott, the watch-house keeper of St Andrew's, Holborn, before the Lord Mayor, on Tuesday morning, and the following circumstances were declared. The prisoner went to the watch-house about a quarter before five o'clock this morning, and said he wished to surrender himself. The officer asked why? and in what way? He replied. 'My name is Robert Dean, and I am the murderer of the child over the water!' He was asked whether he was serious and certain? He said 'Yes;' and that ever since he committed the deed, he had wandered about from place to place in a state of great agony of mind. He went as early as half-past seven o'clock yesterday morning to Bow Street, the neighbourhood of which he perambulated until the same hour at night, in the hope that some one of the officers would recognise him, for he had not courage then to surrender himself. He then renewed his wanderings, and scarcely knew in what direction he proceeded, until he found himself in Moorfields, where he entered the Scots Chapel, and heard a sermon, which had a strong effect upon him. After this he walked about the streets, until, weary both in mind and body, he determined on giving himself up to justice, and espying the watch-house, entered it for that purpose. He confessed most freely to all around, that he murdered the child Mary Albert, whom he adored. But it was not the infant who was his intended victim. Sarah Longman (a girl residing in Aldgate, near his lodgings), was the person whose life he had intended to take. She had disappointed him, and he prepared the knife to kill her. 'The devil, however, tempted him to act otherwise; and while he



held the child in his arms, he thus reasoned with himself— if I kill Sarah Longman, *she will have much sin to answer for*; but if I merely kill the child, the crime will not be so great, as she must be innocent.’ He instantly resolved upon the act, and having done it, he thought he beheld the devil in his court below, while blazing fires seemed to surround him.

“ He is an extraordinarily soft and inoffensive looking young man. He wept much, and occasionally looked wildly around him.

“ He was committed for trial.”

*Trial of Robert Dean at the Surrey Kent Assizes. Kingston, April 3. 1819.*

(*Extracted from the Edinburgh Advertiser, 13th April 1819.*)

“ *Crown side—Murder.*—Robert Dean was put to the bar this morning, charged with the wilful murder of Mary Ann Albert, a little girl between four and five years old, on the 16th October last. \* \* \*. The case was fully proved.

“ Mayott, a police officer, gave in a statement which the prisoner had dictated to him shortly after he was apprehended. It set forth, that he was rendered very unhappy by being forbid by her father all farther correspondence with a young woman of the name of Longman, to whom he had paid his addresses. In a state of despair he first thought of murdering Miss Longman, but considering that she might have some sins to answer for, he determined upon killing the innocent child, and he accordingly took the poor infant out and cut her throat. He afterwards ran about in a state of distraction, and at length surrendered himself.

“ The prisoner, who, during the course of the evidence above mentioned, appeared to be in a kind of idiotic stupor, being called upon to make his defence, merely said in a mild manner that he was not guilty.

“ The case went to the jury under the learned Judges’ direction, and the prisoner was found guilty.

“ Mr Justice Park pronounced sentence of death in an



impressive manner, and the prisoner was ordered for execution on Monday next.

“ During this awful stage of the proceedings, the prisoner exhibited a maniacal apathy to the doom that awaited him, and he was removed from the bar in a state of mental abstraction.

“ He was aged twenty-five years.”

*Execution of Robert Dean.*

*(Extracted from Caledonian Mercury of 17th April 1819.)*

“ On Thursday, Robert Dean, who was convicted of the murder of Mary Ann Albert, was executed on the top of Horsemonger Lane Jail. An immense crowd was collected to witness the execution. The unfortunate man had been at prayer with little intermission from the period of his apprehension. It was needless to recommend devotion to him. He was, on Wednesday night, visited by Lord Rocksavage and Mr Sinclair, both of whom prayed with him. He expressed much gratitude at their kindness. ‘ They came,’ he said, ‘ with Christian feeling to visit the poor wretch in his dungeon.’ Mr Mann, chaplain, had much conversation with him. Dean regretted that, as he was going to a place where there was no gnashing of teeth, he had it not in his power to take with him his beloved Sarah, who was now exposed to a wicked world. The chaplain endeavoured to induce him to speak of the little girl whose life he had taken away, and told him she would meet him in a better world. ‘ No doubt,’ said he, ‘ Christ who is now saving my soul is waiting for me; but I am sorry for poor Sarah, she is in a dangerous world.’ Mr Mann rejoiced to see him so full of penitence, and told him with what happiness his friends would hear that not a wish of escape from punishment had passed his lips, nor a murmur of complaint. ‘ Why should I complain,’ said he, ‘ conscious as I am that the change I am going to make is for the better. Where is now Voltaire?—in hell. Where is Tom Paine?—in hell. *God have*



*mercy upon them as he has upon me.* His general appearance was that of a maniac; but on all subjects he spoke rationally, although often incoherently. After the sacrament had been administered to him he appeared impatient to leave the world, and asked whether every thing was not in readiness for his journey. On being told by Mr Mann that some time was to be allowed for preparation. 'For preparation,' said he, 'who can say that I want preparation? Never was a man more ready to die.' Mr Mann having observed that the preparation of the body, not of the soul, was what he meant, Dean smiled. 'Oh,' said he, 'I shall then be going.' He refused to stand up while any part of the ceremony in the chapel was performing; and he frequently prayed aloud, and with great fervour. When the officers were striking off his irons, he looked wildly about, and at last fixing his eyes on the gallows, he bent towards it, and then gazed at the sky. The name of God was in his mouth when on the platform. He then said, 'God bless you all;' and prayed in so loud a tone, as to be heard by the crowd around the platform. At nine o'clock, the drop fell, and he died after a severe struggle."

As already mentioned, a cast of Dean's head was procured by Mr Donkin of London immediately after his execution. The reader is requested to inspect the cast, if possible; and to contrast it with the heads of men of amiable dispositions and vigorous intellect; the impression will be greatly stronger than can be produced by description. It is to be found in all the phrenological collections. In it the organs of the animal propensities, in large proportions, are combined with the two controlling organs of Cautiousness and Firmness and also the organs of Reflection in a state of great deficiency. Disappointment in love, aided perhaps by other causes, appears to have produced diseased action in the brain; and the different mental faculties are here seen acting like so many limbs of an automaton, when their different organs happen to be excited by external objects, those which are



largest always taking the lead. Thus Amativeness, and apparently Adhesiveness, excite Destructiveness, and Dean first resolved to kill Sarah Longman. The little child, however, fell accidentally in his way, and his Veneration and Benevolence seem to have started into activity in favour of this young woman: he would not kill her because "she would have much sin to answer for." Impelled, however, by the diseased energy of his large Destructiveness, he could not refrain from murder, but slew the infant, to whom nevertheless he had been previously tenderly attached. After giving scope to Destructiveness, his moral organs came into action, and he was overwhelmed with remorse, and gave himself up to the police. His subsequent conduct shews the continued diseased action of his various organs. He prayed fervently,—a manifestation of his large Veneration; he lamented Sarah Longman's exposure to a wicked world,—a manifestation of his Benevolence and Adhesiveness; he spoke of Voltaire and Tom Paine as in hell,—a manifestation of Destructiveness. Next moment he prayed to God "to have mercy on them,"—another manifestation of his Benevolence and Veneration. In short, his whole conduct is marked by the strongest indications of insanity, and I do not envy either the state of intellectual illumination or the feelings of the judge and jury who sent him to the scaffold as a criminal, rather than to an asylum as a lunatic. After a knowledge of Phrenology has reached the public mind, such an exhibition as this will be impossible. It is curious to observe the description of his state given by the reporter. "His general appearance was that of a maniac; but *on all subjects he spoke rationally*, ALTHOUGH OFTEN INCOHERENTLY." "Incoherent reason" is a species of reason certainly unknown to logicians!

The following case is reported in the Phrenological Journal, vol. ix. p. 521:

"On 21st October 1835, Mr Combe, accompanied by Mr D. B. White, Mr T. M. Greenhow, surgeon, Mr William



Harcastle, surgeon, Mr W. A. Mitchell, editor of *The Tyne Mercury*, Mr William Hutton, and Captain Hooke, visited the Asylum kept by Mr Wilkinson at Dunstane Lodge, two and a half miles from Newcastle. Mr Wilkinson is not a phrenologist. Mr Combe explained that, in cases of decided monomania, the character of the insanity generally has a perceptible relation to the development of the brain; and with the view of shewing that this is the case, he proposed to examine the heads of a few of these patients, and to write down his observations on them before any information was given of the particular affections under which each laboured. In pursuance of this purpose, several patients were introduced, examined, and commented on; one of whom was,

“ Patient W. A.—*Mr Combe’s Observations*: The cerebellum is very large; the whole region of the animal propensities is large; Cautiousness approaches to the very summit of the head; Conscientiousness is extremely deficient; and altogether the coronal region, which manifests the moral sentiments, is very deficient. The intellectual organs are not defective; Concentrativeness is rather large. The natural character will be bad; the dispositions low; and the love of women will probably be the characteristic feature of his insanity.—*Mr Wilkinson’s Remarks*: When brought here, he attacked the men in consequence of women being out of his reach; when restrained he was like a raging bull, and actually bit off part of his own thumb when he found that he was mastered. A very low and brutal character.”

*Cases of Disease in the Cerebellum accompanied by Affections of the Testicles.*

“ To George Combe, Esq.

“ Dear Sir, 31 *Buccleuch Place*, May 12. 1823.

“ In compliance with your wish, I with pleasure send you a note of a circumstance, that seems illustrative of Phrenology, and which was brought to my recollection by some ob-



servations made by you one evening at the Clyde Street Hall. I think it must have been in the year 1819 that I had under my care a young married man, whose general health was delicate, and whose appearance indicated a scrofulous tendency. Without any perceptible cause, an inflammation, and very considerable enlargement, of one of the testicles occurred, and before it had yielded to the usual treatment a similar affection attacked the other. The active stage of disease was in neither instance of long continuance, but both testicles remained tender, irregularly enlarged, and incapable of performing their natural functions. The individual in question became soon afterwards the subject of an acute general disease, under which he sunk. On examination after death, sacculated depositions of a caseous-like substance were found scattered through the enlarged testicles, while the lower portion of the cerebellum exhibited a singular pulpy appearance, which was quite inexplicable, as no symptom during life had led to the suspicion of that part of the brain being the seat of disease. At that time I had never indulged in phrenological speculations, and therefore merely considered this appearance as one of the morbid changes of structure connected with a scrofulous diathesis. I am, dear Sir, yours very truly, GEO. HOGG."

In the London Medical and Surgical Journal, vol. v. p. 649 (21st June 1834), Dr William Stokes of Dublin, after quoting several of the cases reported by Serres, mentions two which had fallen under his own observation, illustrative of the connection between disease of the cerebellum and excitement of the genital organs. "I have now seen two cases," he says, "in which this connection was observed. In the case of a young man who was brought into the Meath Hospital some time ago with paraplegia, it was observed that the penis was in a state of constant erection, and there were continual seminal emissions. On dissection, an effusion of blood was found in the cerebellum, and another in the hemisphere opposite the paralyzed side. There was



another case of a patient who was attacked with apoplexy and paralysis of one side, but with the unparalyzed hand he continued to attempt the act of masturbation, so that it was necessary to tie down his hand. On dissection, there were several effusions in the substance of the cerebellum. All these facts strongly go to prove the connection which subsists between the cerebellum and the generative function; and I think it would not be unsafe to make the diagnosis of disease of that organ in cases of cerebral disease, where the genital system was much excited."

In the sixth volume of the same Journal, p. 125 (23d August 1834) there is quoted from the *Revue Medicale* a statement, that, at a meeting of the Academy of Medicine, there was exhibited a compact tuberculous mass, of the size of a hazel nut, "taken from the cerebellum of a young man of nineteen years, who, in life, had been given to excessive masturbation, and was subject to continual erections."

*Cases of Disease in the Cerebellum, accompanied by excessive manifestation of the Instinct of Reproduction.*

An officer, says Dr Broussais, of the African army, became extremely addicted to sexual indulgence. He died of a febrile nervous affection, and Dr Baudens, a distinguished military surgeon, found in his cerebellum a concretion which weighed more than an ounce.—*Broussais, Cours de Phrenologie*, p. 785.

In the 5th volume of the Phrenological Journal, p. 311, Dr Fossati mentions the following case: "Professor Metaxa, a philosopher of much merit (at Rome), having heard that we regarded the cerebellum as the organ of Amativeness, communicated to me the case of a lady who to a mature age lived a quiet and regular life, and then gave way to extreme dissipation; shortly after which she was attacked by a disease of which she died. Dissection exposed supuration of the cerebellum."



In closing these cases, I beg leave to add, that I have private notes of the development of this organ, as observed by myself, by feeling the head in more than 150 individuals, of both sexes, and in all periods of life, and with whose natural dispositions I was acquainted, and, although restrained by the respect due to confidential communication from publishing them, I have no hesitation in declaring that they have produced in my mind the most insuperable conviction that the cerebellum manifests the amative propensity.

*Cases in which it has been affirmed that the Cerebellum was wanting, or extremely deficient, while the sexual feeling was strongly manifested.*

In Magendie's *Journal de Physiologie* for June 1831, a case is reported, in which it is said that the cerebellum was found on dissection to be wanting, having apparently been destroyed by disease; yet the patient enjoyed to the last the power of executing combined movements, and performed none of the evolutions described as the result of Magendie's experiments. The case alluded to, I may notice, is that of a girl named Labrosse, who was addicted to masturbation. It is reported likewise in *Ferussac's Bulletin* for October 1831, and has been proclaimed by the enemies of Phrenology to be utterly subversive of the science. Mr Cowan, however, a member of the Ethical Society in Edinburgh, stated to that Society, that he was in Paris at the time of the girl's death; that MM. Blainville and Spurzheim, and several other eminent physiologists, declared, that, in consequence of the imperfect history and anomalous nature of the case, no legitimate conclusion could be drawn respecting either the actual pathological conditions of the brain, or the connection of the lesion with the general symptoms; and this opinion is now entertained by some of the most able physiologists. Dr Caldwell, however (Professor of the Institutes of Medicine in Transylvania University, United States), has well shewn,



in the American Annals of Phrenology, quoted in the Phrenological Journal of this city, vol. ix. p. 226, that such a conclusion as is drawn by the opponents is altogether unwarranted. Although the cerebellum was found, on dissection, to be almost obliterated, the appearances were such as to indicate plainly that the obliteration was recent, and had been caused by inflammatory excitement of the organ—an excitement perfectly in harmony with the manifestations referred to. Dr Caldwell's remarks are as follows :

“ The subject of the case was a female child, born of a *sickly*, and *irregular*, if not *profligate* mother—‘ *usée par des excès de toute genre*’—and was herself deeply diseased from her birth. She became early addicted to self-pollution, and died near the close of her tenth year. On an examination of the brain, it was found that the cerebellum *was not entirely wanting*, but greatly diminished in size, and so changed in structure as to resemble a *gelatinous* membrane, surrounded by a large quantity of serum—‘ *une grande quantité de sérosité.*’—‘ *Je trouvai à la place du cercelet une membrane gelatinéforme, de forme circulaire, tenant à la moelle allongée par deux pedoncles.*’

“ This gelatinous membrane was doubtless the *remains of the cerebellum*, reduced to its then present condition by disease; and that disease was of a highly *excitive*, if not *inflammatory* character, calculated to throw the part prematurely into a state of preternatural action. Hence the vice into which the child fell at so early a period. The precocity and strength of the sexual propensity can be in no other way explained. And this explanation seems satisfactory. That an inflammatory or highly excited condition of the cerebellum awakens strong libidinous desires, is proved by hundreds of instances, occurring at various periods of life, from early childhood to advanced old age. That this is the pathology of *erotomania*, is proved, not only by the symptoms and successful treatment of that complaint, but also by dissections after death.

“ Are we asked, why we consider the cerebellum, in the



present case, to have been in a state of *chronic inflammation*? We reply, because it presented the effects of chronic inflammation. A superabundant secretion of serum is a common result of subacute inflammation of serous membranes, in every part of the body; and the structure of portions of the brain is known to be often reduced, by the same morbid condition, to a sort of *gelatinous* mass. Why should it not? We might almost ask, how can it be otherwise? The brain consists, in a high degree, of albumen, tenderly organized. Demolish its structure by subacute inflammation, and that substance shews itself in somewhat of a pultaceous or jelly-like form. Such is the condition in which the cerebellum of confirmed onanists has been found after death. Softened, and somewhat disorganized, by a constant state of high and unnatural excitement, that portion of the brain has assumed the appearance of a gelatinous mass, surrounded by serum or penetrated by it. Several dissections in the large hospitals of Paris testify to this. So does the late dissection of a case, reported in No. II. of the Journal of the Phrenological Society of Paris. And, from its extreme tenderness and imperfect organization, the brain of a mere child must be more easily reduced to a semi-fluid condition than that of an adult."

In the Annals of Phrenology, from which this extract is made, Dr Caldwell presents a summary of the evidence in favour of the doctrine that the cerebellum is the organ of the sexual propensity, which, before the publication of the present work, was the fullest exposition of this subject that had appeared in the English language. This extensive circulation of the facts will speedily bring all controversy on the subject to an end.

Another case of a somewhat similar description is reported in the Phrenological Journal, vol. xi. p. 78, as follows:

"*M. Magendie on Amativeness.*—'The region of the cerebellum is but moderately developed, a fact which is in con-



tradition of the doctrine of Gall, who placed, as you know, the seat of amateness in that organ; now the present subject, affected with this passion or instinct in a very high degree, should have presented a corresponding development in that part of the nervous system in which Gall supposed the instinct to reside. The only remarkable circumstance connected with this portion of the head is the excessive thickness of the skull where it covers the cerebellum. I am far from wishing to deny the possibility of any relation existing between the cerebellum and generative system; but, on the other hand, I cannot prevent myself from thinking that such relation is anything but constant, when I find in so many cases high irritation of the genital apparatus coinciding with atrophy or more or less destruction of the cerebellum. The back part of the brain, then, is, as I said, but slightly developed in the present case: on the contrary, the whole mass of the cerebrum presents a greater volume than we usually find at this age. The same purulent effusion, already noticed, may be observed on the superior and inferior surfaces of the cerebellum,' &c. (*Lancet*; June 24. 1837. *Report of Magendie's 22d Lecture on the Physiology of the Nervous System.*)

“*Note.*—The case here spoken of was one of ‘Nymphomania’ in a child aged only twelve years, who had become excessively addicted to practices such as phrenologists would suppose likely to be induced by morbid activity of the cerebellum. The passion was exhibited in a way which could not be called either morally or physically a healthy manifestation of the instinct, and from the state of the bone and the surface of the cerebellum, we may reasonably infer disease of some duration. Unlike M. Magendie, we regard the case as altogether countenancing the doctrine of Gall, a morbid manifestation of function being coincident with a diseased state of the organ; and to us it appears that such a manifestation was not incompatible with a small organ while in a state of unnatural irritation. Disease of organ and disease of function we should expect to find concomi-



tant; but why functional manifestations should be sometimes increased and at other times lost or enfeebled, by physical changes apparently almost the same, is beyond our power of explanation at present. It is highly instructive, in a psychological view, that Magendie should adduce the fact of 'irritation of the genital apparatus coinciding with atrophy or more or less destruction of the cerebellum' as evidence of the want of constant relation between them. He must be well aware that atrophy and partial destruction are frequently (the latter, always?) preceded by irritation of the organ."

It ought to be noticed at the same time, that the tendency to masturbation is not to be received as an unequivocal proof of the energetic activity of Amativeness. Where the impulse is towards the opposite sex, no doubt can exist. But experience proves, that in schools and institutions for the young, the vice of masturbation is often indulged in almost without an idea of the natural sexual function being entertained. The practice is taught by one or two bad subjects, and spreads throughout a school, partly from the children not being in the least aware of the nature of the mischief, and partly from mere companionship, and for the sake of the local titillation. In one instance, indeed, it was from a boy of a decidedly amative disposition being shocked by the practice, that its prevalence in a school was discovered; and cases have occurred, in both sexes, of its being indulged in, where Amativeness was neither large nor actively manifested in the dispositions. Often, however, masturbation does arise from an unnatural amative impulse.



ALLEGED CLAIMS OF REIL AND OTHERS TO DR GALL'S  
ANATOMICAL DISCOVERIES.

It has been much the fashion among the opponents of Phrenology to affirm, that Drs Gall and Spurzheim had borrowed much of their anatomy, without acknowledgment, from Reil, the celebrated German anatomist. This charge was first made by Dr Gordon, in the controversy which followed his attack on Phrenology, in the 49th Number of the *Edinburgh Review*; and although triumphantly refuted by Dr Spurzheim in his answer to Gordon, published in 1817, it is still occasionally brought forward by those who look only to one side of the question. No one would have been more astonished at this assertion than Reil himself, if it had met his eye in print.

In the 4th volume of Dr Gall's large work, "*Sur l'Anatomie et Physiologie du Systeme Nerveux en Général, et du Cerveau en Particulier*," at p. 378, the following extract is given from a publication by Professor Bischoff, who was acquainted with Reil and Loder, both of Halle, and both eminent as anatomists, and before all of whom Dr Gall dissected several brains. "The worthy Reil," says Professor Bischoff, "who, as a profound anatomist and a judicious physiologist, stands in no need of my commendation, has declared, in rising above all the littleness of egotism, 'that he had found more in the dissections of the brain performed by Gall, than he had conceived it possible for a man to discover in his whole lifetime!'"

Ignorance and prejudice have done so much in this country to represent Dr Gall as a mere ignorant and empirical adventurer, that justice to his reputation, and also to the rising generation of physiologists, renders it a duty to present the opinions of him entertained by some of his co-



temporaries, who had the best means of judging of his merits. The following testimonies were published in 1805 and 1806, and are reprinted by Dr Gall in the work just cited.

“Loder,” continues Professor Bischoff, “who certainly does not yield the palm to any living anatomist, has expressed the following opinion of the discoveries of Gall, in a friendly letter written to my excellent friend Professor Hufeland.

“Now that Gall has been at Halle, and that I have had an opportunity not only of being present at his lectures, but of dissecting along with him, sometimes alone, sometimes in presence of Reil and several others of my acquaintances, nine human brains, and fourteen brains of animals, I consider myself to be qualified, and to have a right to give an opinion regarding his doctrine.

“I have to inform you, then, since I am called on to explain my sentiments, that I am, to a great extent, of the same opinion with yourself, in so far as the organology is concerned, without, however, believing that there is any thing in it in contradiction with the anatomy; being, on the contrary, persuaded that it is true in its foundation and principles. There are still details to rectify, and the body of his doctrine is still (in 1805) too much in its infancy to be explicable in the manner followed by some persons who abuse it. It is evident, nevertheless, that the faculties of the soul and of the intellect, which are very strongly marked, may be discovered by indications on the skull. Ackermann of Heidelberg lent me the skulls of Schinderhannes and of six others of his accomplices: they are strikingly in harmony with the craniological indications of Gall. Gall gave such an exact description of the little H. of Jena (who drowned herself in the Saale after having committed several thefts), merely from seeing her skull, (which I had secretly procured, and which nobody supposed to be in my possession), that S., in whose house the little H. lived, was really astonished when I revealed to her the secret. These facts,



and many others which have occurred, are not the results of chance.

“ ‘ The discoveries of Gall in the anatomy of the brain are of the highest importance; and many of them possess such a degree of evidence, that I cannot conceive how any one with good eyes can mistake them. I refer to the great ganglion of the brain—to the passage of the corpora pyramidalia into the *crura* of the brain and the hemispheres—to the *fasciculi* of the spinal marrow—to the crossing of the fibres under the pyramidal and olivary eminences—to the recurrent fibres of the cerebellum—to the commissures of the nerves—to the origin of the motor nerves of the eyes, of the trijemenal nerves, of those of the sixth pair, &c.:—I pass over other points, which, although very credible, do not appear to me to be sufficiently demonstrated. These discoveries alone, would be sufficient to render the name of Gall immortal; they are the most important which have been made in anatomy since the discovery of the system of the absorbent vessels. The unfolding of the brain is an excellent thing. What have we not to expect from it, as well as from the ulterior discoveries to which it opens the way. I am ashamed and angry with myself, for having, like the rest, during thirty years, sliced down hundreds of brains as we cut a cheese, and *for having missed seeing the forest on account of the great number of trees which it contained*. But it serves no purpose to distress one's-self and to be ashamed. The better way is to lend an ear to truth, and to learn what we do not know. I acknowledge, with Reil, that I have found in Dr Gall more than I believed it possible for a man to discover in a lifetime.\*

\* The reader is requested to contrast the opinions contained in this letter, published in 1805, by an anatomist of the highest attainments, who had dissected, along with Dr Gall, nine human brains, and the brains of fourteen of the inferior animals, with the estimates of his merits, which I shall present in a subsequent part of this work, expressed by Dr Gordon of Edinburgh in 1815, Dr Roget of London in 1818, Sir Charles Bell in 1836, and to form his own judgment which is entitled



“ ‘ I do not mean yet to publish any thing on these subjects, because I wish to obtain the highest degree of evidence in regard to them, to point out the modes of proceeding most suitable to be followed, and perhaps even add plates to elucidate the facts. It is with this view that I have already examined ten human brains, and that I shall examine as many more as I shall be able to procure. I wish, besides, to compare several examples of brains of wild and domestic animals, of birds and fishes ; to inject delicately the veins and arteries of the brains ; to prepare several with alcohol, acids, and solution of corrosive sublimate, maceration, &c. ; and to write down my different observations. I hope then, in due time, to publish such a work as you expect from me.’ ”

“ It is thus that the estimable Loder thinks and writes. It is thus that he, who has devoted himself to the study of anatomy for thirty years, judges. His conduct proves, that true greatness consists in recognising the merits of others, and laying aside egotism for the sake of truth.”

At page 143 of the same work, M. Hufeland, before commencing his critical remarks, expresses himself thus concerning Gall :—

“ It is with great pleasure, and much interest, that I have heard this estimable man himself expound his new doctrine. I am fully convinced that he ought to be regarded as one of the most remarkable phenomena of the eighteenth century, and that his doctrine should be considered as forming one of the boldest and most important steps in the study of the kingdom of nature.”

“ One must see and hear him, to learn to appreciate a man completely exempt from prejudices, from charlatanism, from deception, and from metaphysical reveries. Gifted with a rare spirit of observation, with great penetration,

to the greater weight ; and also to say whether it be not time that the young physiologists of this country should at last emancipate themselves from the trammels of prejudice, which these able but blinded men have industriously woven around them.—TRANSLATOR.



and a sound judgment,—identified, as it were, with nature,—become her confidant from a constant intercourse with her,—he has collected, in the kingdom of organized beings, a multitude of signs of phenomena, which nobody had remarked till now, or which had been only superficially observed. He has combined them in an ingenious manner,—has discovered the relations which establish analogy between them,—has learned their signification,—has drawn consequences and established truths, which are so much the more valuable that, being based on experience, they emanate from nature herself. From this kind of labour has arisen his manner of looking upon nature, and the relations and functions of the nervous system. He himself ascribes his discoveries solely to the circumstance of his having given himself up ingenuously and without reserve to the study of nature,—following her in all her gradations, from the simplest results of her productive power to the most perfect. It is an error, therefore, to give this doctrine the name of a system, and to judge of it as such. True naturalists are not men to form systems. Their observations would not be sufficiently accurate if they were prompted by a systematic theory, and realities would not always square with the narrow limits of their notions. Hence the doctrine of Gall is not, and cannot be, according to the opinion which he has expressed of it himself, any thing except a combination of instructive natural phenomena, of which a part consists at present only of fragments, and of which he makes known the immediate consequences.”

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HAVING now presented the evidences on which phrenologists found their opinions regarding the functions of the cerebellum, I shall proceed to lay before the reader the objections which have been urged against the truth of Phrenology itself by several of its ablest opponents, Rudolphi, Roget, Prichard, and Tiedemann. To prepare the reader, however, to form a sound judgment of the validity of their objections, and of the answers which have been made to them, I beg leave to offer some remarks on the nature of the evidence by which the functions of the different parts of the brain may be successfully ascertained. They are reprinted from the *Phren. Journ.*, vol. x. p. 556.

ON THE NATURE OF THE EVIDENCE BY WHICH THE  
FUNCTIONS OF DIFFERENT PARTS OF THE BRAIN MAY  
BE ESTABLISHED; BY GEO. COMBÉ.

In perusing the remarks of physiological authors on the functions of the brain, every one must have been struck with the great importance which they attach to the experiments of Flourens, Majendie, and others, and with the contempt with which they have regarded the observations and reports of phrenologists on the same subject. The preference which they have shewn for the experiments alluded to is probably owing to their having inconsiderately committed themselves against Phrenology, whence has arisen in them a desire to discover the functions of the different parts of the brain by other than phrenological means, so as at once to wipe off from themselves the stain of ignorance, which they cannot conceal, and to deny to the phrenologists all merit in accomplishing this end. The same feeling makes them shut their eyes and ears to the evidence which phrenologists place before them. Their rejection of it, however, is not an act of their understandings, but arises from a revulsion of their self-esteem at the pretensions of a class of



men whom they have ridiculed and condemned, to instruct them, even by the humble method of reporting facts in nature which they have observed.

While, however, these feelings unquestionably operate in many of the more advanced physiologists, there is a younger class of inquirers who perceive that the experiments alluded to, have not accomplished the ends for which they were instituted, and who, less hostile to Phrenology, are willing to embrace truth, by whomsoever presented; but who, from the inherent difficulties of the question, are at a loss to decide on the real merits of the experiments, and on the value of the cases reported by phrenologists. The following remarks are offered to assist this class of inquirers in forming a judgment for themselves.

The established practice with physiologists is, to cut through, or cut away, different nerves and different portions of the brain in living animals, and to observe the results. The experiments are first made and announced by one experimenter, such as Flourens, Majendie, or Sir Charles Bell; they are then repeated by several other inquirers; and if all obtain the same results, the facts are generally received as established physiological science. If contradictory reports be made, further experiments are resorted to; and belief is suspended until a strong body of concurring testimony appear in favour of one conclusion. In some instances this method appears adequate to attain the end in view. When Sir Charles Bell cut the root of a motor nerve, and saw that the power of motion was lost in the muscles on which it was ramified, and when he cut across a nerve of sensation at its root, and observed that sensation was lost, the evidence of the functions of these nerves was complete. But four conditions are necessary to the success of this method of investigation:—*First*, The part destroyed must be a distinct organ with a specific function; *secondly*, The part injured must be such that it can be cut without necessarily involving the disorder of the functions of a variety of other parts; *thirdly*, The functions of the organ to



which the cut nerves are distributed must be known; and, *fourthly*, After the operation, the state of these functions must be completely within reach of observation. These conditions were present in Sir Charles Bell's experiments in irritating or cutting roots of the nerves of motion and sensation. For, 1st, These nerves were distinct organs, each having a specific function; 2dly, It was possible to cut a branch of the fifth pair, or a root of a spinal nerve, without involving the functions of the nervous system in general in derangement; 3dly, It was known that the muscles manifested voluntary motion and sensation; and hence, when one of these powers was suppressed, it was possible to distinguish its absence; 4thly, The muscles on which the cut nerves were ramified were so much within reach of observation, that they could be forced into action or sensation at the will of the experimenter, and hence he could discover what effect had resulted from his operations.

When, however, Flourens proceeded to cut out, in living animals, the cerebellum and different parts of the hemispheres of the brain, these conditions were wanting. For, 1st, He could not say whether the parts were or were not distinct organs, executing specific functions; 2dly, These parts could not be laid open and cut away without involving the functions of the nervous system generally. This proposition is now admitted by Sir Charles Bell and many other physiologists. 3dly, He did not know beforehand what mental power the part destroyed manifested, and he could not therefore judge of its suppression; and, 4thly, The animals in whom the cerebellum and parts of the convolutions were destroyed, were not, after the operations, in a condition of health, or placed in external circumstances calculated to shew whether they were or were not capable of manifesting any propensity which might be connected with the injured organs. There is not a shadow of evidence, for example, that these creatures manifested the propensity of Amativeness *after* the cerebellum was destroyed. Yet, if our doctrine be correct, that this feeling is connected with that organ, the suppression of the manifestations after the abstrac-



tion of the organ, might, according to sound principles of induction, be viewed as the direct result of the destruction of the organ.

The experimenters proceeded on the assumption that nothing was known concerning the functions of the cerebellum and of the cerebral lobes, and yet they expected to discover the functions of these parts by observing the powers which were *not* manifested when they were destroyed. The reasonableness of this expectation may be judged of by a short analogy. Suppose that an instrument capable of emitting an unknown number of sounds by means of an unknown mechanism, were presented to one of these operators, and that his object was to discover, by experiments, what sounds it was capable of producing, and by what precise pieces of machinery each sound was emitted. Imagine that he opened its covering, and seeing a number of wheels and springs, he, at random, broke two or three of them, and that he then set the machine agoing. If it refused to emit *any* sounds, he would discover that he had destroyed it all. But if it still emitted twenty or thirty sounds, how could he tell *what sounds were wanting*, when he did not know the *whole* originally within its compass? And how could he discover, by this silence, the particular sounds which the broken wheels and strings were calculated to emit when entire? Yet this is the precise condition in which the experimental physiologists stand in regard to the faculties of the mind and functions of the brain. They do not know what propensities, sentiments, and intellectual powers the mind is capable of manifesting in its entire compass, and they do not know what particular powers are manifested by each particular part of the brain; they therefore proceed to discover unknown faculties, by destroying at random convolutions whose functions are unknown! This is precisely like breaking the strings of an unknown instrument to discover the notes attached to these strings. The philosophical maxim, *Ex nihilo nihil fit*, is set at defiance; for they destroy the organ, and expect that, after it is destroyed, it will reveal to them its function. To bring into clearer



light the inadequacy of this method, we may suppose that the physiologist is presented with a machine capable of emitting, not an unascertained, but a definite number of sounds, say thirty-five, by means of thirty-five distinct strings, every one of which is visibly separate from the rest. It is clear that, even in this more favourable case, it would be necessary for him, in order to discover the sound emitted by each string, to proceed as follows: 1st, To cause the machine, while entire, to emit all its sounds, and to become so familiar with each that he could recognise its presence or absence with positive certainty. 2dly, When he cut a particular string, to cause the machine to emit all its sounds so far as it retained the power of doing so, in order that he might judge *what* sound was now wanting:—This would be indispensable before he could have sufficient reasons for inferring that any particular sound depended on the string which he had destroyed. If he were not acquainted with all the sounds, each by itself, when the machine was entire, he could not tell which sound was wanting when a particular string was broken. Or, if the machine were so constructed that he could not break one string without rendering several more mute, he could not discover which of the sounds which were now wanting was connected directly with the string which he had cut, and which were only incidentally involved in its fate. And, 3dly, If he had it not in his power, after having cut one particular string, to cause the instrument to emit all the other sounds which it was still capable of emitting, he would, if possible, be still more completely obstructed in his attempt to discover the particular note attached to the particular string on which he operated, for he would want the first element for observation, the presence of sounds to be compared with the strings which remained entire. If the broken wire gave only a few discordant jingles, he might be sadly in error, if he imagined these sounds to be its proper notes.

The only way in which he could hope to succeed by this method would be, *first*, by becoming familiarly acquainted



with each of the thirty-five notes by itself. *2dly*, By ascertaining that each string was so far independent of all the others that he might cut it, without impairing them. And, *3dly*, by placing the machine in circumstances calculated to elicit all its sounds at distinct intervals,—by making it emit them accordingly, and then observing which of the thirty-five was wanting.

When we apply this illustration to the case of the physiologists we perceive,

*1st*, That they are unquestionably ignorant of the character of each primitive propensity, sentiment, and intellectual faculty, which may be manifested by the mind. Hence, on seeing a certain number of manifestations, they cannot tell to what primitive powers they belong, nor how many are still wanting to complete the manifestations of the full catalogue of primitive faculties. In this condition of ignorance, they can never tell whether any particular power be the sole faculty suppressed or not, and therefore they cannot say that that particular power, and no other, depends on the part of the brain which they have destroyed.

*2dly*, They are avowedly unacquainted with the particular parts of the brain which manifest particular primitive powers. Hence, in cutting away portions of the brain, they may destroy half of one organ and half of another, or one entire organ and half of each of two others adjoining it. They are not convinced that the organs are double, and do not cut away precisely the corresponding portions of the brain in the two hemispheres. In their ablations they resemble an experimenter on the supposed machine who should smash a few wheels and strings at random, and then listen to discover what sounds he had rendered the machine *incapable* of producing. By their incapacity to remove precisely the two organs of any one faculty, and neither more nor less, they can never place themselves even in the condition of the experimenter, whose machine possessed thirty-five, easily distinguishable, strings, on each of which he could operate with a certainty that he was cutting only one at a time.



Their operations are really smashing wheels and strings at random, and then listening to discover what sounds shall *not* be emitted.

*3dly*, After having destroyed a particular part of the brain, they cannot make the animal manifest in distinct succession all its propensities, sentiments, and intellectual powers, which it may be still capable of manifesting. They cannot cause it to love its mate of the opposite sex, to love its young, to fight, to conceal, to fear, to build, to sing, to be proud, at their pleasure ; and if these powers were not manifested after the ablation, the legitimate conclusion would be, that they all depended on the portion of brain abstracted. As the same non-manifestation would follow from cutting out a great variety of parts of the brain, the conclusion would, by this method, be reached, that all of these powers depended on each part cut away in succession, or that each part manifested all the faculties.

*4thly*, The physiologists do not pretend that they can cut out particular organs from the brain without impairing the functions of other organs. It is impossible, therefore, to compare particular manifestations lost with particular parts cut out.

For these reasons, while I admit the competency of experiments by vivisection to discover the functions of the nervous system, where the four conditions before described exist, viz. *1st*, Where the part cut is a distinct organ ; *2dly*, Where the part can be cut without seriously involving other parts ; *3dly*, Where the functions of the organ on which the cut nerves are ramified are known ; and, *4thly*, Where it is possible to compare the state of the function after the operation with its condition before it ; yet I deny its competency to lead to any valuable results, where these conditions are wanting ; and I respectfully maintain that all of them have been wanting in the experiments performed by ablations of the cerebellum and of particular parts of the brain. I consider, therefore, that this method is fundamentally defective, unphilosophical, and unproductive, when relied on for disco-



vering the primitive faculties connected with particular parts of the brain.

These observations are all fortified by the following excellent remarks of Sir Charles Bell. "I have endeavoured to discover the truth by the examination of the structure, and the observation of the phenomena of life, without torturing living animals. It is too common a belief that, in physiology, experiments on living animals is the best and surest way of pursuing an inquiry, although it be certain that the supposed issue of experiments is as much affected by the preconception, as the process of reasoning can be. The experimenter on brutes is not to be called a philosopher merely, because he goes counter to the natural feelings of mankind; nor is he the more entitled to favour, that he gives a character of cruelty to the Medical Profession, thereby contracting its sphere of usefulness.\* *It is but a poor manner of acquiring fame, to multiply experiments on brutes, and take the chances of discovery. We ought at least to try to get at the truth without cruelty, and to form a judgment without having recourse to torture.* At all events, it is our duty to prepare for experiments upon living animals by the closest previous application of our reason, so that we may narrow the question, and make it certain that advantage shall be gained by the experiment."†

• "That I have known the best and most virtuous men hold a different opinion, I must allow. But I have not been able to suppress the expression of my sense of this matter, that dissections of living animals attended with protracted suffering must be wrong. I can affirm, for my own part, that conviction has never reached me by means of experiments on brutes, neither when I have attempted them myself, nor in reading what experimenters have done. It would be arraigning Providence to suppose that we were permitted to penetrate the mysteries of nature by perpetrating cruelties which are ever against our instinctive feelings. I am, therefore, happy in believing that the examination of the natural structure, and the watchful observance of the phenomena of life, will go farther to give us just notions in physiology than the dissections of living animals."

† An Essay on the Circulation of the blood, by Charles Bell, &c. 1819. P. 25.



When Dr Gall started in his career of discovery he was equally ignorant of the fundamental faculties and their particular organs, as the physiologists in general now are ; but his method of removing this ignorance did not involve self-evident absurdities and impossibilities. He met with living and healthy men who were capable of manifesting a great variety of faculties at pleasure, perhaps with the exception of one, say Tune. He met with others who had an instinctive facility in manifesting this faculty, but who were deficient in others. Here was a power so specific that its nature could not be mistaken, and here were individuals who were able and willing to manifest the faculty, so far as they had the power, as often as he pleased. On comparing their heads, he observed that the one had a particular part of the brain large, and the others had that same part small, and that the power of manifestation was in proportion to the size of the part. This case was the same as if Dr Gall had met with two self-acting and intelligent machines capable of emitting a variety of sounds by distinct strings, and had found that in one of them a particular string was very large and strong, and in the other that it was broken ; and had, by observing the notes which each emitted, discovered that a particular musical note was deficient in the machine whose string was broken, and vigorous in that whose string was large and sound. There is neither absurdity nor impossibility here. Men with particular organs deficient, such as Milne with Colouring, Haggart with Conscientiousness, are instruments having particular strings damaged, yet capable of sounding all their other notes ; while other individuals, in whom these same particular organs are very large, are like machines in which these strings are remarkably strong, and as they are intelligent and self-acting machines, and as a defect in one particular organ does not impair the others, we can induce them to sound their notes, and hence we may compare the power of manifestation with the size of the string until we are satisfied.

The circumstances which I have here mentioned shew



that it is in vain to expect that cases will ever be recorded of the artificial abstraction of particular parts of the brain, and the suppression thereby of particular powers, so as to produce a satisfactory physiology of the brain; and if physiologists will not condescend to resort to the observation of the size of particular parts of the brain, as indicated by the skull during life, and to the comparing of that size with the power of manifesting particular mental faculties, they must remain long uninstructed regarding the functions of the different parts of the brain. They have a great aversion to this method of proceeding, because they conceive it to be particularly liable to fallacies. There is the want, they say, of that precision which is so desirable in science. There is no measure of the size of an organ. It cannot be estimated in inches, nor by weight. Again, there is no standard by which to try the force of the manifestations. They therefore reject the whole method as empirical and unphilosophical, and incapable of leading to scientific truth.

We at once admit that the two elements in our method of investigation are both in their own nature *estimative*. We cannot accurately measure or weigh the size of particular parts of the brain during life; but we affirm, that if an observer possess an average natural endowment of the observing faculties, he may, by due practice, learn to *estimate* it with sufficient precision to lead him to positive conclusions. Again, we confess that we cannot measure the force of each manifestation of the faculties by ounces or inches, but we maintain, that, by proper instruction and the exercise of the understanding, we may *estimate* it also. Phrenology, in its evidence, rests on the same foundation as the practice of medicine. The existence of disease cannot in general be determined by weight or measure, and the characters of diseases can be judged of only by their appearances, or the symptoms which they present. The organs affected,—the degree to which they are affected,—and the extent to which medicines act on them, are all *estimated* by the exercise of observation and reflection on mere symptoms. In the prac-



tice of medicine—anatomy, physiology, and pathology, shed their lights to help the judgment in its estimates, but they do not reveal the theory of medicine *à priori*, nor do they render it a demonstrative science.

The same general laws of evidence must necessarily apply to the study of Phrenology. The mental manifestations are not ponderable nor measureable any more than the capacity for pain or pleasure, or the powers of hearing or sight, are so. We *estimate* the degree in which these susceptibilities and capacities are possessed by different individuals, and regard our knowledge as substantial; and we must of necessity learn to *estimate* the force of the mental manifestations by a similar exercise of observation and reflection, or remain for ever ignorant of mental science. Again, the differences between the forms of particular organs, and between their sizes when large and small, are so palpable that it is absurd to deny the possibility of distinguishing them in favourable cases; and, in proving a science, we are not only entitled, but bound by the dictates of common sense, to select the simplest and the most striking cases, the *instantia ostentiva* of Bacon, as best calculated to bring the truth to light.

It must therefore be by the exercise of observation and reflection, or by the practice of the method of *estimating*, that we shall discover the primitive faculties connected with particular parts of the brain, if we shall ever discover them; and it will be only after these discoveries shall have been made that anatomy, physiology, and pathology, will shed light on our path. Until we have followed this method, they are as little adapted by their own beams to reveal the functions of the different parts of the brain, as they are to unfold *à priori* the symptoms and best modes of treatment of diseases.

Those individuals, therefore, who object to the evidence on which Phrenology is founded and supported, appear to me not to understand the nature of the inquiry. In the phrenological books, there is as clear a specification of the localities and appearances of the organs, of the functions which they perform, and of the effects of their different de-



degrees of development in point of size, as there is in treatises on the practice of physic of the organs affected, and the symptoms which constitute particular diseases. The authors of medical treatises do not record all the cases, by which the propositions which they announce were first ascertained, and may be still traced. They assume that the inquirer has qualified himself, by previous study, for understanding and appreciating what they describe, and they refer him to the sick beds of the people for verification of their remarks. We teach our student how to observe, and refer him to the active theatre of the world, where he will find faculties manifested, and developments of organs exhibited, to an unlimited extent, and we bid him verify our observations there. We refer him to prisons and lunatic asylums, and to pathological cases reported by phrenologists, for evidence of excessive, of deficient, and of diseased, manifestations. The opponents, however, object to pathological cases reported by phrenologists, because they say they are interested in representing them in favour of their own views.

We may truly say, in this science, that every man who is not for us, is against us; and the objection might be urged, that we cannot trust to reports made by antiphrenologists, because they are interested in finding evidence to justify their opposition. But I go farther, and maintain, that the most honest *non-phrenologist* is incapable of reporting pathological cases calculated to establish the functions of the different parts of the brain. A *non-phrenologist* is a man who has not studied Phrenology, and who is ignorant of its details. Now, such a person does not know the primitive faculties of the mind, nor their modes of manifestation, and he does not know whether different parts of the brain have or have not different functions. He cannot point to one portion of the convolutions, and say this manifests such a power, and, when it is diseased, *this* power, and no other will suffer. He cannot say that it is an organ at all. In short, persons, ignorant of Phrenology, that is, of the functions, situations, and healthy manifestations of the mental



organs, are no better qualified to report accurately pathological cases of these organs, with a view to the elucidations of their functions, than a person would be to report pathological cases of the abdomen, who knew only in general that it contained the organs of digestion and assimilation, but without being aware that one part serves for chymification, another for chyfication, another for the secretion of bile, and a fourth for absorption, and so on. For these reasons, it is only phrenologists who are capable of reporting such cases, so as to give them a bearing on the subject. In the case of Mr N. (reported by Mr Craig in the *Edinburgh Medical and Surgical Journal* for October, and by me in the *Phrenological Journal* for December, 1836), Mr Craig, so far as can be discovered by his report, did not know that the function of a part of the posterior lobe of the brain, which he saw extensively injured, was to manifest combativeness, and, in consequence, he did not mention whether Mr N.'s temper was or was not affected by the disease of that part. In consequence of knowing the function of that part in health, I saw the importance of investigating this point minutely, and ascertained that the manifestations were as morbid as the organ. Again, Mr Craig reported, that Mr N. spoke ten, and knew four more languages; yet, although he had his brain in his hands, he did not report whether any particular part of it was large or small in concomitance with that great gift. Apparently he did not know, because he had not studied, where any convolution connected with that talent was to be met with. From previous study, I was informed that a certain convolution lying above each superorbital plate was regarded as the organ of a faculty for languages, and, in consequence, I earnestly observed its size, and was able to report that it was very large. I advert to this case, because it is fairly illustrative of my proposition, that a person who has not ascertained the situations of the different mental organs, and the manifestations which accompany them in a state of health, is not capable of reporting pathological cases of these organs



with success. We should estimate at a very humble value pathological reports on the organs of the thorax, made by a person ignorant of the separate functions of the lungs, heart, and bloodvessels, however high his general talents might be; and equally valueless and inconclusive will similar reports relative to the brain in all probability appear, when made by those who are ignorant of the uses of its different parts.

I therefore respectfully maintain, not only that the principles of investigation adopted by phrenologists are sound and adequate to attain the ends in view in employing them; but that there is no *other* method by which the primitive faculties attached to particular portions of the brain can be discovered.



ANSWERS TO THE OBJECTIONS URGED BY DR PETER  
MARK ROGET AGAINST PHRENOLOGY.

In the year 1818, Dr Roget published, in the Supplement to the Encyclopædia Britannica, an article under the title of "Cranioscopy," in which he brought forward a multitude of objections against the doctrines of Drs Gall and Spurzheim. These objections were answered by George Combe in his "Essays on Phrenology," published in 1819, and by Dr A. Combe in the Phren. Journal, vol. i. p. 165. A new edition of the Encyclopædia Britannica itself was subsequently published, in which the article "Cranioscopy" was omitted; and Mr Combe, in publishing the second, third, and fourth editions of his work, also omitted his answer to it. From this cessation of hostilities, phrenologists had conceived that Dr Roget no longer desired that his article Cranioscopy should be regarded as expressive of his more mature opinions of the merits of Phrenology, and, under this belief, they for many years abstained from regarding him as an active opponent.

In his Bridgewater Treatise, however, on "Animal and Vegetable Physiology," published in 1834, Dr Roget has again pronounced an opinion hostile to this science, and has referred to his previous article "Cranioscopy" as containing his views on the subject. He expresses himself thus: "Although the brain is constructed with evident design, and composed of a number of curiously wrought parts, we are utterly unable to penetrate the intention with which they are formed, or to perceive the slightest correspondence which their configuration can have with the functions they respectively perform. The map of regions which modern



phrenologists have traced on the surface of the head, and which they suppose to have a relation to different faculties and propensities, does not agree either with the natural divisions of the brain, or with the metaphysical classification of mental phenomena.\* Experiments and pathological observations, however, seem to shew that the hemispheres of the brain are the chief instruments by which the intellectual operations are carried on; that the central parts, such as the optic lobes and the *medulla oblongata*, are those principally concerned in sensation; and that the cerebellum is the chief sensorial agent in voluntary motion.”—Vol. ii. p. 566.

Dr Roget, therefore, apparently still abides by this article, and relies on it as sufficient to warrant not only his own condemnation of Phrenology, but its rejection by all whom his authority as a scientific author may influence. On this last account, it becomes indispensable, in the defence of truth, to republish the answers to “Cranioscopy,” that young inquirers, who have entered on the field of study since 1819, may be able to judge for themselves to what degree of weight Dr Roget's condemnation is justly entitled.

\* Dr Roget adds the following words in a note to the text:

“For a summary of the doctrines of Drs Gall and Spurzheim, I beg leave to refer the reader to an account which I drew up, many years ago, for the *Encyclopædia Britannica*, and which composed the article ‘CRANIOSCOPY’ in the last Supplement to that work, edited by Mr Napier.”



*Answers by George Combe to Dr Roget's Objections to Phrenology, reprinted from his Essays on Phrenology, published in 1819.*

We have still another formidable medical opponent in Dr P. M. Roget, F.R.S. and author of the article entitled *Cranioscopy*, in the Supplement to the *Encyclopædia Britannica*; and we must now, therefore, dedicate a few pages to the consideration of his observations.

We observe, *in limine*, that the titles which Gall and Spurzheim give to their science are Phrenology, when the philosophy of the mind is chiefly considered; and Physiognomy, when the outward development of the organs is principally in view. They attribute functions connected with the manifestation of the mind, to the *brain*, but not to the *skull*.

Their science consists in comparing the development of the brain with the manifestations of the mind. The name *Cranioscopy*, therefore, is gratuitous on the part of this author, and incorrect in itself. This circumstance would not be worth noticing, were it not the practice of some opponents to shew an ignorant contempt of the doctrines, by fabricating names which do not indicate the true nature of the subject.

This author, like every other anatomist, industriously keeps out of view the *principles* on which the true merits of the system must be decided, and which have been so often repeated. He collects only such superficial objections as have a tendency to delude without enlightening. He does not advance one idea of his own upon the subject; but sets himself to throw all manner of suspicion upon those of Gall and Spurzheim. In short, he is one of the "philosophers who darken, and put out—eternal truth by everlasting doubt." He says, "Let us, however, for the sake of argument, suppose, that the form of each organ within the skull, could really be ascertained by external examination of the head, shall we allow it to be an easy task to determine the



real character of the individual who is the subject of observation? Are we always able to discriminate between real and affected sentiment; or to mark with certainty, the operation of all the various motives which constitute the springs of action?"

No, certainly; Gall and Spurzheim nowhere maintain that we are "always" able to discriminate between true and affected sentiment; but they say that, if we observe long enough and close enough, it will be in our power to discriminate; and hence, that in every case where we can discriminate the true character, the comparison betwixt manifestations and development ought to be made, and the result stated, before the conclusion is drawn, that the manifestations observed by them were not those of the real character. Gall and Spurzheim say that it is possible, by close and accurate observation, to discover the true character; and it is so especially in children, who cannot effectually disguise their true feelings, and never conceal their talents. The author of *Cranioscopy* replies, It is not "always easy" to discover the true character; and hence, as it is possible to observe erroneously, he wishes his readers to draw the conclusion, that Gall and Spurzheim did observe erroneously, and that their conclusions are entitled to no credit. This reasoning cannot be praised as logical. The true conclusion appears to be, that if it be possible, by patient and enlightened observation, to discover the true character, the presumption is, that the true character was discovered by Gall and Spurzheim, till the contrary be shewn. If we are never to receive a statement as true, where there is a danger of its being incorrect, farewell science and philosophy; for both are founded upon statements which we must receive upon the credit of the philosophers who make them, and which it is impossible for each ordinary individual to verify to the extent of a thousandth part, by his own observations.

The author, however, proceeds thus: "Is the transient glance of a passing observer, sufficient for unravelling the



complex web of our affections, or unveiling the secret and tortuous recesses of the human heart, so as to assign to each principle its precise sphere of agency?"

No, certainly; Gall and Spurzheim nowhere affirm, that "the transient glance of a passing observer," is sufficient for such a task. Then, to what purpose does this observation tend? To throw a suspicion over their statements by a side wind, when the author did not choose to attack them manfully in front. It will be observed, besides, that the above sentence speaks of "the complex web of our affections" alone. Now, as formerly stated, it may be possible for men occasionally to disguise the motives of a single action, because nature has given them the power of doing the same act from various motives; for example, a person may give charity to a public hospital from ostentation, as well as from benevolence: but it is impossible, permanently, to disguise dispositions. The person who gives charity to an hospital merely from ostentation will not habitually do private acts of benevolence; and it is by observing the general tenor of a person's conduct that his true character can be known. Let it be observed, however, that it is impossible to disguise capacities, if the individual manifest them at all. He who has heard, for the most fleeting moment, the prodigious bursts of melody which flow from the throat of Catalani, cannot be deceived as to the fact of her possessing a great endowment of the faculty of Tune. Or he who has heard, but for five minutes, the splendid eloquence which flows from the lips of Chalmers, can have no doubt that he possesses the faculty of Ideality. These, then, are cases in which even "the transient glance of the passing observer," may compare manifestations and development together; and from such comparison just conclusions may be drawn.

The author, however, goes on to ask, "Can the most profound moralist, or acute metaphysician, pronounce with confidence, what are the natural dispositions of any human being, when these dispositions have been changed or modified, exalted or subdued, perverted or refined, by the force



of habit, education, example, and a multitude of other powerful causes, which, in the course of life, have moulded his intellectual and moral constitution? Can he trace them through the guise of falsehood, artifice, and dissimulation, which so commonly hide his real character from the world, and which occasionally deceive the eye of the closest and most vigilant observer?"

Yes, I humbly maintain, that the man of plain common sense, and still more, "the most profound moralist and acute metaphysician, can pronounce with confidence, what are the natural dispositions of any human being," provided he observe with moderate patience and intelligence. The Scripture says, "Can the Ethiopian change his skin, or the leopard his spots?" We say, Can any human being change his nature? And can education do more than exalt powers? Can it create them? But, as we have already shewn, mankind act every day on the belief, that they can discover the true characters of men, by observing their actions.

"Is it (proceeds our author) to the behaviour of a person who knows that he is watched; is it to the partial report of his friends; is it to the testimony of the individual himself, the most fallible of all, that the cranioscopist is to trust for his knowledge of human character? Such, however, is the kind of experience from which it appears, that all the doctrines relative to the functions of the different parts of the brain have been derived; and it is in this experience, as in an impregnable fortress, that the adherents of the system make their last and most resolute stand."

It is difficult to say, with what sentiments such unfounded assertions ought to be regarded. One is almost tempted, on reading them, to use some of the courtly epithets of the Edinburgh Review; but let us rather observe, that as Gall and Spurzheim's mode of philosophising is the only one capable of leading to any rational results in this branch of science, their observations, however imperfect and inaccurate, are entitled to more respect, from the very circumstance of their being made in the right way, than any opi-



nions which the author now quoted can be possessed of, if he never followed their mode of philosophising. He must, necessarily, be in a state of profound ignorance on the subject on which he writes.

Dr Spurzheim has said, "I again repeat, that I could here speak only of the results of the immense number of facts which we have collected. Several may complain of my not mentioning a greater number of these facts; but in reply, I need only answer, that were I to write as many books of cases as there are special organs, still no one could, on this subject, attain personal or individual conviction, before he had practically made the same observations. I may farther remark, that the detailed narrative of a thousand cases would not improve the science, more than that of a few characteristic ones, which state our meaning, and shew what is to be observed, and how we are to observe. Self-conviction can be founded only on self-observation; and this cannot be supplied by continually reading similar descriptions of configuration. Such a proceeding may produce confidence, but not conviction. This requires the actual observation of nature." (Outlines, p. 222.)

Now, the author of *Cranioscopy*, with this passage in view, goes on to say, "Quitting the airy region of Theory, they, that is Gall and Spurzheim, fancy themselves posted on a rock, secure against the insidious minings of scepticism, and bidding defiance to the rude assaults of argument. The appeal to the evidence of induction, as the supreme authority in the court of philosophy, is made with confidence; and all the wild effusions of a bewildered fancy, are presumed to be sanctioned by a supposed conformity with experience. You may speculate or reason, they exclaim, as you please; observation shews, that such and such forms of the head, are the invariable concomitants of such and such predominant dispositions and faculties."

But the author does not meet the observations of Dr Spurzheim by counter observations of his own. It is difficult, then, to perceive, how he knows that their doctrines



are the "wild effusions of a bewildered fancy." I regret much that he has not discussed the principles upon which the system is founded; for I should be glad to be informed by him, if it be possible to discover the functions of the brain, supposing it to be the organ of the mind, by any way except by comparing manifestations and development together; and whether a person, who has not made the comparison, can know any thing whatever on the subject. The author does not say, "I have compared manifestations with development, and found the results to be different from those which you state;" for, "this would have been taking a broad and liberal ground, stating it fairly, allowing what there is of truth, or an appearance of truth, and then asserting his own judgment by exposing what is deficient, and giving a more masterly view of the subject." But this would not have suited the author of *Cranioscopy*. He, therefore, finds an easier way of getting over the assertion of Gall and Spurzheim, that certain manifestations and a certain development go together.

He says, "Who will dare to set up his opinion in opposition to ascertained facts? We certainly pretend not to such boldness." What, then, does he pretend to do? "We shall venture only," says he, "to express doubts as to the reality of these facts, on which so much is made to depend; and to suggest the expediency, previously to any admission of their truth, of inquiry not only into the manner in which the knowledge of these pretended facts has been obtained, and in which inductions from them have been made, but also into the talents and qualifications of the observers, on whose testimony we received them."

Such inquiries may, no doubt, appear exceedingly proper, to persons ignorant of the first rules of philosophising. But, why not inquire at once into the facts themselves, when they are submitted to our own observation, as a more direct mode of coming at the truth, than an inquiry into the talents and qualifications of the persons who say they have discovered them? Is this like a philosopher? Gall and Spurzheim do



not affirm, that the facts were observed in the Moon, or in the interior of Africa, or in some place, or in some way, to which no person has access but themselves. If they had done so, then it would have been exceedingly proper to inquire into their talents and capacity for observing, before believing their statements; because, on the result of such preliminary inquiries, would have depended the character of the only evidence that could be obtained, and, of course, the credibility of their assertions. But, when they affirm that the subjects of observation are patent to the whole world, who have eyes to see and understandings to comprehend, and when they say, Compare manifestations with cerebral development, and you are at the bottom of the problem yourself; what need for inquiry into their talents and qualifications to observe? Surely the author of *Cranioscopy*, who thought himself qualified to refute their doctrines, could never doubt of his own qualifications to make the observations on which the system is founded. If so, why did he not make them? If he thought himself not qualified to observe the facts, that is to say, to become acquainted with the basis of the system, why did he attempt to refute it?

When Gay Lussac hears that Sir Humphry Davy has made a discovery in chemistry, and reads Sir Humphry's statement of the way in which it was made, does he begin by inquiring first, whether it be possible to make the discovery at all, seeing natural substances are "so changed and modified, exalted and subdued," by "a multitude of powerful causes?" And, after settling this point, does he, in the second place, proceed to inquire into Sir Humphry Davy's talents and qualifications as a chemist, and into his capacity to make the discovery, and then believe in it, or not, according to the result of this investigation? No man who knows the very first rudiments of philosophy would follow so absurd and preposterous a course. Gay Lussac would make the experiment forthwith himself, in the mode directed by the discoverer; and he would state the result honestly and candidly. If he found the discovery real, he would say so,



and give Sir Humphry his merited fame. If he found that he could not produce the result, he would repeat frequently his experiments; and if he could not at all succeed, he would then publish an account of his experiments, and of the results, and submit to correction if he had erred in his method; or, if he had followed the right course, and always obtained a different result, he would overthrow the alleged discovery. What should we think of Gay Lussac's refutation of Sir Humphry's discovery, founded on a metaphysical inquiry into the possibility of making it, and into the "talents and qualifications" of the discoverer? We should pity him for his ignorance of the rudiments of philosophy.

Now, I beg of the author of *Cranioscopy*, to tell me if the mode of verifying conclusions, said to be drawn from facts, be not the same in every science? Why, then, not in the science of Phrenology, alleged to be discovered by Gall and Spurzheim? Who would listen for a moment to a person pretending to refute a result in Chemistry, by inquiring into the character of the discoverer? Who would hear any man argue against a result said to be obtained by observing facts, if that person admitted that he had never even attempted to observe them? And, more especially, what should we think of his objections, if, at the very time he made them, he concealed, but did not dispute, that there was no way on earth of making the discovery, but the one which was said to have been followed; but which, nevertheless, he refused to follow himself? Should we not require him at least to demonstrate, beyond all possibility of doubt, that observation of the facts was impossible? And has the author of *Cranioscopy* done so? Will any man rest satisfied that he has demonstrated that the real character cannot be discovered by observing actions; or that the development of the brain cannot be observed?

But even allowing him to have proved, by demonstration, that the true character cannot be discovered by observing actions, could he not have compared actual manifestations with actual development, and stated the result? Let us



even allow to him, that men never manifest their true faculties, and that the true form of their brains cannot be discovered by observing their heads; yet surely they manifest some faculties, and their heads have some development. Now, if certain dispositions and capacities actually manifested, correspond uniformly with a certain actual development, it may be of little consequence whether the dispositions manifested be true or false, or whether the development be of the brain or of the skull. The fact is the only thing of importance; and may not the fact be as Gall and Spurzheim state, for any thing he knows to the contrary? In the whole of his article, he does not say that he has made a single observation; and yet he comes forward to refute the facts, by an inquiry into the "talents and qualifications" of those who observed them! If he has made observations, and found no concomitance betwixt specific powers and specific development, the statement of this fact would have been worth a thousand arguments; for it would have proved that the brain and the mind are not connected in the way that Gall and Spurzheim suppose; and of course, that their observations are absurd. But he never ventures thus far.

It appears impossible to dispute, that Gall and Spurzheim proceed in the right road to attain the end they profess to have in view. Unless, therefore, that end be in itself trifling and ridiculous, their success, however small in degree, must be a valuable addition to science; and their errors, however numerous, cannot be detected but by following in the very course in which they have proceeded. No absurdity, therefore, can equal that of attacking their observations and conclusions, by an attempt to shew by argument, that Gall and Spurzheim themselves possess stupid and unphilosophical understandings; when, by following the steps of a rational philosophy, their doctrines might at once be either refuted or confirmed by an appeal to facts.

The author of *Cranioscopy*, indeed, complains, like many other persons who do not attend to the nature of the subject, that Gall and Spurzheim do not specify sufficient pre-



mises from which their conclusions can be deduced by the rules of a rational logic. But the nature of the subject precluded the specification of all the cases which they have seen, and from which their conclusions are drawn. They would have swelled volumes had they attempted to specify the hundreds of instances on the evidence of which, each organ is admitted ; and such a specification would have done no good, because we see that their observations are doubted, as well as their conclusions. The object of their writings is to point out the manner in which observations ought to be made, and to specify the conclusions which they have drawn from innumerable cases. Every one, therefore, who complains that Gall and Spurzheim's works produce no conviction on his mind, and that their conclusions appear to want premises, mistakes the nature and the object of their works. The premises are found in nature, and the conclusions only in the books. If the reader will go patiently, and without bias, to interrogate Nature, she will afford him premises ; and the conclusions of Gall and Spurzheim will then be found to be drawn with a degree of accuracy of which those who shut their eyes against observation have no conception.

After these observations, we may hear how the author of *Cranioscopy* completes his paragraph, already so auspiciously begun, and completes, at the same time, his inquiry, the result of which is to establish or overthrow the system under his consideration. He continues :—" We should know in what spirit he conducted the inquiry ; with what previous dispositions he examined the objects of his contemplation ; what motives led him to these researches ; and what interest he may have in the event. Experience, we should recollect, leads to very different results, according to the sagacity and good faith of the person who acquires it. Minds already prejudiced, collect from it only a confirmation of their errors, and become, by its means, only the more obstinately wedded to their opinions. The sailor, stedfast in his belief that his whistling to the sea will raise a wind, or conjure up a storm, instead of being undeceived by experience,



is only the more strengthened in his faith, by the observations which it furnishes to him. In what a multitude of instances do we not find men deceiving themselves as grossly, when they draw inferences from what they see, if prepossessed with the expectation of meeting with a certain coincidence, or succession of events? How disposed are we all to disregard the exceptions to a preconceived rule, and to allow undue weight to every example that conforms to it. How willingly we repel the evidence that opposes, and how eagerly we catch at whatever corroborates our previous notions, especially when these notions have originated with ourselves, and are viewed as the darling offsprings of our own lucubrations."

These observations are exceedingly judicious in themselves, taken in disjunction from the conclusion which the author plainly intends us to draw from them. He means us to dismiss the whole doctrines as a delusion, without farther inquiry, because Gall and Spurzheim were liable to be deceived in their investigations. If he had meant otherwise, he ought to have expressed himself so. If he intended only to recommend more accurate observation, he ought to have stated, in justice to the founders of this philosophy, that they have at least the merit of pointing out the proper ways in which the inquiry should be conducted. He no doubt was at liberty to state, if he pleased, that, in his opinion, they had followed that mode to so little purpose, that not one of their conclusions could be trusted; yet justice required that they should get the merit of discovering a mode of philosophising, the want of which has been the cause of the long ignorance of physiologists concerning the functions of the brain.

Magendie says, "La physiologie, toute brillante qu'elle paraisse dans les traités écrites de nos jours, est encore une science à son berceau. Il faut absolument qu'elle sorte de cet état affligeant d'imperfection. Pour obtenir ce résultat, le premier pas à faire, doit être de changer la forme et par conséquent la marche qu'elle a suivie jusqu'ici; elle doit



prendre la marche analytique et la forme théorique: alors seulement elle pourra se perfectionner et se mettre au niveau des sciences naturelles les plus avancées."—(Precis Elementaire de Physiologie.—Préface.)

Whatever imperfections may be chargeable against the observations of Gall and Spurzheim, it cannot be denied that they have the merit of pursuing the mode of philosophising here so strongly recommended. Their antagonists, on the other hand, who *argue* against their observations, instead of bringing them to the test of their own experience, still cling to the hypothetical mode of philosophising, and seem incapable of considering the statements of Gall and Spurzheim, as facts, and not as speculations; so inveterate are the habits of speculating, instead of observing, in those who generally cultivate the science of physiology.

The objections now combated, are not brought forward the first in point of order in the article Cranioscopy; but they ought to have been so, as they touch the principles on which the system is founded; and, on that account, they have been here considered first. I proceed now to notice some other objections, which relate more to the substance than the principles of the system.

"The truth is," says the author, "that there is not a single part of the Encephalon, which has not, in one case or other, been impaired, destroyed, or found defective, without any apparent change in the sensitive, intellectual, or moral faculties. Haller has given a copious collection of cases which bear upon this point; and a similar catalogue has been made by Dr Ferriar, who, in a paper in the fourth volume of the Manchester Transactions, has selected many of Haller's cases, with considerable additions from other authors. The evidence afforded from this mass of facts, taken conjointly, is quite sufficient to overturn their fundamental proposition. This evidence is not impeached, by the feeble attempts of Dr Spurzheim to evade its force, by a general and vague imputation of inaccuracy against the observers, or by having recourse to the principle of the duplicity of each of the cere-



bral organs ;—a principle of very dubious application, on a subject of so much uncertainty as the physiology of the brain !”

If the cases here alluded to be true,—if the mind has really been manifested with complete effect, when the whole, or the greater part, of the brain has been wanting, then the brain is not, and cannot be, the organ of the mind. The conclusion, therefore, which the author ought to have drawn, and which he ought boldly to have advanced, is, that the brain is not the organ of the mind : and that all theories which are founded on such a supposition, are absurd. But amid the innumerable and incontrovertible facts, obvious to the common observer, which renders the affirmative of this question probable, and seeing that all enlightened medical writers maintain it, such a denial would have been hazardous ; and this author, therefore, like many others, chose rather to embarrass the discussion with difficulties, than to strike out light himself.

But in answer to the objection contained in the foregoing cases, I remark, that when we find the report of such cases in books, we must always inquire what notions the authors had regarding the meaning of the word “ faculties,” when they used it. It is an undeniable fact, that hitherto the most obscure and undefined notions have been entertained regarding what a faculty really is ; and that no opinions whatever have been entertained regarding the functions of any faculties implanted in the mind by nature. No philosopher has hitherto conceived the lower propensities of our nature, or even the higher sentiments, to be manifested by means of distinct faculties. These propensities and sentiments have been considered merely as modes of affection of the single power, the Mind ; and the term Faculties, was held to refer to the intellectual powers alone. Such being the case, it is not difficult to conceive that a person who entertained such notions might for months attend a patient who could not manifest the faculties of the lower propensities, or even those of the higher sentiments, and might not



discover these wants. He might believe that the patient was able to manifest all his faculties unimpaired, seeing he meant by the word Faculties, only the intellectual powers; while, in point of fact, he was nevertheless unable to manifest several of the propensities or sentiments. In such a case, if the patient's brain was dissected after death, and those parts of it found diseased which served as the organs of the faculties which he really could not manifest, the medical attendant might, from mere ignorance of this system, be led to conclude that the brain was not the organ of the mind, and yet err egregiously in doing so.

In the next place, we may observe, that the anomalies recorded in books are easily accounted for by the fact, that the brain, and consequently the organs of each faculty, are double, like the organs of the external senses; and that the faculty may have been manifested by the one organ, although the other was diseased.

But it is of importance also to observe, that these cases are recorded by authors, who could not have had the philosophy in question in view when they reported them; and that, as human nature is the same in the present day that it was ten, twenty, or a hundred years ago, it is much better to resort at once to Nature for evidence to refute the system, than to appeal to inaccurate observations contained in old volumes. No person would resort to the books of the alchemists for evidence to overthrow a modern discovery in chemistry; and there appears no good reason for resorting to books for evidence to overthrow the opinions of Gall and Spurzheim, when, in their case, also, Nature herself can be appealed to. The discrepancy betwixt the observations of Gall and Spurzheim, and those of the authors cited by Dr Roget, may arise from the ignorance of the latter as probably as from the incapacity of the former. After what has already been said, the presumption is much stronger that these authors were deceived, than that Gall and Spurzheim were mistaken. The authors were unacquainted with the true mode of discovering the functions of the brain. Gall



and Spurzheim discovered it, and they say that they followed it in their investigations.

We may even allow, however, to the Anatomists, for the sake of argument, that many anomalies and apparent exceptions actually exist in regard to the system; and we may ask, whether there may not, nevertheless, be such a great body of well ascertained facts in support of the general conclusions, that no doubt of them can remain; and if so, whether we ought to reject the general conclusions entirely, because difficulties exist, or to hold by the conclusions as certain, and regard the anomalies as points which time and farther observation may enable us to explain. No science was ever founded, that was, in the first instance, free of difficulties; and they are only shallow minds, who seize on the difficulties, and stifle the rising truth, rather than assist in removing them. The author of *Cranioscopy* says, "The anatomy of the brain is so complex, and so void of apparent adaptation to any purpose we can understand, that it will suit any physiological system, nearly equally well." Why, then, would he wish such a state of ignorance to continue, and why does he not admit that Gall and Spurzheim deserve at least the merit of attempting to remove it, in the only way in which it can be removed?

The author of *Cranioscopy* proceeds also to refute what he is pleased to call Dr Spurzheim's "arguments" in favour of the system founded on analogy. But if we have been successful in shewing that the "arguments" on which the system is founded are direct facts, we need to trouble ourselves very little about the additional evidence which it derives from analogy. Analogy can afford only illustrations or cumulative proofs. While the fundamental truths remain, the arguments founded on analogy may be sound or unsound. It was only because the author of *Cranioscopy* overlooked the fundamental principles on which the system is founded, that the analogies appeared to him so imperfect; but if the facts bear out the doctrines, the analogies will appear in another light; and hence, as the strength of the ana-



logies depends on the truth of the previous propositions, it appears to me unnecessary to waste a word upon the subject.

Before leaving this branch of the discussion, it may be proper to notice, that several anatomical gentlemen have stated as an objection to the system, that certain appearances in the brain, mentioned by Dr Spurzheim in his account of the anatomy of that organ, arise from optical illusion, and, of course, that they are not founded in fact. I am not able to say, whether the illusion is on the part of Dr Spurzheim or of his opponents, being unacquainted with the minute anatomy of the brain myself; but it is of great importance to observe that however great Dr Spurzheim's mistakes as to the anatomy of the brain may be, such mistakes do not necessarily invalidate his account of the functions of that organ. Dr Spurzheim does not say that he has discovered the structure of the brain to be so and so, and inferred from that structure, that the brain is the organ of the mind, and that different parts of it are the organs of different faculties. If he had done so, it is clear, that an opponent, by shewing that his notions of the structure are erroneous, would have overturned the whole system. But Dr Spurzheim, on the contrary, observes, "that the deepest perspicacity would not, *a priori*, have attributed the smell to the pituitary membrane of the nose; the taste to nervous papillæ of the tongue; the sensation of light to the optic nerve, &c. Who, says he, in seeing the structure of the stomach could conjecture its digestive power? Who, from the structure of the viscera, could decide, that the liver secretes bile, and the kidneys urine? Who, from the structure and form of the nerves, can determine what kind of impressions they propagate? It is the same with the brain. Let the directions of its fibres be known, and let anatomists distinguish their greater or less consistence; their more or less white colour; their different size, length, &c. What conclusions as to the functions, can they draw from these circumstances? None. Thus, it is certain, that the anatomical knowledge of any part does not indicate its



function ; and it is, therefore, necessary, to have recourse to other means in order to discover it. On this account, the physiology of any part often precedes its anatomy. Thus, it was generally known, that we see by means of the eyes, before anatomists were acquainted with their structure." Hence, " many organs of the brain were discovered before its structure was demonstrated ; and these discoveries might have subsisted for many centuries, without the structure of the brain being known." (Spurzheim, p. 205.) When, therefore, an anatomist shews that Dr Spurzheim is mistaken in his ideas of the structure of the brain, it only proves that Dr Spurzheim is not yet perfectly master of the anatomy of that organ, but he does not prove that he is mistaken in his notions of its functions.

The author of *Cranioscopy* goes on to say, " The possibility of discovering the size and shape of the different parts of the brain from the external examination of the head, is also discountenanced by anatomy." It is amusing to see Doctors, who, without making a single observation on the subject, agree perfectly in pronouncing Gall and Spurzheim's system to be absurd, differing among themselves *toto cœlo*, regarding the truth of the fundamental principles on which it rests. The *Edinburgh Reviewer*, in direct opposition to the author of *Cranioscopy*, says, " But we will acquiesce implicitly for the present in the proposition (familiar to physiologists long before the age of Gall and Spurzheim), that there is, in most instances, a general correspondence between the size of the cranium and the quantity of cerebrum." (Page 246.) Of course, if the general size of the skull correspond to the general quantity of brain, it will be difficult to persuade us, that the figure of the skull in particular parts does not correspond with the development of the brain also in particular parts. We have, moreover, the testimony of Magendie, \* concurring with the *Reviewer* in favour of Dr Spurzheim, and in direct opposition to the assertion of the present author.

\* *Précis Elementaire de Physiologie*, tom. i. p. 115. Paris 1816.



This author likewise objects to what he calls Dr Spurzheim's "arguments," in support of the system, founded on Pathognomical indications, such as gestures, looks, and voice; but what he supposes to be arguments are not given as such. Dr Spurzheim found by observation that in an individual who manifests great self-esteem, a certain part of the brain is fully developed; and likewise, that the individual carries his head high, and reclining backwards. But these two facts are independent; and the one was never stated as an evidence establishing the other. It may appear fanciful to those who have not observed nature, that such a concomitance of sentiment, development of brain, and carriage of the body, should exist; but, as we have already repeatedly observed, any supposed absurdity of this kind is no evidence that the allegation is not true; and if it be true, it ceases to be ridiculous. We are in great danger of erring, when we slight an account of a fact in nature, because the fact appears to us ridiculous. The appearance of absurdity may arise from our own ignorance, as probably as from the errors of the observer. We know so little of the constitution of nature, that we ought more frequently to examine, and more rarely to scoff, than we generally do. For my own part, I have verified many of Dr Spurzheim's pathognomical signs by observations; and it appears to me that he is correct in saying that they indicate certain propensities and sentiments. But it would be absurd to argue from the nature of these signs to the existence of the organs; and Dr Spurzheim never does so. In some cases, where he has perceived strong indications of the existence of an unascertained faculty by manifestations, which could not be referred to any one already ascertained, he has suggested the probable situation of the organ of the unascertained faculty, from observing the pathognomical signs attending the manifestations which he conceived to belong to it; but he never proceeds farther. And such conjectures, *a priori*, were legitimately made, if Dr Spurzheim had found, which he says he had, a general coincidence between the character of the signs and the situation of the organs,



in regard to the faculties which he had ascertained. Again, therefore, the supporters of this system have reason to complain of its opponents, for mistaking the outworks of it for the citadel, and for representing one conclusion as unsound, merely because they themselves have misapprehended another.

The author of *Cranioscopy* appears to me to have employed the authority of his name, and the force of his talents, in propagating prejudices, instead of removing them. He appears clearly not to have perceived the basis of the system, or the importance of the questions involved in it; and hence he seems, carelessly perhaps, to have taken for granted that it was absurd, and then to have set about proving it to be so. "Such," says he, "is the body of doctrines, and such the reasonings in their support, which have emanated from the school of Gall and Spurzheim, and which they have dignified with the appellation of a new science!"—"We shall refrain," continues he, "from employing the weapons of ridicule against a system so vulnerable in its attacks, and which would have been so capable of affording Swift a new incident for the history of the Philosophy of Laputa. The simple explanation of the sandy foundation on which it has been built; of the flimsy materials of which it has been compounded; and the loose mode in which they have been put together, will suffice to enable our readers to form their own conclusions as to the soundness and solidity of the edifice."

No person who had viewed this question with a philosophic eye could have made such observations as these. The author of *Cranioscopy* could not be unacquainted with the imperfect state of knowledge in the medical profession, in regard to the functions of the brain, and the organs of the mind; and he would take it as no compliment, if we were to suppose him ignorant of the causes of this imperfect state of information. Yet, if he saw the deficiency, and the causes of it, he must have perceived the peculiar fitness of Gall and Spurzheim's mode of philosophising to remove both, and to wipe the darkest stain of imperfection from the



science of physiology, with which it is at present blotted. It is inconceivable, therefore, how he could, without once appealing to nature, stigmatize their system as one "so capable of affording Swift a new incident for the history of the Philosophy of Laputa !" If the followers of Gall and Spurzheim were disposed to employ "the weapons of ridicule" against opponents "so vulnerable to its attacks," they have materials enough afforded them for an amusing picture of the absurdity of men coming forward to instruct others, who are necessarily ignorant themselves. They could, perhaps, without much difficulty, excite a strong feeling of "just indignation," against their "confident nonsense;" but we shall at present only use the words of Dr Johnson, part of which form the motto to this section; and which appear completely applicable to the present case. "There are (says he) some men of narrow views, and grovelling conceptions, who, without the instigation of personal malice, treat every new attempt as wild and chimerical, and look upon every endeavour to depart from the beaten track, as the rash effort of a warm imagination, or the glittering speculation of an exalted mind, that they may please and dazzle for a time, but can produce no real or lasting advantage.

"These men value themselves upon a perpetual scepticism,—upon believing nothing but their own senses,—upon calling for demonstration, where it cannot possibly be obtained,—and sometimes upon holding out against it, when it is laid before them,—upon inventing arguments against the success of any new undertaking,—and, where arguments cannot be found, upon treating it with contempt and ridicule.

"Such have been the most formidable enemies of the great benefactors of mankind, and to these we can hardly doubt, but that much of the opposition which 'Gall and Spurzheim' have met with, is to be attributed; for their notions and discourse are so agreeable to the lazy, the envious, and the timorous, that they seldom fail of becoming popular,



and directing the opinions of mankind."—Johnson's *Life of Drake*.

The true merits of Gall and Spurzheim, on the contrary, are correctly expressed in the following passage, quoted from Dr Reid, and applied by him to certain Philosophers on the Mind, who had struck out at least some important views in that interesting branch of science, although they had not been so fortunate as to bring it to perfection.

"Instead of despising," says he, "the dawn of light, we ought rather to hope for its increase: instead of blaming the philosophers I have mentioned, for the defects and blemishes of their system, we ought rather to honour their names, as the first discoverers of a region in philosophy formerly unknown; and, however lame and imperfect the system may be, they have opened the way to future discoveries, and are justly entitled to a great share in the merit of them. They have removed an infinite deal of rust and rubbish, collected in the ages of scholastic sophistry, which had obstructed the way. They have put us in the right road, that of experience, and accurate reflection. They have taught us to avoid the snares of ambiguous and ill-defined words. They have made many openings that may lead to the discovery of truths which they did not reach, or to the detection of errors in which they were involuntarily entangled."—Reid's *Inquiry*, chap. i. § 4.

*Correspondence betwixt the Author and Dr P. M. Roget, author of the Article Cranioscopy, in the New Supplement to the Encyclopædia Britannica, referred to in the preceding Answers.*

To P. M. Roget, Esq. M. D.

*Edinburgh, 5th May 1819.*

SIR,—As I have not the pleasure of being personally known to you, I avail myself of the introduction of our common friend Dr Dewar, to afford me an apology for intruding



upon your attention. I am at present engaged in preparing some Essays on the system of Gall and Spurzheim for the press, and in these essays I find it necessary to advert to your article Cranioscopy, on the same subject, but to differ very widely from your views. In consequence, your article is spoken of in terms rather of severity than otherwise. It gave me pain to be obliged to do so, but the interests of truth appeared to require it. As however, I may have misapprehended the scope of your arguments, or may myself be mistaken; and as the furtherance of truth is the only object I have in view, I take the liberty to trouble you with this letter, and to say that I shall have great pleasure in modifying my observations if you are in the right, and in being corrected myself, if I am in the wrong. If your views and mine shall not after all correspond, I shall be happy to print any observations with which you may be kind enough to favour me on the subject, either in defence of your own views, or in refutation of mine.

The principles, then, on which I proceed, are the following:

*First*, It is a principle in physiology, now universally granted, that functions can never be inferred, *a priori*, from structure. Hence, the brain *may* be the organ of the mind, and different parts of it *may* be the organs of different faculties, and yet mere dissection or examination of structure could never reveal such facts.

*Secondly*, Consciousness gives us no intimation of the existence or functions of any organs of the body, made use of by the mind as instruments for manifesting its powers: For example, we are not conscious of the existence or functions of the nerves of voluntary motion, although it is by means of these nerves that we act on the external world. In moving the arm, we are not conscious of the operation of the muscles which produce the motion; in speaking, we are not conscious of the functions performed by the muscles which contract the larynx; in hearing, we are not conscious of the functions of the auditory apparatus. And in like manner, we



are not conscious, when in health, of the existence or functions of the brain, whatever these functions be.

Hence, he who has sought to discover the functions of the brain only by dissecting it, and he who has sought to discover the organs of the mind only by attending to the subjects of his own consciousness, must, from the necessity of the case, be ignorant equally of the functions of the brain, and of the organs of the mind; for, by neither means could the fact be discovered, that the brain is the organ of the mind, even although it were indisputably so. These principles are distinctly stated by Dr Spurzheim, at p. 205 of his octavo work, from which I perceive your article is taken.

Now, in the *third* place, the only way of discovering the functions of any organ of the body, serving as an instrument for manifesting the mind, appears to me to be, by comparing the power of manifestation with the state of the organ; and this mode of discovering functions, if pursued long enough, and with sufficient attention, must lead ultimately to results which cannot be doubted.

When, therefore, Dr Spurzheim said, that by comparing the development of the cerebellum with the power of manifesting the amative propensity, he found the size and power to be generally in proportion to each other; when he said, that by comparing the development of the brain under the middle of the parietal bones, with the power of manifesting the sentiment of cautiousness, he found the size and power to be generally in proportion; and when he said that, by comparing the development of the brain in the upper part of the forehead, with the power of manifesting the reflecting faculties, he found the size and power to be generally in proportion;—when he said so, did he not proceed on principles which, if followed out with sufficient attention, would lead to indubitable results? and did he not proceed on the *only principles*, by means of which either the functions of the brain, or the organs of the mind can possibly be discovered?

Farther, if it be a fact that dissection cannot reveal the functions of the brain;—if it be a fact that reflection on the



subject of our own consciousness can never reveal the organs of the mind ; and if it be a fact that the functions of the brain and the organs of the mind can be discovered only by comparing the power of manifesting the mind with development of brain, is it not philosophical to make such a comparison, and to state the results candidly and fairly, and to solicit the aid of every philosopher in checking the observations and results, for the sake of insuring accuracy and certainty ?

Now, in your article *Cranioscopy*, you appear to me not to have stated these principles to the public, and not to have shewn that the principles are unsound, or that, *de facto*, the observations made in following them out, have been inaccurate. You appear only to have used every argument to shew, that inquiry is unnecessary, and that the system is self-evidently absurd.

This, however, with all deference, is arriving at the conclusion, a little too rapidly ; for, if it be impossible to discover that any particular part of the brain is the organ of any particular faculty, by any way, except by comparing development and power of manifestation together, and if you have not made such a comparison, which you nowhere say you have done, you appear to me to be arguing without an inch of ground to stand upon yourself. When Dr Spurzheim says he has made the comparison, and found so and so ;—when his followers say they have made the comparison, and found so and so, on what principle do you, if you have never made the comparison, teach the public that Dr Spurzheim and his followers have not, and could not find so and so ? Every word that Dr Spurzheim says may be true, for any thing that dissection or consciousness can reveal to the contrary ; and you do not say that you proceed on any other data than those furnished by dissection and consciousness, when you call the system a “fantastic edifice” and “the wild effusions of a bewildered fancy ;” and you do not specify any data, which can enable you, without comparison, and *à priori*, to predicate that the facts stated by Dr Spur-



zheim are absurd ; which, it appears to me, you ought to have done before you could philosophically condemn the system without comparing it with nature. Such is the cause of my speaking of your article in terms of disapprobation; and my reason for troubling you with this letter, is to state fairly the principles on which I proceed, that I may yet, ere the Essays go to press, avoid doing you an injustice, if I have inadvertently misapprehended the scope of your argument, or fallen myself into a mistake as to the principles of the science. I am, &c.

LETTER from P. M. ROGET, Esq.

BERNARD STREET, RUSSELL SQUARE,  
*London, May 18. 1819.*

SIR,—I am obliged to you for the honour of your communication relative to the system of Drs Gall and Spurzheim ; and have read with great interest and pleasure the pamphlets you were so good as to send me, extracted from the Literary and Statistical Magazine. Although I fear there will still remain a considerable difference in our opinions as to the merits of this system, I flatter myself the difference lies in a narrower compass than you seem to imagine ; and that my endeavours to convince you, that the censures you have passed upon my Essay, which has appeared in the Supplement to the Encyclopædia Britannica, are founded upon a misconception of the scope of my reasoning, will not be altogether unsuccessful.

The essential point, I conceive, on which the disciples and the opponents of Gall and Spurzheim are at issue, is, whether there really exists such an uniform correspondence between certain forms of the head, skull, or brain, and certain characters of mind, as can be distinctly recognised by observation. Until this be ascertained as a matter of fact, all speculation as to what may possibly be the functions of different parts of the brain, founded on reasonings *à priori*, can, in my opinion, lead to no positive or certain result.



The question then is, Whether sufficient evidence has been obtained to establish the fact, that certain forms of brain are accompanied by certain powers of mind? What I principally contend for is, that, as far as that evidence is derived from the observations of Drs Gall and Spurzheim, it is quite inconclusive, not only because the general accuracy of their observations is extremely suspicious, but because the several propositions with respect to the functions and particular parts of the brain do not appear to have been obtained by fair induction from experience. In the article *Cranioscopy*, I have fully explained the grounds on which this scepticism may, I think, reasonably be entertained. From the remarks you have made on this part of my Essay, it appears to me that you have not exactly appreciated the nature of my objections, which are principally directed against the sufficiency of the evidence brought forward by Gall and Spurzheim in support of these pretended facts.

You find fault with me for not having stated more explicitly, that the truth of the theory must ultimately depend on its conformity with experience. But the proposition really appeared to me to be too self-evident to require even the most concise enunciation. A theory, being merely the generalization of facts, must, of course, be built upon facts. When the facts have been ascertained and collected, and when we have shewn them to admit of being comprehended under a smaller number of more general facts, we have, in other words, established their theory. Neither the one nor the other has, I think, been accomplished, in the present instance. I have no confidence in the authors of the system, either as faithful observers, or as sound reasoners; and, as far as I have myself had an opportunity of comparing the results they pretend to have obtained, with my own experience, I cannot say that I have found them in any degree verified. I at one time took some pains to make observations on this subject; and am persuaded that I met, in the course of them, nearly as many exceptions to the rules, as instances in confirmation of them. One conse-



quence of my endeavours to satisfy myself by a reference to nature, was the conviction I received of the extreme difficulty of conducting an investigation of this nature. I have stated some of the sources of difficulty, not with a view of discouraging inquiry, if conducted on the true principles of philosophical induction, but as ultimately contributing to its success, by inspiring a salutary caution against a fault, to which it seems to me that the founders of this system have shewn a strong propensity, that of too hasty and imperfect generalization.

You complain that I have not proved that the observations of Gall and Spurzheim are, *de facto*, inaccurate. It appears to me, on the contrary, that where doubts can be reasonably entertained of the accuracy of the observers, the exposition of the grounds of doubt leaves the *onus probandi* on the side of those who seek to establish the facts in question. With those who are satisfied with the testimony of Drs Gall and Spurzheim on these points,—who esteem them accurate observers, and give them credit for having exercised that coolness and caution of judgment which alone can secure us from deception, even by our external senses,—and who can trust to their fidelity in generalizing the results of observation, and are satisfied with the logic which they are in the habit of employing, I cannot presume that any thing I might say would have much weight. I am, therefore, inclined to let the matter rest here; since, after all, the judgments we are likely to form on the validity or insufficiency of this sort of evidence must be as much the result of feeling as of reasoning. On this account I do not feel disposed to enter into any further controversy on the subject; though, at the same time, I remain open to conviction, if the evidence in favour of the theory, furnished by observers worthy of confidence, shall ultimately be found to preponderate. I am, Sir, your very obedient humble servant,

P. M. ROGET.



To P. M. Roget, Esq.

*Edinburgh, 28th May 1819.*

SIR,—I have had the pleasure of receiving the letter of 18th May, which you did me the honour to write to me, under cover to Dr Dewar. I fear there is no prospect of you and I agreeing on the merits of the system of Gall and Spurzheim; so I drop all private controversy on that subject. But I am sorry that we do not yet agree on the apparent tendency of your article *Cranioscopy* in the *Encyclopædia*; and it is on this topic alone that I trouble you with another letter.

If it was your object, in that article, to state, that the great cause of the present deep ignorance in regard to the functions of the brain, was to be found in the circumstance of anatomists having hitherto confined themselves to dissection alone of that organ;—that Gall and Spurzheim had at least the merit of suggesting the only way in which this ignorance was likely ever to be removed, viz. by comparing manifestations and development together; but that from their enthusiasm, or incapacity, or some other cause, they have pursued their own mode of philosophising with so little advantage, that their conclusions cannot be depended upon;—and, hence, that other anatomists ought to follow industriously in the same course, and seek to perfect science by obtaining correcter results;—if it was your object to make such a statement in the article *Cranioscopy*, then I have done you great injustice in the *Essays* intended for publication; and I now write, to say, that, with your permission, I shall print your letter to me in the *Appendix*, or any *note* with which you may be pleased to furnish me, in explanation of your object.

On reading your article, it appeared to me, that your object was to represent both the mode of philosophising, and the conclusions of Gall and Spurzheim, as absurd, and deserving of no consideration; and that you meant to shew, first, that the *true* faculties and dispositions of the mind,



could not be discovered by observing manifestations, except with extreme difficulty and uncertainty; Secondly, That the true development of the brain could not be discovered with any moderate certainty through the cranium and integuments; and, thirdly, That experience shewed rather that the mind might be manifested without perceptible diminution of power, even although the brain were partially, if not indeed totally destroyed by disease, than that the brain was the organ of the mind; and, hence, that the whole system, both in foundation and superstructure, was only "the wild effusion of a bewildered imagination."

Since I received your letter, I have again read your article, and with all the candour in my power; and I am sorry to say I cannot see the object of it in another light from that now stated. It is with regret, therefore, that I cannot alter the observations made on it in the work which I intend to print; and although I am well aware that any remarks of mine will give you no uneasiness, yet it is so painful to be supposed capable of wilfully misrepresenting an author, in order to make an attack upon him, that it would be a real favour to me to be allowed to establish at least my *bona fides*, by publishing the correspondence, or the substance of it, and thus to afford the reader a safeguard against my misconceptions, if I have so egregiously erred. I have the honour to be, Sir, &c.

*P. S.* I cannot but regret that you did not state the result of your *observations* in the article *Cranioscopy*; as a number of well authenticated facts in contradiction to Gall and Spurzheim's statements would have outweighed, in my humble apprehension, a whole *Encyclopædia* filled with arguments.

G. C.



## LETTER from P. M. ROGET, Esq.

BERNARD STREET, RUSSEL SQUARE,

LONDON, *June 3. 1819.*

SIR,

I have to acknowledge the honour of a second letter from you on the subject of my article on Cranioscopy in the Encyclopædia. In answer to your inquiries as to my object in writing it, I can only say, that I have endeavoured to give such a statement of the arguments for and against the system, as might enable the reader to form his own judgment respecting its truth. My comments of course applied solely to the evidence brought forward by its founders, Drs Gall and Spurzheim; I accordingly thought it right to omit all reference to my personal experience on the matter, more especially as I was not exactly writing in my own name; and I felt it nowise incumbent in me to lay the foundations of any similar system myself, or presume to direct others in the pursuit, by laying down a plan of operations to be followed for that purpose. But, as I have before stated, I shall certainly not refuse my assent to the facts in question, when I shall be convinced they have been fairly established. I have the honour to be, Sir, your most obedient humble servant,

P. M. ROGET.

*P. S.* I have no objection to your publishing this and my former letter to you, if you think it worth while.

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NOTE.—It is nineteen years since the preceding answers to Dr Roget were composed, and I then wrote according to the state of my own knowledge. Subsequent experience has convinced me, that his objections to the anatomy of the brain, as taught by Drs Gall and Spurzheim, are as unfounded as those which he urges against the other parts of



their discoveries. During the interval between 1819 and the present date, I have seen many human brains, and also many brains of the lower animals, dissected; and from the evidence of my own senses, can testify that their anatomy is a correct representation of nature. I long experienced a difficulty in regard to the converging fibres; but in June 1837, Dr Seiler, the Director of the Medical Academy in Dresden, a zealous phrenologist, shewed me some admirable preparations of the brain in his museum, in which the representations of this organ in Dr Gall's "*Physiologie du Cerveau*," even in regard to the converging fibres, were distinctly realized. Mr Solly, in his excellent work on the human brain published in 1836, has adopted Dr Gall's plates and descriptions of the diverging fibres, and acknowledges that to this "wonderful man we are indebted for our present knowledge regarding the course of the motor tract." P. 484.

The cases collected by Dr Ferrier, and published in the 4th volume of the Manchester Transactions, of injuries of the brain having been sustained, without being accompanied by any affection of the mental faculties, referred to by Dr Roget on p. 208, have since been critically analyzed by Dr A. Combe, and demonstrated not only to form no legitimate grounds of objection against Phrenology, but expressly to support its principles. His Remarks are published at the end of the 2d volume of my System of Phrenology, and also in the Transactions of the Phrenological Society. I entertain a strong conviction, that the reported integrity of the mental faculties in these and similar cases, rests on no foundation except inattention in the observer. Similar examples have been reported of the absence of all symptoms indicating disease in other important organs, which, nevertheless, were found extensively disorganized after death; and I am disposed also to account for these apparent anomalies, by imperfect observation of the phenomena in the reporters. I knew one case of this kind, which I may state as an illustration. In August 1832, I visited a friend who resided in the pastoral district bordering Gala



Water, on the confines of the county of Edinburgh, and walked fully ten miles with him, during one forenoon, over the hills which rose close behind his house. He was then apparently in perfect health. During our walk, he told me that, for some time previously, when rambling among these hills, he had *occasionally* felt a sensation come over him which he could not describe; it was, he said, as if life were about to leave him instantly; he did not feel any acute or local pain, but solely a sense of sinking. He lay down on the grass on his back, and in this attitude he recovered. His recovery was so complete that he pursued his walk, but at a slower pace, and felt no farther inconvenience probably for some weeks. He and I resorted to various speculations concerning the probable cause of these sensations, but none of them satisfied him as being correct, and he concluded by saying that his health, in all other respects, was complete. In October of the same year he died suddenly, and a large aneurism was found on the *aorta descendens*, about the point where it enters the diaphragm. For some weeks before his death he had complained of pains in the back, which were treated as rheumatic; and neither his family nor his medical attendant suspected aneurism till the *post-mortem* examination revealed it. If this case had been reported, the medical attendant who, I believe, was not consulted about the sensations now described, might have stated that there were here no symptoms which indicated the existence of aneurism, although he would have formed a different opinion, if he had heard the patient describe his feelings in the way he did to me as a mere topic of conversation.

G. C.

*Edinburgh, 26th February 1838.*



*Remarks on the article Cranioscopy, by Dr Roget, in the Supplement to the Encyclopædia Britannica.* By Dr A. COMBE.

(Reprinted from *Phrenological Journal*, vol. i. p. 165.)

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“Io domando ciò che tutti desiderano e pochi ottengono, di esser giudicato dalla ragione, non dalla opinione. Non facciamo un dogma della stima di alcun autore. Vediamo co' nostri occhi. Gl' inconvenienti che possono nascere da questo liberissimo metodo non saranno mai tanti quanti sono quelli che derivano dal guidicare colla fama e colle prevenzioni.”—VERRI.

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The phrenologists have been often blamed for attributing too little weight to the numerous attempts which have been made to sap the foundations of their science. But where, in the whole course of phrenological warfare, is even an opponent to be found who has declared himself satisfied with the refutations and arguments of his coadjutors in the field? In what page of the grand attack in the 49th Number of the *Edinburgh Review* (1815), does the antiphrenological champion of that day refer to, or quote the facts or arguments of the antiphrenological champion of 1803? In what page of the “able and elegant article” of Dr Roget does he refer to any of the numerous death-blows given to our science before his time? In what page of “*Life and Organization*” can we gather any thing as to the existence of a refutation of Phrenology, prior to Dr Barclay's own? In what page of *Antiphrenologia* does its illustrious author refer to those of Dr Barclay, or any other of his predecessors! Where, in short, is the magazine, journal, or review, which has not, by some strange mistake, deemed it necessary to be original in this particular alone, and to avoid scrupulously any reference to the assistance of predecessors or contemporaries? And yet our opponents ask us to resign our convictions on evidence with which they are not satisfied,



and to own ourselves vanquished, when they see us standing before them vigorous and unscathed.

But to satisfy those who may differ from us in opinion, and also to redeem the pledge given in our first Number, when we stated our readiness to meet any opponent whose object seemed to be truth, although he should not have displayed much philosophy in his mode of attack, we now take up Dr Roget's article,—CRANIOSCOPY, which is still regarded in the south as the most formidable attack Phrenology ever had to sustain; and our doing so will afford us a good opportunity of undeceiving the public on the supposed credit due to the opinions of the members of the medical profession above that due to the opinions of other men.

Phrenology being a system of philosophy founded on the discovery of the functions of the different parts of the brain, there are only two circumstances which can entitle a professional man to dispense with the preliminary step of examining the nature and evidence of the doctrines, before giving an opinion for or against them. He must have previously ascertained, either that there is another function which is inconsistent with the phrenological one, or that the latter is incompatible with the anatomical structure. Now, even Dr Roget himself expressly declares, that "the brain is *still as incomprehensible in its functions* as it is subtile and complex in its anatomy;" and that "its structure is so void of apparent adaptation to any purpose we can understand, that it will suit any physiological system equally well,"—thus leaving the educated part of the public on a perfect footing of equality with himself as to the possession of knowledge on those points. We state this explicitly, because, to use the words of our motto, "we wish to be judged by reason, and not by opinion;" and while, on the one hand, we are disposed to listen attentively to the opinions of those medical men who have really examined the evidence, and who know what Phrenology is, we cannot, on the other hand, regard the decision of any one of them, who has not informed himself, as entitled to more consideration as



evidence, than the opinion of an ignorant fisherman regarding the theory of the tide when placed in opposition to that of Sir Isaac Newton.

In entering upon his refutation, Dr Roget takes credit to himself for some very slender virtues. After representing Phrenology as "admitting so easily of being held up to ridicule by PARTIAL or EXAGGERATED statements," he claims merit "for refraining from employing the weapons of ridicule" against it, and for contenting himself "with the simple exposition of the sandy foundation" on which, and the "flimsy materials" of which, the new system is constructed. We willingly leave to him all the praise of which forbearance from ridicule founded on "partial or exaggerated statement" is worthy.

The first, and what Dr Roget calls the most important of all his objections, is, that injuries of different parts of the brain have occurred without corresponding derangement of the function assigned to them; and Haller and Dr Ferrier are referred to as authorities. These cases, our readers are aware, have been already amply discussed in the Transactions of the Phrenological Society, and shewn to be not only perfectly consistent with and explicable by the phrenological view of a plurality of organs, but also to be at utter variance with Dr Roget's idea of unity of mental organ. We therefore pass on to what he denominates Gall and Spurzheim's "analogical arguments;" in the attempted refutation of which he displays much of that inconsistency into which a man unavoidably falls when writing on a subject with which he is unacquainted.

Dr Roget very judiciously objects to any theory of the functions of the brain founded on analogy alone, and stigmatizes, "as a gross violation of logic," the assumption "of any such analogy as equivalent to *positive proof*, which can only result," he adds, "from the *evidence of direct observation*." Drs Gall and Spurzheim take the same view of the matter, and, therefore, wherever they lay down any point as established, they found only upon positive proofs furnished



by direct observation, and never upon analogy alone. They, like Dr Roget, think analogies calculated to afford merely "indications of what *may* possibly happen," and therefore as often useful "to direct and stimulate our inquiries to the discovery of truth, by the legitimate road of observation and experiment." But, as they consider analogies as always inferior to proofs, they never cease to urge their readers to "appeal to the evidence of induction, as the supreme authority in the court of philosophy." Can we admire, then, Dr Roget's consistency, when, quite forgetting even his own preference of the evidence of induction, and all that he has said about Gall and Spurzheim "making their last and *most resolute stand in experience*, as in an impregnable fortress," and about the mode of obtaining "*the facts upon which so much is made to depend*," he turns round and says, Although I am ignorant of the structure and functions of the brain, and have admitted that the former will suit Gall and Spurzheim's physiological views as well as any others, and although I object to the evidence of analogy, as proving any thing at all, yet *by analogy alone* I will overturn statements which Gall and Spurzheim allege to be founded, not on analogy, but on *positive proofs and observations?*" and shew that "these gentlemen have ventured to found all the leading propositions of their doctrines" "upon analogical assumptions and preposterous imaginations." If the phrenologists displayed half as much inconsistency, they would deservedly be laughed at as sadly deficient in "logical acumen."

Even granting Dr Roget the privilege of refuting direct inductive evidence by that drawn from analogy, we suspect he is only at the beginning of his task, for he has not yet advanced a single argument of this kind which is able to stand on its own foundation, much less fitted to be employed to beat down the arguments of others. Indeed, he never attempts to combat the *principles* of Phrenology, but merely magnifies the difficulties of putting them in practice; and in this he acts wisely, for it is only thus that the real feebleness of his reasoning has any chance of escaping de-



tection. With all his caution, however, he is not altogether safe. To prove that one organ may perform all the operations of the mind, he argues thus: "Does not the same stomach digest very different and even opposite kinds of aliments? yet we do not find that one portion of that organ is destined for the digestion of meat, and another for that of vegetables." Very true; but the *function* is the *same* in all, the *subject* only is different. Digesting is no more than digesting, whether it be performed on turtle or roast beef, animal food or vegetable. In like manner, no phrenologist ever asserted that one part of the organ of Causality reasoned in political economy, another in metaphysics, and a third in medicine; or that one portion of the organ of Tune was destined to produce soft and plaintive notes, and another bold and warlike music. We only maintain, that as the stomach cannot secrete bile, nor perform the office of kidneys, neither can the organ of Causality produce a relish for music, nor that of Tune a talent for logical reasoning. We have never said that Causality cannot be exercised on all sorts of subjects, sacred or profane, important or trifling; but we have said, that no change of subject will ever change its specific function of reasoning, any more than any change of diet will change that of the stomach from digestion to the secretion of bile.

Dr Roget states, as strong analogical arguments against a plurality of mental organs, that "nerves perform the double office of volition and sensation; but no anatomist has yet separated the different bundles of fibres which convey each impression;" and that "the same organ serves for the hearing of acute and of grave sounds," and "the whole retina, and not merely different portions of its surface, receives the impression of different kinds of colour: there is not one organ for the perception of blue, and another for the perception of red rays." "Guided by such analogies as these," says he, "might we not be equally justified in concluding that the same part of the brain may serve for the memory of words and for the memory of things, and that the same portion of



that organ which enables us to conceive the idea of figure may also suggest to us that of size."

The first of these analogies is the only one that, if sound, would be applicable, because in it alone there is a real difference of function, or a manifestation of two distinct powers, volition and sensation. But, unfortunately for Dr Roget, it has been demonstrated since he wrote, that for each of these functions *there is a different bundle* of fibres, although they are enclosed in a common sheath, and seem to constitute only a single nerve. This analogy therefore falls completely to the ground as an argument against Phrenology. Nay, it becomes a powerful support of its doctrines. Many years ago, Dr Spurzheim and some other physiologists inferred, from the fact of motion remaining in some cases where feeling was lost, and *vice versa*, that the nerve must really be double. Now, with this fact before us, when we observe that the memory of things frequently remains after that of words is lost, and *vice versa*, as in Mr Hood's and Dr Gregory's cases, mentioned in the Phrenological Transactions, does not the inference naturally follow, that there must be a distinct organ for each of these kinds of mental manifestations? We refer our readers for particulars to an article in our last Number on the functions of the nerves.

The second and third analogies evidently arise from Dr Roget confounding a modification of the same function with two distinct functions. Hearing acute and hearing grave sounds amount to nothing more than *hearing* sounds. Again, the perception of blue and that of red colours are nothing more than perception of colours. We maintain that there is one organ for hearing sounds of every kind, and another for perceiving colours of every hue, but not that one organ perceives one colour, and another another; so that the Doctor's analogy again totally fails him. We are only surprised that he never stumbled upon this plain fact in any of Dr Spurzheim's works, where it is repeatedly mentioned.



Dr Roget thinks the phenomenon of mental fatigue being relieved by passing from one kind of study to another, as from philosophy to music, equally explicable on the supposition of a single organ of mind as on that of a plurality; and he entertains the same opinion of the facility of explaining the phenomena of dreams, somnambulism, partial insanity, the very essence of which is the activity and healthy manifestation of one or more faculties co-existing with the inactivity and diseased manifestation of others; or, to use the analogy of the five senses, Dr Roget *is able to conceive* how sight and smell may be lost or diseased, while hearing, taste, and touch are in a different state, equally well, on the supposition of all being functions of a single organ, as on that of each having an organ to itself. This analogy is a palpable one, and we use it because there cannot be a greater difference between smell and taste than between Destructiveness and Veneration. We can offer no argument against Dr Roget's power of conception; but, to render the analogy effective, he ought to have referred to some created being in which all these different functions are performed by a single organ. This he has not done, and, therefore, we are not enlightened by his argument.

Such, then, our readers will be surprised to learn, are Dr Roget's refutation of the *analogical* arguments adduced by Drs Gall and Spurzheim,—not as proofs of their system, as Dr Roget would have us to believe, but merely as facilitating its reception, by shewing its consistency with the ordinary laws of the animal economy.

Dr Roget proceeds to object to the anatomical evidence in favour of Phrenology, because, says he, “the anatomy of the brain is so complex and so void of apparent adaptation to any purpose we can understand, that it will suit any physiological system equally well. The separation of the parts of the brain and their diversity of shape can no more be evidence of a diversity in their functions” than the lobules of the kidneys, &c. Those of our readers who recollect that it is a principle much and justly insisted upon by the phre-



nologists, that dissection alone is insufficient to reveal the function of any organ, will see at once that Dr Roget is here combating an enemy of his own creation. Gall and Spurzheim had for their object only to shew that the anatomy of the brain was *not inconsistent with* their physiological discoveries. They do not attempt to go farther than this; and the proof of it is, that the physiology was discovered long before they commenced their anatomical labours. We refer Dr Roget to Dr Spurzheim's Outlines, p. 22.

Dr Roget, as if he were instructing Drs Gall and Spurzheim in an important truth for the first time, states, that "comparative anatomy, *upon which so much is made to hinge, is of all guides the most fallible in questions of this nature.*" No person reading this would imagine that Dr Spurzheim himself had previously said, that "although it is of the highest importance to know the gradation observed by nature in perfecting the brains of animals, in order to multiply and enoble their functions, we must allow *that, notwithstanding the most assiduous labour, comparative anatomy has shewn only the mechanical form of different brains, and that these anatomical notions do not at all determine the functions of the cerebral parts.*"—(Outlines, p. 24.)

The next objection of Dr Roget was once much in vogue, but is now little noticed. It is, that the want of parallelism in the two tables of the skull, renders it impossible to ascertain the size and shape of the parts underneath. But this objection disappears, when we recollect that the function of every organ has been determined from *extreme cases* of endowment and deficiency. The whole thickness of the skull varies, in different individuals, from one-tenth to one-fifth or one-sixth of an inch, and therefore when we measure across *both* sides of the head, the greatest possible inequality, within the limits of health, must be comprised within something less than the greatest aggregate thickness of both sides, viz. two-fifths of an inch; so that when we produce two skulls nearly equal in size, one of which presents one inch more in the region of Cautiousness, for example, than



the other, there must of necessity be at least three-fifths of an inch more brain at that part in the former, than in the latter; and when such differences are daily found, they are quite sufficient to enable us to determine the functions. But in point of fact, the divergence from the parallel, when it does exist, is seldom more than to the extent of a *line*, and rarely extends over a whole organ, so far as to affect the accuracy of our observations. In disease and old age, indeed, the difference is often very great; and for that reason the phrenologist never infers any thing whatever from the development in such cases.

Dr Roget next attacks and denies the principle of size of cerebral organs being, *ceteris paribus*, a criterion of energy; and he again represents this principle as founded on a loose analogy, instead of resting on the firm basis of experience. "Let us examine," says Dr Roget, "the logic by which the above fundamental principle is deduced. 'A large muscle,' say Gall and Spurzheim, 'is stronger than a smaller one, and a large loadstone is more powerful than a smaller one; why should it not be the same with regard to the brain?' Thus again," says Dr Roget, "confiding in a loose analogy," &c. Now, Drs Gall and Spurzheim founded this doctrine on positive observations, that large organs are actually accompanied with stronger manifestations than small ones, and *then* they pointed out that this fact is in harmony with the analogies of nature. Dr Roget therefore does not meet them fairly. He next goes on to describe other conditions which must influence the functions as much as that of size, without ever seeming to know that the phrenologists have attended to these as well as himself, and have said that size, *ceteris paribus*, is a measure of energy of manifestations. Dr Roget seems, indeed, not to be aware of the meaning of this proposition, as he adds, that "increase of size in the viscera of the body is more generally the indication of a diseased than of a healthy state;" thus evidently confounding *healthy existence* with *morbid growth*; and because Professor Hufeland has said that



small eyes see better than large ones, he (who never trusts to analogy) asks if it may not be the same "with the organs of the brain?" Unfortunately for Dr Roget's views, however, physiologists and pathologists are agreed, that while too small a brain is constantly attended by idiocy, a healthy brain of a larger size is uniformly accompanied with a greater degree of mental power, as the result of its greater size; and Dr Roget cannot indicate a point where size ceases to exert an influence upon the vigour of the manifestations. He therefore adds, "But really in our present state of ignorance as to the mode of operation by which these organs are subservient to the processes of intellect and sensation, all reasoning, *a priori*, on their functions as connected with their size must be completely illusory." Here we have a specimen of reasoning far surpassing even the celebrated "argument with a vengeance." Dr Roget first avows his ignorance of the conditions which render the cerebral organs more or less subservient to the operations of the mental faculties, and justly objects to any *a priori* reasoning on the influence of size, as *completely illusory*, and trusts to observation alone for knowledge. But when Drs Gall and Spurzheim say that they have made *innumerable observations*, which afford "positive proof" of size of cerebral organ exerting a great influence upon the power of manifesting the faculty, Dr Roget does not attempt to disprove this by an opposite statement of observations affording different results, but, in avowed ignorance, and on the faith of *a priori reasoning alone*, to the validity of which he had just objected, he declares Gall and Spurzheim's statement to be "*preposterous and unfounded*."

We add not a single remark; for nothing that we could say would make any impression upon those who admire this specimen of Dr Roget's reasoning, and to those who do not admire it, nothing farther is necessary. "Even were we to admit so preposterous a doctrine, as that the energies of the parts of the brain are proportional to their magnitude," and that it were possible to distinguish the size of each part, "is it an easy task to determine the real character of the



individual, and to discriminate between real and affected sentiment?" &c. We have dwelt too long on Dr Roget's article to do more than refer to the Phrenological Transactions for a most satisfactory answer to this question; and shall only add, that we have not often had much difficulty in determining whether an individual had a talent for music, a great command of language, or much poetical or reasoning powers. Nor have we ever seen any one to whom nature had denied these reasoning powers, who was able to write a very logical treatise merely by affecting to be logical, or who could, by merely affecting to be poetical, manifest poetical power in such a degree as to deceive the world and pass for a genius.

Those who have not seen the article *Cranioscopy*, but whose fate it has been to hear it confidently talked of as a most satisfactory refutation of our science, will be surprised to learn that we have now stated *all the objections* which a professional gentleman of Dr Roget's talents and knowledge has been able to bring against Phrenology. Since, with all the supposed advantages of a medical education, he has effected so little, we conceive that we are only doing justice to ourselves and readers, when we again beg of them not to be deterred from examining the subject by the mere dicta of any man, however high he may rank, in or out of the profession. The one is as little qualified to judge as the other, until he has put Phrenology to the test of experience. Nor ought any one to refrain from putting it to this test, from a supposed disqualification arising from his ignorance of anatomy. For, in the first place, he has Dr Roget's assurance that the *structure* of the brain, in as far as is known to the medical profession in general, will suit any physiological system equally well; and, secondly, he may feel doubly sure, when he knows that Dr Gall actually discovered the physiology before he began his researches into the anatomy of that organ; and we can safely assure him, that in so far as anatomy is concerned, or, indeed, any other species of general medical knowledge, any man of ordinary understanding



may, in a single day, qualify himself completely for verifying the evidence of Phrenology.

We have purposely avoided entering into Dr Roget's repeated misrepresentations of the doctrines, and of the evidence upon which they are founded, contained in what he calls the history of the science, and have confined ourselves entirely to his *objections*, for upon these alone his adverse opinion rests. The misrepresentations we believe to have been involuntary, and to have arisen from unacquaintance with the subject. The objections, however, are his own, and in their fate his other opinions must necessarily be involved.



## PROFESSOR RUDOLPHI AND PHRENOLOGY.

By Dr ANDREW COMBE.

*(Reprinted from the Phrenological Journal, vol. i. 1823).*

The anti-phrenologists of the present day seem to be bent upon demonstrating the truth of an assertion which has often been made by the advocates of the new system, from a sincere and deliberate conviction of its truth, viz., that, from the clearness, consistency, and irresistible force of the *mass* of evidence which supports their science, it is impossible for any person of ordinary candour and attainments fairly to investigate it, without himself becoming a phrenologist; and hence that it is *impossible* for any one who really knows the subject, and who is not blinded by prejudice, to speak or to write against it. Dr Gordon, Dr Roget, Dr Barelay, Dr Milligan, and Mr Rennel, have all taken considerable pains to prove to the satisfaction of the public the truth of this statement, by exhibiting, in their own attacks, a degree of ignorance, which, in other sciences, it is rare to meet with. And the public is now indebted to the zeal of a celebrated Professor of this city, for having the name of Professor Rudolphi of Berlin added to the above list. We might not, for years to come, have had the pleasure of knowing the extent of Rudolphi's efforts, had not the Scotch Professor, in the course of last session, strongly dissuaded his pupils from wasting their money and time in purchasing and reading works on Phrenology, on the ground that Professor Rudolphi, in a German work then publishing at Berlin\* (and of which scarcely a copy had yet reached this country), had entirely and utterly demolished the whole science, facts, principles, and applications. Anxious as we have always been to meet with a philosophical opponent, we now thought

\* Grundriss der Physiologie von D. Karl Asmund Rudolphi, &c. Berlin, 1821. Vol. ii. 1823.



our wish about to be realized. But when we at last succeeded in getting a sight of this vaunted production, we found it to contain the same kind of misrepresentation, and the same kind of arguments, so often and so unsuccessfully brought forward in Britain against our inductive science. To take up each of these in succession would be tedious and uninteresting to most of our readers. We therefore prefer selecting such specimens as will be sufficient to shew whether the British Professor acted with his usual wisdom, when he rested his friendly and decisive admonition on such a basis. We beg to add, that we entertain a real respect for Rudolphi as a physiologist; but when he chooses to expose himself on a subject of which he is profoundly ignorant, his is the fault, and not ours, if his labours are not rewarded with an increase of his fame.

Professor Rudolphi first states, as an objection to Phrenology, that although we can easily believe different parts of the brain to perform different functions, yet "we can never regard it, *with Dr Gall*, as an aggregate of *unconnected* parts;" whereas *Dr Gall himself* says, that, on account of this very connexion, "*we cannot indicate with precision the limits of all the organs.*" Vol. ii. p. 391.

Professor Rudolphi is next of opinion, that "Dr Gall's marking out of the skull is *arbitrary* and *fantastic*;" and he comments on "the *absurdity* of the circles which surround certain organs." All this would be extremely proper, if the Professor would first take the trouble to prove either that Dr Gall *alone*, and not Nature, gave a particular form to particular organs; or that their form is, *de facto*, inconsistent with the functions assigned to them. Until this is done, we think it may be quite as philosophically objected to the theory of the circulation of the blood, that the heart is of a pyramidal shape, and shut up in a bag; or to the function of the kidneys, that they are shaped like a French bean, and lie imbedded in fat; as to the organ of Benevolence that it is of an oval shape, or to that of Tune, that it is somewhat pyramidal.



Professor Rudolphi remarks it as *an inconsistency* in Dr Gall's system, that "there are spaces left on the skull and brain possessing, in every respect the same properties as those called organs," but to which Dr Gall has assigned no function. If the Professor had had even a glimpse of the real nature of Phrenology, he would never have reproached Dr Gall with *inconsistency for not inventing functions* for parts, *the real uses of which he had not then discovered*. In writing these remarks, the Professor had clearly taken it *for granted*, that the new philosophy had no other or firmer foundation than that of Dr Gall's imagination, and under this impression he conceived that Dr Gall might as easily have fancied functions for 30 or for 50 organs, as for 10 or 20. But in this assumption he will ere long find himself woefully mistaken.

Professor Rudolphi goes on to amuse the German public with Dr Barclay's stale assertion of the impossibility of distinguishing one isolated organ from another, *e. g.* the sublime one of Veneration, as he calls it, from the unhappy one of Murder; and we shall therefore only refer the reader, for an answer, to the refutation of Dr Barclay's objections in the Phrenological Transactions.

The Professor, in another place, confidently avers, that the convolutions of the two sides of the brain "*are not sufficiently symmetrical*" to execute the important functions assigned them by Dr Gall. If the Professor had known any thing at all about their functions, or about the effects of their greater or less degree of symmetry in modifying the performance of these functions, this objection would have been a little more feasible. In ignorance of both, it is too absurd to require a serious answer. What will such a stickler for symmetry say to the very *unsymmetrical* appearance of the right and left lungs, connected, as they are known to be, with the very *important* function of respiration?

Professor Rudolphi next informs us, that not a single organ of Dr Gall's system is ever diseased singly, but "sometimes the whole surface, and sometimes *here and there* a



*particular part* without any particular rule." Query, Is not this "particular part" in the situation of some particular organ? Our experience says that it is; and Rudolphi does not shew that it is not. The diseased appearances, however, generally extend to more than one organ, as it almost always happens that more than one faculty is diseased at the same time.

After enumerating a whole host of facts directly proving that different parts of the brain perform different functions, Dr Gall adduces the analogy of the lower animals in farther support of it, because they possess fewer faculties, and their brains are more simple, and have fewer component parts, than the human brain. Professor Rudolphi objects to this, that among the mammalia we find the brain *consisting of precisely the same parts, only proportionally reduced in size*. But if this were fact, the brains of the lower animals would present the appearance of a human brain in miniature, and we should find in the ass, the horse, and the whale, the *upright head*, the broad coronal surface, and the covered cerebellum of man. Our readers can answer for themselves, whether such animals have ever crossed their path, and whether they have ever read, even in fabulous history, of donkeys, for instance, possessing the upright foreheads of a Shakspeare or a Bacon.

But, adds the Professor, "How little depends on the convolutions" (the very symmetry of which he but a moment before upheld as of vital importance), "is evident from their being wanting in the *human embryo*, and in many of the smaller mammalia." Where, we would ask the Professor, are the proofs of the great energy of character, or of the great intellectual vigour of an embryo, for which convolutions should be needed, supposing them to be necessary for the manifestations of the mind? Is it really possible that Professor Rudolphi could imagine, even in a dream, that the smaller mammalia, or the human embryo, ever manifest the same faculties, and the same scope of mind, as the more perfect of their species, and the adult man, in whom convo-



lutions exist? *We* have not yet seen the *convolutionless* embryo, whose mental powers were equal to the production even of a tirade against Phrenology, the lowest of all kinds of mental exhibitions, much less to that of the *novum organon*, which may be ranked among the highest.

Professor Rudolphi goes on to furnish us with farther irresistible evidence of his unacquaintance with the doctrines which he attacks, by representing Dr Gall as arguing, that since we see in organic structure different organs for different phenomena, there must be "different organs in the brain for the *different kinds of activity of the faculties*;" whereas, every tyro in Phrenology is aware that its supporters only contend for different organs for *different faculties*, and not for the *different kinds* of activity of the same faculty.

Professor Rudolphi proceeds to ask a very important, though simple question.—"But *who can decide*," says he, "whether a plurality of mental organs is indispensably necessary; and if it is, *who can tell* for what purposes the mind requires the same, and for what different organs?"—If we were to answer, that Dr Gall and the phrenologists, in consequence of observation, can decide, Rudolphi would probably smile at our presumption, but the fact would not on that account be the less true. If we fairly analyze the question, it will be seen to destroy most effectually all Rudolphi's arguments; for it amounts to neither more nor less than a confession, that the question cannot be decided either by reasoning or by the analogy of any thing yet known, but only by the evidence of new facts and observations, and that, therefore, it is needless, in the absence of such, to argue about the matter. Rudolphi having no facts to adduce, is clearly *hors du combat*, and has no right to disturb Dr Gall in his possession of the field, otherwise than by wresting his facts and observations from his hand, and supplying their place by those of an opposite nature; but this he will never do.

Professor Rudolphi, still taking it for granted that Phre-



nology is a mere phantom of Dr Gall's imagination, says—  
“ Gall *thinks* he has discovered a great number of organs, and he is quite clear about many of them, but the source of his pretended knowledge is almost entirely an absolutely untenable cranioscopy; he *thought*, for example, that in men distinguished for any particular talent (which they possessed in common), music, for instance, or verbal memory, he had always found a certain formation of the head, and thus, if a part of the skull was prominent, he FANCIED that behind it there was developed a certain portion of the brain on which that talent depended; *vice versa*, he ASSUMED, that where the talent was deficient, the elevation also was wanting, and thence he was obliged to place all the organs on the surface of the brain. Those cases which were calculated to support his hypothesis were brought forward, but the unfavourable ones got rid of in a manner which proves the nullity of the whole.”

Let those of our readers who think us too severe towards our opponents, ponder well the preceding quotation; and, before passing sentence, let them recollect that Dr Gall has published four closely-printed quarto volumes of facts and proofs in support of all his statements—that he has laid down rules for repeating his observations, and for verifying his facts—that he has illustrated these by an immense atlas of plates—and that he has challenged the world to disprove their accuracy—and that Professor Rudolphi, without pretending to refute, utterly disregards all these labours, and, in profound ignorance, and with much presumption, boldly proceeds to charge Dr Gall with deceiving the world by fancies and assumptions; and finishes by the gratuitous and insulting accusation, that he, Gall, has met with cases unfavourable to his cause, but which he disingenuously conceals from the public. If the phrenologists can find differences of development and elevations on the head, only by “*assuming that they are present*,” and “*fancying they see them*,” why do they, with such labour and expense, collect skulls, and casts of heads and skulls, from all quarters of the world,



and not only freely admit, but anxiously *invite*, the public to inspect and to compare them with each other? and, above all, why do they put a pair of calliper compasses into the hands of their visitors, and request them to measure the differences of development in inches and tenths of inches? If these differences had no existence but in the fancies of Dr Gall and his followers, how long could this delusion last with such infallible means of destroying it placed in the hands of our enemies?

As to being obliged to place all the organs on the *surface* of the brain, it has already been stated, for the hundredth time, that they are not confined to the surface, but extend to it from the *medulla oblongata*; and, in truth, we are wearied of hearing this weak and glaring perversion of Dr Gall's words thus eternally repeated.

Such is a sample of the objections with which a great German physiologist chooses to assail Phrenology; and so very desperate is the condition of the opponents at home, that such arguments are hailed and proclaimed by them, as for ever exploding the only philosophy of mind which could boast of resting on the firm basis of Nature!

Professor Rudolphi notices the existence of the Phrenological Society, and kindly adds, that although Phrenology is nonsense, there cannot be a doubt but that some good will result to science from that Association. We would recommend to him an early and attentive perusal of the Society's Transactions.



## DR PRICHARD AND PHRENOLOGY.

By Dr A. COMBE.

*(First published in the Phrenological Journal for August 1824.)*

We have repeatedly heard it stated, as an indirect objection to Phrenology, that Dr Prichard, in his late excellent publication on Nervous Diseases, not only dissents from its truth, but maintains such views of the mutual connection between mind and brain, as, if true, must prove directly subversive of the foundations of our science; and it has been hinted, that the silence of the phrenologists regarding these arose from a lurking suspicion of their truth. To shew that no such fear could possibly enter the mind of any one who was at all acquainted with Phrenology, and that Dr Prichard's opinions have really no just claim to serious notice, it will be quite enough to quote some of them from the short "Physiological Survey of the Functions of the Nervous System," prefixed to his work.

Dr Prichard agrees with us, and most other physiologists, in regarding the brain as that part of the animal system which is most immediately connected with the operations of mind. But he differs from them in believing the instrumentality of that organ to be essential for the operation of some of the faculties only, and not of all. "It appears certain," says he, "that every *sensation* of which the mind is conscious, as well as every subsequent act of apprehension or *perception*, of *recollection* or *memory*, of *conception* and *imagination*, although in itself an affection of the soul, or immaterial part of our system, *must always take its rise, or commence*, with an operation in the organic structure of the nervous system." P. 41. So far we are perfectly agreed; but, in common with almost all observers, we totally differ from him, when he goes on to say, that "the consequent



operations of *judgment* or the rational faculty, as well as the phenomena of *passion* or *emotion*, *desire* or *aversion*, love or hatred, are mental processes or affections of the soul, with which I think it must be concluded, that we have *no* proof of the connection of any co-operating organic process. And this conclusion," he continues, "may be drawn perhaps more confidently with respect to volition."

By comparing cerebral development with mental manifestations, the phrenologists have succeeded in demonstrating what was generally believed, but never before satisfactorily proved, and what is directly in opposition to Dr Prichard's view, viz., that the Reasoning faculty, and the propensities of Amativeness, Destructiveness, Combativeness, Adhesiveness, &c., which are the sources of judgment, and of the passions of love, hatred, desire, aversion, &c., are connected with, and dependent upon, the cerebral organization, as immediately and inseparably as those powers whose modes of activity and different combinations give rise to the various kinds of sensation, perception, memory, and imagination. Undeniable proofs of this assertion abound in all the phrenological writings, and it is amply illustrated and supported by the Phrenological Society's collection of casts, and by all living and healthy heads. When, in opposition to this, we mention that Dr Prichard founds his extraordinary opinions not on *facts* at variance with these observations, but on unsound arguments alone, our readers will admit that, as refutations of our science, they are unworthy of notice, and might still have been passed over in silence but for the reasons already assigned.

So far from considering the propensities and sentiments, or pathemata, as he calls them, as dependent upon, or connected with any part of the brain, Dr Prichard states, that he is "acquainted with *no fact*, either in physiology or pathology, which furnishes any ground for presuming, that those mental phenomena *take place through the instrumentality of any corporeal process whatever.*" P. 30. We hope that we have not a single reader, who is able conscientious-



ly to make a similar declaration. Setting aside the multitude of observations, which every one has in his power to verify, all tending to prove that, in the state of health, the energy of each of the propensities and sentiments bears a direct and certain relation to the size of a particular portion of the brain, we have only to adduce a single case of *diseased feeling or passion, co-existing with integrity of intellect*, in order to disprove Dr Prichard's view. Dr Prichard admits such cases to exist, and he himself puts the question—"Are these examples of disorder and perversion in the active principles induced by morbid states of the nervous system? If this be the case," he adds, "it must be allowed that those actions of mind which belong to this department (*pathemata*), are so closely connected with certain processes carried on in the cerebral texture, that when the latter are thrown into disorder, the mental operations are liable to be disturbed." P. 37.

Dr Prichard feels the necessity of coming to this conclusion, and, with a view to evade it, immediately says—"I believe most of the facts which appear to lead to this inference, admit, when strictly examined, of a different explanation." Such apparent examples of disordered sentiments and affections, Dr Prichard thinks, "are often, if not always, dependent on some *hallucination*. The insane mother, who neglects her offspring, only feels aversion for *little imps or demons*, which *she imagines* to have been substituted in the place of her own children, when they were cruelly torn from her. The *irascible* madman is the victim of some *vexatious disappointment* or mortification, which is continually harassing him," &c. This is, no doubt, both plausible and ingenious reasoning, but facts, stubborn facts, are against it. Many insane mothers hate their children, *knowing* them to be *their own*, and many irascible madmen furiously attack those who, they are at the time perfectly conscious, are full of kindness and sympathy towards them, and whom, on that account, they esteem and admire, and *struggle to save*. In the following case from Pinel, there is no hallucination, no harassing mortification, no vexatious disappointment,



nothing, in short, but *purely diseased propensities*. On the invasion of the paroxysm, the patient was seized with a "*fureur forcéné*, which drove him, by an irresistible impulse, to lay hold of whatever instrument came in his way, and to attack the *first person he met*." He stated to Pinel, "that he felt an incessant internal struggle *between this ferocious impulse and the horror with which it inspired him*. There was *no wandering of memory, imagination, or of judgment*. The propensity to commit murder was absolutely forced and involuntary," and led him to attack his wife, whom he tenderly loved, as well as the superintendant of the hospital, who treated him with great kindness. If this had been the result of *hallucination*, or of disappointment, or of any thing different from purely diseased propensities, surely the patient, instead of *struggling against* the impulsion, would have gladly obeyed it,—just as the mother, who sees imps in place of her children, would gladly put them to death.\*

So very far is Dr Prichard behind in his researches, that he professes utter ignorance of the functions of the brain. "I am sanguine enough," he says, "to hope, that the time *will arrive* when we may be enabled to ascertain the nature of the cerebral functions, and, perhaps, to understand thoroughly the whole of the process which is carried on in this part of our bodily fabric. *At present*, however, we must confess that we are not in possession of *one fact* that belongs to it." P. 41. This single sentence stamps the value of Dr Prichard's dissent from the truth of our science. HE is not in possession of *one fact* regarding the cerebral functions, and yet his mere gratuitous *opinion* is seriously set up in opposition to many volumes of *unrefuted and undeniable facts* published by Dr Gall, and to many more of *unquestioned facts* published in our own country; and, to crown the whole, the phrenologists are daily accused of presumption and impudence, for daring to believe and to teach, *on the*

\* Pinel, Sur l'Aliénation, p. 103.



*evidence of facts*, what others disbelieve and reprobate on the evidence of ignorance!! Our readers will *now* know what weight ought to be attached to the following declaration by Dr Prichard, contained in a note to p. 35, being the only place where Phrenology is *directly* alluded to:—

He says: “The conclusion to which I have been led by the foregoing considerations,” (for Dr Prichard never thinks of adducing *facts*), “is directly at issue with the inferences which Drs Gall and Spurzheim have deduced from their observations. They insist, as it is well known, on the correspondence of certain protuberances or projections of the cranium (from which a greater than usual development of the subjacent portion of the brain is inferred), with the prevalence of particular propensities. Although I entertain a high respect for the latter of these gentlemen, as well on account of his known qualities as his talents, and the services he has rendered to anatomy and physiology, *I must take the liberty of doubting altogether that part of his system which refers to Cranioscopy.*

“Dr Gall, in his work on Craniology, has mentioned some pathological observations, tending to evince the dependence of various active as well as intellectual powers on the brain, its organization and condition. Some of these refer particularly to the propensities. I shall cite his account of one incident, which is adduced with this view, in which a disorder of the propensities is stated to have followed an injury of the head.

“The accident happened to a boy in Copenhagen, who, until between his fourteenth and fifteenth years, gave but very little promise of future abilities. At this epoch, however, he fell over a staircase from the fourth story, and subsequently to the fall he displayed great intellectual acuteness. Nor was this the only change. Nobody was previously aware of any bad qualities in his disposition; but after this accident, he displayed a depraved moral character, which eventually proved the cause of his ruin.

“A relation of this kind proves nothing. That an indivi-



dual at the age of this youth should *begin* to display the influence of powerful passions on his mind is nothing extraordinary. If stories of this kind gain credit, the College of Surgeons may expect one day to march in triumph, and take possession of the vacant seats of the criminal judges; and we shall proceed forthwith to apply the trepan, where now the halter and gibbet are thought most applicable."

The appropriate answer to the first part of this note will be found in what we have already said, and need not now repeat. Dr Prichard requires no apology for presuming *to doubt* the truth of Drs Gall and Spurzheim's doctrines. It would be the height of absurdity in any rational being to adopt principles as true, without due examination and experience of their uniform consistency with nature; and as Dr Prichard never entered into the examination of the facts and observations upon which Phrenology rests, he could not do otherwise than doubt. All the favour that we ask of him is, that, before *deciding* either for or against, he will diligently use the means of converting doubts into certainty. We care for truth alone, and for Phrenology only because we believe it to be true. If Dr Prichard, therefore, will satisfy us that it is not true, we shall consider ourselves as greatly indebted to him, and shall make haste to put it from our sight. In the mean time, he must excuse us for acting on our belief, and attempting to shew him his error.

In regard to the merits of the case mentioned in the second part of Dr Prichard's note, we also differ from him. The point at issue is, whether the propensities, sentiments, and intellect, *are* connected with, and dependent on, the condition of the brain for their power of manifesting their activity. Now, the case of the boy seems to us to shew, that, *in a particular instance, a change in the state of the intellect and of the propensities or passions, and a change in the condition of the brain, were concomitant*; and as Dr Prichard does not deny either of these changes, and as all evidence is made up of an accumulation of single facts, we certainly see no reason why this one fact should not count as *one*.



And if we find that the development of certain propensities, that of Amativeness, for example, can be proved by an extensive observation and accumulation of evidence to be *always concomitant, cæteris paribus*, with a change or increase of size in the cerebellum, and especially if no contradictory fact has ever been observed, surely this affords a strong presumption, at least, that the two phenomena are intimately connected with each other. Now, this is the real state of the case on the side of the phrenologists, and if Dr Prichard, by some unaccountable misconception, supposes that the *single* case of the boy forms the *whole* evidence in support of Dr Gall's belief, then he is lamentably mistaken, for it forms neither the hundredth nor the thousandth part of it, as he may soon discover by looking into Dr Gall's work, or into the translation of the part of it in the present volume, and thereafter consulting the works of nature, which are alike open to all.

Dr Prichard thinks it not at all extraordinary, that powerful propensities should *begin* to display themselves at the age of puberty. Neither do we; but we would ask, *why* does an individual only *begin* to display passions at that age, and *why* do not all the passions and faculties appear at the same early period, if they are not dependent on the condition of the brain? We state, as the result of innumerable observations, under every variety of circumstances, that each faculty, active power, or propensity, is connected with a particular organization, in the same way as the senses of sight or smell; and that, therefore, neither the one nor the other can appear, or act with energy, before its organization is fully developed, any more than sight can be present without eyes; and we state it as an *ascertained* and *ascertainable* fact, that the different faculties and propensities do appear, and act with an energy exactly proportioned to the period and extent of development of their respective organs. Thus the feeling of Amativeness is observed to keep pace, *cæteris paribus*, with the degree of development of the cerebellum, and the reasoning faculty with that of the anterior, lateral,



and superior part of the forehead, and the same relation is observed to hold with all the other organs and faculties. Dr Prichard, however, *without advancing any fact against it*, chooses to deny the existence of this relation between faculty and organ, and as a necessary inference from his not believing the operation of the propensities to be at all dependent on "any organic process," he must suppose the late appearance of some of the faculties to be the result of a change in the immaterial soul itself, which must thus assume qualities at one time which it had not at another. To say nothing of the improbability of this notion, it is one, at least, which is perfectly gratuitous, and utterly incapable of proof, and which we may reasonably "be permitted to doubt," for we can know nothing whatever of the nature of the soul, as it exists and acts, unconnected with, and uninfluenced by *matter*. The phrenological statement, on the other hand, has the great advantage of *being susceptible of verification and demonstration*; since any one that has eyes may observe, whether any increase of size, corresponding in point of time to the increased activity of the passions, does take place in the cerebellum and other portions of the brain, at the age alluded to. And supposing, for a moment, that the faculties and organs *are* really connected with and dependent upon each other, surely the method adopted by Dr Gall of investigating the fact by *observation*, is much more likely to lead to the discovery of the extent and nature of the connection, than the plan adopted by Dr Prichard, of "doubting," under the influence of certain "considerations," whether it be so, without using any active means to convert his doubts into certainty.

Dr Prichard ridicules the idea of the College of Surgeons "marching in triumph to the vacant seats of the criminal judges." We confess that there are many instances in which we should not regret to see a well informed and philosophical physician or physiologist taking his seat at the Bench, along with the judges, and benefiting them by his advice. The unfortunate but innocent insane would then



have a still better chance of escaping the cruel fate, which, even in the present enlightened age, not unfrequently overtakes them; and our best feelings would be still more rarely outraged by seeing the victim of disease loaded with the infamy of guilt. If "the halter and the gibbet" could boast of success in the prevention of insanity, and of the crimes not unfrequently arising out of it, and in the reformation of offenders, we should not be disposed to quarrel with Dr Prichard for recommending them, even although they should act through the sole principle of fear, which we regard as at best an inferior motive. But if it is notorious that the terrors of the halter and the gibbet neither diminish the number nor reform the morals of the criminals, we cannot help thinking that their praise comes with a bad grace from a member of that profession, whose chief aim is the relief and prevention of both bodily and mental suffering, in all their numerous forms and varieties.

In concluding, we can assure Dr Prichard, from our own experience, that if he really knew Phrenology, he would speedily be convinced that it is to the labours of the physiologist, much more than to those of the judge or the lawyer, that we must look for the discovery and removal of the most powerful and immediate causes of crime, and for the best means of treating those unfortunate beings who perpetrate them. We beg also to add, that while we differ from him in opinion as to the propriety or wisdom of the course which he has pursued, we feel all due respect for the purity of his motives, and believe that he is really desirous of arriving at truth, although he has attempted to reach it in a very round-about way. On this account we are only the more anxious to see him fairly enter upon the plain path which lay directly before him, and which has already conducted us, and which would also conduct him to more successful results, than any he has yet obtained in this interesting field of inquiry.

*Note.*—The preceding answer to Dr Prichard's objections



appeared first in 1824. Whether Dr Prichard ever read it we cannot tell, but at all events he never replied to it. In his next work on the "Natural History of Man," published three or four years afterwards, he merely alludes to Phrenology in a depreciating way, without venturing on any details; but in 1834, in his article on Temperament in the Cyclopædia of Practical Medicine, he returns to the attack with greater explicitness and determination, and once more tries to extinguish it by additional "considerations," unsupported, however, by even a single fact applicable to the point in dispute. To this article the following answer was published in the fortieth Number of the Phrenological Journal for June 1834.

*Answer to Dr Prichard's Second Attack on Phrenology.*

After a pause of ten years, Dr Prichard once more enters the lists against Phrenology, in an article on Temperament in the Cyclopædia of Practical Medicine (Part xxi. April 1834), and again denounces its doctrines as untrue. He begins by stating, that nearly all that has been advanced of late by English writers against the science, was brought forward many years since, in the most forcible manner, by the Edinburgh reviewer; and then complains that "*similar objections are still frequently repeated*, THOUGH MOST PERSONS HAVE BECOME, OR MIGHT HAVE BECOME, AWARE OF THEIR INCONCLUSIVENESS." It is pleasant to find an able opponent like Dr Prichard thus doing justice upon his coadjutors, and declaring that *their* arguments go for nothing. We only fear that his successors will repay him in kind, and affirm, with equal truth, that both his former and his present objections are not less inconclusive than those which he so properly and unreservedly condemns.

Dr Prichard tells us that, unlike many philosophical theories which can neither be proved nor refuted, "*Phrenology certainly admits of proof or disproof, and would obtain it*



*if the measurements of a sufficient number of heads, and those belonging to persons of marked qualities of mind, could be accurately and indisputably known.*" Substituting the word "developments" for "measurements" this proposition is perfectly accurate; and accordingly, when we read it, we expected it to be followed by a statement of proofs or disproofs, drawn from this as the only authentic source recognised by Dr Prichard and the phrenologists, viz. *the comparison of actual developments with actual mental character.* But, strange to say, Dr Prichard no sooner points out the road which ought to be followed, than he suddenly turns his back upon himself, and walks away in an opposite direction, as quietly as if he had never seen it, or known where it led to.

The direct course of inquiry above referred to, seems not to be congenial to the minds of antiphrenologists in general, for one and all of them carefully avoid it. Instead of pursuing it, Dr Prichard goes back twenty-six years, to the report of the Committee of the National Institute of France against Gall and Spurzheim's *anatomical* discoveries, as a serious stumbling-block; and tells us, that Ackermann, Hartmann, and Rudolphi, agree with that report in denying the justice of Gall and Spurzheim's anatomical claims. We answer, as we have done before (for Dr Prichard does not assume the merit of inventing a NEW objection), that, even granting Gall to have erred in the anatomy, the phrenological doctrines *may* nevertheless be true; since it is well known that the foundation of the new science was firmly laid several years before Gall began his investigations into the *structure* of the brain; and that it rests almost entirely upon *physiological* evidence. But, in reality, so far from the most important facts discovered by Gall and Spurzheim in the anatomy of the brain being now denied or proved to be false, the exposition of its structure, as given by them, is essentially that received by the majority of continental anatomists, and taught at this moment in most of the medical schools of this kingdom. The very plates of Gall's large



work have been extensively copied. Cloquet reproduces almost every one of them in his splendid folio atlas; and Solly, in his late work on the Brain, also copies several of them. Some errors have, no doubt, been discovered and since corrected, because the subject was then confessedly in its infancy; but it is rather too much, on that account, to cast suspicion on the whole anatomical discoveries of Gall and Spurzheim.\* As to Rudolphi's evidence, we have elsewhere shewn how far it is deserving of confidence, and doubt whether, on perusing our analysis of it, even Dr Prichard would be disposed to exempt it from the fate of that which he has already denounced as "inconclusive." The value of Ackerman's authority may be estimated from the fact, that he founds one of his chief arguments against Phrenology on the position that the existence and activity of an organ are inseparable—that if an organ exist, it must be continually performing its functions; † so that, because we have eyes, we *must* be always seeing; and because we have a nose, we must be always smelling.

Dr Prichard quotes from Jacobi a long and spacious argument to prove that Gall's appropriation of particular functions to certain parts of the brain cannot be correct, because insects and reptiles exist which possess similar instincts, without the corresponding cerebral organs,—which,

\* Dr Prichard seems at one time to have had a higher respect for Gall and Spurzheim's anatomical labours than he now entertains. In speaking of these gentlemen in his work on Nervous Diseases, he says, "Although I entertain a high respect for the latter, as well on account of his known qualities as his talents, and *the services he has rendered to anatomy and physiology*, I must take the liberty of doubting altogether that part of his system which refers to cranioscopy." We should like to know whether the respect *then* felt for Dr Spurzheim's anatomical and physiological services was the result of a verification of them by Dr Prichard; and if so, by what means he has been convinced of his own error in entertaining it? If it was not such a result, Dr Prichard must have formerly admitted Dr Spurzheim's merits on doubtful evidence; and if so, may he not *now* be denying them on equally untenable grounds?

† See Gall sur les Fonctions du Cerveau, i. 291.



he says, could not be if the two were really related to each other, as Phrenology affirms. According to our view, the way to throw light on a difficult subject is, not to involve it in additional complications, but rather to separate every thing extraneous from the main proposition, that the truth of the latter may be fairly tested. Thus, when it is said that in man the feeling of the love of offspring bears a relation in intensity to the size of a particular portion of the brain, it is certainly easier to obtain evidence of the fact, by directly comparing the strength of the feeling with the size of the part in a great number of human beings, than by entering on a long dissertation about the instincts of reptiles, which are alleged to have no brains at all, and about whose appetites and feelings almost nothing is known. The former is the mode of investigation pursued by the phrenologists, and acknowledged by Dr Prichard to be the best; but, nevertheless, he adopts the latter, and voluntarily encumbers himself with difficulties and obscurities foreign to his subject, and thus naturally enough arrives at unsound and contradictory conclusions.

But even supposing his mode of proceeding to be correct, his inferences are not warranted by the facts. For in reality the same organ undergoes such modifications in different tribes of animals, that not unfrequently its identity can be established only by proving the similarity of function,—so that it is unsafe to infer from external appearance alone, that any given organ possessed by one tribe does not exist in another. Look, for example, at the organs of respiration. Dr Prichard might deny that in man the lungs are the organs by which the blood is oxygenated, because fishes and reptiles have a similar function *without any lungs* (for in reality they have none); but will any one say that the want of a strict analogy destroys the *direct fact* of the use of the lungs in man being to oxygenate the blood? We know, on the contrary, that fishes have no lungs because they live not in the air but in water, and that in them the blood undergoes the requisite oxygenation in passing through the gills,



which are expressly adapted for the purpose; while in insects, the same process is carried on over the whole surface of the body, which also is intentionally modified to execute this office.

Here, then, we have a proof of similar functions being performed by organs corresponding in their objects, although so modified in appearance as to have almost no resemblance either in structure or in local situation. Again, on Dr Prichard's principle, it may be denied that in man the stomach is necessary for digestion, because some of the lowest of the animal kingdom have nothing corresponding to the bag which we call stomach, but receive their food into a hollow cavity, from which it is again ejected when the nourishment has been withdrawn from it. But we ask, does this want of a human stomach in a zoophyte, warrant the conclusion that in man the stomach cannot serve for digestion?

To give the slightest force to his reasoning, then, Dr Prichard must shew, by incontrovertible evidence,—1st, That modifications, analogous to the above, do not and cannot occur in the *nervous system* as well as the respiratory and digestive organs of animals; and, 2dly, That the functions alleged to be identical are so in reality. As he has done neither, it would be a waste of time to pursue this argument farther. We cannot help remarking, however, that the objection *applies not less to the received doctrine, that the brain, as a whole, is the organ of the mental faculties, than to Phrenology*. Indeed Dr Prichard himself must have had, whilst stating it, some suspicion of its irrelevancy; for, at its close, he admits once more that “*the phrenologist need not go beyond the human species, in order to establish his doctrines on the basis of experience.*” This is sound doctrine, and well delivered by Dr Prichard: but why, we would ask, does he forsake the human species and go to reptiles and insects, in order to disprove Phrenology? His only apology must be his gratuitous belief, that experience is *not* “uniform and unquestionable;” for, wonderful to relate, he says, that even



*the phrenologists do not pretend* that the main principle of the system holds throughout: "They are aware," says he, "of numerous and striking exceptions, the evidence of which, however, they elude, by asserting that when a certain portion of the cranium and brain is greatly developed, while the faculty there lodged has never been remarkably distinguished, it nevertheless existed by nature, though the innate talent, through the want of cultivation, has failed to be displayed; the predominant organic power bestowed by nature was never discovered by the owner, though, according to the fundamental principle of the doctrine, the natural preponderance of talent and propensity is alone sufficient to determine the habitudes of the individual, and communicates of itself a strong impulse to particular pursuits. When, again, a strongly marked propensity or decided talent has been manifested without the corresponding amplitude of structure, it is, in like manner, pleaded that, by sedulous exercise and cultivation, a natural deficiency has been overcome. But should it even be admitted that some few exceptions to general observations may be thus accounted for and allowed not to overthrow the whole system, this concession can no longer be claimed if the exceptions are numerous."

Dr Prichard is much more generous than we are disposed to be in conceding so much. Not only do we deny the existence of "the numerous and striking exceptions" to which he alludes, but we never had the fortune to meet with a single phrenologist who was "aware of their existence." It seems to have been otherwise with Dr Prichard; and as truth is our common object, we shall be most thankful if he will inform us where either the exceptions or the phrenologists who meet with them are to be found. In justice to his own character as a medical philosopher, as well as to the science which he has attacked, he is bound, AND WE NOW CALL UPON HIM, to support his averment by substantial evidence, and thus at once to put an end to the delusion under which, according to him, we and so many of our countrymen are labouring.



If the evidence which Dr Prichard *has produced* be a fair sample of that which he *has in store*, we fear he will have little to boast of. To prove his position, he says that, "if, for example, we should examine a hundred monomaniacs, *in all of whom certain feelings and propensities have been developed, even to morbid excess, and it should be discovered by a person competent to form a judgment on the subject, that NO evidence displays itself in the cranioscopy of so many individuals tending to support the doctrine, we should hold that it ought, in all fairness, to be abandoned.* Some hundreds, and even thousands, of such persons have passed a part of their lives under the inspection of M. Esquirol, who possesses most extensive resources for elucidating almost every subject connected with the history of mental diseases, and has neglected no inquiry which could further the attainment of that object. The result of his observation will be allowed to be of some weight on the decision of this question, in which the appeal is principally to facts of the precise description of those with which he has been chiefly conversant. At his establishment at Ivry, he has a large assemblage of crania and casts from the heads of lunatics, collected by him during the long course of his attendance at the Salpêtrière, and at the Royal Hospital at Charenton, which is under his superintendence. While inspecting this collection, the writer of the present article was assured by M. Esquirol, that the testimony of his experience is entirely adverse to the doctrine of the phrenologists: it has convinced him that there is no foundation whatever in facts for the system of correspondences which they lay down between certain measurements of the heads and the existence of particular mental endowments. This observation of M. Esquirol was made in the presence of M. Metivié, physician to the Salpêtrière, and received his assent and confirmation."

We really join with Dr Prichard's conclusion, that if, in a hundred monomaniacs, in all of whom certain feelings and propensities have previously been so prominently developed as to induce disease, no evidence tending to support the



doctrine can be "*discovered by a person competent to form a judgment on the subject,*" it ought, in all fairness, to be abandoned for ever. But if Dr Prichard believes that the intelligent and benevolent Esquirol is that person, and if his collection of crania and casts be the hostile evidence which is relied on, this only proves, in a forcible manner, that Dr Prichard is himself not competent to judge, or that he has not taken time either to examine the collection of crania, or to ascertain the competency of Esquirol and Metivié to decide on the merits of the question on which they volunteered an opinion. We, too, can say, that "while inspecting this collection;" "we were assured by M. Esquirol, that the testimony of his experience is entirely adverse to the doctrine of the phrenologists;" and that, on the same occasion, Metivié repeated the assurance of his master. But when we asked for the exhibition of a proof, what was the result? Metivié took up one of the lunatic skulls, on the exterior of which were two considerable depressions, to which there was nothing corresponding internally; and nevertheless, said he with triumph, Gall affirms that the outer surface of the skull represents faithfully the form of the inner! We asked if he considered the skull to be *diseased*. He answered that it was. We then asked if he was aware that Gall expressly limited his proposition to *healthy* skulls, and declared that, in disease, great aberrations of thickness and form might exist, *without* any necessary relation to the form of the brain? Metivié looked utterly incredulous, and asked "What right Gall had to make any exceptions!!" To this strange question it could only be answered, that Gall *made* nothing, but only *recorded what he saw existing in nature*; and that, accordingly, when he stated that the rule did not hold in disease, he merely gave expression to a truth in natural history, discoverable by observation alone. But no mortal before Metivié would thence argue that a healthy nose, for example, was not of a shape called Roman, simply because another nose, *in a state of disease*, had a greater resemblance to a Dutch cauliflower. Gall *observed* the want



of conformity caused by disease between the two tables of the skull, and wisely inferred that, on account of that source of error, *proofs* must be sought for only in the state of health, and in so far gave evidence of his accuracy and honesty; whereas Metivié, by his total ignorance of this, and even of the first principles of Phrenology, only proved his own incompetency to judge in the matter to which his testimony was applied.

So far from the evidence which these six hundred skulls and casts afford being really adverse to Phrenology, the fact is so much the reverse, that we would willingly peril the whole science on the very experiment which Dr Prichard proposes. We would even admit Messrs Esquirol and Metivié to be the sole judges, provided they would previously prove to the satisfaction of impartial persons, their "competency" to decide on the form and phrenological indications of the skulls, by each accurately pointing out the situations and natural size of the cerebral organs, in any three skulls in the collection. But we expect, in return for this concession, that if they fail in the preliminary attempt, and prove themselves ignorant both of the doctrine and its practice, Dr Prichard will in turn admit that their opinion is valueless on account of that incompetency. We know it to be a fact, that when we visited Ivry in September 1831, they were in the state of ignorance above represented, and that nevertheless, they then gave the same unhesitating testimony against Phrenology which Dr Prichard says they gave to him. But we had the means of estimating its real worth, which he had not; and hence the error into which he has fallen. In the mean time, we would ask Dr Prichard, whether the ready reception of Esquirol's adverse opinion thus weakly supported, in opposition to that of Georget, Voisin, Falret, Elliotson, Conolly, Caldwell, Barlow, Broussais, Ferrus, Sir W. Ellis, Evanson, and a host of others, supported by evidence which only requires careful scrutiny to be universally admitted, does not look a little like that unphilosophical attribute called *Prejudice*?



Dr Spurzheim used to expatiate on the interest and value of Esquirol's collection, and express a hope that it would one day be given to the public; and we ourselves have ever viewed it in the same light. It was when following Esquirol's Clinique that we first became thoroughly satisfied of the truth of Phrenology. When, therefore, on a more recent occasion, Esquirol told us that he knew the history of most of the individuals to whom the skulls had belonged, we entreated him not to deprive the public of such an opportunity of obtaining accurate information on a deeply interesting subject, and if he could not go into detail, to publish at least a catalogue raisonnée of the collection. He spoke so despondingly, however, of being able to complete his work on insanity, then in the press, that we had little hope of his ever executing his intention of describing the cases which refer to the skulls.\* But not to lose what little hope we had, we urged his and our friend M. Royer, incessantly to remind him of his purpose, and to excite him to its fulfilment by every means in his power. M. Royer being not less impressed than ourselves with the value of the collection, stated that he had already urged, and would persist in urging, the subject on M. Esquirol's attention; but, to our great regret, our friend is now numbered with the dead, and the prospect of the collection being made available to science is now more remote than ever.

If the testimony afforded by these skulls be adverse to Phrenology, it says something, at least, for the candour and love of truth of Dr Spurzheim and the phrenologists, that they should be so anxious to have it produced and verified. Truth alone is consistent with itself, and the phrenologists knew that if their science be true, the evidence to be derived from the collection must be favourable to their views, whether the collection itself have been made by a friend or an opponent. And accordingly, M. Georget, whose talents are admitted, whose opportunities of observation were

\* We are happy to see this work at last announced as on the eve of publication.



long equally extensive with those of Esquirol (seeing that they were for some years derived from the same cases in the same hospital), and who was equally conversant with the history of many of the individual skulls, has avowed to the world his deep and well founded conviction of the reality and importance of Phrenology;—a result which could scarcely have happened, except from the force of truth, considering that he is the nephew of Esquirol, was brought up under his professional auspices, and was naturally inclined to receive his tenets in preference to those of any other authority. Voisin and Falret, also young and able writers, educated essentially in the same school, and with the full knowledge of the nature of Esquirol's collection, have not hesitated to publish their belief in the principles of Gall's doctrine. Are these facts, then, entitled to so little weight in Dr Prichard's estimation, that he unhesitatingly overlooks them, and attaches his faith to the alleged experience of Esquirol and Metivié? We repeat that we are ready to peril the whole doctrine on the real bearing of Esquirol's collection as given by persons competent to decide, and that we ask no greater boon to the science, than to have the specimens and their history made accessible to the public, and their bearings accurately tested.

We regret that we have been forced into this discussion about the value of Esquirol's phrenological opinions; because we can never forget the advantages which we enjoyed in being admitted to his instructive clinical lectures on insanity at the Salpêtrière, and the very high respect which we felt for the talent, zeal, and unwearied interest, and conciliating kindness, which characterized his whole conduct towards the unhappy inmates of that vast establishment. His visits and lectures were not only valuable professional, but highly important moral lessons. No one could attend them for a season, and witness their effects in soothing misery, alleviating wretchedness, and inspiring hope into the sinking heart, without being improved in his own moral feeling, and impressed with a higher sense of the dig-



nity of the profession. Neither can we forget the personal kindness and attention with which, on a more recent occasion, he conducted us through the hospital at Charenton and his splendid private establishment at Ivry, and dedicated three hours to the gratification of our curiosity in regard to the details. Gratitude for these advantages and acts of kindness would have effectually prevented us from dragging forward opinions which we think he has inconsiderately emitted, and which, so far as we know, he has never obtruded. But when we find a physician of Dr Prichard's reputation strenuously founding on them in a work of high authority and wide circulation, and which exercises great influence over the opinions of the rising generation, and thus may become the means of retarding the progress of the greatest discovery of the age, we can no longer allow personal considerations to stand in the way of their entire refutation. But we have endeavoured to expose the fallacy of Dr Prichard's arguments, and the erroneousness of M. Esquirol's opinion, without failing in that respect which is due to both of them as men of science and men of sense; and trust that in this effort we have not been unsuccessful.

*Remarks on Dr Prichard's third attack on Phrenology in his "Treatise on Insanity."*

Apparently not satisfied, any more than ourselves, with either of his preceding refutations of Phrenology, Dr Prichard once more returns to the attack, and, in the form of fourteen pages of a "survey of phrenological evidence," in a "Supplementary Note" to his *Treatise on Insanity*, aims another blow at its devoted head; but whether with any greater effect than before, we shall speedily enable our readers to judge for themselves. This last "refutation" was published two years ago (in 1835), notwithstanding which Phrenology, to the best of our information, was never before in such robust health, or so much in favour with men



of science, as at the present moment. We rather fear, therefore, that, if Dr Prichard shall live a few years longer, and again meet with his foe in all the vigour of youth, he will feel himself constrained to admit, that his past efforts to consign him to the tomb of all the Capulets have been wholly ineffectual, and that his time might have been better bestowed than in the vain attempt to extinguish truth. We feel compelled, indeed, to add our conviction, that, if clever sophistry, ingenious misrepresentation, and utter disregard of evidence, could have extinguished Dr Gall's immortal discovery, Phrenology would have been dead long ago, even without Dr Prichard's able and unwearied assistance. But although such weapons are not without effect in *retarding*, they are wholly ineffectual in *extinguishing*, truth; and we are therefore surprised that the very circumstance of Phrenology continuing to advance and flourish, in spite of all his efforts to annihilate it, should not have led Dr Prichard to suspect that his plan of attack was essentially unsound, and that, in persisting in its use, he was merely exposing to view the strength of his own prejudices.

If the consequences of Dr Prichard's proceedings were to be limited to himself, we should much prefer leaving him to discover and regret at his own leisure the pains which he has taken in his different works to arrest the progress of physiological discovery. But when his talents and influence are directed to turn aside his numerous readers from the direct path of philosophical inquiry, and to prejudice the young physician against a doctrine of which every day reveals more and more clearly the great magnitude and utility, his errors at once assume an importance which may be denied to the author as an individual, and demand a full refutation and exposure. Hence it is that we continue the disagreeable duty, *forced upon us by Dr Prichard himself*, and shall now proceed to offer some remarks on his third and most recent attack.

In his "Survey of Phrenology," Dr Prichard says, "I have had my attention directed for many years to this in-



quiry," (whether the peculiarities in the shape of the head in deranged persons correspond with the observations of Dr Gall and the school of phrenologists), "and have omitted no opportunity that has presented itself of gaining information on this subject." On this we are compelled to remark, in the first place, that Dr Prichard must either have overlooked the many very accessible opportunities of obtaining information from the phrenological works published during the last fourteen years, or have treated with utter disrespect all the information contained in them which did not chime in with his preconceived opinions. Any man but himself, who was really in quest of information on the subject, would not have failed to consult its most authentic record, the Phrenological Journal, now in the fifteenth year of its existence, and to have assigned some reason for wholly disregarding the several answers contained in it to his own specious but most inconclusive objections. So far, however, is Dr Prichard from pursuing this rational course, that he once more appeals to Esquirol's collection of skulls as affording demonstrative evidence against Phrenology, although, a full year before, we had assigned very strong reasons for believing it to have directly the opposite tendency—reasons which he has not only not answered, but to which he does not even allude. Dr Prichard may no doubt affirm that he has not seen the articles alluded to. Probably not; but if so, is any man justified in pronouncing an authoritative opinion, *calculated to injure an important cause*, without previously making himself acquainted with the facts and pleas of its defenders? Nobody—not even Dr Prichard—will answer in the affirmative; and if he is zealous for truth alone, why should the phrenological books be the only ones which he does not consult, and the phrenological facts the only ones which he thinks himself entitled to throw overboard unexamined, when the truth or futility of Phrenology is the point which he is professedly discussing?

Dr Prichard attempts to destroy Phrenology by theoretical reasoning and false analogies; but why not go honestly



and at once to the matter of fact? Phrenologists have all along insisted that the truth of Gall's discovery is a *question of fact, which can be determined only by direct observation of its accuracy; that the functions and localities of the organs were discovered successively by actual observation of phenomena, and not by previous guessing or invention; and that therefore the direct and only conclusive way of refuting them is to disprove them by opposing and inconsistent observations.* Dr Prichard is so far aware of this, that, although his grand objection is founded on an erroneous analogy drawn from the lowest of the brute creation, he distinctly admits, 1st, That "*the phrenologist needs not to go beyond the limits of the human species in order to establish his doctrines on the basis of experience;*" 2dly, That, "*if relative amplitude in a given region of the brain were always coincident with a proportional display of one particular faculty or quality of mind, the constant coincidence would prove a connection between the two phenomena,*" (p. 475); 3dly, That "*Phrenology certainly admits of proof or disproof, and would obtain it if the measurements of a sufficient number of heads, and those belonging to marked qualities of mind, could be accurately and indisputably known;*" and, 4thly, As a necessary result from acting on the above admissions, he justly infers, that "*if the testimony of facts on a great scale should be found adverse to the alleged coincidences, or to the correspondence of given mental qualities with certain conditions of the brain, Phrenology will not continue to make proselytes, and it will be ultimately discarded as an hypothesis without foundation. At present most persons seem to be in doubt on the subject, and to be looking out for evidence.*" (P. 476.)

Dr Prichard, then, is not ignorant of the proper mode of conducting the inquiry. After the above distinct enunciation of the means by which Phrenology is to be proved or disproved, if he were consistent with himself, he would at once resort to experience, and adduce "*the testimony of facts on a great scale,*" in support of his "*strong persuasion that the time is not far distant when the whole theory will be*



abandoned!" So entirely, however, does he turn his back upon this only rational mode of investigation, that, in the very next sentence, he goes on to say—"I have taken every opportunity that has occurred to me for many years, of *making inquiries of persons* who had a great field of observation within their reach, what had been the result of *their* experience on this subject." It is true that this passage refers specially to the evidence to be drawn for or against Phrenology from cases of derangement, but as Dr Prichard states that he has been himself conversant with the insane for many years, and thus had an ample field of investigation within his own reach, what good reason can he assign for resorting in preference *to the experience of others*, in a matter which, so far as the truth of Phrenology is concerned, (and that is the real question in dispute), is capable of direct proof, not only by physicians to asylums, but by every observant and reflecting person who lives in the society of his fellow-creatures? If Dr Prichard distrusts his own powers of observation, what are his superior grounds of confidence in those of others? It will not do to answer that *he* wants an adequate field of observation. A physician in good practice has peculiar advantages for testing the truth of Phrenology, because he is daily and hourly in contact with human nature stripped of most of its conventional disguises. Why, then, does Dr Prichard shrink from the responsibility of judging for himself from facts before his own eyes? If he has more confidence in the testimony of others than on his own, would it not be more consistent in him to leave also *the public expression* of that testimony to others, seeing that it comes weakened and at second-hand through him? And if, notwithstanding such considerations, he still insists on being the medium of communicating the testimony of others, on what grounds does he rest his conviction of the superior accuracy of the one party over the other? And by what strange accident is it, that he attaches weight to the observations of *those only who are opposed* to Phrenology, while the "facts on a great scale," of those who advocate its



truth, go for nothing with him, and are not only not refuted, but not even alluded to?

To a phrenologist who has become convinced by extensive and diligent observation, and who has made himself familiar with the multitude of facts recorded in the eight volumes of Gall, in the works of Spurzheim, Vimont, Combe, and others, and, lastly, in the pages of the Phrenological Journal, it seems not a little extraordinary, that Dr Prichard should so quietly set aside all the soundest rules of philosophical investigation, and attempt to disprove such a mass of direct evidence, by a few pages of theoretical assumptions and round-about analogies. He tells us, no doubt, that "*great and striking facts display themselves which are adverse to the hypothesis.*" But when we read on, in the hope of at last finding some specific statements of an adverse kind, we are again thrown back upon the old authority and the old analogy, and assured that "*birds and reptiles, as Jacobi has observed, are nearly or wholly destitute of many cerebral parts, which, in mammifers, are held as of high importance for the manifestation of psychical properties, and yet they display psychical phenomena similar to those of mammifers.*" We have already noticed this statement, and must now ask Dr Prichard whether, when he brought forward Jacobi's hostile assertion, he was aware that Dr Vimont had published a magnificent work, in two quarto volumes, with a folio Atlas of Plates, on Human and Comparative Phrenology,—not only shewing the exact coincidence between the brains and "*psychical phenomena*" of the lower animals, but proving his positions by upwards of 700 admirably engraved specimens of the *skulls* and *brains*, not of man alone, but of *mammifers, birds, and fishes*, and the perfect accuracy of which has never yet been disputed? If Dr Prichard was aware of the fact, was it fair in him thus to pitch the mere phrase "*as Jacobi has observed,*" as conclusive evidence against the many volumes of direct facts published and submitted to ocular demonstration by Gall, Vimont, and other phrenologists? If he was not aware of the existence



of Dr Vimont's work, was he justified in reposing such unbounded confidence in Jacobi's opinions, merely because they were hostile to Dr Gall?

But let us even take Jacobi's observation in its fullest force, and see of what avail it will be to Dr Prichard. As shewn in our former answer, it amounts simply to this, that, in one order of animals, in the dog for example, a given organ, say the lungs, cannot be the organs of breathing; *because*, in another order of animals, say a fish, adapted for a totally different mode of life, the same organ does not appear in the same identical form and place, but is modified to adapt it for breathing in water instead of air, as the dog does. Or, in other words, the lungs cannot be the organs of breathing in the dog, because a fish has no lungs and yet breathes! and yet, notwithstanding this startling Jacobian objection, nobody ever thinks of denying that dogs really breathe by means of lungs, although a fish has the misfortune to breathe by means of gills. And since it is so, why may not the nervous organs of instinct be modified as well as the lungs, to suit them for their special purposes in reptile or insect life? When we consider the true bearing of the evidence, then, ought we not to *reverse* Dr Prichard's proposition, and say that "great and striking facts display themselves which are FAVOURABLE to the hypothesis, for Drs Gall and Vimont have observed, &c." seeing that the real analogy, as well as the direct evidence, is all in their favour? And is there no want of candour in Dr Prichard thus adopting Jacobi's and rejecting Gall's and Vimont's observations, when he has verified neither, and can assign no grounds for his preference of the former? We leave him to answer.

Dr Prichard, however, does not absolutely withhold his own evidence, although he prefers that of his neighbours. After the above quotation of Jacobi's observations, he continues: "Whenever an undoubted and tangible fact can be laid hold of in the different proportional development of cerebral parts, which can be brought into comparison with the relative differences of animal instinct or of psychical pro-



perties in general, there is, *if I am not mistaken*, a manifest failure of correspondence between the two series of observations," p. 473.

Such is the weight of Dr Prichard's testimony; and who that has perused Dr Gall's published evidence in proof of the functions of even a single organ—the cerebellum—evidence to every line of which Dr Prichard had access before he penned a sentence of his work, can fail to regard the mere expression of *his* opinion, backed by his modest admission "if I am not mistaken," as an unanswerable refutation of all Dr Gall's facts? It may be thought that, as many of Dr Gall's cases are drawn from authors hostile to Phrenology, they ought to be considered as strong evidence in its favour. Ordinary inquirers would certainly view the matter in this light, but not so Dr Prichard; on him this circumstance makes so little impression, that he never even hints at the existence of the cases referred to; but, on the contrary, takes the cerebellum under his especial protection, and supported of course by "facts on a large scale," enobles its functions by locating the intellect in its folds!

The reader may smile when we make such a statement, but let him listen to Dr Prichard, and his gravity will be restored by the closeness and solidity of his logic. "When we consider the great amplitude which the cerebellum attains in man in comparison with its size in the lower animals, *we are obliged, if we really attach any importance to such a system of correspondences, to acknowledge some relation between this circumstance and the transcendent superiority of the human intellect*, compared with the psychical powers of brutes. Other paths of observation lead us to a similar conclusion. Cretins, in whom the cerebellum is very defective, display, in different degrees, idiotism or deficiency of intellect, but no correspondent weakness in the sexual instinct, which, on the contrary, often exists in such unhappy beings in the greatest intensity, and impels them to violent excesses. Again, injuries of the posterior part of the head are observed to be followed by stupor and loss of memory, indi-



eating the function of the cerebellum to be connected with the exercise of the mental faculties rather than with that of the animal propensities." P. 474.

It is singular, indeed, into what inconsistencies even a talented man will run, when his opinions are not based on actual observation of nature, and his object is ostensibly the pursuit of truth, but in reality to combat a proposition against which his prejudices are arrayed. The Dr Prichard who, in his zeal to annihilate Phrenology, is thus ready on the slenderest semblance of evidence to connect the "transcendent superiority of the human intellect" with the size of the *cerebellum* in man, is nevertheless the same Dr Prichard who, when not combating Phrenology, and when consequently his prejudices are asleep, speaks in the following very different terms. When discussing the question in his "Natural History of Man," whether, as alleged by Camper, the intelligence of an animal is proportioned to the openness of its facial angle, or, in other words, to the development of its *forehead*, Dr Prichard says, "As far as we can at present form an opinion on this matter, I think we must allow that *experience is in favour of the general position* assumed by Camper and other writers on physiognomical subjects. *It is certain that every man is struck with the expression of dignity or elevation of mind and character in the ancient busts which have a great facial angle, and that this expression would be lost if the facial angle were contracted. This perception must be founded, as it would appear, on experience.* THE FACT SEEMS, INDEED, TO BE A GENERAL ONE, THAT MEN OF GREAT INTELLECT HAVE FULLY DEVELOPED BRAINS, AS INDICATED BY ELEVATED AND CAPACIOUS FOREHEADS. It hence appears probable, that there is a foundation in nature for Camper's physiognomical estimate of the mental capacities of individuals." (Vol. i. p. 162.)

Here, then, we have Dr Prichard, on the one hand, ascribing the transcendent superiority of human intellect to a well developed *cerebellum*, and on the other admitting the "general fact" that great intellect is indicated by an "ele-



*vated and capacious forehead.*" But as both propositions cannot possibly be true, to which of them will Dr Prichard *now* adhere? If he admits the one sanctioned by "experience," then he must in so far countenance Phrenology; whereas, if he adopts the other, because of its hostility to that science, whence will he derive the facts on a large scale required for its support, seeing that it is *not* sanctioned by "experience?" And of what value, we earnestly ask, can Dr Prichard's opinion be considered against Phrenology, when his ignorance of the subject is so complete as to admit of his advocating two different views so wholly incompatible with each other?

Dr Prichard may think we were severe in animadverting upon his preference of the authority of others to that of self observation in a question of fact. But can he candidly declare that the above contradictions do not warrant all that we have expressed? If he had founded either of his opinions on observations made by himself, and repeated till he obtained positive conviction of its truth, he could not, by any possibility, or by any amount of prejudice, have been induced afterwards to run his own head against it on the mere dictum of another, even although that other was Esquirol.

When Dr Prichard wrote the above comments on the connection of the intellect with the cerebellum he must either have never read a word of Dr Gall's evidence, or have calculated that none of his readers would ever look into the original work in which it is recorded. And truly, when its inaccessibility to the British public leads to such commentaries as the above from men of science and reputation, the interests of truth call loudly for its translation. If the mass of facts adduced by Dr Gall were generally accessible to English readers, the absurdities thus gravely propounded by Dr Prichard might be safely left to their own weight; for if there be one point more than another on which all physiologists, in common with all other observers, and even as we have seen with Dr Prichard himself, are agreed in



regard to the functions of the brain, it is that intellect is seated, not in the cerebellum, but in the forehead. So thoroughly biassed, however, is Dr Prichard's mind on this subject, that it would be in vain, we suspect, to ask *him* whether a large cerebellum is *always*, or even commonly, accompanied by transcendent superiority of intellect? or whether, in the first twenty men he could meet, fifteen would not disprove the alleged concomitance? In Dr Prichard's eyes, it would be of no avail to produce a hundred cretins with large cerebella, accompanied with the transcendent intelligence of IDIOCY! We assert positively, that we have seen at least twenty such cases, some of them living cretins, and others skulls in anatomical collections, the histories of which were known. Upwards of three years ago, *we called upon him to produce his hostile facts*, and WE NOW REPEAT THE CALL. Standing in the responsible and influential position of an able physician and successful author, he has no right to continue, year after year, throwing out unfounded opinions, calculated to retard the progress of momentous and beneficial truths, and to imbue the minds of his youthful readers with prejudices hurtful to themselves, and hurtful also to the society of which they will soon become the active and influential members, and to shrink from proving them. It is from the importance which we attach to his influence, and not for his individual benefit, that we have written thus earnestly on the subject.

After stating that he has taken every opportunity that has occurred to him for many years, of making inquiries of persons who had a great field of observation within their reach, what had been the result of their experience on this subject, Dr Prichard adds, "Many of these persons have been physicians who were superintendents of extensive lunatic establishments. Some of them have *been men who had addicted themselves to the study of Phrenology, and were predisposed to imbibe the opinions of its authors*; some have been persons distinguished by their researches in the anatomy and physiology of the brain and nervous system. Among



them. *I do not remember to have found one who could say that his own observations had afforded any evidence favourable to the doctrine.* Yet we should imagine that a man who lives amidst hundreds of monomaniacs, must have constantly before his eyes facts so obvious that he could not be mistaken in their bearing."

When we read these observations, we could not help thinking that Dr Prichard's "opportunities" must either have been very rare, or his memory very treacherous, seeing that in the whole course of his inquiry, he does not remember to have received *a single answer* favourable to Phrenology. We also know persons who have been, or are still connected with the management of asylums, and not a few of them are "men who have addicted themselves to the study of Phrenology;" and we must declare unhesitatingly that Dr Prichard's statement is not supported by our experience, or by the testimony we have received from others. We do not mean to say that he misrepresents the nature of the answers elicited by his individual inquiries, but merely that he must have been singularly unfortunate in his opportunities when he received such answers. When Dr Prichard penned the above assertion, had he pursued his inquiry in such a careless or one-sided spirit, as not to be aware that Georget, the nephew, and for years the assistant, of Esquirol, in the asylum over which the latter has so long presided, not only believed the evidence to be favourable to Phrenology, but published his unhesitating conviction of its truth and importance in the very work from which he (Dr P.) quotes, without alluding to the fact? \* If Dr Prichard knew this, why does he make an assertion at variance with its spirit? If he did not know it, what weight is due to an inquiry conducted so loosely that he overlooked a feature which pervades the whole work?

When in his recent treatise Dr Prichard again adduced Esquirol's collection and opinion as *fatal* to Phrenology,

\* Georget, de la Folie. Paris, 1820.



was he aware that the true bearing of the former, and the utter irrelevancy of the latter, had been exposed in detail a full year before in the pages of the Phrenological Journal, and himself called upon to gainsay, if he could, any one of the facts on which that exposure was founded? If he was, why does he shrink from our challenge, and repeat his statement as if it had never been questioned? If he was not, we must again ask, Is it fair, where our object is truth, to consult the authorities of only one side? Again, when Dr Prichard quoted the work of Voisin,\* (who also studied under Esquirol), was he aware that he also takes Phrenology as the groundwork of the pathology of insanity? If he was, why did he venture the assertion that he remembered no opinion favourable to its pretensions from men devoted to the study of derangement? If he was not, what weight is due to his testimony when he could overlook the fact? When Dr Prichard represented Esquirol's skulls as adverse evidence, and affirmed that he had never met with a physician to the insane favourable to Phrenology, was he aware that the same Dr Voisin, at p. 354 of the same work, thus speaks of that very collection? "We shall add that M. Esquirol, having made a numerous collection of skulls and busts of deranged persons, will one day be able to publish valuable information on the relations between the form of the head and the different disorders of intellect, *and thus illustrate many points of the physiological doctrine of the brain taught by Dr Gall.*" If he was, why did he confine his commentary to the opinion of Esquirol, and not also state what those "addicted to the study of Phrenology" thought of these skulls? If he was not, was it fair to bring forward his one-sided view as irrefragable evidence?

When Dr Prichard could not recollect having received any favourable answer to his inquiry among those conversant with the insane, did he know enough of Phrenology to discover that the work on Suicide by Falret (also a pupil of

\* Voisin des Causes Morales et Physiques de l'Alienation Mentale. Paris, 1826.



Esquirol), from which he quotes, is based throughout on the principles of that science, although in a less explicit form? Or was he still ignorant of the fact, that the celebrated Broussais, who, like himself, has written a treatise on Derangement, not only adopts Phrenology, but has written expressly in its defence? And, lastly, is Dr Prichard yet aware that Dr Ferrus, the physician to the immense Asylum at Bicetre, and whose opportunities of observation cannot be surpassed, has also published his conviction of the truth and importance of Phrenology in relation to insanity? With these evidences before his eyes, will Dr Prichard still abide by his extraordinary assertion, that those who have had opportunities of testing its merits are hostile to Phrenology?

But we have not yet done. In this country, as well as in France, we are acquainted (although Dr Prichard is not) with many physicians to lunatic asylums whose observations have proved favourable to Phrenology, and we shall direct him to the names of a few. In the appendix to the fourth edition of "A System of Phrenology," by George Combe, a number of testimonials are printed from persons "who have addicted themselves to the study of Phrenology;" among whom the following are surgeons or physicians to lunatic asylums, viz. S. Hare, Esq. Proprietor and Medical Attendant of the Retreat for the Insane at Leeds; Dr James Scott, Surgeon and Medical Superintendent of the Royal Naval Lunatic Asylum at Haslar; Sir William C. Ellis, M. D. Superintendent of the Lunatic Asylum for the county of Middlesex, at Hanwell; Dr Disney Alexander, late one of the Physicians to the Wakefield Dispensary and the Pauper Lunatic Asylum; W. A. F. Browne, Esq. Surgeon and Medical Superintendent to the Montrose Lunatic Asylum; H. A. Galbraith, Esq. Surgeon to the Glasgow Royal Lunatic Asylum: Every one of these gentlemen has borne solemn testimony "that their own observations have afforded evidence favourable to the doctrine." A great extent of additional evidence from persons of distinguished re-



putation in favour of the truth of Phrenology, was laid before the Town Council of Edinburgh by Mr George Combe, in 1836, and subsequently published.\* We call on Dr Prichard to publish the names or testimonials of *his* men "who have addicted themselves to the study of Phrenology," and "who cannot say that their own observations have afforded any evidence favourable to the doctrine." We defy him to produce half a dozen who will preface their testimonies by a solemn declaration that they know the situations and functions of the cerebral organs, and have compared the size of these with mental manifestations, and found the results to contradict Phrenology. Unless thus qualified, their observations were worthless.

Dr Prichard, after introducing the statement about M. Esquirol and M. Foville, already answered, continues to say: "Testimonies to the same result may be collected from unbiassed witnesses whose evidence taken collectively may have nearly equal weight. Among these are men unscientific, though capable of correct and unprejudiced observation, as well anatomists and physiologists. In the number of this latter, is RUDOLPHI, who declares that he has examined many hundreds of brains without finding any thing that appeared to him favourable to the phrenological theory," p. 477.

After the exposition which has been given on p. 241, *et seq.* of the real value of Rudolphi's objections to Phrenology, we leave Dr Prichard to enjoy the whole benefit of his testimony unenvied and undisturbed. But we must again remark, that with these answers to Rudolphi's opinions before him, Dr Prichard was bound to have replied to them, or to have admitted their weight. In a litigation, a barrister states his client's case, and says every thing in his power to support it, reckless of the weight of testimony and force of argument adduced by his opponent. One would imagine Dr Prichard to be a lawyer practising his profes-

\* Longman & Co. and Simpkin, Marshall, & Co. London, and Maclachlan & Stewart, Edinburgh.



sion, and that he viewed himself as hired to plead every possible objection, sound or unsound, true or false, against the claims of Phrenology, he being answerable only for the zeal and talent with which they were brought forward. The cause of truth, however, cannot be promoted by such means.

*Note by the Editor.*—To the preceding remarks, we beg to add, that Dr Prichard concludes by observing, that, “If the examples of accurate research into the anatomy of the different parts of the brain, connected with physiological and pathological inquiries, of which Dr Abercrombie, Dr Bright, and Dr Hodgkin have set examples in this country, should be followed they cannot fail of leading to a full elucidation of this (the functions of the brain and cerebellum) and many other subjects hitherto involved in doubt.

With all deference to Dr Prichard, we are humbly of opinion, for the reasons before stated on p. 181, *et seq.*, that the researches of these able men have failed, and will continue to fail, in leading to a full elucidation of these subjects, because the fundamental principles on which they proceed are palpably defective. They attempt to establish a pathology of the brain, without previously ascertaining its physiology. They confound the phenomena of the vital, with those of the animal, moral, and intellectual functions of the brain; in short, to make use of the illustration before adduced on p. 184, they observe and record, with an air of solemn gravity, the discordant jingling sounds of worn out and broken down musical instruments, as the best means of discovering the notes and compass of such instruments when in a state of perfection; and they and Dr Prichard treat the phrenologists with contempt for affirming that this method is absurd, and that it is *impossible* to discover the notes and compass which any musical instrument possessed *when entire*, by observing the sounds emitted by it *when broken*.

In surveying the utterly incompetent and unphilosophical methods of investigation which are extolled by the opponents of Phrenology, as examples of philosophical wisdom and deep penetration; in reading the shallow sophisms ad-



duced even by able men as grave arguments against this science; and in weighing the flimsy assertions which are at once admitted by these authors as conclusive evidence against it,—we are amazed at the extent to which prejudice can blind the understanding and pervert the judgment. Sooner or later the public mind will free itself from the degrading bondage in which it has so long been held by prejudice on this subject; and we venture to predict, that whenever it shall arouse itself, the authority and philosophical reputations of these names, will vanish like vapours that have obscured the morning rays, but which are dissipated for ever by the meridian blaze of the sun of truth.

*Remarks on the Opinions of Tiedemann, Arnold, and Prichard respecting Phrenology, and the Inconsistency of the Antiphrenologists.*

(Reprinted from *Phrenological Journal*, vol. ix.)

Our readers will be amused, if they will take the trouble to contrast the *sayings* and *opinions* of the antiphrenologists with each other, and see how much each admits, which the other denies. In the article on Temperament, by Dr Prichard, to which we have already alluded, that learned opponent gives sundry weighty reasons for believing *the cerebellum to be the seat, not of Amativeness, but of the intellect!* and, as a proof, he avers, that many Cretins with small cerebella manifest strong sexual desire, but little or no intellectual power,—facts which he says he can reconcile with the above theory, but not with Phrenology. Tiedemann, the celebrated professor at Heidelberg, propounds a different view of the matter; and while he is equally hostile to Phrenology, and to the connection of Amativeness with the cerebellum, he chooses a more dignified habitation for the intellect, and declares in his lectures to his wondering students, “*that persons with large foreheads are endowed with superior intellects, and that individuals with small heads have inferior intellects. The brain of Cuvier, which was unusually*



*large, will illustrate the first, and the skull of this idiot (shewing one) the second."* This is not amiss for a great antiphrenologist like our friend Tiedemann; but what says Dr Prichard on the same subject? He disapproves altogether of this doctrine, and gives the palm to the head of moderate or smallish size. "It would rather seem probable," says he, "that the state of interior organization, from which the highest degree of energy in its" (the understanding's) "appropriate action may be supposed to result, would be found in a brain, the volume of which, both generally and in its parts, has the medium degree of development, or is neither greater nor less than the average dimension. As far as our experience and observation reaches, it bears out this presumption: the individuals whom we have known possessed of the greatest intellectual powers have been those in the form and size of whose heads, compact and of moderate volume, nothing remarkable presented itself."\*

It would be curious to discover whether Dr Prichard has a moderate-sized head, and Tiedemann rather a big one! The result might enable us more easily to reconcile them to each other. In the mean time, it is not too much to conjecture, that the intellectual persons known to Dr Prichard are somewhat inferior to such men as Napoleon, Sully, Chatham, Franklin, and Washington; and, moreover, that possibly he is not an adept in the art of distinguishing the signs of intellectual talent.

We cannot help thinking that Dr Prichard has examined the Cretins very imperfectly, when he speaks of their intellects, and not their appetites, bearing a relation to the size of the cerebellum. We have seen numbers of them with unusually large cerebella, in whom reason was but a ray, compared to the energy of the sexual passion which they manifested; and we can state, as an additional fact, that, in such cases, the forehead is either unusually small and contracted, or presents the appearance of *morbid* distention.

\* Cyclopædia of Practical Medicine, article TEMPERAMENT, No. xxi. p. 174.



In a very few instances, nothing remarkable appears in its external configuration, but the whole expression and aspect of the body indicate structural disease in the brain itself.

But to return to Tiedemann. "This," he continues, "would appear to shew that there is some truth in the doctrines of Gall and Spurzheim, and it would be well if the heads of individuals intended for an intellectual or studious life were measured before they commenced their studies, as many disappointments would be avoided. The assertion, however, that in one part of the brain resides this faculty, and in another that, I cannot believe. In dissection of intellectual persons, the convolutions are found more numerous than usual, and the anfractuositities deeper. In women the sulci are less deep than in men."

We are glad to perceive that Dr Tiedemann is a man of the practical understanding which the above quotation betokens. No doubt, a statement of facts like his *does* "appear" to support Phrenology, but it is Nature and not Tiedemann that must be blamed for the coincidence. It is evident that he would have avoided every appearance of supporting such doctrines, if truth would have allowed him. As it is, we suspect that he is a sounder Phrenologist than many who arrogate the title. He distinctly, although by implication, admits the fundamental principle of size of brain being an index of mental power; and he farther admits, that intellect has a direct relation to the cerebral convolutions situated in the anterior lobes. If, after these admissions, he differs as to the functions of other parts of the brain, it is a difference only as to details; and when *principles* are once established, details can be easily verified and corrected. We are bound, indeed, to declare, that the learned Professor is not conscious of being a phrenologist; but his evidence in its favour is only the more valuable on that account, and whatever he may now do or say about the cerebellum is of little consequence, as time and farther progress in his new field of study will ultimately remove all his present difficulties.



Having noticed the opinion of one of the Heidelberg professors respecting Phrenology, we take the opportunity of advertizing to those of another. We learn from excellent authority, that Professor Arnold stated in his lectures last summer, that he agrees with Gall in thinking that the cerebellum is the organ of Amativeness; though he believes it—for what reason we know not—to be also in some way connected with involuntary motion. Personal observation has satisfied him that the animal, moral, and intellectual faculties are connected with different regions of the brain; and he entirely concurs with Gall as to the individual regions occupied by each class of faculties, but, like Tiedemann, thinks that Gall has gone too far in asserting that these regions consist of a number of smaller organs. Arnold, then, admits, *from observation*, the grand fundamental principle, that different parts of the brain perform different functions; and, in particular, that on the basilar and occipital regions depend the propensities, on the coronal region the moral sentiments, and on the forehead the intellect. As a commentary on his and Tiedemann's refusal to admit the existence of organs of individual faculties—in other words to assent to the details of Phrenology—we shall extract, but without meaning to apply the whole of it to the two professors, a lively and forcible passage from a work published in 1829 by Dr Caldwell of Lexington.

“Nothing is more common,” says Dr Caldwell, “than for physicians and others, who ought to be better informed, to observe very gravely, and, as some may think, *very knowingly*, ‘We believe in the *general* principles of Phrenology, but not in its details.’ But a few years ago, those same sage and cautious gentlemen denounced it, ‘by the lump,’ ‘principles,’ and all. This they will not deny. But times have changed, and they have changed their creed and their tone. Phrenology has gained *strength*, and, in the same ratio, have their opposition and hostility to it gained *weakness*. They think *by fashion*, as they shape their apparel. They feel the breeze of popular sentiment with as much attention



and accuracy as they do their patients' pulses, or as they examine the state of respiration by means of the stethoscope, and 'turn and turn' as it turns, yet 'still go on.' Thus do they completely verify the common adage, that those who 'talk at random should have good memories.' Although *they* may forget, the *world* will remember.

"But let them occupy their new ground undisturbed. What have they gained by it? What are the meaning and force of their objection to Phrenology? Literally nothing. In the 'general principles' of the science they avow their belief; and in that avowal they concede every thing. What are 'principles?' Generalizations of 'details,' and nothing more. They are but aggregates or classifications of recognised facts. 'Details' are parts—'principles' the whole. Of Phrenology, this is proverbially true. By those who know the history of it, it is perfectly understood, that, in all his discoveries, in developing the science, the march of Gall was from 'details' to 'principles'—from individuals to generals—not the reverse. His method, like that of Bacon, was strictly inductive. In this consisted his chief merit as a discoverer and a philosopher. Could he, then, out of *false* 'details' construct *true* 'principles?' No antiphrenologist will answer in the affirmative. No such alchemy pertained to Gall or any of his followers. Nor did they ever profess it. It is by their *opponents* that it is *virtually* professed; and to them belongs the task to reconcile the inconsistency, or to bear the burden of it.

"But they cannot reconcile it. As well may they attempt any other impossibility, and as soon will they succeed in it. If the 'general principles' of Phrenology are true, so are its 'details.' If the *parts* be corrupt, the *whole* cannot be sound. The enemies of the science, then, have but one alternative; to reject or receive it *in toto*.

"But wherefore is it that the opponents of Phrenology do not believe in its 'details?' The reply is easy. They have not studied them, and *do not, therefore, understand them*. It is praise enough for any one, to say of him, that he tho-



roughly understands what he has carefully studied. What he has not thus studied, no man ever yet understood, nor ever can. But to pursue 'details' is much more troublesome and laborious, than to comprehend 'principles' when completely established and clearly enunciated. Hence the reason why, as relates to Phrenology, gentlemen profess a belief in the latter and not in the former. Let them first acquire a correct and thorough knowledge of the latter, and then deny and subvert them, if they can. As soon would they dream of denying, or attempting to subvert, the facts of the descent of ponderous bodies, the reflexion of light, or the pressure of the atmosphere. Why did the prince of Ceylon disbelieve in the consolidation of water by cold? He was ignorant of 'details.' Why have the Chinese denied the possibility of throwing balls to a great distance, and with a destructive force, by means of water acted on by fire? For the same reason, an ignorance of 'details.' Why did the world remain so long incredulous of the identity of electricity and lightning, and of the compressibility of water? Franklin and Perkins had not yet instructed them in the requisite 'details.' Away, then, with such idle affectation of sagacity and wisdom! It is but a tattered covering for a want of information; a hackneyed apology for a neglect to inquire. In truth, with men who make a *pretence* to knowledge, a 'disbelief in details,' and an entire ignorance of them, are too frequently synonymous expressions. As relates to the opponents of Phrenology, this is certainly true. To know the 'details' of that science, and to believe in them, are the same. No one has ever thoroughly studied them, by a faithful examination of man as he is, without arriving at a conviction of their truth. If such an instance has ever occurred, it has been in some individual whose cerebral developments were unfavorable; in plainer English, *whose head was badly formed*. Neither Homer's Thersites, whose cranium was 'misshapen,' nor any of Shakspeare's personages, with 'foreheads villanously low,' could have been easily proselyted to the doctrines of Phrenology. The



reason is obvious. Their own heads would not have 'passed muster.' Their belief, therefore, would have been *self-condemnatory*. And as no man is bound, in common law, to give evidence against himself, neither is it very consistent with the laws of human nature, for any one to believe, more especially to avow his belief, to his own disparagement. As the hump-backed, knock-kneed, and bandy-legged, have an instinctive hostility to the science of gymnastics, it is scarcely to be expected that the flat-heads, apple-heads, and sugar-loaf-heads, will be favourably disposed to that of Phrenology. Nor will those whose brains are so ponderous behind and light before, that their heads seem in danger of tilting backward." \*

We have no doubt that, on widening the sphere of his observations, Arnold will become satisfied with respect to the *details*, as well as the *principles* of Phrenology. Should he ultimately declare himself a phrenologist, of which we have little doubt, the cause of the science will be greatly forwarded in Germany; for he is there universally known, and it is all but certain that he will succeed Tiedemann as Professor of Anatomy and Physiology in the University of Heidelberg.

\* Caldwell's New Views of Penitentiary Discipline, &c. Philadelphia, 1829. Preface, pp. 5 and 6.



REMARKS ON THE FALLACY OF PROFESSOR TIEDEMANN'S COMPARISON OF THE NEGRO BRAIN AND INTELLECT WITH THOSE OF THE EUROPEAN. By ANDREW COMBE, M.D.

(Reprinted from *Phrenological Journal*, vol. xi.)

Professor Tiedemann's elaborate essay \* is remarkable in several points of view. It proceeds from the pen of one of the first physiologists of Europe; its materials have been gathered from a personal examination "of the most celebrated anatomical museums, both on the Continent and in Great Britain;" and its subject is considered to be "of great importance in the natural history, anatomy, and physiology of man; interesting also in a political and legislative point of view." The mode of inquiry pursued in it is based upon the two principles long considered by the phrenologists as demonstrated, but still scouted by many as unworthy of serious discussion—namely, *first*, that the brain is the organ of the mind; and, *secondly*, that there is a constant relation, *cæteris paribus*, between mental power and cerebral development. A fallacy, however, runs through almost all the author's applications of the above principles, and consequently vitiates many of his most important conclusions. In these circumstances, a more detailed examination of the original essay, than the notice already given in the *Phrenological Journal* (No. lii. page 627), seems to be called for, as Tiedemann's name and influence will mislead many, and for a time give to error all the authority of truth.

Tiedemann's grand objects are, to prove, 1st, that the

\* On the Brain of the Negro compared with that of the European and the Ourang-Outang. By Professor Tiedemann, of Heidelberg. (*Philosophical Transactions* for 1836. Part ii.)



opinion of Negro inferiority expressed by Camper, Sœmmering, Cuvier, and almost all naturalists of any eminence, is incorrect; 2dly, that the Negro brain is equal in size and similar in structure to that of the European; and, 3dly, that consequently the former is equally capable of civilization as the latter, and owes his present inferiority entirely to bad treatment and unfavourable circumstances, and will lose it when placed in the position in society, which has been recently assigned him by the "noble British Government." These positions are urged by Tiedemann with so much philanthropic warmth, and with so much hearty zeal in the cause of the Negro, that we feel no small reluctance to enter the lists against him; but having a thorough reliance on the supremacy of truth, and believing its diffusion to be fraught with more ultimate happiness to the Negro himself than he can possibly derive from the propagation of an amiable error, we offer no apology for attempting to shew that the prevailing opinion remains unaffected by any evidence brought against it by Tiedemann, and that *de facto* the genuine Negro brain is inferior in intellectual power to that of the European.

In pursuance of the above objects, Tiedemann first inquires, whether "the Negro has the same quantity of brain as the European?" and to ascertain the fact, he institutes an elaborate comparison between the weight of the brain, as determined in upwards of fifty Europeans of different ages and countries, and its weight in several Negroes examined either by himself or others, and the results obtained are not only full of interest to the phrenologist, but well worthy of the attention of those among our opponents who still continue to ridicule the principle of size of brain being *cæteris paribus* a measure of mental power. Every fact mentioned by Tiedemann adds to the already overwhelming proofs adduced by the phrenologists; but coming in this instance from the pen of a hostile authority, they may probably carry more weight with them than if found in a phrenological essay.



After quoting the statements of many authors, and detailing the weights of fifty-two European brains examined by himself, Tiedemann mentions that "the weight of the brain in an adult male European varies between 3 lb. 2 oz. and 4 lb. 6 oz. troy. *The brain of men who have distinguished themselves by their great talents is often very large.* The brain of the celebrated Cuvier weighed 4 lb. 11 oz. 4 dr. 30 gr. troy, and that of the celebrated surgeon Dupuytren weighed 4 lb. 10 oz. troy. *The brain of men endowed with but feeble intellectual powers is, on the contrary, often very small, particularly in congenital idiotismus.*" Here then is ample confirmation of the phrenological evidence, and from a source which cannot be considered as biassed in our favour. Tiedemann proceeds, "The female brain is lighter than that of the male. It varies between 2 lb. 8 oz. and 3 lb. 11 oz. *I never found a female brain that weighed 4 lb.* The female brain weighs on an average from four to eight ounces less than that of the male; *and this difference is already perceptible in a new-born child.*" This also corresponds entirely with the long repudiated statements of the phrenologists, and it is pleasant to see the fact thus broadly admitted.

Tiedemann goes even beyond the phrenologists in his applications of the principle of size being a measure of power. He says, "*There is undoubtedly a very close connexion between the absolute size of the brain and the INTELLECTUAL powers and functions of the mind.*" This is evident from the remarkable smallness of the brain in cases of congenital idiotismus, few much exceeding in weight of a new-born child. Gall, Spurzheim, Haslam, Esquirol, and others, have already observed this, which is also confirmed by my own researches. The brain of very talented men is remarkable on the other hand for its size." (p. 502). Here certainly is ample corroboration of the influence of organic size on mental power; but Tiedemann has fallen into the very serious error of taking absolute size of the brain as a measure of *intellectual* power only; whereas, it indicates, as might be expected à



*priori*, absolute *mental* power, without determining whether that power lies in extent of intellect, in strength of moral feeling, or in the force of passion or affection. A brain of four pounds' weight may be large in the anterior lobe, and smaller in the middle and posterior lobes; or its chief size and weight may be in the posterior lobes, and the anterior portions be actually small. In both cases Tiedemann would infer equal "intellectual" power, whereas the phrenologist would perceive at a glance, that in the former the intellectual ability would far preponderate; while in the latter the power of mind would consist entirely in intensity of feeling, and the intellect, properly so called, be rather weak than strong.

If, for example, we compare the Charib with the Hindoo brain, we find the entire mass of the former considerably to outweigh the latter, and, according to Tiedemann, we should find more intellectual talent in the Charib. The fact, however, is notoriously the reverse, and the explanation is very easy *when we distinguish the regions of the brain in which the size exists*. In the Charib, the anterior lobe is very small, in perfect harmony with his poverty of intellect; but the posterior and basilar regions of the brain are very large, also in harmony with his ferocity and energy of passion. In the Hindoo, again, the reverse holds; the anterior lobe is well developed, and so is his intellect; but the basilar region, so large in the Charib, is small in him; and consequently, in vehemence of passion, active courage, and general force of character, the Hindoo is greatly inferior to the more savage Charib.

The same distinction occurs every day in social life. We meet with an individual—a criminal, for instance—in whom the brain is absolutely large, but who is nevertheless stupid in intellect, and powerful only in the department of the propensities; while, on the contrary, we find many an amiable member of society possessed of a brain smaller in absolute size, but far superior to the criminal in the size of its anterior lobe or organs of intellect, and consequently far supe-



rior to the criminal in thinking power and general talents ; results at utter variance with Tiedemann's rule, but perfectly reconcilable according to the phrenological application of the principle.

Hence, it is obvious, that of two brains, both precisely equal in absolute weight, one may be very deficient in intellectual endowment, compared to the other, and this deficiency be perfectly apparent on inspection when we attend to the regions of the brain in which the preponderance lies. But as Tiedemann, throughout the whole of his experiments, utterly disregards this distinction, confounds intellectual power, moral feeling, and brute propensity, under one head, and treats of the brain as if it consisted of only one lobe, with only one function, namely, the manifestation of intellect, his inference, that because the Negro brain is equal in weight to the European, therefore the Negro is also his equal in intellectual power, falls to the ground as unwarranted by the evidence. To render his conclusions worth any thing, he must shew, not only that the two brains are equal in absolute size, but that *the anterior lobe, or seat of intellect, is equally developed in both* ;—a position which he never attempts to substantiate, and which is at variance with some parts even of his own facts.

Having obtained the weight of a sufficient number of European brains, Tiedemann next endeavours to ascertain the weight of the Negro brain ; but from the very small number of Negroes to be found in Europe, he has great difficulty in obtaining any thing like a fair average : in fact, he gives the weight of *only four* Negro brains,—one of a boy of fourteen years of age, stated, on the authority of Sœmmering, to have weighed 3 lb. 6 oz. 6 dr. ; a second, of a tall and handsome Negro of twenty years of age, which weighed 3 lb. 9 oz. 4 dr. ; a third, of a large Negro, mentioned by Sir Astley Cooper, of 49 oz. ; and a fourth, examined by himself, of a man twenty-five years of age, which weighed 2 lb. 3 oz. 2 dr.

In comparing these results with the average weight of the



European brain as stated by Tiedemann himself, it is singular to observe the extent to which they are at variance with his inferences. The European average runs, he says, from 3 lb. 2 oz. to 4 lb. 6 oz., while the average of the four Negroes rises to only 3 lb. 5 oz. 1 dr.—or 3 oz. above the *lowest* European averages; and the *highest* Negro falls 5 oz. short of the highest *average* European, and no less than 10 oz. short of Cuvier's brain. And as if these facts were not inconsistent enough with his conclusions, Tiedemann first affirms that, in the Negro, "the length and height of the cerebral hemispheres do not visibly differ from that of the European; their breadth only being somewhat less," (p. 515); and immediately after subjoins three tables of the "Dimensions of the Cerebrum of Negroes," "Dimensions of the Cerebrum of European Males," and "Dimensions of the Cerebrum of European Females;" the figures of which directly contradict his assertion! This seems almost incredible, but on summing up the averages we find the following results; namely—

		Inches.	Lines.
Average length of brain in	4 Negroes . . .	5	11
Do. do.	7 European males .	6	2½
Do. do.	6 European females	5	10½
Average greatest breadth in	4 Negroes . . .	4	8½
Do. do.	7 European males .	5	1½
Do. do.	3 European females	5*	4½
Average height of brain in	3 Negroes . . .	2	11½
Do. do.	7 European males .	3	4
Do. do.	4 European females	2	9½

From these tables, it is evident that the dimensions of the brain are smaller in the Negroes measured by Tiedemann than in the European; but for our own parts we are not disposed to lay much stress upon results drawn from

\* This result is an example of the unsatisfactory mode of proceeding pursued by Tiedemann. It makes the breadth of the female *greater* than that of the male brain, in opposition to all his other evidence, and must have arisen from the unequal spreading out of the soft brain; if, indeed, it is not a mere error of the press.



such a limited number of facts; and we notice them merely to shew that, such as they are, they directly contradict the arithmetical proportions or conclusions drawn from them by Tiedemann. The latter, indeed, grants all that we contend for, when, in his description of the Negro brain (page 515), he states that "*the anterior portion of the hemispheres is something narrower than is usually the case with Europeans,*" because, as the anterior portion is the seat of *intellect*, this is really equivalent to conceding that the Negro is naturally inferior in intellectual capacity to the European.

Not having access to a sufficient number of the actual brains of Negroes, Tiedemann has endeavoured to supply the want of direct evidence by comparing the capacity of the Negro skull with that of the European, and thus obtaining an index to the relative size of their contained organs. For his purpose he filled the skulls with millet seed, and carefully noted the quantity which each contained. In a general sense, no objection lies against this mode of proceeding; but it is useless as a means of determining the proportions of size between the different lobes of the brain, and consequently in estimating intellectual capacity as distinguished from power of feeling. For Tiedemann's special purpose, his time, trouble, and zeal, are utterly thrown away, and one cannot help regretting that such should be the case. Among other collections which he visited, was the Phrenological Museum in Edinburgh, to which we twice accompanied him, and from which twenty-three of his observations were taken; and we can bear witness to the pains which he took to ensure accuracy in the individual details; and yet, strange to say, on summing up his table of results, and striking an average (a proceeding which he seems not to have thought of), HIS FACTS AND INFERENCES ARE ONCE MORE AT UTTER VARIANCE, and we only regret that the numerous editors, who have quoted his authority, should not have examined the matter more closely before so widely disseminating his errors.

After giving several pages of tables comprehending the



weight of the quantity of millet seed required to fill Ethiopian, Caucasian, Mongolian, American, and Malayan skulls, Tiedemann says, "It is evident, from the comparison of the *cavum cranii* of the Negro with that of the European, Mongolian, American, and Malayan, that the cavity of the skull of the Negro in general is *not smaller* than that of the European and other human races. The result of Hamilton's researches is the same. I hope this will convince others, that the opinion of many naturalists, such as Camper, Sœmmering, Cuvier, Lawrence, and Virey, that the Negro has a smaller skull and brain than the European, is *ill-founded, and entirely refuted by my researches.*" (P. 511). Now we have already seen, that the real question of interest, as regards Negro improvement, is not so much the general size of the brain, as the relative size of its anterior lobe and coronal surface compared to the basilar and posterior portions. But even as concerns the absolute size of the whole brain, it is an extraordinary fact, that Tiedemann's own tables give a decided superiority to the European over the Negro brain, to the average extent of nearly four ounces! The average capacity of forty-one Negro skulls in his own tables amounts only to 37 oz. 1 dr. 10 gr., while the average of seventy-seven Europeans of every nation, also in his own tables, amounts to 41 oz. 2 dr. 30 gr. Of the Negroes, indeed, three are females, but even subtracting these, the Negro average amounts only to 37 oz. 6 dr. 18 gr. Here, then, on Tiedemann's own shewing, we have, first, an inferiority in the dimensions of the Negro brain and a greater narrowness of its anterior lobe; and, secondly, a marked inferiority in the capacity of the Negro skull to the extent of about one-tenth, and yet he very strangely infers that *both are equal* to those of the European; and the Royal Society, and half of our scientific men and journals, adopt and propagate both facts and inferences as literally correct and of vast importance!! If the phrenologists had perpetrated such a series of blunders, Sir William Hamilton and his allies would have shouted in triumph over their stupidity.



After treating of the organization of the Negro, Tiedemann attempts to prove that, intellectually and morally, as well as anatomically, the Negro is naturally on a par with the European, and contends that the opposite and popular notion is the result of superficial observation, and holds true only in regard to certain degraded tribes on the coast of Africa. We shall not follow him into this branch of the inquiry farther than to express our concurrence with him in believing some of the tribes of the interior to be endowed with much higher intellectual and moral faculties than those with which we have been longest acquainted. Park, Denham, Clapperton, and others, unite in testimony to the superior civilization of some of the interior nations. But then these are the tribes which the same authors state to "vary little except in colour from the European," and to have "neither the broad flat noses, thick lips, prominent cheek-bones, sloping contracted forehead," &c. "which most naturalists consider as the characteristics of the genuine Negro. Most of them have *well-formed skulls*, long faces, handsome, even Roman or aquiline, noses, thin lips, and agreeable features," and "the Caffres and Bachapins, or Betchuanas, have *the same form of skull and the same high forehead* and prominent nose as Europeans." (P. 512). If this be a correct description, what can be more natural than that races thus approximating to the European standard of organization should also approach to the same standard in function, and present an affinity to the European character? But also, what can be more opposed to Tiedemann's main proposition, that the ordinary Negro type and mind are on a level with the European? Because a black race with a brain like that of the European is capable of approaching to European civilization, it does not therefore follow that the thick-lipped and flat-nosed Negro with the narrow forehead is equally good; and even granting Tiedemann to have established the first point (which nobody denied), he has still left the latter precisely where he found it, or rather he has left it enveloped in a new cloud of fallacy and obscurity.



That a physiologist of Tiedemann's talent and merited reputation should have failed so signally in an investigation which he recognises as one of so much importance, and upon which he has bestowed so much labour and with so benevolent an intention, is much to be lamented; but the cause which has led to his failure is still more to be lamented, because it is humiliating to him as a man of science, and is the natural and just result of his own conduct. Well did Tiedemann know that the great discovery of his immortal countryman Gall lay directly in his way in the inquiry in which he was engaged, and that, if true, it must be of immense use to him in conducting his inquiry. Had he availed himself of its aid, he would have seen at once the futility of any investigation based on considering the whole brain as the organ of *intellect*, and would thus have avoided becoming the instrument of authoritatively diffusing mischievous error, where he was anxious only for beneficent truth. Tiedemann, however, confiding in the strength of his own merits and the durability of his own fame, chose to treat the phrenological physiology of the brain with contemptuous silence, to disregard its facts, and to reject its aid as a guide. He has preferred being a leader in the train of error, to being a subordinate in the march of truth, and as he has chosen his path so shall he be rewarded. His contribution to the Royal Society's Transactions, although hailed at present as an honour to its author, will ere long be regarded as a beacon to warn others how very little a first-rate talent, great industry, and a European reputation, can accomplish when employed in a false direction, and how indispensable to true greatness is the direct and unprejudiced pursuit of truth.

These observations may be thought severe, and were Tiedemann a mere man of straw, whose opinions nobody cared about, they would be uncalled for. But we cannot acquit him of culpable negligence in overlooking, as he has done, a principle which he is well aware has been much insisted on, and backed by an immense amount of evidence, in the



phrenological works; and which, indeed, is broadly admitted, in a general way, by Cuvier and almost every physiologist of any note—including even Tiedemann himself;\*—namely, that different lobes or parts of the brain have different functions, and consequently that it cannot be judged of as a whole in relation to only one function. Whether Tiedemann's omission of all reference to this distinction arises from intention or from forgetfulness, he is almost equally to blame; first, because it lies at the very foundation of the inquiry; and, secondly, because it was doubly incumbent on him who stands as one of the first physiologists of Europe, and whose opinions on all physiological subjects are received with a degree of deference, both in this country and on the Continent, which leads many to adopt and diffuse them with implicit confidence, neither as in the present instance to give his sanction to errors, the mischief of which is proportionally increased by the wide circulation which his authority ensures them, nor to treat with the contempt of silence the discoveries and labours of men whose names will be handed down to posterity as the most distinguished of the age to which they belonged. If Tiedemann was really ignorant of what Gall has done, then is his ignorance still more blameable; because he was aware of the nature of Gall's claims, and of the existence of his works, and *ought* to have made himself acquainted with their contents and truth, before erecting himself into an authority entitled to condemn them to oblivion, and to deter the young physiologist from their examination. But perhaps the strangest thing of all is, that Tiedemann's statements or arithmetical results are so directly at variance with the evidence of his own facts and figures, and so strongly confirmatory of the opinion which it is his sole purpose to refute.

It is with much pain we have felt ourselves compelled thus freely to criticise the philosophical fallacies involved in

\* See Phrenological Journal, vol. ix. p. 48.



Tiedemann's mode of proceeding; but the interests of truth imperatively required the exposure, and little as we are disposed to contend against a man of Tiedemann's eminence, we shall not shrink from the contest even against greater odds when truth requires it. Phrenology has suffered sufficiently already from the contempt and obloquy of the "Great in Science," and it would be folly to allow the evil to be done without at least attempting to neutralize its effects, and to pave the way for better days.

Before parting with Tiedemann, it is worth while once again to remark, how implicitly he adopts the long repudiated phrenological principle of cerebral size being *cæteris paribus* a measure of mental power, and how expressly he confirms other contested phrenological doctrines. Our readers can scarcely have forgotten the strenuous efforts made by Sir William Hamilton to upset the phrenological statements that the cerebellum is relatively smaller in females than in males, and that the brain generally decreases in size in old age. Tiedemann, however, shews by his Tables, that not only the female brain but the female cerebellum is actually smaller. In the list of "conclusions" given on page 502, he expressly affirms that "the female brain is lighter than that of the male, and weighs on an average from four to eight ounces less," and adds, that "the difference is already perceptible in a new-born child." In the 4th conclusion, after stating that Sir William Hamilton denies the decrease of the brain in old age, he continues, "It is remarkable that the brain of a man eighty-two years old was very small, and weighed but 3 lb. 2 oz. 3 dr., and the brain of a woman about eighty years old weighed but 2 lb. 9 oz. 1 dr. *I have generally found the cavity of the skull smaller in old men than in middle-aged persons.* It appears to me therefore probable, that the brain really decreases in old age, only more remarkably in some persons than in others." What will Lord Brougham say to this, when he considers the mind to become more vigorous in old age?



Again, in regard to Sir William Hamilton's assertion of the equal or superior size of the female cerebellum, we find at page 514 a table entitled "Dimensions of the cerebellum and nodus encephali." In it the greatest breadth of the cerebellum in six male Europeans varies from 4 inches 3 lines to 3 inches 6 lines, being the highest and lowest measurements. But in the three female Europeans, the *highest* is only equal to the *lowest* male, namely, 3 inches 6 lines; the other two being 3 inches 5 lines and 3 inches 3 lines respectively. Here again the superior accuracy of the phrenologists is proved even by hostile testimony; and did time permit, other confirmations might be extracted from Tiedemann's pages.



## LIST OF PHRENOLOGICAL CONTROVERSIES.

Having now presented the reader with specimens of phrenological investigations, of the objections which have been urged against them, and of the answers to these objections, I shall conclude this branch of the present work by the following remarks on "Phrenological controversies," which appeared in the *Phrenological Journal*, vol. x. p. 150.

It would be refreshing to hear new arguments adduced against Phrenology; but this is a gratification which, in these days, never falls to our lot. For several years Phrenology has been assailed only with weapons shattered and blunted in previous warfare, each new opponent coming boldly into the field with a firm reliance on their irresistible power. It is, in truth, ludicrous to see arguments which have been repelled again and again borrowed from the works of preceding antiphrenologists, and triumphantly flourished, without the slightest allusion to the replies which have been made to them. This probably arises in some cases from dishonesty, but in others may be the result of ignorance. For the benefit, therefore, of all who design to attack Phrenology with artillery which has already been in the field, we subjoin a list of the principal aggressive and defensive champions, with references to the places in which the controversy has from time to time been carried on. After examining both sides of the question, they will perhaps feel disposed to exhibit their skill in some more profitable arena.

Dr JOHN GORDON. *Edinburgh Review*, 1815, No. 49; and *Observations on the Structure of the Brain*, Edinburgh, 1817.—Answered by Dr Spurzheim, *Examination of the Objections made in Britain against the Doctrines of Gall and Spurzheim*, Edinburgh, 1817; and by an anonymous



writer in the *Medico-Chirurgical Journal*, 1817, vol. iv. p. 53, 117; see also vol. iii. p. 425.

Dr P. M. ROGET. Supp. to *Encyc. Brit.* article *Cranioscopy*.—Answered by Mr G. Combe, *Essays on Phrenology*, Edinburgh, 1819, p. 62; also by Dr Andrew Combe, *Phrenological Journal*, i. 165.

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Rev. R. W. HAMILTON, Leeds. *Essay on Craniology*, London, 1826.—Answered by Mr William Wildsmith in an *Inquiry concerning the Connection between the Mind and the Brain*, &c. London, 1828.

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Dr JOHN BOSTOCK. *Elementary View of Physiology*, iii. 263.—Answered by Dr Elliotson, *Phren. Jour.* v. 96.

Mr T. STONE. *Evidences against the System of Phrenology*, Edinburgh, 1828; answered by Dr James Kennedy of Ashby-de-la-Zouch, in *London Med. and Surg. Jour.* i. 153, 249, 349, 435; ii. 46, 130, 507. *Obs. on the Phrenological Development of Burke, Hare, &c.* Edinburgh, 1829; answered by Mr G. Combe, *Phren. Jour.* vi. 1; see also pp. 93, 180, 232, 234, 317. Mr Stone published a *Rejoinder to the Answer of George Combe, Esq. &c.* Edinburgh, 1829.

Dr JOHN WAYTE. *Antiphrenology*, London, 1829.—Answered anonymously in *An Exposure of the Unphilosophical and Unchristian Expedients adopted by Antiphrenologists for the purpose of obstructing the Moral and Philanthropical Tendencies of Phrenology*, London, 1831.

Sir CHARLES BELL. *Anatomy*, fifth edition, i. 189; answered in *Phren. Jour.* viii. 333.—Also *Phil. Trans.* cviii. 306; answered by Dr Spurzheim, *App. to Anat. of Brain*, or *Phren. Jour.* vi. 606.

FOREIGN REVIEW, No. 8, 1829.—Answered by Mr G. Combe, *Phren. Jour.* vi. 222.

Mr JAMES MONTGOMERY, Sheffield. *Essay on the Phrenology of the Hindoos and Negroes*, London, 1829.—Answered by Dr Corden Thomson, Sheffield, in *Strictures on Mr Montgomery's Essay*, annexed thereto.

EDINBURGH WEEKLY JOURNAL, 1829.—Answered by Mr G. Combe, *Letter on the Prejudices of the Great in Science and Philosophy against Phrenology*, *Phren. Jour.* vi. 14, 211.

NORTH AMERICAN REVIEW, No. 80, July 1833.—Answered by Dr Caldwell, *Annals of Phren.* i. 1; also by Mr R. Cox, *Phren. Jour.* viii. 638.

CHRISTIAN EXAMINER, Boston, Nov. 1834, article *Pretensions of Phrenology examined*.—Answered in *Annals of Phren.* ii. 1, and *New England Magazine* for March 1835;



likewise by Dr Caldwell, *Phrenology Vindicated, &c.* Lexington, Kentucky, 1835.

It deserves to be remarked, that some of the more eminent and well informed antiphrenologists, while they seem to regard their own refutations as unanswerable, speak very lightly of those of their predecessors. Dr Prichard, for instance, states that "nearly all that has been said of late by English writers on this (the antiphrenological) side of the question was advanced many years since in the most forcible manner, by the author of a critique in the *Edinburgh Review* (Dr Gordon). Similar objections," he adds, "are still frequently repeated, though most persons have become, or might have become, aware of their inconclusiveness. It must, for example, be evident to those who reflect upon the subject, that the arguments against Phrenology founded on the difficulty of applying measurements to particular portions of the brain, is no objection at all against the truth of the doctrine itself, or the principle on which it is founded. With equal justice might the obstacles arising from the imperfection of instruments, or from states of the atmosphere, which interfere with the observations of astronomers, be urged as invalidating the most noble of human sciences. Not less ill-judged have been the attempts of those who have argued against the speculations of the phrenologist on the ground of their dangerous tendency, and the alleged fact that they lead to fatalism and destroy moral responsibility. The pernicious results to be deduced from any new and specious doctrine are not reasons for shutting our eyes against the evidence on which it rests, but ought rather to render us more anxious to sift the matter to the bottom. If the thing be true, let this be known: '*fiat justitia; ruat cælum.*' The real merits of the case will sooner or later be made apparent, and the sooner the better." (*Cyclopædia of Practical Medicine*, 1835, article *Temperament*, vol. iv. p. 168.) Dr Bostock, also, in speaking of the strictures which have at various times been published on Phrenology, says, "It must be acknowledged that they



have been more characterized by the brilliancy, or perhaps flippancy, of their wit, than by the soundness of their arguments: it would seem, indeed, that the writers did not regard it as a subject for serious consideration." (Elementary System of Physiology, second edition, 1830, iii. 276.) He excepts from this censure Dr Roget's article on Crani-oscropy in the Supplement to the Encyclopædia Britannica, which, says he, "is truly characteristic of the cultivated and candid mind of its author." We doubt whether any unbiassed person who has carefully perused the answers which were made to Dr Roget's article will concur in this laudatory remark; and we know Dr Bostock's candour and acuteness too well to suppose that when it was penned he had looked at more than one side of the controversy. For Dr Roget's talents and attainments, we, in common with the mass of his educated countrymen, entertain a high respect; but we must be permitted to add, that his article in the Encyclopædia indicates unequivocally that before writing it he had not bestowed on Phrenology that patient study without which no man, however eminent his abilities and general knowledge, can render himself qualified to form a correct judgment on the questions at issue.

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I shall conclude the present publication with a translation from the German of a Petition and Remonstrance presented by Dr Gall to the Provincial Government of Lower Austria, against an Edict issued by the Emperor of Germany in 1802, prohibiting him from lecturing. The work from which it is translated was presented to me in Vienna, in July 1837, by a talented young physician, who is devoted to Phrenology, but whose name I am not authorized to publish; and as it has never appeared, so far as I am aware, either in the French or English language, and as it is interesting in itself, and valuable as a historical document, I am induced to give it a place here.



## PETITION AND REMONSTRANCE

BY

DR GALL

*Against an order issued by Francis the First, Emperor of Germany, prohibiting him from delivering Lectures on the Functions of the Brain, without special permission previously asked and obtained. Translated from the German.\**

To the Right Honourable the Imperial and Royal  
Government of the Province of Lower Austria.

On the 9th day of January 1802, the high command of his Majesty the Emperor was communicated to me, by the very worshipful the Government of Lower Austria, that I should immediately discontinue my private lectures on the functions of the brain, and state in writing whether I had obtained permission to deliver them.

On receiving the first intimation that this high command was about to be issued, I immediately, on the 29th December last, discontinued my then current course of lectures, in order practically to manifest my profound respect and implicit obedience to the ordinance of his Majesty.

I have never hitherto obtained any special permission to deliver these lectures; and, for the following reasons, have never understood that it was imperative on me to ask for such.

\* Neue Darstellungen aus der Gallschen Gehirn-und-Schedellehre, als Erläuterungen zu der Vorgeordneten vertheidigungsschrift des Doktor Gall eingegeben bey der niederösterreichischen Regierung, &c. Herausgegeben von WALTHER der Philosophie, Med. und Chir. Doktor, kur-bairischem medizinalrath, ausübendem Arzt und Augenarzt in Bamberg. MÜNCHEN in der Scherrerschen Kunst-und Buchhandlung, 1804. 12mo, pp. 168.



1. I am not acquainted with any law, by virtue of which private lectures in a teacher's own house, on any scientific subject, are forbidden.

2. The physicians and surgeons of Vienna have practically enjoyed the following rights, vested in them from the remotest times:—"Dedimus ac damus ei potestatem, cathedram doctoralem conscendendi, ac de Medicina respondendi, praxin cæteraque exercendi, quæ medicinæ doctores exercere solent. Tribuimus ei insuper privilegia omnia ac prærogativas, quæ vero medicinæ doctori legibus vel CONSUETUDINE tribui solent."

In consequence of the privileges here conferred, the following courses of private lectures are at the present time in the progress of being delivered:—

By the Chief Physician Joseph Frank, on Pathology, Materia Medica, &c.

By Dr Capellini, assistant of the Medical Clinique, on the Practice of Physic.

By the Surgeon-in-Chief Rudorfer, on Surgery.

By the assistant-surgeon Härtel, on Operative Surgery.

By Professor Boer, a public and also a private course of lectures on Midwifery.

By Mr Loser, on the Practice of Midwifery, *in collegis privatis et privatissimis*.

By Dr Beer, on Diseases of the Eyes.

Last year, Dr Schmit lectured on Diseases of the Eyes, and on Syphilitic Affections;

While Dr Nord lectured for three years undisturbed; and also many more.

Although all this is known, and although, for example, Dr Beer presents his prospectus every year to the censor for revisal, so completely is the practice treated as a matter of accustomed right, that no impediment is ever thrown in the way of any one physician or surgeon.

Whether, then, my newly discovered theory be viewed as a doctrine concerning the skull—or as a doctrine regarding the head—or (as it should properly be named) as a



doctrine concerning the brain, that is to say, concerning the functions of the brain in health and in disease—in short, a physiology and pathology of the brain;\* I have been able to discover no reason why I should be excepted from the benefit of this prescriptive privilege.

3. During a period of five years, I have delivered these lectures with the utmost publicity, and indeed have been frequently visited, not only by strangers from all countries, but by persons of all ranks and conditions in Vienna. They could not, therefore, have ever been a secret. This is proved by the numerous public notices of them which appeared in Vienna. Nevertheless, no admonition regarding them was ever received by me.

4. It is two years since the manuscript from which I used to lecture received the IMPRIMATUR of the censor; because, at that time, I intended to publish my discoveries on the subject without copperplates.

5. Since my system began to be generally diffused, numerous notices and accounts of it, containing my whole theory, have been circulated and sold without impediment; nay, reprinted and published openly in supplements to the Vienna newspapers, and even advertised by means of handbills stuck up on the corners of all the streets; and they continue to be sold to this hour. How, then, could I suppose that I committed any wrong, when I only delivered, in a superior manner, and to more enlightened individuals, the same doctrine which was thus universally spread abroad without offence?

6. I have never drawn on myself the attention of the police, neither by my conduct, nor by my speech, nor by my actions, nor by any secret combinations, nor by any intentional selection of my hearers; and neither have I, in my lectures, abused the privilege which we physicians are universally understood to enjoy. I hope, therefore, that the

\* This sentence affords a complete answer to those opponents of Phrenology, who assert that Dr Gall at any one time ascribed the mental faculties to different parts of the *skull*.



Right Honourable the Provincial Government will not allow me to suffer, because I omitted specially to request, and expressly to obtain, a permission to lecture.

In regard to the grounds of the accusation implied in the imperial command, I regard it to be due to myself, to the subject, to the public, and to his Majesty, to solicit a preliminary inquiry before I be condemned. I anticipate the granting of this request, not only because such a right belongs to every subject, but also on account of that stedfast love of justice which distinguishes our revered monarch; and I await with confidence the favourable issue of the investigation.

I beg, therefore, the Right Honourable the Provincial Government, to lay the following dutiful explanations at the feet of the Emperor.

Occasionally, but only in comparatively few instances, were ladies present at my lectures, and they were always of the superior class. As to young women, if by this term is meant simply unmarried ladies,—in the course of the whole six years of my teaching, not more than four of them attended my expositions. Of these, three were foreigners, all above twenty-four years of age,—one was of this country, and she was above twenty. All of them were of the highest respectability, and on every occasion were accompanied by their mothers or their aunts. In consequence of my giving previous notice, they were always absent on the day on which I treated of the sexual propensity, in so far as its organ is seated in the brain; although, in the lecture itself, nothing indelicate was ever brought forward. The presence of women at the rest of the course, appeared to me to be far from unnecessary, because I often seized the opportunity to lay before them important instructions concerning the physical and moral education of the young. I had the less scruple about their presence, as I treated absolutely of no other parts than the skull and the brain,—the portions of the human body, certainly, which are least of all offensive to delicacy.



It has occasioned to me infinite distress, that his Majesty has been led to entertain the unfounded apprehension, that my theory appears to lead to materialism, and consequently to militate against the first principles of morals and religion. In all ages, it has happened that truths entirely new, or even truths only better demonstrated, have appeared to threaten the existence of all previously established principles. But experience has uniformly proved, that old and new truths soon cordially combine, and mutually support each other,—that opposition to them is only pernicious,—and, especially, that obstacles thrown in their way tend only to promote their advancement. We need only reflect on the discoveries of the true planetary system—of the circulation of the blood—of the existence of antipodes—on the fate of Galileo and of Wolf under William the First—on the introduction of antimony—of bleeding—and of inoculation for the small-pox—to be convinced of the truth of these remarks.

Assuredly, my doctrine concerning the brain, has attained the high degree of interest which it at present possesses, only in consequence of the great number of new discoveries which it contains. Every one, who is still a stranger to these discoveries, must, in proportion to his ignorance, substitute a greater or less degree of error for the truth, in forming a judgment of them. This precipitate judgment must be indifferent to every discoverer when it is confined to the people. To the scientific classes, I communicated the principles and proofs of my doctrine in my lectures, and have, by this means, sent the results of my experiments into circulation with universal approbation, until I shall be able to bring forth an expensive work on the subject. I avail myself, however, of the present opportunity, and consider myself bound in duty to the Emperor, especially in consequence of his excited apprehensions, to make the substance of my doctrine more intimately known; and I trust, with confidence, that the Right Honourable the Provincial Government will strenuously support me in doing so; because



this is called for, in order to arrive at a right judgment, not only regarding myself, but also in regard to the propriety of promoting one of the most beneficent discoveries, and in an especial degree supporting the scientific honour of our native country.\*

The first principle, then, in my doctrine, concerning the functions of the brain, which appears liable to the objection of Materialism, can be no other than this—that the brain is the organ of the mind.

We, physicians and surgeons, limit our investigations to facts cognisable by the senses, and leave every thing else to the clergy and Revelation. It is, however, highly important to know how the soul operates in this life in its alliance with the body, and according to what laws it acts. The most decided and undeniable experience teaches us, that the brain, of all parts of the body, is the one by means of which the mind manifests its powers; that animals which have no brains, and human beings who are born without brains, exhibit no mental functions; that all mental manifestations cease when the brain is destroyed, or the head cut off; that effusion into the cavities of the brain, inflammation and injuries of this organ, &c. derange the mental functions, and produce insanity, *furor*, monomania, imbecility, or general or partial obstruction of the mental powers; that too small a size in the brain, or a skull fractured or deformed by violence, also water in the head, and crettenism, degrade man almost below the level of the beasts, in regard to manifesting the mind; that our powers of thinking are very different in childhood, in manhood, and in old age; that men think and feel differently from women; that we think and feel differently in the full vigour of life, in drunkenness, and in the heat of a fever, from what we do

\* How must Austria blush hereafter, when she claims the honour of this splendid discovery, to be forced to record, that she, in the face of this warning and appeal, shut the mouth of her instructor, and banished him from her territory!—*Translator.*



in times of exhaustion, through affliction, hunger, sickness, and so forth.

These phenomena prove the dependence of the mental manifestations on the bodily organs—or that influence of the physical world on the mental functions, of which, from time immemorial, no philosopher, moralist, theologian, physician, teacher, judge, or lawgiver, has ever doubted. On this is grounded the old doctrine of the influence of the temperaments on the qualities of the soul; and for this reason, it has been held, even from the time of Alkmæon, that a favourable organization of the head and brain is an indispensable condition to the favourable manifestation of the mental powers. Augustin says, *Libr. 9, de Civit. Dei*, i. 9.—“*Anima etiam pessima melior optimo corpore;*” and in another passage—“*Corporis debilitas nimia etiam vires animæ frangit.*” *Greg. hom. 3, 2 pars. Ezech.*—“*In hoc mundo cum nostris tentationibus nascimur, et caro nobis aliquando adjutrix est in bono opere, aliquando autem seductrix in malo.*” And Chrysostom, *hom. 2, 3, super Epist. ad Hebr.*—“*Corpori molli reddito et delicato necesse est animam participare ex corporis morbo.*” Paul, in his *Epistle to the Galatians*, 5th chapter, verse 17, says—“*For the flesh lusteth against the spirit, and the spirit against the flesh: and these are contrary the one to the other; so that ye cannot do the things that ye would.*” *Epistle to the Romans*, chap. vii. verses 21, 22, 23—“*I find then a law, that when I would do good, evil is present with me. For I delight in the law of God, after the inner man. But I see another law in my members, warring against the law of my mind, and bringing me into captivity to the law of sin, which is in my members.*”\*

\* In Britain, Drs Gall and Spurzheim have been ridiculed for citing the Fathers of the Church and the Scriptures in support of their philosophical opinions; but it will be observed, 1st, That Dr Gall here applies them merely to rebut a charge of hostility to religion brought against his views, and it is both logical and philosophical to refute this accusation by shewing that similar opinions were entertained by the greatest



It leads no nearer to materialism when, instead of maintaining the dependence of the mental manifestations on the whole body (which has been demonstrated to be true by the foregoing facts and authorities), we limit the proposition to the brain, as being the special organ of the mental functions; a proposition on which also all physicians and philosophers are long since agreed. Boerhave says—"Cranium in junioribus intropressum, in adultis post fracturam intropressum, cerebrum premit, pro varietate loci pressi, pro varia magnitudine, profunditate, acutie, punctura prementis, producit hebetudines, sopores, vertigines, tinnitus, caligines, deliria, &c." Van Swieten adds—"Cum autem ab illis, quæ in cranio continentur, tota vita et humanitas, pendent, &c." In another place, he says—"Cerebrum esse illud organum corporeum, a cujus integritate idearum perceptio, illarum combinatio, judicium inde sequens, animi adfectus pendent, ex physiologicis notum est. Tom. i. 5, 434." After citing from Hildanus the case of a boy, who, after a permanent compression of the brain, caused by a fall on the head, had lost all his previous faculties, and had continued idiotic, he adds—"Observatur, plerosque, qui a nativitate fatui sunt, vitiata habere capitis figuram." The same thing is proved even by the example which Haller adduces, of a previously stupid man who became intelligent in consequence of a wound in the head, and whose stupidity returned after the wound was healed. Clem. Physiol. tom. iv. p. 293, 294. Father Mabillon was more fortunate. He also possessed very limited capacities; but after having been trepanned, in consequence of a wound in the head, he acquired and retained excellent abilities. Prochaska also says, (*Lehrsätze aus der Physiologie des Menschen*, § 340), that it is beyond all doubt that the internal senses, (the faculties of Perception, Attention, Imagination, Memory, and

authorities in Christian doctrine; 2dly, That Dr Gall was addressing a well meaning but ignorant and bigoted monarch and government, in whose eyes the Fathers of the Church were greater authorities than the most distinguished philosophers.—*Transl.*



so forth), are the results of the mutual action of the organization of the brain and of the faculties of the soul. For, in proportion as the development and perfection of the brain advance through the influence of time and exercise, the understanding equally increases; and, on the contrary, whatever impedes the development of the brain, weakens or mutilates these powers. On this account, also, the faculties of the understanding are not alike in all men; and for the same reason, any disease which operates either directly or indirectly on the brain, may produce insanity, the entire loss of consciousness, or only the loss of memory.

On this subject, the whole chapter of Weikard's Philosophical Physician is well worthy of being studied, where he treats of organization, and other circumstances, from which diversity of talents, and also the distinction between man and the lower animals arise. I cite only the following passages:—"We know positively that the brain, soft in children, acquires greater firmness in age: That among adult persons, one possesses a larger, another a smaller brain; that in one it will be found softer, in another firmer or harder. Shall these differences in this noble instrument of thought produce no differences in its workings? As well might it be maintained that a dark or a clear, a convex or a flat eyeball, or a hard or a soft skin, should cause no difference in the impressions or feelings of these organs of sense."

Although, however, the truth that in this life the mind cannot manifest its functions without corporeal instruments, without its proper organ the brain, has, in every age, been recognised by moral and physical observers,—nevertheless, there has been, at all times, an agreement among them, that the bodily mass—the brain—is not of itself alone capable of manifesting thought; but that to accomplish this result, another independent power—the soul—is necessary. The desire to ascertain the changes which take place in the organs of the internal faculties, has hitherto led only to vain efforts. On this point, we should never forget that these



material changes are not ideas, but only the external conditions by means of which the ideas formed by the internal faculties are perceived or felt. Lud. W. Heinr. Jakob, *Grundriss der Erfahrungs-seelenlehre*, § 303, and Platner's *Aphorismen*, Thl. i. § 124.

Now, in my doctrine on the functions of the brain, I move a great step in advance. Having shewn, by irresistible evidence, that the mental functions are essentially different from, and independent of one another, I draw the inference, that the whole brain cannot be regarded as a single organ, but that its entire mass is composed of as many distinct and independent organs, as there are different, independent, and particular mental qualities. Does this truth lead any nearer to materialism than the facts already stated?

Before answering this question, I shall demonstrate the antiquity and universality of this opinion.

Philosophers had very early and abundant opportunities of observing, that the different mental faculties are possessed in very different proportions by different individuals; that they neither appear simultaneously in the advance of life, nor decay simultaneously in its decline; that we may exercise one power most actively, while several others remain at rest; that, by wounds and diseases of the brain, frequently, only certain mental faculties are affected, while the remainder suffer either not at all, or in quite a different manner,—and so forth. From these facts, inferences were early drawn by some philosophers, that the mental faculties are essentially different and independent of each other; by others, that they must have different instruments, or that they are situated in different parts of the brain. The Jesuits, besides recognising the well-known distinctions between Memory, Judgment, and Imagination, taught likewise the existence of three different kinds of memory, *memoriam realem*, *localem*, *verbalem*, which have been established also by experience. Boerhaave inferred, certainly from phenomena erroneously observed, (namely, that, in sleep, the faculties of the Sentiments rest, while Imagination is active), that



the different mental functions must have their seats in different parts of the brain. Steno, and the Academy of Dijon, made fruitless researches, while Arnemann and others mutilated the brains of living animals, in order to discover the different organs of the mental powers. Winslow made great efforts to open up a way to this discovery, *Epist. Anatom.* p. 641; Haller, in *Boerhaav. Prælect.* tom. iv. p. 426, 428, 429; Unzer, *Physiologie*, p. 60; and Van Swieten,—express the liveliest wish that the different organs might be discovered; of which result, however, almost all have hitherto despaired. *Quis auderet determinare loco cerebri, unde singuli nervi, sensibus externis inservientes primam suam originem ducunt? QUIS MEMORIÆ, RATIOCINII, &c. sedem in mirabili hoc et intricatissimo organo determinare poterit?* Van Swieten, tom. ii. p. 454.—Joh. Christ. Andr. Mayer, *Anat. Physiol. Abhandlung vom Gehirne* (Treatise on the Brain), 1779, § 38, asks—“Are all the functions of the rational soul of man localised in those parts where it manifests vivifying power; or are the operations of a single mental power effected in a single and specially appointed part of the brain? and does not the mind perhaps localise its ideas by the operation of single faculties in particular departments, one in one place, and another in another?” SÆMMERING, in his *Hirnlehre und Nervenlehre*, 1791, § 83, says that it is not improbable that certain kinds of ideas arise in determinate parts of the brain; that certain mental functions are executed in determinate parts; in short, that these different powers appropriate to themselves different provinces of the brain. PROCHASKA, in his *Lehrsätze aus der Physiologie des Menschen*, § 309, says, that the impressions which are excited in the sensorium of the mind through the external senses and their nerves, are arranged and prepared by means of numerous faculties and organs, in a wonderful, and to our understanding, an incomprehensible manner, so that from them the most beautiful and interesting functions arise, which are called Thought and Consciousness; and the parts into which they can be



divided are called the internal senses. And in § 340, he adds: But we cannot yet tell with certainty, what part of the brain is necessary to this or that internal sense. JACOB, in his *Grundriss der Erfahrungsseelenlehre*, § 225, says: It is highly probable that the operations of the internal, as well as those of the external senses, and of all the perceptive powers of man, are connected with particular organs; but very little is yet known of the organs and the manner of acting of these internal senses. When a change is experienced in the mental feelings, probably there is a certain determinate change also in the organization connected with it; and this change will be determined partly by the degree in which the soul operates, and partly by the degree of sensibility belonging to the organization itself. The same sentiments are expressed by SCHELHAMMER, WILLIS, VIQ D'AZYR, GLASER, HOCBOE, LANZISI, MORGAGNI, SCHMIT, REIL, BLUMENBACH, CUVIER, PLATNER, TIEDEMANN, METZLER, HERDER, in short, by every celebrated and able anatomical, physiological, psychological, anthropological, and metaphysical author.\* Consequently, my fundamental prin-

\* Twenty-four years after the publication of these remarks, Lord Jeffrey, with all the authority of a master in philosophy, continued to assure the British public, that "*there is not the smallest reason for supposing that the mind ever operates through the agency of any material organs, except in its perception of material objects, or in the spontaneous movements of the body which it inhabits.*"—*Edinburgh Review*, No. 88, p. 267. And Lord Brougham, after the lapse of thirty-two years, gravely taught that "the ordinary course of life presents the mind and the body running courses widely different, and in great part of the time in opposite directions; and this affords strong proof that the mind is independent of the body."—*A Discourse on Natural Theology*, p. 128. Dr John Abercrombie, a distinguished practising physician, continues to this day to publish works on the moral and intellectual faculties of man, without the least reference to the influence of organization, in a state of health, on their manifestations, and goes so far as to say that we are probably advancing a step beyond what is warranted, when we speak of the brain being necessary even to perception, as "we do not know whether impressions made upon the nervous fabric connected with the organs of sense, are conveyed to the brain, or whether the mind perceives them directly, as they are made upon the organs of sense."—*Enquiries concerning the Intellectual Powers, &c.*—TRANS.



ciples have at all times been expounded by the greatest men, without any one having ever, on that account, become alarmed on the subject of materialism.

Wherein, then, does my system differ from all the foregoing? What doctrine does it contain which renders its effects suspicious in regard to materialism and the principles of Religion? When one person maintains, as a general proposition, that the mind cannot execute the functions of the external senses, except by means of certain material instruments,—while another renders this idea special, by adding that the organs of sight are the eyes, the optic nerves, and the thalami nervorum opticorum in the brain, and by enumerating also the distinct organs of the other senses;—which of the two approaches nearest to materialism? Certainly neither the one nor the other. And yet this is precisely the case in regard to my doctrines! I have had the good fortune to rectify the hitherto confused ideas entertained in regard to the human faculties; to establish the difference and independence of a greater number of mental qualities, by means of the most numerous, long continued, and expensive researches and observations on their organs, and to determine the situations of these organs in the brain; in short, to fulfil, at least to some extent, the wishes of many great men. Le Bouvyer Desmottiers, in his *Memoir, or Considerations on the Deaf and Dumb* (p. 191), after remarking that his deaf and dumb pupil Maurice sung very willingly, and with all the natural expression of the most delicious enjoyment, proceeds thus: “Assuredly these effects take their rise, and are accomplished in the brain, without the participation of hearing. There is, therefore, in general, an internal sense for sounds, independently of the action of sonorous bodies on the ears. This observation, which I have had an opportunity of making on the labourer Braud, who varies the inflexions of his tones according to the passions which agitate him, and which he wishes to excite in others, is well worthy of being investigated.” I had long before made this discovery, and



pointed out to my hearers an organ of Tune (*organ des Tonsinnes*) quite distinct from the organ of hearing. One of the greatest calculators of the Academy of Paris, M. de Lagny, when he was dying, and recognised nobody, was asked the cube of the number 12. In an instant he answered, 144. Here, after consciousness was dead in all his other faculties, a particular sense was found to be still capable of action, one for the relations of numbers; partly perhaps owing to its greater exercise, and partly on account of its greater original powers. I have established, by evidence, the existence of an organ for the faculty of numbers, and proved its situation in the brain; and am I, on this account, deserving of blame? Because I have demonstrated the situations of organs of melody and calculation, do I say that, therefore, material organs feel and judge of the relations of tones and of numbers? He who permits himself to draw such inferences, or who desires to prevent researches which appear to ignorant men to lead to them, must absolutely condemn the study of man, of natural philosophy, of chemistry, of anatomy, of the science of the functions of the essential parts of man and animals, of mental philosophy, of ethics, of education, and so forth.

Although superfluous, I add to these remarks an extract from my manuscript, which was printed in the *Teutschen Merkur*, so early as the year 1798, and which is now reprinted in the third edition of Froriep's work, and in which, from the commencement of my lectures, I endeavoured to prevent these anticipated conclusions.

OBJECTION.—“ If we establish corporeal instruments for the mental manifestations, will not the spiritual nature, and consequently the immortality, of the soul, be called in question?

“ The inquirer into nature explores merely the laws of the material world, and assumes that no natural truth can come into conflict with any truth of revelation. He knows besides, that neither spirit nor matter can be annihilated



without the interposition of the Creator ; and that man can decide nothing concerning the life of the spirit. He sees only, and learns that, in this life, the soul is incorporated with organization.—These observations apply to the general question. In regard to particular objections, I answer as follows.

“ In this objection, the acting being is confounded with the instruments by means of which it acts. Every proposition which I have maintained concerning the internal powers and the internal organs of the mental functions (to prove their difference and independence), holds undeniably true in regard to the external senses. While the wearied eye reposes, we can listen attentively with the ear. Hearing may be destroyed, without sight suffering in the least. One sense may be imperfect, and another extremely acute. Worms neither hear nor see, but they have delicate feeling. The newly pupped dog is for several days still deaf and blind, although his sense of taste be already perfectly developed. In old age, hearing usually declines earlier than sight, while taste generally remains in excellent condition. These same facts afford indications and proofs of the independence and separate existence of the senses, of which, indeed, nobody doubts. Has any one, then, drawn the conclusion, that, on account of the essential difference of the senses, the soul must be material and mortal ? Must we conclude that it is a different soul which hears, from that which sees ? I carry this comparison a little farther.

“ People err when they believe that the eye sees, and the ear hears, and so forth. Every external organ of the senses is, by means of its nerves, in connection with the brain. At the origin of these nerves, a proportionate mass of cerebral matter, the proper internal organ, completes the functions of the senses. Although, therefore, the eyes themselves be ever so sound, and although the optic nerves be perfectly entire, yet if the internal organ be diseased or injured, the eyes and optic nerves are no longer serviceable. Consequently, the external instruments of the senses have



also their operating organs in the brain ; and these external instruments are only the means by which their internal organs are brought into communication with external objects, or, on the contrary, are excited into activity by them. This is the reason why, without the interposition of external objects, or of the external organs of sense, we can experience in our minds representations of tones, of light, and so forth, as in dreams.

It never, however, occurred to Boerhave, to Haller, to Mayer, &c. not even to the pious Lavater himself, who sought for the seat of the intellectual faculties in the head and of the feelings in the trunk, that from the difference and individuality of the faculties and inclinations, and from the difference and independence of their internal organs, any conclusion could be drawn against the doctrine of their spirituality and immortality. The same soul which sees through the medium of the organ of sight, and smells by means of the organ of smell, learns by heart through the organ of Memory, and does good through the organ of Benevolence. It is always one and the same spring, which moves in you fewer, in me a greater, number of wheels."

Allow me to add, that, during the whole period of five or six years during which I have lectured, I have never heard the objection stated by any one of my hearers, and least of all by any clerical persons, who, along with physicians, have the greatest extent of previous knowledge on this subject, that my doctrine favours materialism. Farther, several hundreds of them are ready to give me a testimonial that I have directly furnished the strongest weapons for overthrowing the doctrine of materialism. In all the expositions, which are already so numerous, of my views, the unfounded nature of the charge of materialism is stated. I request that my letter to FREYHERRN VON RETZER, the "*PATRIOTISCHE TAGEBLATT*," the "*MUSICAL TIMES*," and FRORIEP'S work, may be read. Above all, I maintain that scarcely any one inquirer into nature, among so many scientific men, has spoken with deeper conviction, more warmth, and greater



force of reason concerning the indubitable necessity (grounded in the very nature of man), of education, of morals, of laws, of reward and punishment, and of religion, than I have done. And on what do all these rest, except on the nature of man? And who can know this nature to greater advantage than the philosopher who is acquainted with the organs of the mental faculties, and consequently with the spheres of activity of its several powers?

To be still more fully convinced of the perfectly innocuous tendency of my doctrine, you are requested to read Herder's chapter: "The kingdom of human organization is a system of spiritual powers. The most important doubt which is usually urged against the immortality of organized powers, is drawn from the instruments with which they act; and I dare maintain, that the clearing up of this doubt will illuminate us not with the hope only, but with the confidence, of everlasting activity." He proceeds then to adduce all the principles which I have always so earnestly pressed on the attention of my hearers. *Ideen zur Philos. der Menschheit*, I. Thl. § 309 to 322.

Another weighty objection, in reference to religion and morality, may probably be adduced against me by such persons as judge of my doctrine concerning the functions of the brain, only from contemplating its single and incomplete relations, and who are not previously conversant with similar subjects. Namely, they ask, Whence come the faculties and propensities of man? After I have adverted to the opinions of my predecessors on this point, of Socrates, Plato, Aristotle, Des Cartes, Locke, Leibnitz, Wolf, Helvetius, Tiedemann, and so forth, I shall establish firmly the fundamental principle of my System, that the dispositions, or, in other words, that certain faculties and inclinations in mankind are innate. Some proofs of this point are these: The internal feeling of every one,—the hereditary transmission not only of more marked intellectual and affective dispositions, but often also of their shades,—the very different effects of institutions for education, and so forth, on different



men,—the existence of many peculiar mental qualities, the origin of which cannot be accounted for by external things, such as genius without previous proportionate instruction or exercise,—also of determinate, and frequently, at the same time, very conspicuous qualities in uncultivated men, in the deaf and dumb, and in persons whose education has been totally neglected. Even the testimony of many profane writers, and of the holy fathers, all lead to the same conclusion. Although we found our educational establishments, our laws, our system of rewards and punishments, our results in mental philosophy, and so forth, solely on the presupposition of a determinate and stedfast natural constitution in man,—although every step proves to us that we did not constitute ourselves, but that all that we are, and all that we can do, arise only from our Creator, and through him ; yet still another objection is urged against the doctrine in the following terms.

If the fundamental elements of our faculties and dispositions be innate, then are we rather the instruments than the masters of our actions ; we are given up as slaves to our internal impulses. What, then, becomes of liberty ? How can good and evil be ascribed to us ?

I again transcribe the answer to this objection, word for word, from my manuscript.

“ Those persons who would persuade themselves that our mental faculties are not innate, derive them from education. Have we not been, in every case, equally passive, whether we have been constituted in a certain manner by creation, or have received our qualities from education ?

“ In this objection, the notion of faculty and disposition—of simple tendency—is confounded with the manner of acting of these powers. Even the lower animals are not subjected absolutely, without any will, to their faculties and propensities. How strongly soever the dog is excited to hunting, and the cat to mousing, they may be restrained by repeated punishment, from following these inclinations. Birds repair their nests which have been destroyed, and



bees do over with wax a piece of carion which they cannot remove.

“Man, in addition to the qualities possessed by the lower animals, enjoys the faculty of speech, and the most extensive capacity for education, two inexhaustible sources of knowledge and motives. He has a faculty for distinguishing truth from error, justice from injustice, for conceptions of an independent existence; the past and the future are able to influence his actions; he is endowed with the feeling of morality, with clear consciousness, and so forth.

“Armed with these powers, man takes the field against his inclinations. These last, indeed, are excitements, which lead him into temptation; but they are not temptations of such a kind that they cannot be blunted or subdued by means of opposite or stronger motives. Man has, for example, the propensity to sexual enjoyment; but morality, the pleasures of wedded love, health, social decency, religion, and so forth, serve him as antagonist principles that Amateness may not subdue his mind. From this struggle virtue, vice, and culpability first arise. Wherein lies the merit of that so-highly-recommended virtue—self-denial—if no struggle with our internal inclinations is presupposed? Thus, the more numerous and powerful the opposing impulses are, the greater is the extent of will and moral liberty which man has received. The more powerful and determinate the internal tendencies are, the more do controlling principles become necessary. Hence arise the necessity and the utility of education, law, reward and punishment, and religion. On this also is founded the utility of the knowledge of mankind, and of the present doctrine concerning the origin and difference of the human faculties and dispositions.”

From the propositions now stated, it follows, that all tendencies, according as they are directed by external circumstances, may be turned to good or bad ends; in short, that education, religion, legislation, and so forth, have a powerful and undeniable influence over them;—that the primitive



dispositions are essentially the same in all men;—that thus the before-mentioned things are capable of exerting an influence on all men; but that every remarkable degree of any quality, which cannot be properly deduced from the influence of external objects, must depend on unusual strength of natural, innate dispositions; and these, again, on an unusually favourable constitution of the organs.

I teach farther, that it is only this extraordinary degree of development of an organ which becomes discernible to the initiated on the surface of the brain and skull; and that although we can thus judge, in many cases, of the leading dispositions of an individual, we cannot estimate the use which he has made of them through education, principle, and exercise. *Crimen in rebus non est, sed in usu agentis.* Isidor. lib. 3, *de summo bono*. When I observe a great power of memory in any one, I cannot tell whether he has used this talent in reading, in speaking, or in learning history, and so forth.

Thus the charge is unfounded, when I am accused of distinguishing the worthless and useless from the virtuous by the shape of their skulls. All this is impossible,\* because moral, social, civil, and religious conduct, is the result of many and different concomitant causes, and especially of many powerful external influences; for instance, education, example, habits, laws, religion, age, society, climate, food, health, and so forth. Thus, in place of my doctrine being injurious to any human being, it leads us to forbearance with our mutual weaknesses, and teaches us, when we know their sources, to avoid their detrimental consequences. “*Nec tardum ingenium, nec labilis memoria, nec inquietus appetitus, nec sensus obtusus, nec vita languens reum per se statuerunt hominem, sicut nec contraria innocentem. Et hoc non ob aliud, nisi quia et hoc necessario ac præter*

\* This was written in 1802. I consider it quite possible, in the present state of Phrenology, to distinguish the naturally worthless and useless from the virtuous by the shape of their skulls. See Combe's System, vol. ii. p. 695. 4th edition.—TRANSL.



voluntatem posse provenire probatur. Aug. de lib. arbit.  
et Bernard de lib. arbit.

The conclusion against the freedom of the will drawn from the innateness of our intellectual and affective faculties, arises from not duly distinguishing the meanings of the words—inclination, propensity, desire, and will. It is, and will continue to be, an everlasting truth, that the mental organs bestowed by the Creator, are the fundamental causes of all the states which, according to the differences in their intensity, we name inclination, propensity, tendency, desire, or passion. That we are not entirely masters of these feelings nobody doubts. But let us suppose several human faculties to act together (those of which I have spoken in the extract from my MS.); and let us suppose that they have been enlightened and improved by education, by social converse, by knowledge of the laws, and by the dictates of the sentiments of religion, and of right and wrong; and that the powers of judgment and controul resulting from all these combined causes are directed against any powerful organ acting singly,—then a contest will arise between its desires and their controlling influence; the possibility of choice will be enjoyed; and at last, and often in spite of strong and counter-acting desire, a resolution of the Will will be formed, which cannot be based on any organ, but is the free act of the soul itself.\*

I hope that I have now been so fortunate as completely to

\* There is some obscurity here in the original work, and I have given what I conceive to be its import. The text runs thus: "Dass wir hierüber nicht ganz Herren sind, bezweifelt kein Mensch. Aber lassen wir mehrere menschliche Kräfte zusammen wirken, von denen ich in der Stelle aus meinem Manuskripte geredet habe; setzen wir den jezt noch bloß einzeln wirkenden Organen die Resultate seiner Erziehungs- und Sprachfähigkeit, seines moralischen Gefühls von Recht und Unrecht, der Gesetze, der Religion, selbst der übrigen innern Kräfte entgegen—so erfolgt Kampf und Wahl zwischen Ja und Nein; es kommt endlich und oft ungeachtet der heftigsten, widerstrebendsten Begierde zum Willen, der schlechterdings auf kein Organ gegründet seyn kann, sondern das freye Werk der Seele selbst ist."—P. 59.



tranquillize his Majesty in regard to morality and religion. As, however, I am painfully led to conclude, from the imperial and royal motives already answered, that his Majesty has received generally unfavourable impressions of my doctrine, I consider it to be my bounden duty to add some remarks on its truth and utility.

With the single exception of the organic powers of the body, the brain is the source of the most important nervous functions,—namely, of the senses, of the affections, inclinations, and faculties,—in short, the source of all the human mental powers. Nevertheless, nothing has hitherto been known by physicians concerning this most important organ, except, at the most, the form of its larger and most essential parts. No one has been able to penetrate either into the universal functions of the brain, or into the use of any single essential portion of it, notwithstanding of all the pains which have been directed to that object.

But nobody can deny that, on the true knowledge of the functions of the brain in a state of health, depends the true knowledge and judgment of its diseased functions, and consequently the knowledge and conformable treatment of diseases of the intellectual and affective faculties, as well as of all the operations of the brain on the other parts of the body. Only on such a basis, is it possible to ground a rational philosophy of the mind, and of human nature in general. The difficulties which opposed the attainment of this object have been heretofore unsurmountable, as I have already repeatedly shewn. I have, therefore, made this difficult and important work my object in conjunction with my practice. I have, partly by myself, partly by the aid of favourable circumstances, which I have still more favourably employed, and especially by comparison with all the other departments of nature, found the means at last of advancing some steps towards fulfilling the greatest desire of Boerhave, Haller, Van Swieten, and Viq d'Azyr. Who would not rejoice, says likewise Herder, at a philosophical anatomist undertaking to give a comparative physiology of seve-



ral of the creatures, but particularly of those most nearly allied to man, according to the powers which have been pointed out and established by experience as different, and considered in relation to the whole organization of the animal. On this account I have received the warmest approbation of many of my hearers, who are certainly better authorities for the success of my efforts, than the suppositions and erroneous conjectures of all those who have either never heard me at all, or heard me very imperfectly. Nobody can deny the possibility of farther progress in subjects depending on experience and physical investigations. It would, therefore, be more wise to wait quietly the unfolding of events. If my system should prove unfounded, the teeth of time would devour it, and the world would have become acquainted with one additional error. If, on the contrary, it be grounded on facts, to which end all my efforts have been directed, what can misrepresentation do against it, and what can obstacles accomplish to destroy it?

It has always appeared to me to be perilous to speak of the practical uses of a science which is partly in its infancy, and partly imperfectly comprehended by those who study it. How many things are there whose future importance no human being could have proved, or even conjectured, at their first appearance? When the leading ideas of my system first stood clearly before my mind, many things which are now ascertained were only hypothetical. I had not anticipated that the influence of my discovery would have such beneficial effects on the education of the deaf and dumb of this city, as is shewn in the accompanying testimonial of the director May: and although I have studied solely to draw practical and useful doctrines from my discovery, yet I did not at all allow myself to maintain that, by means of the more certain knowledge of diseases of the intellectual and affective faculties, and through their more successful cure, I should do to mankind that essential service which the distinguished physician of the Lunatic Asylum, Dr Nord, ascribes to me in his testimonial. What may we not



hope for, then, when we shall be able to act more certainly on the particular organs of the brain; when all concomitant circumstances shall become favourable to the practical application of my principles to the treatment of the unhappy, nay nearly abandoned insane?\*

Above all, do not all diseases of the brain and nerves pass into aberrations of the mind itself, but which, by means of my doctrine, will be placed in a new light. To the same class belong the effects of over excitement of mind, of the excessive enjoyment of love or of wine; also the consequences of strong affections, such as grief, rage, avarice, and so forth.

When we consider, then, the lucid explanations of the most important phenomena of life, which have become possible by means of my doctrine; the infinitely numerous, great, and new views in regard to social practices, history, and so forth, which it has brought to light; it is impossible for me to believe that his Majesty can have meant to suspend my farther researches into the healthy and diseased functions of the brain.

I venture, therefore, with the fullest confidence, to propound the humble request that his Majesty may have the supreme grace not to arrest my labours in the farther advancement of this object, which concerns so many depart-

\* Dr Gall was forced to leave Vienna, his doctrines were proscribed, and they continue under the ban of government to the present day. One consequence is clearly traceable. The public Lunatic Asylum in that city is a disgrace to any civilized country. The physicians are not to blame; they see and deplore the miseries which are inseparable from an hospital built on the principles of a jail for felons, and destitute of every accommodation for the proper treatment of patients labouring under mental disease. If Dr Gall had been allowed to continue his lectures, light would long ere this time have reached the public and the members of the government, and this extensive cause of human suffering would at least have been mitigated. Prince Metternich was an early pupil of Dr Gall, and he knows something of Phrenology. The third volume of Dr Gall's great work is dedicated to him; and he now exercises supreme authority in Austria. If he desires to leave a monument of philanthropy behind him, he should build a new pauper lunatic asylum for Vienna, in harmony with the lights of the age.—TRANSL.



ments of science, but again to permit my lectures to proceed, in virtue of the prescriptive right belonging to us physicians, which I have never abused. I support this dutiful request by the following considerations.

1. As my doctrine on the functions of the brain has been taught to several thousand hearers, and as it has been spread abroad among a still greater number of persons by the sale of Froriep's Treatise, in three editions, and by means of smaller extracts and notices in almost all the German, English, and French journals, it is no longer in the power either of myself or of any human being to arrest its progress, or to set bounds to its circulation.\* As, moreover, nobody has hitherto discovered in it any danger to religion and morality; and as, on the contrary, many have found in it the most powerful reasons against Materialism; it appears to me that equivocal and dangerous consequences are inevitable, if the stigma of Materialism be now forcibly set upon it, and the virtuous and useful prosecution of it be denied to me,

2. Farther, to act in this manner would seriously injure me personally; for the public at large would believe that I had really taught immoral and irreligious doctrines, and Materialism; and thus I should stand forth before the world branded with the mark of a dangerous man. I should be robbed of the confidence of the public, on which a physician

\* It is thirty-five years since this sentence was penned, and the Austrian government may now read its verification. While the doctrine of Gall is exerting a powerful, a beneficial, and an increasing influence on the medical practice, the literature, and the philosophy of England, France, and America, Austria reposes in the darkness which she evoked when she banished this great discoverer from her soil. The full extent of the mischief which, by this act of bigotry and oppression, she inflicted on herself is not yet discernible, either by her own people or by the surrounding nations; but before the end of the present century, no prophet will be needed to unfold its magnitude and consequences.—  
TRANSL.



lives; and so forth.\* These consequences must be the more severely felt in proportion as the individual has laboured, all his life long, to benefit mankind, and as he has been fortunate, partly through the successful treatment of disease, and partly by his writings (which have been received with universal approbation), to acquire a distinguished reputation both at home and abroad.

3. To this perilous injury to my reputation, involving the loss of all the advantages arising from the hard-earned confidence of the public, must be added a consequence deeply affecting my interest. My collection of plaster-casts,—of the skulls of men and animals, and of the brains of men and animals in wax, has cost me above seven thousand Gulden; and I have already made very expensive preparations, exceeding in amount fifteen thousand Gulden, for a splendid work on the functions of the brain, which has been universally demanded of me: this property will be rendered useless by destroying my reputation.

4. Nothing valuable can be discovered regarding the animal economy by conjectures *à priori*, but solely by observing real phenomena, and by experience. I should have produced only sadly deficient and partial views on this subject, if I had confined myself to my own reflections in prosecuting this extensive subject. To a great extent I attribute whatever I have brought forth in regard to this subject that is true, good, and valuable, to the conferences which I have enjoyed with so many scientific men of all classes, to which the lectures gave occasion. In them the objects were presented in their true lights, and in their logical combinations.

5. The many useful truths before mentioned, referring partly to medicine, and partly to other subjects, cannot pos-

\* Dr Gall was at this time in the forty-fifth year of his age, and in the exercise of very high and extensive practice as a physician in Vienna. The rejection of the prayer of this petition rooted him up, and sent him virtually into banishment.—TRANSL.



sibly be expounded except connectedly; and consequently cannot, at least at present, be distributed among the various other departments of science. Besides, no ordinary public teacher is at present able to treat of these various subjects, because no one possesses the necessary proofs, namely, a suitable collection.

Should it be possible, after all that has been said, that any one should entertain so mean an idea of our exalted holy religion, and of the excellence of our christian morality, that he still fears that their foundations can be undermined and endangered by physical researches on the living corporeal condition of man,—then I conclude by begging,

That, in order that not even the smallest doubt may remain concerning the perfect innocuousness of my system;—on the contrary, that every reasonable man may be convinced of the intrinsic adaptation of it to purposes of general utility, his Majesty will be graciously pleased to appoint a commission, composed of men selected from the four faculties of Theology, Law, Medicine, and Philosophy, who are conversant with the sciences, and enjoy in the highest degree the confidence of his Majesty; and that this commission, while it consists of men worthy of every confidence, may be taken from individuals belonging to these faculties who have at different times heard my lectures, and who will be thereby qualified to give evidence, free from all suspicion, of the perfect conformity of my lectures themselves with the representation of them now given. Before these men, thus graciously appointed by his Majesty, I shall produce my system, with all its evidences in due connection, yet with every possible brevity; and thereafter await, with the profoundest respect and unflinching deference, my future fate.



## NOTE BY THE TRANSLATOR.

The only effect of this most reasonable supplication was the publication of a new order of the cabinet, rendering permanent the previous prohibition against Dr Gall's lectures; and he left Vienna and the Austrian dominions a short time afterwards, and never returned to them.

Every individual interested in the freedom of thought, and in the welfare of mankind, must deplore the dark and narrow minded bigotry which dictated this act of oppression; but in visiting it with our just indignation, we shall do well to remember, that the most enlightened men in Britain, even much later than 1802, and after they had enjoyed ample time and opportunities for ascertaining the truth of Dr Gall's discoveries, persecuted his name, his doctrines, and his followers, with a degree of virulence and determination which shewed that the power only, and not the will, was wanting, on their parts, to extinguish them for ever. In 1803, Dr Thomas Brown, the celebrated metaphysician, and himself a doctor of medicine, condemned Dr Gall's discovery *in toto* in the Edinburgh Review. He says, that he "trusts that his readers are already sufficiently convinced that the principles on which Dr Gall has founded his theory are erroneous;" and that "it is unfortunate for Dr Gall's theory that he has entered into the detail of it with such minute exactness, as it enables every one too easily to compare its predictions with the skulls of those around him."—(No. iv. April 1803.) In 1815, Dr John Gordon, a lecturer on anatomy of great reputation in Edinburgh, wrote an article on this subject in the 49th number of the Edinburgh Review, in which he says: "We look upon the whole doctrines taught by these two modern peripatetics (Drs Gall and Spurzheim), anatomical, physiological, and physiognomical, as a piece of thorough quackery from beginning to end." The Quarterly Review, in their notice of Madame de Stael's



"L'Allemagne," censured her for being "by far too indulgent to such ignorant and interested quacks as the cranio-logist Dr Gall;" and in No. xxv., p. 128, the same review rejected the new science as "sheer nonsense," and designated Dr Spurzheim as a "fool." Dugald Stewart, in his Preliminary Dissertation to the Supplement to the Encyclopædia Britannica, exclaimed, "Is there no Arbuthnot now to chastise the follies of our modern craniologists!" Dr Peter Roget, in the article "Cranioscopy," published in the Supplement to the Encyclopædia Britannica, complacently concludes by observing, that his own "simple explanation of the sandy foundation on which craniology has been built; of the flimsy materials of which it has been compounded; and the loose mode in which they have been put together; will suffice to enable our readers to form their own conclusions as to the soundness and solidity of the edifice." The Rev. Thomas Rennell, christian advocate in Cambridge, published "Remarks on Scepticism, especially as it is connected with the subjects of Organization and Life," in which, to the utmost of his ability, he sounded the alarm of Materialism against Mr Lawrence, and maintained the independence of the mind, even in this life, on matter. He asserted that "Sleep is an affection of the mind, rather than of the body;" and made many similar statements. In a note on the "system of Gall and Spurzheim," he says: "It must certainly be allowed that this system does not, of logical necessity, terminate in Materialism;" nevertheless he assured his readers, that it "is annihilated by the commonest reference to fact:" he designated it as "the flimsy theories of these German illuminati;" and spoke of its "absurdities," and of "this master-piece of empiricism." In 1826, Lord Jeffrey designated the doctrines as "crude," "shallow," "puerile," "fantastic," "dull," "dogmatic," "incredibly absurd," "foolish," "extravagant," and "trash;" and said, that "long before this we expected to have seen the 'plaster-heads' turned into toys for children, and this folly consigned to that great limbo of vanity to which the



dreams of alchemy, sympathetic medicine, and animal magnetism, had passed before it." On 6th February 1830, Dr Hope, Professor of Chemistry, from the Chair of the Royal Society of Edinburgh, stated, that, "as different organs are provided for each of the external senses, it is extremely probable that each of these organs has a particular share in the general mental operations of the brain assigned to it; that it is a study truly philosophical, and strictly physiological, to investigate the special use of each of these (cerebral) organs, and the particular mental function to which each was subservient;" and added, that "the phrenologists of the present day were not in the right path, and had not advanced a single step in this physiological investigation." In 1836, Sir Charles Bell printed, without modification, the following observations, which first appeared in his treatise "*On the nervous circle which connects the voluntary muscles with the brain*," published in the Philosophical Transactions. "The most extravagant departure," says he, "from all the legitimate modes of reasoning, although still under the colour of anatomical investigation, is the system of Dr Gall. It is sufficient to say, that without comprehending the grand divisions of the nervous system, without a notion of the distinct properties of the individual nerves, or having made any distinction of the columns of the spinal marrow, without even having ascertained the difference of cerebrum and cerebellum, Gall proceeded to describe the brain as composed of many particular and independent organs, and to assign to each the residence of some special faculty."\* Let

\* The extravagant ignorance of Dr Gall's real character as a physician, and of his attainments as an anatomist, implied in these remarks of Sir Charles Bell, may be estimated by referring to Cuvier's testimony to the value of his and Dr Spurzheim's contributions to the anatomy of the brain, on p. 100. The whole records of the opposition to Phrenology do not afford a more inconceivable extent of prejudice than these remarks contain. Dr Gall stood in the first rank of practising physicians in Vienna, and was in the forty-fifth year of his age in 1802, when his lectures were prohibited by the Emperor. To affirm that such a station could be acquired and maintained in the Austrian capital in the 19th



the odium of mistaking and misrepresenting the nature, and of opposing the progress, of this beneficent and important discovery, be shared by these distinguished men with the Austrian Emperor; and let it be mentioned to his honour, that, in 1815, he made the only atonement in his power, by personally inviting Dr Gall to return to his capital,\* while they have never yet retracted one syllable of the censure and condemnation which they have thus recklessly directed against him. On the contrary, in so far as their influence with the world extends, they continue to obstruct, by the authority of their names, all serious examination into the subject.

century by a person who had not "even ascertained the difference of cerebrum and cerebellum," is strange enough; but to repeat this assertion in 1836, when Dr Gall's method of dissecting the brain is generally recognised in France, Britain, and America, as the best in existence, surpasses all ordinary hardihood of detractive assertion.

\* See Phrenological Journal, vol. xi. p. 28.

FINIS.



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