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AN

# ELEMENTARY SYSTEM

OF

# PHYSIOLOGY.

BY JOHN BOSTOCK, M.D.

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TO

# WILLIAM BABINGTON, M.D. F.R.S. F.G.S.

&c. &c. &c.

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Dear Sir,

I trust that you will excuse the liberty which I have taken in prefixing your name to my third volume. The friendship with which you have honoured me, since my residence in the metropolis, might plead my excuse; but I will acknowledge, that I have been principally induced to make use of your name from the kind interest which you have taken in the progress of my work, and the approbation which you have bestowed upon the former parts of it. It is impossible not to feel the value of commendation when it proceeds from such a quarter, from one who is so thoroughly conversant with all the topics which form the subject of my treatise. The profound knowledge and the ample experience which you possess of the morbid actions of the animal œconomy enable you to form a correct judgment of the degree in which the practice of medicine may be expected to derive benefit, by investigating the functions of the living body in their healthy state, and the interest which I know you attach to these inquiries affords the best warrant of their importance.

I am, dear Sir,

With every feeling of esteem and respect,

Your obliged friend and obedient servant,

J. BOSTOCK.

Upper Bedford-Place,
May 23d, 1827.

# PREFACE.



The present volume completes the plan which I originally proposed, of giving a summary view of the present state of physiological science. I am far from supposing that the method which I have pursued is the best which could have been adopted, or that it is perfect in its execution; but I may be permitted to say, that the deficiencies of my work do not arise from any want of care or attention on my part, and I believe that no material improvement would have arisen by longer deferring its publication. Many of the topics that are treated of in this volume are such as do not always fall under the cognizance of the physiologist; yet I consider them as bearing so intimate a connexion with the animal frame, as to afford sufficient ground for taking at least a cursory view of them. We have observed in almost every branch of the subject that has fallen under our examination, the greatest diversity of opinion to prevail, and we have found that this is the case even on topics which seem to admit of being decided by a direct appeal to experiment. It cannot, therefore, excite surprize that obstacles almost innumerable should assail us at every point, when we attempt to penetrate the intricacies of metaphysics, where we have to treat upon subjects, the very conception of which is difficult to attain, and where we have nothing to guide our researches but imperfect deductions and doubtful analogies. It is, however, not a little remarkable, that it is on these dubious points that mankind have shown the most pertinacity of opinion, and have been the least disposed to manifest a spirit of candour towards those who have differed from themselves. On such subjects the utmost that I can expect to have accomplished, is to have endeavoured to free my mind from prejudice, and to state my opinion with that cautious moderation, which is fitted to the uncertain nature of the evidence on which it is necessarily founded.

Considering that three years have elapsed since the publication of my first volume, I have found occasion for less alteration and correction in the former part of the work than might perhaps have been expected. My readers will perceive that I have taken the opportunity of introducing in various parts of the notes some of the necessary corrections and additions; the remainder I have placed in an appendix. I will not venture to presume, that these are all the deficiencies and inaccuracies which the work may contain, but they are all that can be ascribed to inadvertency. As to the errors of judgment, or the defects of information, I must leave them to be rectified and supplied by my successors, being satisfied with the reflection, that my attempt may have the effect of smoothing the path to one of the most interesting and ennobling pursuits that can possibly occupy the human mind.

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# PHYSIOLOGY.

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## CHAP. XII.

### OF GENERATION.

THE functions that have hitherto passed under our review are supposed to depend essentially upon the contractility of the muscular fibre, by which either a mechanical or a chemical change is produced, the first constituting the direct, and the second the indirect effect of this property.[1] These functions would appear to embrace all that is absolutely necessary for the support and continuance of animal

[1] Certain objections, which I admit are not without considerable force, independent of the respectability of the sources whence they proceed, have been urged against the designation of *contractile,* which I have assigned to the first class of functions. Although I think it would not be difficult to prove that, at least in the higher orders of animals, muscular contraction is the first step in the process, yet, to avoid all discussions of a merely verbal nature, we may substitute the term *physical,* which is indicative of their obvious and essential effects.

life, and probably to be each of them, to a certain extent, essential to its existence. There are, indeed, certain species of animals which may seem to afford an exception to this remark. In some cases the minuteness of the object scarcely permits us to acquire any satisfactory information concerning the œconomy of the individual; and there are others, where the organization is so imperfect, as to prevent us from forming any direct comparison with the more complicated animals.

And yet even here we have a certain analogy, which may guide us in our investigations. Although many of the inferior orders have no circulating system,[2] by which the fluids are progressively carried to the different parts of the body, yet we may conclude that they are deposited in appropriate cavities or reservoirs, from which they are taken up, as occasion may require, so as to afford each organ its due supply of nutrition. Although not possessed of lungs, by which to inhale the air, and thus bring it into proximity with the blood or other analogous substance, yet we find that the same chemical change is produced by the external surface of these animals, as by the lungs of the vertebrata. It appears probable that all animals possess some power of regulating their temperature, although this power exists in a very small degree only in many of the lower orders. It is obvious that every organized being must possess

[2] Blumenbach, Comp. Anat. by Lawrence, c. xii. § 166, 7, note F; Cuvier, Leç. d'Anat. Comp. No. 24. t. iv. p. 188; Lamarck, Anim. sans Vert. t. i. p. 149, 360; t. iii. p. 248.

a power equivalent to digestion, that the formation of their different parts and textures can only be performed by the power of secretion, and that a species of absorption must be the mode in which they appropriate to themselves the materials of which they are composed.

The next order of functions, those which depend upon the operation of the nervous system, are much more confined in their existence and more limited in their operation. None of the functions which have been enumerated above are necessarily connected with a nervous system, and accordingly there are numerous tribes of animals in which no nerves have been detected. The presence of this system invests the animal frame with a set of powers and actions that are essentially different from those that depend upon contractility. They do not necessarily produce either a mechanical or a chemical change. The mode in which they act is indeed altogether beyond our comprehension, and the effects which ensue from this action are not referrible to any other of the known operations of physical agents. Still, however, we are enabled to observe the operation of cause and effect with as much minuteness in the action of the nervous as of the muscular system; and by observation and experiment we can, in most cases, predict with sufficient accuracy the results of the impressions of various kinds which are made upon the brain and nerves.

But before we enter upon the consideration of this class of vital operations, it will be proper to inquire into the mysterious phenomena of generation, a function which may be regarded as, in some measure, intermediate between the other classes; for although it can scarcely be referred to any modification of contractility, yet it is not essentially connected with nervous action, as it exists in animals that are without a nervous system, as well as in those that are the most amply provided with it.

In treating upon this subject, I shall divide my remarks into three heads. After a few observations upon the anatomical structure and physical properties of the generative organs, I shall, in the second place, inquire into the part which the sexes respectively perform; and, lastly, I shall examine the nature of the process, and give a brief sketch of some of the most celebrated hypotheses that have been adopted to explain this function.

### § 1. *Remarks on the Structure of the Generative Organs.*

There is no function which exhibits a greater variety in the comparative anatomy of the parts that are subservient to it than the one now under consideration. But all the warm-blooded vertebrated animals agree in the essential particular of there being two sexes, the congress of which is necessary for the completion of the process; the one furnishing a peculiar secretion, and the other an appropriate situation in which the secretion may be deposited.[3] The male consequently possesses an apparatus necessary for the formation of this secretion, and a duct by which it may be afterwards conveyed into the body of the female, while the latter is provided with a cavity into which the secretion is received, and in addition to this, with an organ of a more complicated structure, the functions of which are perhaps not in all respects thoroughly understood, where the rudiments

[3] In all the vertebrated animals, with a few exceptions which appear to be incidental, and, as it were, anomalous varieties (see Phil. Trans. for 1823, pl. 15, 20) the sexes are distinct. Some of the mollusca possess both sets of organs, and require no co-operation; while in others, where both sets of organs are present, mutual congress is necessary, each animal impregnating the other; Haller, El. Phys. xxix. 1. 1..6; Lawrence in Blumenbach's Comp. Anat. note K. in ch. xxiv. p. 460, 1; Cuvier, Leçons d'Anat. Comp. No. 29, sect. 4. t. iv. p. 164, et seq. We are informed that there are certain of the microscopical animals which require the co-operation of three individuals; Senebier, Introd. to Spallanzani, Opusc. de Phys. p. lxxvi. There are large classes of the inferior animals, which are propagated by simple division, or by the separation of a smaller body from the larger one of the parent, without the intervention of any thing analogous to sexual congress; but the mode of their propagation bears no analogy to the function of generation as exercised by the more complicated animals; Blumenbach's Comp. Anat. by Lawrence, ch. xxiv. note K. p. 461. The mode of generation in the lowest classes of animals may be traced in the various parts of Lamarck's Work, Anim. sans Verteb. t. i. p. 404, 433; t. ii. p. 8, 21, 31, 46, 69, 213, 407; t. iii. p. 61, 67, 73, 141, 159, 197, 207 237, 245, 274..7; t. iv. p. 85, 94, et alibi. We may remark, that the perfection of the function of generation, or the analogy which it bears to the same function in the higher orders, does not proceed in a regular progression with the other functions of the invertebrated animals.

of the young animal are originally formed, and upon which the male secretion produces its specific action.

For one of the earliest descriptions of the male generative organs, which contains a correct account of the minute anatomy of the parts, and of their relation to each other, we are indebted to De Graaf. A point upon which he insisted, and which he elucidated with more accuracy than had been done before his time, is the complete vascularity of the testis. The most distinguished authors who had preceded him conceived that a large part of this organ consisted of what they termed a glandular or medullary substance, while De Graaf, on the contrary, states his opinion, that the testis is merely an assemblage of very minute vessels which elaborate the peculiar secretion.[4] These are described as being folded up, as it were, into regular bundles or lobules, which do not anastomose with each other, and which may be drawn out into

[4] De Vir. Org. Gen. inserv. p. 55. tab. 1.. 4; this treatise, which was published in 1668, and which seems to have been composed from actual observation, contains an ample account of what had been done by his predecessors. His plates, although not executed with the elegance of modern engravings, and partaking, perhaps, in some cases, rather of the nature of plans, than of actual views of the parts, are characteristic and expressive. The anatomists, whose descriptions of the testis he particularly contrasts with his own, are Galen, Riolan, Fallopius, Spigelius, and Vesling, with others of less note. For figures of the testis, see also Bidloo, Anat. Hum. Corp. tab. 45 .. 7; Albinus, Acad. Annot. lib. 2. tab. 7. fig. 1, 2, 3; and tab. 3, fig. 1. On the subject of De Graaf's discoveries the remarks of Mr. Bell may be consulted; Anat. v. iv. p. 191, et seq; his 2d, 3d, 4th, and 5th plates, also the plan in p. 195, exhibit good views of the testis and the parts connected with it.

single vessels of a very extraordinary length. Their diameter has been stated to be no more than $\frac{1}{200}$ of an inch, while it has been estimated that the total length of the vessels which compose one of the testes amounts to more than 500 feet.[5]

Besides the testis, the vesiculæ seminales, both from their size and their situation, have been supposed to perform some important part in the function of generation, although it has been difficult to ascertain the exact nature of the purpose which they serve. The opinion formerly entertained was, that they are merely reservoirs in which the semen is deposited as it is secreted. In consequence, however, of the observations of Hunter, who remarked that the fluid contained in these cavities appeared to be different from that found in the testis, many of the later anatomists have supposed that the vesiculæ seminales produced a secretion of a peculiar nature, the

[5] Haller, El. Phys. xxvii. 1. 16, adopts the opinion of De Graaf, and remarks, that except these lobules and the cellular texture which separates them from each other, the testis contains "nihil "ultra et neque aliud parenchyma, neque glandulosi quid." See also Boerhaave, Prælect. t. v. part 1. p. 137. § 644, cum notis; Haller, "De Vas. Sem. Obs." cum Tab. Op. Min. t. ii. p. 1, et seq.; also a paper of Haller's in Phil. Trans. for 1750, No. 494, p. 340, et seq. For the diameter and length of these vessels, see Monro de Test. et Sem.; Blumenbach, Inst. Phys. § 523. p. 278. Monro, 3us. in his Outlines, v. iii. p. 46. fig. 39. and also in his Elements, v. ii. p. 179, et seq. gives an account of the testis, principally from his father's observations. See also Bichat, Anat. Des. t. v. p. 188, 9; for figures of the part, see Cowper, Anat. Corp. Hum. tab. 45, 6. and Caldani, Icones Anat. pl. 131.

use of which may probably be to dilute the semen or to add to its bulk.[6]

The principal physiological point connected with the male organs, that requires our attention, respects the mode in which the secretion is produced. It might be supposed that a gland capable of secreting a fluid, endowed with such extraordinary property as that derived from the testis, would exhibit something peculiar in its structure, or what would lead to some indication of the means by which it acquired its specific properties. But we are scarcely entitled to say that this is the case. We find, indeed, that the

[6] See Hunter on the Animal Œconomy, p. 31, et seq. Haller, El. Phys. xxvii. 1.23..6, gives a list of animals who are provided with this organ, and of those who are without it; he also enumerates many in whom there is no direct communication between its duct and the vas deferens. This circumstance appears to afford a strong anatomical argument against the opinion of the vesicle being merely a reservoir for the semen; the case of the gall bladder, which Haller supposes to be analogous, scarcely applies to this part. M. Magendie, however, still adheres to the old opinion, t. ii. p. 406, and the same is the case with Blumenbach, Inst. Phys. § 529, cum notis, and with Sœmmering, Bibl. Med. t. iii. p. 87, as referred to by Blumenbach; see also Dr. Elliotson's judicious observations in note H. But whatever opinion we may form on this point, it is obvious that the testis is the only essential organ, the seminal vesicle being frequently absent. With respect to the animals in which this organ is wanting, besides the above reference to Haller, see Blumenbach's Comp. Anat. by Lawrence, § 315, and note D; Cuvier, Leç. d'Anat. Comp. No. 29. t. v. p. 29..41. For figures of this organ, see Albinus, Acad. Annot. lib. 4. tab. 3. fig. 1, 2; Cowper, Anat. Corp. Hum. tab. 47. fig. 2; Caldani, Icones Anat. pl. 131. fig. 13, 14, 15.

arteries which are sent to the testis exhibit the peculiarity of being of an unusual length in proportion to their diameter, and of pursuing a singularly tortuous course[7] (a circumstance which we have seen above is much more remarkably the case with the seminal ducts themselves) so that the blood must necessarily pass very slowly along them, but we do not perceive any other circumstance in which they differ from the ordinary state of the vessels which supply the secreting organs. We are then led to inquire, what is the precise change which would be induced upon the blood, or any of its components, by being slowly propelled through a long narrow tube. We may conceive that it would be separated into its proximate principles, that only the more fluid or soluble ingredients would be carried forwards, while there would be sufficient opportunity for all the changes to be effected which arise from the action of these principles upon each other. Such considerations do not, however, tend to throw any light upon the action of the spermatic vessels, nor is the result of their action what might perhaps have been previously expected from the operation of the cause.

The semen, as I have already had occasion to remark, belongs to the class of mucous secretions; it

[7] Haller, El. Phys. xxvii. 1. 10, 1; Blumenbach, Inst. Phys. § 509. From the mechanical structure of these vessels, Keill estimates, that the blood must move in them 150 times slower than if they had been disposed in the ordinary manner; Essays, page 153; we may conclude that his calculation is not without some foundation, although probably the effect is considerably overrated.

has, however, some properties of a specific nature, which are unlike what we meet with in any other of the animal fluids. Its odour is specific, the fibrous substance which it contains, and the changes which it undergoes by its spontaneous decomposition are peculiar to itself, while we cannot perceive any relation between these circumstances and the mode in which the fluid is secreted, or the purposes which it afterwards serves in the animal œconomy.[8]

An observation was made about the middle of the 17th century, which is in itself a very curious matter of fact, and which gave rise to an almost infinite number of experiments and speculations; I allude to Leeuwenhoek's discovery of the existence of animalcules in the semen.[9] So extraordinary a fact, and

[8] Haller, El. Phys. xxvii. 2. 11..16; Fourcroy and Vauquelin, Ann. Chim. t. ix. p. 64, et seq.

[9] I have spoken of this as the discovery of Leeuwenhoek, in conformity with the common opinion; although there is sufficient reason to believe that the observation was first made by Hamme. Leeuwenhoek informs us that this was actually the case, but we may conceive that the fact would have passed almost unnoticed, had it not been brought so fully before the public by his zeal and activity. See Haller in not. 1. ad Boer. Præl. § 651; and El. Phys. xxvii. 2. 3. The discovery was also claimed by Hartsoeker, Essay de Diop. Art. 88. p. 227, but apparently with little justice, for it appears pretty evident, that, even if he saw these bodies, he was not aware of their peculiar nature, until it had been ascertained by Leeuwenhoek. In a letter to Van Zoelen, dated Dec. 17, 1698, Op. t. iii. p. 57, et seq. Leeuwenhoek refers to the claim that had been just made by Hartsoeker, that he had discovered the animals twenty years before, and had published an account about the same time in the Journ. des Scavans, No. 30, for 1678. This claim led Leeuwenhoek to give a full account of

more especially as taken in connexion with the peculiar properties of the fluid, could not fail to excite

the transaction with Hamme, from which it appears that Hamme showed the bodies to Leeuwenhoek the year previous to their alleged discovery by Hartsoeker, and that Leeuwenhoek immediately transmitted an account of them in a letter to Brouncker, President of the Royal Society, dated Nov. 1677; see Phil. Trans. v. xii. No. 142. p. 1040, et seq. Leeuwenhoek refers to the same subject, Op. t. iii. p. 285. and t. iv. p. 169, in which passages he speaks of Hartsoeker with a degree of severity which is not usual with him; his general habit was to state his opinions with great moderation. See also Valisneri, Op. t. ii. p. 102; Senebier, Introd. to Spallanzani, Opusc. de Phys. p. xli. In Haller's Bibl. Anat. t. i. p. 663, 4, we have a list of Hartsoeker's works, with their respective dates; from this it appears that the animalcules were first mentioned by this writer in the Journ. des Sçav. for 1678; in p. 371, et seq. we have an article by the editor, containing an account that had been transmitted to him from Hartsoeker, of his having observed these animalcules; but the fact is simply stated without any comment. There is, however, a reference to a previous account that had been given in a former part of the same volume. This is contained in a letter from Huygens to the editor, in which, speaking of a new microscope that had been lately brought from Holland, he alludes to the discovery, but without naming the discoverer, p. 345 . . 7. The passage in the "Essay de Dioptrique," in which Hartsoeker speaks of the discovery is as follows: "Il y a plus de vingt ans que j'examinai le premier, à ce que je crois, la semence des animaux avec des microscopes, et que je decouvris qu'elle est remplie d'une infinité d'animaux semblables à des grenouilles naissantes, comme je le fit mettre dans le 30me Journal des Sçavans de l'Année 1678—," p. 227. This work was published in 1694. Haller says, that Hartsoeker gave a more full account of the animals in Journ. des Sçav. for 1695, but this paper I could not find. In his "Suite des Conjectures Physiques," published in 1708, he speaks of the animals, but without claiming the discovery or alluding to the

the greatest interest, and accordingly the observations were repeated by many physiologists both at that period, and among our contemporaries. As is frequently the case on those points which depend upon the evidence of the microscope, various opinions were advanced respecting the appearance and properties of these animalcules. Their existence, however, was generally admitted by those physiologists whose authority is, on all accounts, the most to be relied on, and even those who opposed Leeuwenhoek differed from him rather as to the nature of the bodies, and their supposed operation in the animal œconomy, than with respect to their actual existence.[1]

But notwithstanding the weight of evidence which was brought forwards, a degree of doubt still remained in the minds of many physiologists as to the specific nature of these animalcules.[2] It seemed impossible

controversy to which it had given rise; Dis. 7. p. 105 . . 7; this was ten years after Leeuwenhoek's letter to Van Zoelen. In the "Cours de Physique," published in 1730, Hartsoeker enters fully into the subject, claims the discovery, as having been made by himself in 1674, and bitterly complains of Leeuwenhoek's injustice towards him.

[1] This was particularly the case with Buffon, whose remarks and observations will be noticed below, as involving a peculiar hypothesis of generation.

[2] We find that Linnæus was one of those who discredited the observations of Leeuwenhoek. A thesis was published under his presidency in the year 1746, in which the author supports the opinion, that the bodies observed in the seminal fluid are not independent animals, but merely inert particles, which are set in motion by some physical cause. Several eminent names are brought forwards, who are said to countenance this doctrine, and

not to admit, that living bodies were present, but it was contended that they were merely a species of the animalcula infusoria, those minute beings, which, by the aid of the microscope, may be detected in all fluids that are impregnated with any animal or vegetable substance, and which, it may be presumed, are produced from ova, that are constantly floating in the atmosphere, and which make their appearance in all situations that are favourable to their evolution and subsistence.[3]

It was to remove these doubts that the researches of Spallanzani, and still more lately those of MM. Prevost and Dumas were particularly directed. The

Lieberkuhn is supposed to have proved it by his microscopical observations; see Wahlbom's Diss. "Sponsalia Plantarum," in Amœnitates Acad. t. i. p. 79; also Spallanzani, Opusc. de Phys. t. ii. p. 131, 2.

3 This was the case with Tuberville Needham, a naturalist, whose accuracy and candour give his opinions every claim to our respectful attention. In several parts of his "new microscopical discoveries," published in 1745, he refers to the observations of Leeuwenhoek, and endeavours to show, that what he saw were not proper animalcules, but certain inanimate bodies, which had a mere mechanical motion; p. 56..9; 60..2; 82..4. See also the corresponding passages in the translation of the above work, "Nouvelles Observations Microscopiques;" p. 65..0; 71..4; 97..2; with the translator's remarks and Needham's reply. In his letter to Folkes, written in 1748, which is subjoined to the latter work, he admits the bodies in question to be animals, but does not suppose them to have any specific relation to the semen; he merely regards them as belonging to the class of infusory animalcules; Nouv. Obs. Micr. p. 183, 212..4, 242. It will be found that in his replies to the translator's notes, Needham considerably modifies the opinions maintained in the text: the translation was published in 1750.

first of these naturalists applied himself, with his accustomed zeal and industry, to the refutation of the hypothesis of generation which had been proposed by Buffon and Needham. In the prosecution of this object he was led to inquire into the nature and appearance of the infusory animalcules, and to compare them with those bodies which are found in the seminal fluid. He has detailed his observations on both these points, with what may probably be thought an unnecessary degree of prolixity, but in such a manner as to impress the reader with a full conviction of his accuracy. The general result is, that the observations of Leeuwenhoek are, on every essential point, confirmed by those of Spallanzani, that the seminal animalcules have a definite figure, which it would seem not difficult to recognize, and that they are obviously different from the animalcules that are found in infusions.[4]

The observations of MM. Prevost and Dumas appear to have been made with the greatest care, and

[4] Spallanzani's observations are detailed at full length in his volumes entitled "Opuscules de Physique," as translated and edited by Senebier; the first volume is principally occupied with an account of the different infusory animalcules, as they are found in various animal and vegetable fluids. The account of the seminal animalcules is contained in the second volume, ch. i. p. 90, et seq. tab. 3. fig. 1, 5; in the following chapter, p. 122, et seq. he compares his observations with those of Leeuwenhoek, and notices their complete coincidence. We have various observations in the remaining chapters, which prove the independent existence of the seminal animalcules, and at the same time their distinction from the infusory animals.

are related with minuteness. They examined the spermatic fluid of various animals from the different orders of the mammalia, birds, and those with cold blood, and they found the animalcules as described by Leeuwenhoek and by Spallanzani, so as to leave no doubt of their existence, as specifically belonging to the secretion of the testis.[5]

The semen, whether prepared in the testis alone, or by the conjoined operation of the testis and the vesiculæ seminales, is so disposed, that by the excitement of the termination of the excretory duct, it is projected with considerable force into the body of the female.[6] The excretion of this fluid may be sup-

5 Memoires de la Societé de Physique de Geneve, t. i. p. 180, et seq. pl. 1, 2. The authors afterwards published an account of their observations in the Ann. des Sciences Naturelles, in a series of papers contained in the first and second volume of this work.

6 The congress of the sexes, attended with the entrance of the male, takes place only in the two first classes of the vertebrated animals; in the amphibia there is the congress without the entrance, while in fishes there is neither congress nor entrance. Among the invertebrated animals the action is accomplished in various ways, which bear no exact relation to the other functions, or to the circumstances upon which their classification is founded; Blumenbach's Comp. Anat. c. 23, 24, with Mr. Lawrence's notes; Cuvier, Leç. d'Anat. Comp. No. 29, t. v. p. 164, 5. In these cases the excitement of the termination of the excretory duct must be effected in a different mode from what it is in the former. In the change of form in the male organs, we have one of the most remarkable examples of that structure which has been termed by anatomists erectile. The final cause of the change in the figure of the corpora cavernosa is sufficiently obvious, and there appears little reason to doubt that the immediate physical cause consists in the injection of blood into a series of membranous

posed to obey the same laws which influence the other analogous organs of the animal body, but it is so far peculiar, as being connected with a specific and very powerful nervous sensation. This constitutes the most urgent of what are termed the appetites or passions; in the lower tribes of animals, it would appear to be altogether instinctive, and in the human race, if not instinctive, is so violent in its operation, as frequently to counteract every other feeling both of a physical and a moral nature.[7]

cells of which the body is composed, produced by preventing the return of the blood through the veins. It is not, however, very easy to explain the way in which the various mental and physical causes operate so as to produce the injection, nor how, upon the cessation of these causes, the removal of the blood is so quickly effected; see Haller, El. Phys. xxvii. 3. 7. 10, 11. Anatomists have differed in opinion respecting the muscularity of the urethra; Haller thought that it was not a muscular organ, El. Phys. xxvii. 3. 31, and Mr. Bell coincides with him, Anat. t. iv. p. 17. Prof. Monro informs us that his father conceived it to be muscular, Elem. v. ii. p. 171. See farther remarks on this subject by Prof. Monro, in the Edin. Journ. Med. Scien. v. ii. p. 8, 9. Sir E. Home is of the same opinion, and Mr. Bauer supposes that he has been able to demonstrate the muscular fibres; Phil. Trans. for 1820. p. 183, et seq. pl. 22, 3. Hunter supposes that the cells of the corpora cavernosa are muscular, Anim. Œcon. note in p. 43. Moreschi has attempted to prove that the part is entirely vascular, not, as had been generally supposed, cavernous or cellular; whatever opinion we may be induced to form upon this point his elaborate plates may be studied with advantage; Comment. de Ureth. Corp. Struct.

7 So far as we can form an opinion from a single case, it would appear, that in the human subject this passion is not, strictly speaking, instinctive; Edin. Trans. v. vii. p. 67.

The female organs are necessarily much more complicated and elaborate than the male. The part which the male bears in generation is quickly accomplished, whereas the female has to undergo a series of operations, which occupy a great length of time, and are connected with the most important changes in the animal œconomy. There are two orders of parts, upon each of which it will be necessary to offer a few observations; the organ on which the seminal fluid acts, so as to produce or evolve the fœtus, and the organ which is destined for the reception and nutrition of the fœtus after it has been so produced or evolved. The former of these constitutes what is termed the ovarium, and the latter the uterus.

In all animals that possess different sexes, the female is provided with a glandular body, containing a number of cells or vesicles, from which the substance proceeds, that is either converted into a fœtus, or serves to compose its basis or substratum. The way in which this is accomplished will more particularly fall under our consideration in the next section; at present it will be sufficient to remark, that the ovary is usually situated at some distance from the part into which the semen is introduced, and, as might be supposed from this circumstance, out of its immediate or direct influence. In the mammalia, the fœtus, when thus elaborated, is deposited in the uterus, to a portion of the interior surface of which it attaches itself, by a very curious and almost inexplicable process, where it remains fixed for a certain period, until its organs and functions shall have been sufficiently matured to enable it to resist external violence and to provide for its own support. From the mode in which animals of this description bring forth their young, they are named viviparous. Birds, on the contrary, and a large part of the cold-blooded quadrupeds,[8] compose the class of oviparous animals; here the fœtus is not transmitted into any cavity analogous to the uterus, but after being provided with the necessary protection from external injury, and a due supply of matter for its growth and nourishment, is immediately discharged from the body of the mother.

The nature of the female ovarium, and its connexion with the other parts of the uterine system,

[8] Spallanzani indeed informs us, that his observations led him to conclude, that what had been termed the ova of the cold-blooded quadrupeds are, many of them, not properly entitled to this appellation. The substance that is discharged from the female is merely the fœtus moulded into a round form, and is not enclosed in any shell or other extraneous body from which it is afterwards evolved. The objection appears, to a certain extent, to be well founded; but it may be remarked, that it applies rather to the anatomical structure of the part than to its physiological relations; See his Experiences sur la Gener. by Senebier, c. 1, et alibi. Harvey, on the contrary, contends that all animals proceed from ova; Exer. de Gen. 1: the difference of opinion evidently depends upon the different sense in which that term is used, the first being derived from its anatomical structure, the second from its physiological relations. Many eminent authors, as Fabricius and Harvey, speak of the uterus of oviparous animals, but this term can scarcely be admitted even as analogically correct. I may remark, that the sense in which Harvey employs the word ovum is so general as to reduce it to little more than a verbal analogy; see Exer. 62. p. 287; Exer. 63. p. 292, et alibi.

have been the subject of very minute anatomical examination. The name of ovarium is said to have been first applied to it by Steno, in consequence of the analogy which it was conceived to bear to the organ, from which the eggs of the oviparous animals are produced.[9] De Graaf appears, however, to have been the first writer who ascertained the actual structure of this organ, and who pointed out, with at least much more accuracy than those who had preceded him, the changes which the several parts undergo in the different periods of impregnation. He describes the ovarium as composed of a particular kind of cellular texture, in which are contained a number of vesicles, filled with an albuminous fluid, each of which is capable of producing a fœtus by the application of the male secretion, through the intervention of a process, which will be more particularly described hereafter.[1]

[9] Myolog. Spec. p. 145. It would appear, indeed, that anatomists before the time of Steno had conceived an analogy between the organs of viviparous and oviparous animals, but that he was the first who clearly expressed the opinion and applied the term to the former class; see Boer. Præl. § 699, 0. cum notis; Haller, El. Phys. xxviii. 2. 33.

[1] See his work, De Mulier. Organ. Gener. inserv., particularly Ch. 12, and tab. 1, 5, 6, 7, 8, 11, 13, 14. These vesicles have accordingly been termed Ova Graafiana, or Vesiculæ Graafianæ, by Haller, ubi supra, Blumenbach, Instit. Physiol. § 550. and other writers. Haighton enumerates the circumstances which De Graaf was supposed to have ascertained respecting the ovaria; but some of the positions are not correct, and others are doubtful; Phil. Trans. for 1797, p. 160, 1. Bartholine "De Ovar.

These glands are connected with the uterus on each of its sides by two ducts, which, from the name of the anatomist who first correctly described them, are termed the Fallopian tubes. These, in the unimpregnated or quiescent state of the uterine system, are narrow, tortuous, and almost impervious, particularly at their lower extremity. The other end is more capacious, and is so situated as to possess the power, on certain occasions, of embracing the ovarium, being furnished with a loose fringed membrane, which seems to be the part in which its specific action more immediately resides.[2]

The uterus, in its unimpregnated state, is a compact, dense, membranous body, provided with a copious supply of blood-vessels, which run through its substance in all directions, and is also furnished with a considerable number of lymphatics and nerves.[3] It is likewise possessed of great muscular

Mulieb." and Drelincour "De Fœm. Ovis." may be consulted in connexion with De Graaf; although the latter is to be noticed principally as a specimen of laborious research. Among the modern works on this subject I may select Sabatier, Anat. t. ii. p. 455..462; Boyer, Anat. t. iv. p. 584..7; Bichat. Anat. Descript. t. v. p. 294..6, and the long article "Ovarie," by Murat, in Dict. Scien. Med. We have good views of the ovaria in Smellie's Anat. Tables, No. 5; in Monro's Elem. v. ii. pl. 8. fig. 2, 3, and pl. 9. fig. 2, 3, 4; and in Caldani, pl. 134, 149, and 152.

2 Fallopius's account of his observations is contained in his Instit. Anat. Opera, p. 438. See Haller, El. Phys. xxviii. 2. 29..32.

3 For a view of the lymphatics of the uterus I may refer to Mascagni, "Vas. Lymph. Hist." p. 94. tab. 14, and to Cruik-

power, as is proved by the mechanical force which it exercises during parturition. Yet, notwithstanding this circumstance, it would appear that it is somewhat difficult to demonstrate the fibres, for we learn that some anatomists of eminence have even doubted of their existence.[4]

shank on the Absorbents, p. 156..8; for the nerves to Walter, "Tab. Nerv. Thor. et Abd." No. 1. fig. 1. 472, 3; and to Tiedemann, "Tab. Nerv. Uteri." Haller has given two good plates of the uterus and its appendages, Opera Min. t. ii. p. 32. and Icon. Anat. Fas. 1. tab. 3. fig. 1. We have good plans of the uterus in Bell's Anat. v. iv. p. 229, et seq., which very aptly illustrate its form in all its different states and its connexion with the contiguous parts. By way of comparison the plates of Swammerdam may be consulted in his "Miraculum Naturæ."

4 Haller, El. Phys. xxviii. 2. 9, admits that the uterus is obviously muscular in quadrupeds, but says that the human uterus is of a peculiar structure. He describes it as being a cellular spongy mass, in which it is not easy to detect any muscular fibres; yet he acknowledges their existence, § 11, and brings forward many proofs of their contractility, § 10. Blumenbach, however, informs us that he could never detect any muscular fibres in the uterus, and in conformity with his general practice, attributes its action to the vita propria, § 547. He assigns other offices for the vita propria of this organ, § 561, 598. This may also be regarded as the opinion of Bichat, Anat. Des. t. v. p. 287. But it would appear that, with some exceptions, the most distinguished modern anatomists are advocates for its muscularity; Dr. Elliotson enumerates the following names among the supporters of this doctrine: Malpighi, Morgagni, Mery, Littre, Astruc, Ruysch, Monro, Vieussens, and Haller. To these may be added, among the earlier anatomists, Vesling, Syntagma Anat. p. 53, 4, and among the moderns, the great name of Wm. Hunter, pl. 14, of his work on the gravid uterus. If any doubt remained upon the subject, we might consider it as removed by the accurate investigations

Besides the immediate and specific use of the generative organs, in the continuance and reproduction of the species, they appear to exercise a peculiar and specific influence over the system at large, affecting its general form and its powers, both mental and corporeal, causing the growth and development of particular parts, and giving to the individual, in a more remarkable degree, those characters which constitute the peculiarity of sex. The constitutional difference of the two sexes during infancy is not very considerable, but at the period of puberty, when the generative organs are developed and their functions established, the difference is very much increased, and continues during the remainder of life. It will not be necessary to enter into a minute detail of these differences; it will be sufficient to remark, that they exist in the anatomical structure of the body, in its chemical constitution, and in its powers, and that besides those organs which are immediately subservient to generation, certain parts, which are more peculiarly characteristic of the two sexes, then become prominent, as the beard of the male and the mamma of the female.[5]

of Mr. Bell; Med. Chir. Tr. v. iv. p. 335, et seq.; also Anat. v. iv. p. 279. Dr. Blundell observes, that the uterus of the rabbit exhibits a distinct muscular structure; Researches, p. 35.

5 See Haller, El. Phys. xxvii. 3. 15. and Blumenbach, Inst. Phys. sect. 35, for an accurate account of these differences. We have a volume by Roussel expressly on this subject, "Système de la Femme et de l'Homme," in which he has described with considerable minuteness the characteristic peculiarities of the two sexes, both physical and moral, with their effect upon the various functions

We have an opportunity of observing this influence of the generative organs over the constitution by noticing the effect which takes place, when, from any cause, their proper development is not permitted to take place. If, in consequence of any original malconformation, the testes remain defective, or when emasculation has been performed, the male never assumes the characters of the sex, and we have reason to conclude that the same is the result of the extirpation of the ovaria, although the changes in the female are less obvious, and we have not such frequent opportunities of observing the effect of the operation.[6]

and organs; it contains many good remarks, although somewhat sentimental and diffuse in its style. In Vesalius, Opera, t. ii. p. 682. tab. 75, 6; in Cowper, Anat. Corp. Hum. tab. 1, 2, 3, and in Bidloo's Anatomy, fig. 1, 2, 3, we have beautiful and characteristic engravings of the male and female form; Sœmmering has pourtrayed the peculiarities of the bones of the female in his "Tabula Sceleti Feminini."

[6] Haller, El. Phys. xxvii. 3. 3. Haighton found that by dividing the Fallopian tubes sexual feelings were destroyed, and the ovarium gradually wasted; Phil. Trans. 1797, p. 173, et seq. Pott gives us a short account of the case of a female, where both the ovaries were extirpated; he remarks that the person "has become thinner and more apparently muscular; her breasts, which were large, are gone; nor has she ever menstruated since the operation, which is now some years." Works, v. iii. p. 330. We have a case related in the Phil. Trans. for 1805, p. 225, et seq. of an adult female, in whom the ovaria were defective, and when there was a corresponding defect in the state of the constitution. To the same general principle we ought probably to refer the partial growth of a beard on females in the decline of life; Blumenbach, Instit. Physiol. § 660. It has been likewise observed that female birds, when they have ceased to lay eggs,

The influence of the generative organs upon the constitution is sometimes singularly illustrated in those individuals who have been styled hermaphrodites, and who have been popularly supposed to unite in themselves the functions of both sexes. We are now convinced of the impossibility of this occurrence, but in these cases, we find that, owing to the imperfection of the parts and the deficiency of their functions, the constitution is so much modified, that it is sometimes difficult to determine which sex predominates.[7]

have occasionally assumed the plumage, and to a certain extent, the other characters of the male; Hunter on the Anim. Œcon. p. 75, et seq.; Blumenbach, Elem. Phys. § 660. note; Home in Phil. Trans. for 1799, p. 174. We have a paper on this subject by Dr. Butter, in the Wernerian Memoirs, v. iii. p. 183, et seq. from which it appears that this change is by no means unfrequent in old female birds; he conceives it not to be, in any respect, a morbid change, "but the natural result of age and time." We have a still later account by M. St. Hilaire, which is translated, with observations by the editor, in Jameson's Journ. for Oct. 1826, p. 302, et seq. It may be remarked that emasculation has a less marked effect on the form of the inferior animals than of the human species. See appendix to this chapter for the observations of Mr. Yarrell on the change in the plumage of birds.

7 It is not easy to determine the exact degree in which the mixture of the two sexes has occurred in the human species, in consequence of the tendency to exaggeration which is so frequently met with in the older writers, but we may venture to assert, that a perfect uterus, with its appendages and perfect testes, have never been detected in the same individual. We have an elaborate memoir by Haller, entitled, "Num dentur Hermaphroditi Commentarius," Op. Min. t. ii. p. 9, et seq. in which he relates some observations of his own, and takes a view of the best authenticated cases that are on record, of irregularities of the

## § 2. *Remarks on the Functions of the two Sexes in the Process of Generation.*

The office of the male is simply to introduce into an appropriate part of the body of the female a portion

sexual organs. In most instances it is easy to point out to which sex the individual ought to be referred, but the author concludes that the evidence is irresistible in favour of the existence of certain cases, in which there was an actual mixture of the two sexes, but with an imperfect development of the organs. Sir Ev. Home comes to a similar conclusion in a paper which contains a number of facts and observations on the same subject; Phil. Trans. for 1799, p. 157, et seq. He remarks, "there is much reason to believe, that no instance of an hermaphrodite, in the strict sense of the word, has ever occurred in the more perfect quadrupeds, or in the human species;" p. 157. He arranges the supposed or reputed cases into four classes: "1st. Such malformations of the male, as led to the belief of the persons being hermaphrodites; 2d. Such malformations in the females as have led to the same conclusion; 3d. Such males as, from a deficiency in their organs, have not the character and general properties of the male, and may be called neuters. 4th. Those in which there is a real mixture of the organs of both sexes, although not sufficiently complete to constitute double organs; which, I believe, is the nearest approach towards an hermaphrodite that has been met with in the more perfect animals;" p. 159, 0. A remarkable case of this last description is related by Dr. Baillie; the external parts resembled those of a female, while there appears likewise to have been the rudiments of the male organs; the internal organs were not examined; Morb. Anat. p. 410, 1, also works by Wardrop, v. ii. p. 371. We have a well detailed case in Mem. Soc. Med. Emul. t. iii. p. 293. . 5, of a male, where the parts were imperfect, and the form of the body resembled that of the female. Mr. Mayo mentions two cases, one of an imperfect male, the other of an of the peculiar secretion which is furnished by the testis. That of the female is considerably more

imperfect female, where the general form of the body resembled that of the other sex; Physiol. p. 368.

An individual was exhibited in this metropolis, in the year 1818, who possessed a remarkable mixture of the characteristics of the two sexes. Without a more minute examination of the generative organs than it was possible to make during life, it was not easy to determine to which sex they should be referred. The countenance resembled that of the male, and there was a beard, although scanty. The form of the body and of the limbs was, however, decidedly female, and the mammæ were considerably developed. Upon the whole I am disposed to conjecture that the individual in question was a female, where the uterus and the ovaria were defective, and where there was an irregularity in the form of the external organs. There are some valuable observations on this subject in Blumenbach's Essay on Gener. sect. 3. p. 81; in Beck's Med. Jurisp. ch. 4. p. 42 .. 52, also in the art. "Generation," in Rees's Cyclopædia; and "Hermaphrodite," in Dict. Scien. Med. by Marc, t. xxi. p. 86, et seq. The belief in the existence of human hermaphrodites was formerly entertained by the most eminent anatomists; see Cheselden's Anat. p. 314. tab. 33. We have two very valuable papers by Dr. Duncan, Jun. in Edin. Med. Journ. v. i. p. 43, et seq. and p. 132, et seq., which, although not directly connected with this subject, contain some observations which tend to illustrate it.

In some of the mammalia we have a decided mixture of the two sexes, although the appropriate parts, and consequently their functions, are imperfect. This is particularly exemplified in the free-martin, of which we have an account by Hunter; Phil. Trans. for 1779, p. 179, et seq., and Observ. on the Animal Œcon. p. 55, et seq. Sir Ev. Home, in the paper referred to above, relates an instance of a similar conformation having occurred in a dog; p. 168, 9. Morand informs us that he examined an hermaphrodite carp, which had ova on one side and a melt on the other, and he states this to be not an uncommon occurrence in complicated; it consists, in the first instance, in providing a substance which, in connexion with the male secretion, is to constitute the fœtus, in furnishing a suitable situation in which the fœtus may be deposited, in affording it the due nourishment, until it is able to support itself by its own powers, and, lastly, after the fœtus is detached from the mother, she continues to supply it with a peculiar kind of food, especially adapted to the digestive organs of the young animal.[8] This long series of operations is, however, exercised only by the females of the class of the mammalia. In proportion as we recede from these, the office of the mother is less complicated and continuous. In birds, the process of utro-gestation is dispensed with, nor is there any organ in the body of the female adapted to the production of a peculiar kind of food for the nourishment of the newly-born animal; in place of the first, incubation is substituted, and the latter is compensated by the instinctive care of the mother, in selecting for her offspring the kind of food which is adapted to the state of its digestive organs. As we descend into the lower classes of oviparous quadrupeds and fishes, we find the functions of the female still farther diminished. In many

fish; Mem. Acad. pour 1737, p. 51, 2. We may presume, that as we descend to the lower orders, even of the vertebrated animals, we shall meet with a nearer approach to perfect hermaphrodism.

[8] Technically speaking, the function of the male is confined to the process of impregnation, that of the female comprehends the various operations of conception, gestation, parturition, and lactation.

tribes it consists merely in furnishing a substance which is to receive the male secretion, in some cases within, and in others out of the body, while the young animal is spontaneously evolved from its ovum, and is supported without any assistance from the mother.

Considerable difficulties, however, attend our explanation of every part of this process, and a great variety of hypotheses have been proposed, some of them supported by numerous and direct experiments, in order to remove these difficulties. That I may be able to throw some light upon the subject, I shall consider each part of the operation separately. And first, with respect to the immediate effect of the male secretion. It has been stated above,[9] that Fourcroy and Vauquelin detected a fibrous substance in the seminal fluid, with which, as being of a peculiar nature, it may be reasonably supposed, that its specific effects are in some way connected. Except, however, the mere existence of this fibrous substance, we are totally unacquainted with it; we know nothing of its properties, either chemical or physiological, and are, of course, unable to explain how it operates in the formation of the fœtus.

The presence of the spermatic animalcules is, however, a still more remarkable circumstance in the constitution of the seminal fluid. If we are to rely upon the experiments and observations of Spallanzani and of the Genevese physiologists, we are unavoidably led to the conclusion, that the secretion of

9 V. ii. p. 350.

the testis contains animalcules of a specific kind, that they are not derived from any extraneous source, but form one of its essential components; that they are present in the various species of males, but that they cannot be detected when, either from age or from disease, the animals are rendered sterile. Hence we can scarcely refuse our assent to the position, that these animalcules are, in some way or other, instrumental to the production of the fœtus, although we are altogether unable to assign the particular mode in which they produce their effect.[1]

When we consider the anatomical structure of the organs of the two sexes in the mammalia, where there is the greatest number of parts, and where the functions appear to be the most elaborate in their nature, a difficulty has arisen in conceiving how the secreted fluid can be brought into contact with the parts of the female where the rudiments of the fœtus appear to be lodged; yet the analogy of some of the lower animals seems to show that this contact is necessary. Many experiments have been instituted to discover the exact part of the female organs into which the semen is projected, or to which it may be afterwards conveyed by absorption, or by any other vital or physical process. It has been doubted whether the fluid is capable of entering the uterus, and various experiments and observations have been adduced, even by Harvey and

[1] See the general conclusions of MM. Prevost and Dumas in Ann. des Sciences Naturelles, t. i. p. 289, and in t. ii. p. 147..9.

some of the most eminent physiologists, to prove that this is never the case.[2] But, upon considering all the facts that have been brought forwards on both sides of the question, the opposite opinion appears to be the most probable, and to this Haller inclines, although fully aware of the objections that have been urged against it.[3] There is much more difficulty in

[2] De Gener. Exer. 39. p. 145; 67. p. 308; 68. p. 312; et de Concept. p. 405. The difficulty of accounting for the passage of the semen to the ovarium, which was supposed to be necessary for conception, gave rise to the hypothesis of the aura seminalis. Harvey's chapter "De Conceptione," appended to his work on Generation, affords a singular contrast to the correct and cautious spirit which so generally pervades his writings.

[3] See Boerhaave, Prælect. not. 6. ad § 673, t. vi. p. 74, 5. and El. Phys. xxix. 1. 11, where the subject is very minutely examined. The testimony of Leeuwenhoek is very direct in favour of the reception of the semen into the cavity of the uterus, a fact which he ascertained by actually detecting the spermatic animalcules in this organ; Arc. Nat. in Op. t. i. p. 155, 166, 169. The confirmation which we have lately had of his observations must lead us to conclude that on this point, in which he could not easily be mistaken, we may rely with confidence upon his statements. We learn also from the same authority that the animalcules have been detected in the cornua of the uterus and the Fallopian tubes of the rabbit and the dog; t. i. p. 170, 1; and t. iv. p. 208, 9. Respectable authorities are not wanting who assert that the seminal fluid has been found in the Fallopian tubes of the human subject, but this scarcely seems to have been the result of direct observation. Should it however appear that this was actually the case, we are unable to say by what power it could have been conveyed there. See the remarks of Dr. Elliotson, with the authorities to which he refers; Blum. Inst. sect. 39, note A, p. 325, 6. De Graaf, de Org. Mulieb. p. 243; Ruysch, Advers. Anat. Dec. 1. tab. 2. fig. 3; and Sauvages, Physiol.

supposing that it can pass beyond the organ; so that it will remain for us to inquire, how the presence of the semen in the uterus can act in the production of the fœtus. This will lead us to examine what part the female acts in the process, and in what way the uterus, or the different organs connected with it, are affected, or what change they each of them experience.

After the period of puberty, when the uterine system has acquired its full size, and the power of exercising its appropriate functions, certain causes, and especially the excitement of the seminal fluid, produce an unusual flow of blood to the ovaria, and the parts connected with them. The fimbriæ of the Fallopian tubes become turgid, and embrace the ovaria, where one of the vesicles, which appears to be more immediately affected, is protruded from its former position, and bursts, discharging a drop of an albuminous fluid, which is received by the tube and conveyed to the uterus; this constitutes the ovum, and is to be regarded as the first rudiment of the future fœtus. The vesicle from which the ovum has escaped, experiences a peculiar change in its texture and appearance, and is converted into what is named the corpus luteum.

The operation by which the uterus receives and supports the ovum, which is transmitted to it, is no less wonderful than that by which it is conveyed there. It is immediately attached to some part of

p. 222. all maintain the opinion that the seminal fluid is deposited in the uterus, as the result of their own observation.

the internal surface of the uterus, a communication is established between them, the exact nature of which is still, in some measure, unknown; while we have an elaborate organization of membranes of various kinds, for the purpose of supporting the fœtus, protecting it from injury, and conveying to it all the substances that are necessary for its nutrition and existence. The uterus, in the mean time, increases in size so as to contain the fœtus, and its membranes; its vessels have their diameters proportionably enlarged, and it acquires a great addition of fluids, by which it is enabled to perform all its extraordinary functions.[4]

In considering the physiological relations of the different parts of this process, the following points will particularly require our attention. Upon what organ does the seminal fluid first produce its appropriate effect? If upon the ovarium, in what does that action consist, and what is the nature of the changes which the ovarium experiences? In what stages of the process of conception is the ovum conveyed into the uterus, and by what power is the conveyance effected? By what means does the fœtus attach itself to the uterus, and how is it afterwards nourished and supported there? What change takes place in the action of the uterus, by which its new

[4] For a view of the changes which the uterus and its appendages undergo at this period, the student may examine Albinus, Tab. Uteri Mulier. grav.; Bidloo, Anatomia Hum. Corp. pars 4. tab. 53..63. "De Ingravidato Utero, Fœtu, ejusque Annexis;" and especially the splendid work of Wm. Hunter.

membranes are formed and its bulk increased? And, lastly, in what manner does it immediately contribute to the support of the fœtus? When we consider how many interesting physiological discussions are involved in these queries, and to what an almost infinite number of speculations and controversies they have given rise, it will be sufficiently evident, that I can only attempt to take a brief survey of some of the most interesting topics, and those which may more directly contribute to throw light upon the other operations of the animal œconomy.[5]

[5] Attempts have been made by various physiologists to elucidate the hypothesis of generation by watching the growth and evolution of the chick in ovo; of these observations the most important and interesting are those of Fabricius, Harvey, Malpighi, and Haller. Fabricius's treatise "De Formatione Ovi" contains much valuable information, and is illustrated by expressive engravings; the same commendation may be justly ascribed to his treatise, "De Formato Fœtu," in which the growth and successive formation of the parts of the fœtus are described and figured. Harvey's "Exercitationes de Generatione," although inferior to his immortal work on the circulation, is not unworthy of its author; if his reasoning appears less conclusive than on the former occasion, we must, in part at least, ascribe it to the intricacy and mysterious nature of the subject. Of Haller's essays "De Formatione Cordis" and "De Formatione Pulli," it is sufficient to remark that they have always been regarded as among the most valuable of his productions. I have already had occasion to refer to the admirable figures of Mr. Bauer; Phil. Trans. for 1822. We have many useful observations on this subject by Dutrochet in an essay "Sur les Enveloppes du Fœtus," published in the "Mem. Soc. Med. d'Emul." t. viii. p. 1..64, particularly the first section, containing an account of the eggs of birds, in which the author animadverts upon the anatomical arguments which were

The first of the above inquiries, the part of the organ upon which the seminal fluid produces its first effect, involves the point that has been already referred to, the situation in which it is originally deposited. We have seen that there is sufficient ground for believing that it enters the uterus, and that it is perhaps conveyed even into the Fallopian tubes. But upon either supposition we can offer no conjecture as to the mode in which it operates, farther than to say, that it produces a specific excitement, the nature of which we are unable to explain; this is directly applied either to the uterus or to the Fallopian tubes, and is thence propagated to the ovarium. An increased flow of blood seems to be the immediate result of the excitement, to which succeeds the enlargement of one of the vesicles, the escape of the ovum, the reception of it by the fimbriated extremity of the tube, and its transmission to the uterus.[6] It appears to be analogous to the ge-

brought forwards by Haller in favour of the doctrine of pre-existing germs. Connected with this topic there is an essay of Cuvier's, "Sur les Oeufs des Quad." in Mem. du Mus. t. iii. p. 98, et seq. and pl. 2.

[6] Many of the older physiologists supposed that, during sexual excitement, the female organs furnished a peculiar secretion, which unites with that of the male; this is well known to have been the doctrine of Hippocrates, "De Genitura," Op. t. i. p. 231..5. Fallopius, in his Observ. Anat. says, "Omnes anatomici uno ore asserunt in testibus fœminarum semen fieri, et quod semine referti reperiuntur," but he adds, "quod ego nunquam videre potui——;" Opera, p. 421. De Graaf's 13th chapter is entitled, "De Semine Muliebri;" p. 194. See Haller, El. Phys.

neral operations of the animal œconomy, that a specific stimulus, acting upon any organ, should call forth the specific functions of that organ, and that there should be a regular succession of its actions, depending upon the constitution of the part, until the ultimate effect is accomplished; but beyond this we can offer no explanation of the process.

But a question has here been started as to the fact; is the seminal fluid the sole and specific cause of the excitement? or can the turgescence of the ovarium, the escape of the ovum, and the consequent formation of the corpus luteum, take place without the co-operation of the male? If so, what is the state of the ovum which is thus evolved, and where is it deposited? While it continues in its unimpregnated state does it remain lodged in the Fallopian tube, or is it conveyed to the uterus?

We cannot doubt that the introduction of the seminal fluid, in the ordinary method of congress, is the sole cause of what is termed conception, the

xxix. 1. 13; but this opinion is now generally discarded. It is, however, adopted by Blumenbach, § 552; and from his expression it would seem that he supposes it to be produced by the uterus, § 561. Blumenbach refers to Harvey, as maintaining the opinion of the female semen, but from the following passage in Exer. 35. I should conceive erroneously; "—— nec fœmina semen profundit, unde ovum oriatur." "—— neque in coitu semen ab utroque proveniat——." The existence of a peculiar female secretion forms the basis of Buffon's hypothesis of generation, but this is regarded as altogether without foundation; Nat. Hist. v. ii. p. 396, et seq. ch. 4th.

production of an impregnated ovum,[7] and it was formerly supposed that the same stimulus was equally the cause of the evolution of the vesicle, and the consequent formation of the corpus luteum.[8] This

[7] Haighton performed an elaborate set of experiments in order to ascertain how far the division of the Fallopian tubes prevented impregnation, and the result was that, after this operation, a fœtus was never produced. Corporea lutea were, however, formed in this case, and in consequence of his considering the production of these bodies to be a test of impregnation, he drew a conclusion which is directly the reverse of what the experiments might seem to warrant. He conceives that the semen penetrates no farther than the uterus, and acts upon the ovaria by sympathy. One important point he has established, that the effect which is propagated to the ovaria, whatever be its nature, is not accomplished until nearly 50 hours after coition. His experiments were performed on rabbits, and bear every mark of accuracy and fidelity; Phil. Trans. for 1797, p. 159, et seq. The experiments of Cruikshank, which were very numerous, and appear to have been made with the requisite degree of skill and correctness, lead to the conclusion, that the rudiment of the young animal is perfected in the ovarium; Phil. Trans. for 1797, p. 197; but it must be acknowledged that we are not sufficiently acquainted with the precise nature of the ovum which he observed to draw any positive inference.

[8] Haller decidedly maintains that the formation of a corpus luteum is a proof of the production of a fœtus; El. Phys. xxix. 1. 15, 16. In the catalogue which he gives of his discoveries, appended to his great work, he inserts the following: "Corpus luteum oritur ex conceptione, neque prius paratum adest." Auctarium ad El. Phys. p. 7; see also Op. Min. t. ii. p. 457. Haighton lays down the position, that wherever corporea lutea are found, "they furnish incontestible proof" of previous impregnation; Phil. Trans. for 1797, p. 164, which compare with p. 166;

opinion, has, however, been lately called in question and as it is one which involves some very important considerations, both of a legal and of a moral nature, it becomes a point of the highest interest to examine the grounds on which it rests. Blumenbach appears to have been the first who decidedly maintained that, under certain circumstances, a corpus luteum may be produced without the co-operation of the male,[9] and some facts in confirmation of the same opinion have been more lately adduced by Sir E. Home.[1] But if we conceive that a corpus

I have already alluded to the error into which he fell on this subject.

[9] Inst. Physiol. § 562, note, p. 312 . . 3; also Comment. Soc. Roy. Scienc. Gotting. t. ix. p. 109 . . 114.

[1] Sir E. Home defines a corpus luteum to be "a solid compact glandular substance in which the fœtus is formed," not as had been previously supposed, a body produced by the ovum, or a consequence of its existence, Phil. Trans. for 1817, p. 256, et seq. The figures which Mr. Bauer has given us of the ovarium in its different states are among the most beautiful of his numerous performances; fig. 8 . . 11; also Phil. Trans. for 1819, pl. 3 . . 9. In the paper to which these plates refer, Sir E. Home brings forward fresh evidence to prove that the corpus luteum is not necessarily preceded by impregnation. See his Lect. on Comp. Anat.; also Beck's Med. Juris. p. 104.

The late valuable experiments of Dr. Blundell confirm the most important part of Sir E. Home's doctrine, that corpora lutea are not the necessary result of impregnation; Researches, p. 49, 56, et alibi. Dr. Blundell, however, appears to conceive that either intercourse with the male, or at least a very high degree of sexular excitement, is necessary for their production, and the same opinion is adopted by Cuvier; Leç. d'Anat. Comp. No. 29, t. v. p. 57. We have drawings of corporea lutea in Wm.

luteum is, in any instance, produced without the co-operation of the male, it must follow, that in such case the specific action of the seminal fluid is not exercised upon the ovarium, and hence it would appear more probable that the evolution of the vesicle and the production of the corpus luteum are, in all cases, essentially dependant upon the actions of the female, and that the office of the male does not commence until after this previous step of the process. We may then conjecture that the stimulating influence of the male secretion only acts in increasing the excitement which already exists in the vesicles of the ovarium, by which the ovum is detached from it, and is either simply received by the tube, or is transmitted to the uterus, in one or other of which situations it meets with the seminal fluid, and where the specific action commences, which gives rise to the impregnation of the ovum and the production of the fœtus.

Perhaps the most natural supposition may be, that the ovum is transmitted to the uterus in the unimpregnated state;[2] but there are certain facts

Hunter's great work, pl. 31. fig. 3.; pl. 29. fig. 3.; and pl. 15. fig. 5. Murat, in an elaborate article, "Ovarie," in Dict. Scien. Med. supports the doctrine of Haller, p. 5, 6.

[2] Haller discusses this hypothesis in El. Phys. xxix. 1. 18 . . 24, and decides against it. The experiments of Cruikshank, to which I have already referred, tend to the same opinion, although they cannot be regarded as actually demonstrating it. See also Fleming's Zoology, v. i. p. 398. Sir E. Home conceives that, in consequence of the general excitement of the female organs, the tubes are so far expanded as to allow the semen to pass along

which seem almost incompatible with this idea, especially the cases which not unfrequently occur of perfect fœtuses having been found in the tubes, or where they have escaped from them into the cavity of the abdomen. Hence it is demonstrated that the ovum is occasionally impregnated in the tubes; and we can scarcely resist the conclusion that it must always be the case. What upon the whole appears most probable is, that a general excitement of the uterine system,[3] not necessarily connected with the co-operation of the male, produces the evolution of one of the vesicles; that this is discharged into the

them to the ovum, which is probably detained at their farther end for some time, in order to admit of the impregnation; Phil. Trans. for 1817, p. 257. A case is detailed by Dr. Granville, in the Phil. Trans. for 1820, p. 101, et seq. of a fœtus, which appears to have been lodged in the body of the ovarium itself; and it is considered by the author as a proof that conception always takes place in this organ. In this instance we may presume that it actually did so; but I do not conceive that it necessarily affects the view of the subject which I have given above. We may conjecture that the ovum had been developed, but by some accident, instead of being discharged into the tube, remained attached to the ovarium, and that, contrary to what usually happens, it was impregnated in this situation. We have a case of ovarian conception very fully detailed by Bœhmer, with illustrative engravings; Observ. Anat. Rar. Fascic.

[3] The excited state of the female organs is described by Cruikshank; Phil. Trans. for 1797, p. 197, et seq. See also Boerhaave, Præl. § 673, cum notis. Haighton's experiments, in his 1st section, p. 162..6, as well as those of Cruikshank, seem to prove that the excitement of coition tends to accelerate the formation of corpora lutea, although we may conceive it not to be essential.

Fallopian tube, where, upon meeting with a portion of the seminal fluid, it becomes fertilized and impregnated, and constitutes the first stage of the existence of the fœtus.

The means by which the ovum[4] is propelled along the tube, whether before or after impregnation, like every other step in the process, is involved in considerable obscurity. We know of no power, except that of muscular contractility, which could cause such an action to take place, and yet, notwithstanding the size of the part, no muscular fibres have been detected in it; we can, however, scarcely doubt of their existence. As the body which is transmitted is small, and as it is only on certain occasions that their contractility is called into action, we may conjecture that the fibres are extremely minute, but that by the momentary application of a very powerful stimulus, they are enabled to perform the office which is assigned to them.

We conceive that the ovum has now arrived at the uterus, and is become impregnated; the next step in the process is its attachment to a part of the inner surface of this organ. This is succeeded by a gradual, but very considerable increase of its bulk, and the formation of new parts, by which it may maintain its connexion with the fœtus, composing the maternal part of the placenta. The change which takes place in the ovum consists in the pro-

4 The term ovum is here employed rather in compliance with general custom than from any idea of its technical correctness.

duction of a membranous envelop, within which the young animal is contained, surrounded by a quantity of an albuminous fluid, a vascular connexion being established between the body of the animal and a part of this envelop, which constitutes what is termed the umbilical cord and the fœtal part of the placenta.[5] The maternal and the fœtal placenta may be considered as composing temporary appendages to the circulating system of the mother and the fœtus respectively, and preserving the necessary connexion between them, although it appears that we have no evidence of their having any vascular communication with each other. The various complicated operations which compose these successive steps of the process, although well established as matters of fact, appear to be altogether inexplicable. We do not know why the presence of the impregnated ovum should cause it to increase in bulk, and produce the placenta; and we are equally ignorant of the powers by which the fœtus is gradually developed.

With respect to the actual change which takes place in the uterus, it appears to consist, in the first instance, in an increased action of the blood vessels, by which a greater quantity of fluid is conveyed to the part. The mass of the solids appears to be augmented at the same time with the fluid; the organ is not only distended to many times its original

5 We have a concise but perspicuous account of these successive operations in a paper by Mr. Burns, Edin. Med. Journ. v. ii. p. 1, et seq.

bulk, but its parietes are much thickened, proving that its increased size is not owing to mere distention.[6] This process is continued until the period of pregnancy proper to the individual is completed, when the muscular power of the organ, which had remained dormant, is now excited into action, and finally succeeds in expelling its contents. But we are not able to explain this part of the operation more satisfactorily than those which preceded it. We do not know why the fœtus, after it had continued to increase for a certain length of time, should cease to grow; or why the uterus, after it has for a number of weeks or months gradually yielded to the increased bulk of the fœtus, now takes upon itself a new kind of action; and why its contractility, which before lay entirely dormant, should now be so suddenly and powerfully excited. That this does not depend upon any mechanical cause acting on the uterus, is proved by those cases of extra-uterine fœtuses, where the uterus undergoes the same change, and has the same disposition induced at the

[6] Haller, El. Phys. xxviii. 2. 9; Hunter on the gravid uterus, pl. 1 .. 5, show its great bulk at the latter periods of pregnancy, and the state of its parietes. Pl. 10, fig. 1, affords a good view of its vessels at the part where the placenta is attached to it; pl. 11, shows the great size of the veins; pl. 15, of its arteries; and pl. 16, 17, and 18, of its blood vessels generally. See also Bell's Anat. v. iv. p. 237; Boyer, Anat. t. iv. p. 594, 5; Bichat, Anat. Des. t. v. p. 346 .. 356. We meet with some very good observations on this subject in Malpighi's dissertation De Utero et Viviparorum Ovis; Manget, Bibl. Anat. t. i. p. 683, et seq.

end of the usual period, as if the fœtus had been contained within its cavity.[7]

The last of the questions which were proposed for consideration was the mode in which the uterus contributes to the support of the fœtus.[8] The support that will be required in this case must be of two kinds, or directed to two objects; the first, the means by which the body has its contractility maintained; the other, those by which it acquires its supply of what is more strictly termed nourishment, equivalent respectively to the two functions of respiration and digestion. The first of these points has already been considered in the chapter on respiration, where I remarked that, although the subject is not free from difficulties, yet that we have reason to believe, that the placenta serves the purpose of lungs for the fœtus, and that it performs this office by having its blood brought into close proximity with the arterial

7 Blumenbach, Instit. Phys. § 598, p. 341; also Comment. Soc. Reg. Scien. Gottin. t. viii. p. 49. 51. We have a valuable paper by Breschet on extra-uterine fœtuses in Med. Chir. Trans. v. xiii. p. 33, et seq.; he remarks, that in these cases the uterus enlarges, and the desiduum is formed, as in the natural process. Dr. Elliotson makes the same observation, ibid. p. 51, et seq.; his paper contains a valuable list of references.

8 We have a very elaborate account given us by Walter Needham of all that was known upon this subject, and of the opinions that were entertained, up to the date of his publication, in 1667, in his essay De Formato Fœtu; this treatise displays considerable acuteness, and contains many remarks which were offered in a conjectural form, but have been confirmed by subsequent discoveries.

blood of the mother, in the same way that the venous blood of the pulmonary artery receives its appropriate change by means of the air which is contained in the vesicles of the lungs. It is admitted that the change which is thus effected is inconsiderable, but the wants of the fœtus in this respect are few, and it may be presumed that the supply is equal to the demand.[9]

We have no direct evidence of the mode in which the fœtus procures its nourishment. We can scarcely doubt that it must be accomplished through the intervention of the absorbents; but whether it be by means of the cutaneous vessels taking up a part of the fluid which is in contact with their extremities, or whether there be some provision for the same purpose, connected with the placenta or any of its appendages, it is perhaps not easy to determine. Some physiologists of the last century supposed that the fœtus, during its immersion in the liquor amnii, received a portion of it into the mouth, and that

9 V. ii. p. 198..3. When we consider the mode in which the fœtus is connected with the mother, it will appear that the opinion formerly entertained respecting the power which the imagination or feelings of the parent has over the physical structure of the offspring is, at least, highly improbable, and very difficult to be explained, if not absolutely impossible. Little can be added to the judicious observations of Haller on this subject; El. Phys. xxix. 2. 21..6. This apparently exploded doctrine has, however, lately received the sanction of Sir E. Home; Phil. Trans. for 1825, p. 75..8. We may feel less surprise that such a doctrine should have formed a part of the creed of Lavater; Essays, by Holcroft, v. iii. p. 156.

this passed into the stomach, and was assimilated there by the ordinary process of digestion; but there seems to be sufficient ground for rejecting this supposition.[1]

A function of the uterus, which is obviously connected with the process of re-production, is the periodical discharges from its arteries to which it is subject. It commences at the period of puberty, continues as long as the power of bearing children remains, and is suspended during pregnancy and lactation; hence we conclude that it serves some useful purpose, either in the production or the support of the fœtus; but I conceive that no plausible explanation has yet been given of the mode in which

[1] This doctrine was supported by Harvey, De Gen. Ex. 58, p. 267. A doctrine which is at least as old as Aristotle, and which has been generally embraced, is, that in the chick its solids are, in the first instance, formed from the albumen, and that it is afterwards nourished by the yelk; Harvey, p. 77. I have already had occasion to refer to an essay by Monro 1us, the object of which is to prove that the fœtus is nourished by means of the placenta; taking into account the state of our information at the time when it was written, it must be regarded as a very learned and judicious performance; Ed. Med. Ess. v. ii. p. 121, et seq. Mr. Bell supposes that the placenta is an equivalent for both the lungs and the stomach; Anat. v. iv. p. 269..275. Wrisberg's Descrip. Anat. Embr. contains an account of five fœtuses that he had an opportunity of examining during the first months of their existence. We have a minute description by Senff of the successive stages of the growth of the bones of the fœtus during its early stages, accompanied by a series of engravings, and a copious list of references. Sœmmering's Icones Embryorum Hum. exhibit views of the fœtus from its earliest stages.

it operates. It appears now to be fully ascertained, that no female, except the human, is subject to this evacuation; but we are not able to assign any reason, either anatomical or physiological, for this peculiarity.[2] Both the period of its commencement and of its duration differs considerably among different nations, depending principally, as it appears, upon the temperature of the climate. In this country it usually makes its appearance about the age of fifteen, and ceases about forty-five or somewhat later.[3]

[2] See Blumenbach, Inst. Phys. note in p. 307.

[3] Haller, El. Phys. xxviii. 3. gives a full account of the phenomena that attend the menstrual discharge, and the various opinions that have been entertained respecting its causes. He attempts to explain it by a reference to Wintringham's experiments on the comparative density and extensibility of the arteries and veins; upon this principle he conceives that we can account for the recurrence of this discharge at a certain period of life only, and from no other female except the human. The reasoning of Haller is ingenious, but I think it may be doubted whether the facts will warrant the conclusion, and perhaps the facts themselves are questionable. As Blumenbach has not referred to these experiments nor to Haller's speculations, we may presume that he did not conceive them adequate to explain the phenomena; he candidly confesses his inability to account for them; sect. 38, § 558. See the remarks of Mr. Bell; Anat. v. iv. p. 234, et seq. The occurrence of the secretion of milk subsequently to parturition is one of the most obvious and remarkable examples of the adaptation of the powers and functions of the animal to the situations in which it may be occasionally placed. Anatomists have pointed out a curious vascular connexion between the uterus and the mamma; but it may be doubted whether this can assist us in explaining the dependence of these organs upon each other; Eustachius, Tab. Anat. pl. 27, fig. 12; Haller,

A very curious question connected with the function of generation regards the circumstances which determine the future sex of the fœtus; but as this is a point on which I conceive that we are completely ignorant, I shall not think it necessary to give an account of the various conjectures that have been proposed respecting it.[5] It is a remarkable

Icon. Anat. Fasc. 6. tab. 2. No. 30. Blumenbach seems to attach more importance to this structure; Inst. Phys. § 610. I may remark that the nipple affords an instance of what has been termed the erectile texture, similar to that which exists in the corpora spongiosa of the penis; it is, in the same manner, excited both by physical and mental causes; and these causes have likewise a specific, and, as it would appear, an inexplicable effect in promoting the secretion of the milk. The occasional secretion of milk by the male, a circumstance which, however singular, it seems scarcely possible not to credit, proves that the action of the uterus is not essential to that of the mamma; See Blumenbach; Instit. Physiol. § 621. p. 349. In that mixture of the sexes, of which I have given some account above, p. 24, we find that animals, in which the male character predominates, occasionally yield milk; this appears to have occurred even in a bull which was capable of impregnating the female; Phil. Trans. for 1799, p. 171..3. The milky or curdy substance which is secreted by the crop of the male pigeon, during the incubation of the female affords a singular instance of the departure from the ordinary analogy of nature; see Hunter on the Anim. Œcon. p. 235, et seq.

5 The doctrine of Hippocrates on this subject was, that the future sex is determined principally by the prevalence of the male or female semen, either as to the quantity of it which enters into the composition of the fœtus, or what he terms its strength; "De Gener." Opera, t. i. p. 233; and this opinion appears to have been generally embraced by the ancients and the earlier of the moderns. Another opinion which was current among the

fact that, although there is no uniform proportion between the number of males and females produced by the same parents, yet that the total number of each sex brought into the world, taking the average of any large community, is nearly the same; or, more exactly, that we have, in all cases, a small excess of

ancients, and which indeed forms one of the aphorisms of Hippocrates, is, that the different sexes occupy different sides of the uterus; Aphor. sect. 5. No. 48. Opera, t. ii. p. 1255. This notion, fanciful as it may appear, has been adopted by some modern writers, but it is completely overthrown by a case that occurred to Dr. Granville of a female, who had borne children of both sexes, and in whom the appendages belonging to the left side of the uterus were entirely wanting; Phil. Trans. for 1808, p. 308, et seq. pl. 17. Sir E. Home remarks that, in the earliest stages of the fœtus, the parts which determine the future sex are scarcely distinguishable, being so formed as to be easily convertible into each other. In pursuance of this opinion, he supposes, that "the ovum, previous to impregnation, has no distinction of sex, but that it is so formed as to be equally fitted to become a male or a female fœtus; and that it is the process of impregnation which marks the distinction——." Phil. Trans. for 1799, p. 175. I may remark upon this speculation, that it almost necessarily involves the hypothesis of pre-existing germs; or that the fœtus exists ready formed in the female, independent of the co-operation of the male. Mr. Knight's ingenious researches into vegetable physiology, and especially his experiments on the production of hybrid fruits, lead him to conclude that the influence of the female over the future offspring is more considerable than that of the male. With respect to the sex, he is disposed to think that this likewise depends more upon the female, because it is observed, in the breeding of animals, that certain females have a tendency to produce one sex in preference to the other, which does not appear to be the case with the male; Phil. Trans. for 1809, p. 392, et seq. See also Prichard's Researches, v. ii. p. 552, 3.

males. The data that we possess, while they prove that this excess exists in all countries, seem, however, to show that the amount of it differs in different countries. From a very extensive examination made by Hufeland, the numbers in Germany are as 21 to 20.[6] The census that was taken in this country in 1821 shows the numbers to be nearly 21 and 20.066.[7] But to whatever cause we may ascribe the relative proportion, it would appear that the greater number of males which are born is compensated by their greater mortality, whether produced by natural or accidental causes, for we find

[6] Edin. Phil. Journ. v. iii. p. 296..9.

[7] The following are the number of the two sexes born in England and Wales during the interval between the two last censuses; Popul. Abs. p. 154.

| | Males. | Females. |
|---|---|---|
| 1811 | 155,671 | 149,186 |
| 1812 | 153,949 | 148,005 |
| 1813 | 160,685 | 153,747 |
| 1814 | 163,282 | 155,524 |
| 1815 | 176,233 | 168,698 |
| 1816 | 168,801 | 161,398 |
| 1817 | 169,337 | 162,246 |
| 1818 | 169,181 | 162,203 |
| 1819 | 171,107 | 162,154 |
| 1820 | 176,311 | 167,349 |
| | 1,664,557 | 1,590,510 |

Dr. Cross, in his account of the medical schools of Paris, states, p. 191, that in the Dublin Lying-in Hospital, during a period of fifty-seven years, in which time 58,000 women had been delivered, the proportion of males to females born was about as ten to nine; during the ten years preceding that in which he writes, the numbers were 13,665 and 12,583; this is nearly in the proportion of 21 to 19.33. From some curious observations, made in different parts of Europe, it appears, that the surplus of males is greater in among adults, that the number of females rather exceeds the males.[8]

## § 3. *Account of the Hypotheses of Generation.*

The preceding observations on the structure and action of the organs of generation, although comprehending a few only of the topics which have been investigated by the modern anatomists, will, I conceive, be sufficient to enable me to enter upon the consideration of the next subject into which I proposed to inquire—the nature of the generative process.

The inquiry, considered in the abstract, may be reduced to the following form, in what manner, or by what means, can an organized body produce another organized body similar to itself in its physical properties and its vital functions? After making ourselves acquainted with the phenomena which attend the process, with the changes which take place in the organs of the parent, and with the first appearances which we are able to detect of the independent existence of the fœtus, we may inquire, whether we are able to refer this succession of changes to the powers or functions which we have seen operating in the other parts of the animal system; whether, in short, the contractile or sensitive

legitimate than in illegitimate births; assuming the number of 50,000 females, we have 52,896 legitimate males, and only 51,249 illegitimates. From some experiments lately made in France on sheep, it would appear that sex depends, in some measure, on the comparative vigour of the parents.

[8] Haller, El. Phys. xxviii. 1. 1; Jameson's Journ. v. xiii. p. 200.

powers, which belong respectively to the muscular fibre or to the nerve, can so far modify or direct the ordinary physical powers of matter as to explain the phenomena. Few persons, perhaps, will be bold enough to assert, that we have any direct facts which can prove this to be the case; yet, on the other hand, I think it would argue at least an equal degree of confidence to conclude, that the generative process depends upon the operation of a different system of laws from those which belong to the other parts of the animal frame. Notwithstanding all the experiments and observations that have been made upon this subject, the parts which are essential to the operation are so minute and so much beyond our most elaborate researches, that the first step of the process seems still to have eluded our observation, so that we are even yet unable to do more than to make our election between one or other of the hypotheses that have been formed, all of which proceed upon the assumption of certain data, which it is extremely difficult either to confirm or to refute.[9]

A circumstance which encreases the difficulty of this investigation is, that we are unable to derive much assistance from analogy. In considering the generative function, as it is carried on by the different classes of animals, there would appear to be three, if not four modes, in which the young animal is formed, and which may be regarded as essentially different from each other. The first is that which

[9] See the judicious observations with which Haller commences his section on this subject; El. Phys. xxix. 2. 1.

occurs in the higher orders of animals, where the sexual congress of the two individuals is essential to the production of the fœtus. The second is where the co-operation of two sexes is necessary, but when both exist in the same individual. The third comprehends those animals, where nothing resembling the sexual organs, or indeed any other organs specifically destined for generation, can be detected, but where we merely observe the fœtus to be detached from the body of the parent; while, in other cases, the body of the parent itself is divided into two or more portions, each portion, after the separation, acquiring those parts which are necessary for its perfect existence. It is, however, to the first of these modes that our attention must be exclusively directed on the present occasion; and, for the reasons already assigned, I shall proceed to state the leading hypotheses of generation that have been proposed, and shall then consider which of them is the most consonant to the facts that have been ascertained upon the subject.[1]

The earliest hypothesis of generation of which we have any distinct account, and one which has also received the support of some of the most eminent of the moderns, ascribes the original formation of the fœtus to the combination of particles of matter

[1] Drelincourt, who lived in the latter part of the seventeenth century, collected 260 hypotheses of generation; Blumenbach on Gen. p. 4. We have an interesting, although diffuse, account of the different hypotheses of generation in Buffon's 5th chapter of his Nat. Hist. v. ii. p. 410, et seq.

derived from each of the parents.[2] The second hypothesis in the order of time is that of Leeuwenhoek, who supposed that the seminal animalcules in the male secretion are to be regarded as the proper rudiments of the fœtus, and that the office of the female is to afford them a suitable receptacle, where they may be supported and nourished, until they are able to exist by the exercise of their own functions. The third hypothesis, that of pre-existing germs, proceeds upon a precisely opposite view of the subject, that the fœtus is properly the production of the female, that it exists, previous to the sexual congress, with all its organs, in some part of the uterine system, that it receives no proper addition from the male, but that the seminal fluid acts merely by exciting the powers of the fœtus or endowing it with vitality. A fourth hypothesis is that of Blumenbach, who conceives that the process of generation is effected by a peculiar principle or power, which he styles the nisus formativus, with which he believes that the living body is provided for the express and exclusive purpose of re-production. These are the only hypotheses which can have any reference to the human species; but there is a fifth, which has been applied to some of the lowest tribes of animals, and which has been the subject of much discussion, as well as of numerous experiments. This is the doctrine of spontaneous, or, as it has been termed, equivocal generation, where a living

[2] See Harvey de Gen. Præf. sub init.; also Haller, El. Phys. xxix. 1. 13. and 2. 2.

organized being is produced, without the co-operation or previous existence of any similarly organized parent.

The first hypothesis, which has obtained the name of epigenesis,[3] is the one which naturally presents itself to the mind, as the obvious method of explaining the necessity for the co-operation of the two sexes, the resemblance in external form, and even in mind and character, which the offspring frequently bears to the male parent, and still more the phenomena attending the production of hybrid animals, which it would seem almost impossible to explain, except upon the supposition that the fœtus is equally indebted to both its parents for the materials of which it is composed. It is upon these general considerations that the truth of this opinion must rest,

[3] The term Epigenesis strictly means no more than the formation of a body by the successive additions of new matter, "partem post partem," according to the expression of Harvey, Ex. 45; although he adopts the hypothesis of epigenesis, he does not suppose that the male furnishes any actual matter to the fœtus, but that it imparts to the ovum the power of assimilating and organizing matter; Ex. 10, 25, 32, 39, et alibi. His explanation of the mode in which the male is supposed to operate, can only be regarded as a metaphorical illustration; Ex. 33, 50. By many of the modern physiologists the term has been employed in a more extended sense, so as to refer both to the source from which the new matter is procured, and to the relation which exists between its origin and the substance which is formed by it. Haller and Spallanzani seem disposed to restrict the word to the speculations of Needham and Buffon. I have not been able to ascertain who it was that first employed the term, or rather who used it in this sense.

for the facts that have been adduced to prove the existence of an appropriate secretion from the female, analogous to that of the male, would appear to be without sufficient foundation. The principal objections to this hypothesis, independent of the want of any direct proof of a female seminal fluid, are of two descriptions, those which depend upon the supposed impossibility of unorganized matter forming an organized being, and those which are derived from the observations and experiments of Haller and Spallanzani, which they brought forwards in support of their theory of pre-existing germs.

The second hypothesis is founded entirely upon the observations of Leeuwenhoek and others of a similar kind. The observations, as we have seen above, would appear to be correct. The seminal fluid, in all cases where we are able to collect and examine it, is found to contain a number of organized living beings, which, although they differ somewhat in the different kinds of animals, bear a close resemblance to each other, and are unlike any other bodies with which we are acquainted. The occurrence of these peculiar bodies in a fluid of such singular properties, naturally led to the suspicion of some connexion between the two circumstances, and it is not wonderful that the original discoverers should have supposed that these animalcules performed some necessary and specific office in the function of generation. Indeed, we are almost compelled to admit that, in some way or other, this must be the case; and yet the extreme improbability that they should be the rudiments of beings which are so totally dissimilar to them, has been supposed, by all the modern physiologists, to be of itself a sufficient refutation of Leeuwenhoek's hypothesis.[4]

It is, however, contrary to all the analogies of nature to suppose, that bodies so peculiar in their properties and in their situation should not serve some specific purpose of utility in the actions of the animal œconomy. The conjectures that have been formed upon this subject are, however, for the most part, so fanciful, as to be scarcely deserving of notice, yet there is one which acquired such a degree of temporary celebrity, as to be entitled to distinct consideration. I refer to the peculiar modification of the hypothesis of epigenesis, which was proposed by Needham and Buffon. These distinguished naturalists conceived that there exists in all animated beings, what they

[4] See El. Phys. xxix. 2. 6. for the objections that have been urged against the hypothesis of seminal animalcules; it seems to have been first formally opposed by Valisneri, who examined it in all its parts in his elaborate dissertation "Della Generazione," par. 1, cap. 3..13. I may refer to Morgan, as affording a proof of the general assent which was given to the existence of these animalcules and of their agency in generation a century ago, (1735) and it is amusing to observe how he reasons upon the subject. Taking it for granted that one of these bodies is the origin of the future fœtus, he calculates at what rate it must grow from the period of conception to parturition. He estimates that the bulk of the animalcules is to that of the fœtus at birth as 1 to 19,200,000,000,000; hence it must double its bulk during every six days for forty-four successive periods, or for 266 days, which he considers the period of uterine gestation; Mech. Practice of Phys. p. 282, 3.

term a vegetative force, which enables them, when placed in suitable situations, to produce or generate vital particles, which have an attraction for each other, and by their union compose living organized bodies.

The speculation, which seems to have been originally conceived by Buffon,[5] was not only embellished by the charms of his eloquence, but was supported by a series of what appeared to be the most minute and elaborate observations, made by himself and by Needham, independently of each other. Upon examining the seminal fluid of various animals, they perceived a number of bodies to be floating in it; these, however, they did not admit to be animals possessed of independent existence and regular organization, according to the opinion of Leeuwenhoek,

5 With respect to the share which these two learned naturalists had in the formation of the new hypothesis, we learn from Needham, in the preface to his "Nouv. Obs. Micros.", in his letter to Folkes, Phil. Trans. for 1748. v. xlv. p. 615, et seq § 18. and in the translation to the same subjoined to the above work, p. 184, that Buffon originally conceived the hypothesis, which he communicated to Needham, and that they afterwards, both of them, performed experiments for the purpose of establishing and illustrating it. It would appear probable, from the above date, that Needham's letter actually appeared before the first part of Buffon's Natural History, which was published in 1749. Haller's account of the hypothesis is in El. Phys. xxix. 2. 13, 4. It is very fully discussed by Spallanzani in the first volume of his "Opuscules de Phys." where he controverts Needham's reasoning, and endeavours to prove that his experiments were incorrect or at least inconclusive. Buffon's account of his hypothesis in his Nat. Hist. v. ii. part 2. c. 4 . . 8.

but to be merely what they termed organic molecules, or vital particles, according to their hypothesis, produced spontaneously in the fluid, and which gave it its generative power. But, notwithstanding the authority of these authors, each of them eminent for their genius and their science, it is now universally admitted, that their speculations are entirely without foundation, sanctioned neither by facts nor analogies, and that many of the observations of Buffon are altogether incorrect.[6]

It is perhaps not very easy to determine who it was that first proposed the hypothesis of pre-existing germs,[7] but it is to the arguments and experiments of Bonnet, Haller, and Spallanzani, that it was principally indebted for the favourable reception which it met with in the middle of the last century. The first of these authors seems to have embraced it from an impression of the inadequacy of the other hypotheses to account for the facts, for we do not find that anything is urged by him in its favour, except those general considerations which were founded

6 See Haller, El. Phys. xxix. 2. passim, sed precipue in § 5. Spallanzani, Opus. de Phys. t. ii. was at considerable pains to detect the sources of Buffon's error; he seems to have clearly proved that at least one of them depended upon Buffon having mistaken the animalcules which are produced in putrescent fluids for the seminal animalcules.

7 Some of the older physiologists maintained that the rudiments of the fœtus were principally derived from the female ovum, this was the case with Malpighi and Harvey, and the opinion is very elaborately defended by Valisneri in the 2d part of his treatise "Della Generazione."

upon its comparative probability. He conceived that no mechanical or chemical operation, with which we are acquainted, bears the least analogy to the power by which a body is originally formed and organized, and therefore, in order to avoid this difficulty, he concluded, that when the first animal was created, the creation of all its future offspring took place at the same time, and that nothing farther was afterwards necessary but to evolve, or put into action, those beings which were previously existing in a dormant state.[8] A great defect in this mode of reasoning is that it is entirely founded upon our ignorance; and yet, at the same time, it so far presumes upon our perfect acquaintance with the subject, as to entitle us to employ a supposition perhaps more extraordinary than any which had ever been advanced

[8] Bonnet defines the germ "un corps organizé reduit extremement en petit;" he supposes that there is, in all cases, "un fond primordial dans lequel les atomes nourriciers s'incorporent ou s'incrustent, et qui determine par lui-même l'ordre suivant lequel ces atomes s'incrustent et l'espece d'atomes qui doivent s'incruster. Je suppose par-tout que ce fond primordial pre-existe dans le germe;" Œuvres, t. vii. p. 295. Palin. Part 11. c. 3. See also Mem. sur les Germes, Œuvr. t. 5. p. 1, et seq. This hypothesis obtained the appellation of "Emboitement;" Œuvr. t. iv. p. 273. note 3; Tabl. des Considerations, § 71, et alibi. Spallanzani always defends the opinion that the germ contains every organ that is subsequently found in the fœtus. Bonnet's account of his hypothesis and his defence of it against the objections of his opponents, will be found in his "Considerations sur les Corps Organ." ch. 58, 9. and in his "Contemplations," par 7. ch. 8, 9, 10. There is a prolixity and diffuseness in the works of this author which make it difficult to obtain a correct knowledge of his opinions.

in physiology. And this is the less excusable in this instance, because the whole of Bonnet's reasoning, as originally brought forward by him, rested not upon any direct facts or experiments, which he adduced in its favour, but solely upon its accordance with the general principles which direct the operations of nature, and upon the circumstance of its not involving any of those absurd or improbable positions which he thought were attached to the speculations of Buffon and Needham.

The arguments that were adduced by Haller in favour of the doctrine of pre-existing germs are of a different description, depending upon a very ingenious inference which he deduced from his observations on the gradual evolution of the chick in ovo.[9] He remarks that the greatest part of the matter which constitutes the egg, is obviously the production of the female, and that although the fœtus, when it first becomes visible, is extremely minute, yet that, in its earliest stages of existence, it must have been very much more so, and that we can assign no limits to its minuteness, except those of the imagination. From a careful examination of the structure of the chick, and the mode in which it is connected with

9 In El. Phys. xxix. 2. 7. we find a summary of the arguments which induced Haller to embrace this hypothesis; in the two next sections he candidly states the difficulties which attend it, yet in spite of these circumstances, he seriously argues in its favour. The subject is farther considered and additional arguments brought forward in the paragraphs 27. . 37; see also Op. Min. t. ii. p. 399 and 418.

the different parts of the ovum, Haller concludes that the membrane of the yelk is continuous with that of the intestine of the chick, and that they are in fact parts of the same substance; hence it follows, that as the yelk existed in the egg before impregnation, the intestine, and consequently the embryo generally, must have done so likewise.[1] The accuracy of Haller's observation, as far as the anatomical fact is concerned, has not, I believe, been called in question, viz. that the vessels of the chick are continued into certain parts of the yelk, constituting them portions of the same organized substance. The fact was eagerly seized by Bonnet, as a most powerful argument in favour of his hypothesis, and regarded as the most complete triumph over his opponents, and indeed it was generally regarded by all the contemporary physiologists, as a very convincing proof of the pre-existence of the germ in the uterine system of the female, independent of the congress of the male.

We may, however, remark upon this argument of

[1] Senebier gives a view of Haller's experiments and the deductions from them in the Introd. to Spallanzani, Opusc. de Phys. p. xcii. et seq. See the experiments in Haller, Op. Min. t. ii. passim. It is curious to compare the earlier opinions of Haller on this subject, as they are stated in his Prim. Lin. § 881, et seq. with those which he afterwards adopted. Even after making his observations on the chick in ovo, he at first only spoke of them as favouring the hypothesis of pre-existing germs, nor was it until after Bonnet had adopted them with so much zeal, that Haller himself regarded them as decisively proving the doctrine; see the remarks of Blumenbach, de Gen. p. 31, et seq. and Inst. Phys. § 584, 5.

Haller's, that it is not until the embryo has attained a certain size that this continuation can be observed, so that its existence before this period is an assumption which does not form a part of the anatomical fact, nor is it a necessary consequence of it. It is quite consonant to what we daily observe in the operations of the animal œconomy, that the vascular continuation, which was described by Haller, should be produced by a connexion between the two parts in the early stage of the existence of the fœtus, the vessels of the fœtus extending themselves into the previously organized substance of the yelk. This circumstance is less extraordinary than that two surfaces, not originally belonging to each other, should be able to form a vascular connexion merely by being placed in accidental contiguity, because we must conceive that, in the former case, although the yelk and the chick had no original communication with each other, yet that the parts are so constructed that a provision is laid for the communication afterwards taking place. Under these circumstances, there is nothing singular in the vascular connexion between the two bodies being produced in the most complete manner, so as to render them, in the ordinary acceptation of the term, parts of the same structure; but it does not necessarily follow that they were so originally. It is to be remarked, that Haller himself explains the union of the ovum with the internal surface of the uterus upon this principle.

Spallanzani's support of the hypothesis of pre-existing germs is principally derived from his microscopical observations, which, on this, as on so many other occasions, he appears to have prosecuted with unwearied industry, and the results of which, we have every reason to believe, he has related with perfect fidelity. The facts which he brought forward may be reduced to two heads, those in which he shows that the ova of animals exhibit precisely the same appearances before and immediately after impregnation, and those in which he made observations similar to Haller's, in which he attempts to prove, that at the very earliest period when we can behold any trace of a fœtus, it possesses a complete vascular connexion with those parts of the ovum which must evidently have pre-existed in the female. But, however valuable we may consider these observations, as exhibiting to us the condition of the fœtus in its very earliest stages of existence, all beyond this is merely conjectural.

Spallanzani remarks, that the size of the fœtus, when it first becomes visible, is such as to prove that it must have previously existed for some time, although, in consequence of its transparency, it could not be perceived. Hence he argues, that as it had existed for a certain length of time in an invisible state, it may have done this for an indefinite period, and draws the general conclusion, that the circumstance of a part not being visible, is no proof of its non-existence.[2] This position is certainly true in the abstract, but it is a principle which we must

[2] Haller remarks, that when the chick first becomes visible, it is a line in length, and justly infers that its not being sooner

employ with caution, and we must bear in mind, that it requires a high degree of antecedent probability, and the concurrent support of various independent considerations, before we can admit of the existence of a body, which we are, under no circumstances, able to detect.

We have a series of curious experiments by Spallanzani on artificial fecundation, which were regarded by himself and by Bonnet as affording a strong confirmation of their favourite hypothesis. Having ascertained that in many of the oviparous quadrupeds impregnation takes place out of the body of the female, he was induced to try whether he could not effect the operation, by applying a portion of the seminal fluid to some of the detached ova. He found that the quantity which was necessary was very small, and upon diminishing it in order to obtain the minimum, he diluted the semen with water until the mixture contained not much more than $\frac{1}{3000}$ of the secretion, when he found that a drop of this diluted fluid was capable of producing impregnation.[3] But he contended that it was impossible that so minute a quantity of the seminal fluid could afford any actual addition of matter to the ovum, and that it could only operate as a stimu-

visible, does not depend upon its minuteness but upon its pellucidity; El. Phys. xxix. 1. 26. The extreme minuteness of the parts, and the consequence deduced from this, of their existing before they are visible, are insisted upon by Spallanzani in various parts of his works on Generation.

[3] Experiences sur la Generation, Mem. ii. p. 125, et seq.

lus to the heart, or to an eminently contractile part, and thus produce a commencement of that train of actions which constitute the proper life of the fœtus.[4]

It must be admitted that these results of Spallanzani's are very curious and unexpected, but, although we might have supposed that a considerably greater quantity of matter from the male would have been required to have produced impregnation, there can be no limits assigned to the powers of nature on this subject, nor have we any analogy to guide us in the opinions that we may form respecting it. As far, therefore, as this point is concerned, it becomes a question of relative probability, whether it be more difficult to conceive of this minute particle of matter from the male uniting with a certain part of the ovum appropriated to this purpose, with the magnitude of which we are totally unacquainted, and thus constituting the fœtus, or whether we are to prefer the "emboitement," as it has been styled, of Bonnet, where hundreds and thousands of individuals lie one within the other, each of which possesses a complete series of organized parts, where, in many cases, the rudiments of the second generation alone are visible, and this only so by the aid of the most powerful microscope.

But it is not merely a question of relative probability. I have already observed, that it seems impos-

[4] A similar difficulty attends the generation of the warm-blooded oviparous animals; Harvey maintains, as the result of his own observations, that there is no perceptible difference between an impregnated and an unimpregnated egg; Ex. 6. p. 22, et seq. And the same opinion is implied in the remarks of Fabricius.

sible to reconcile the hypothesis of pre-existing germs with the resemblance which the fœtus bears to the male parent, or with the phenomena of the production of hybrids; to account for the operation of accidental circumstances upon the fœtus during pregnancy, or for the power which many animals possess of repairing lost parts, and of being multiplied by division, unless we suppose that there is in the animal itself a formative power, a faculty of producing what did not previously exist, and, if this be admitted to be necessary in one instance, we destroy the ground upon which the whole structure rests. From these considerations, I feel authorised in concluding, that the hypothesis of pre-existing germs is deficient in proof, is inconsistent with acknowledged phenomena, and is antecedently in the highest degree improbable.[5]

[5] The principal arguments in favour of the doctrine of pre-existing germs are, 1st, the difficulty of conceiving of the original formation of an organized body, as no one part can exist without the simultaneous existence of other parts; 2, the fact that several successive generations can be actually detected in certain classes of animals and in plants; 3, the visible evolution of the fœtus, which can in some cases be distinctly traced; 4, the extremely minute quantity of male semen which is sufficient to produce impregnation; 5, the analogy of the various species of inferior animals, where there is no co-operation of the male; 6, that oviparous animals produce eggs, and plants seeds, without the co-operation of the male, which, although not fertile, do not differ in their physical structure from those that are impregnated; and, lastly, the anatomical arguments of Haller, founded upon the mode in which the yelk of the egg is connected with the viscera of the chick. The objections to the doctrine are the

Blumenbach appears to have been the first physiologist who was strongly impressed with the difficulties and contradictions of this hypothesis, and his reasoning tended materially to its downfal.[6] Yet he agreed with Bonnet and his friends in the idea that the ordinary powers of vitality are not adequate to explain the process of generation ; and he therefore, according to his usual custom, ascribed it to the operation of an imaginary agent, which he created for the purpose of executing this particular office, and denominated the nisus formativus.[7] The cele-

difficulty of conceiving how the ovaria of the first created female could contain the germs of all her descendants; the phenomena of hybrids, the resemblance which, in certain cases, the offspring bears to the male parent, the existence of monstrosities of various kinds, and the re-production of mutilated or lost parts.

6 Essay on Gener. sect. 2, where the subject is treated with singular acuteness.

7 Inst. Phys. sect. 40. p. 333..9; also De Gen. Hum. Nat. var. sect. 2. § 33. p. 82, et seq. Those very curious cases, in which certain organized bodies have been found in the ovaria of unimpregnated females, have been supposed by some physiologists to be a proof of the existence of a certain generative power in the female, which may be referred to the nisus formativus. But I think that this hypothesis affords no real explanation of the facts in question, nor am I acquainted with any which throws any light upon the process. Cases of this kind are not uncommon, and there appears to be no doubt of their occurring in unimpregnated females; the following may be adduced as specimens; Haller, El. Phys. xxx. 1. 14. and Phil. Trans. for 1745, p. 71, et seq.; Baillie, in Phil. Trans. for 1789, and Morbid Anat. p. 293..5. and series of engravings, p. 199. Fasc. 9. pl. 7. fig. 1. and works by Wardrop, v. i. p. 137, 142, v. ii. p. 348. The Edin. Journ. Med. Scien. v. ii. p. 227, et seq. contains an account of four cases that were examined by Velpeau.

brity which is so justly attached to the name of Blumenbach, and our respect for every thing which issues from his pen, can alone render this speculation deserving of our attention. It will be sufficient to remark concerning it, that it affords an instance of that incorrect method of introducing new terms into science, which as they do not express the generalization of facts, throw no real light upon the subject in question, and which must therefore retard the progress of knowledge, by inducing the mind to remain satisfied with the acquisition of a new language, without having acquired any new ideas. The error with which Blumenbach is chargeable, consists not in his having failed to give a satisfactory explanation of the difficulty, which probably the present state of our knowledge will not allow us to do, but upon his having proceeded upon a wrong principle in the mode in which he has made the attempt.[8]

From these considerations it will follow, that we must have recourse to that modification of the hypothesis of Epigenesis which supposes the fœtus to be formed, in the first instance, by matter derived from each of its parents, and that the germ, when thus

[8] See Comment. Soc. Reg. Scien. Gotting. t. viii. p. 41, et seq. also Essay on Gen. sect. 3. The very circumstance which Blumenbach seems to consider as the essential merit of his hypothesis, its uniting physical with what he terms teleological principles, in the formation of a physiological hypothesis, is in fact its radical error; see his Inst. note to § 587. His account of what takes place in the female organs, when disencumbered of hypothesis, is probably correct, and contains all that we actually know upon the subject; see § 588, et seq.

produced, continues to receive additional matter, for a certain period, from the mother alone, until its own functions are sufficiently established to enable it to absorb and secrete what is necessary for its growth and increase.[9] It must be admitted that the original congress of the particles, by which the first rudiments are brought together, is an operation which is not analogous to any other with which we are acquainted in the animal œconomy, but it would appear that the subsequent stages do not essentially differ from the ordinary processes of vitality.

Although the hypothesis of Epigenesis does not afford us any satisfactory explanation of the generative function, it is the only view of the subject which we can take, that does not involve some position, either absolutely contradictory to the laws of nature, or, which appears in the highest degree improbable, if not altogether beyond our conception.

There is one hypothesis, on which it remains for me to offer a few observations,—an hypothesis which, although now very generally exploded, was at one period almost universally prevalent; I refer to what has been termed spontaneous or equivocal generation.[1]

9 For Blumenbach's definition of the process of Epigenesis, see p. 5. of his essay.

1 Fabricius commences his treatise "De Formatione Ovi," by observing, "animalium autem fœtus, alius ex ovo, alius ex semine, alius ex putri gignitur . . . . ;" and it would appear that Harvey admitted of this doctrine by the following observation, as well as others of a similar tendency: "sive ab aliis generantibus (animalia) proveniunt, sive sponte, aut ex putridine nascuntur," De Gen. p. 385.

It is sufficiently obvious, that in the higher orders of animals generation never takes place except by means of a parent; but it was contended, that in many of the lower tribes this intervention is not necessary, but that, under certain circumstances, an animal is formed from matter not previously organized. The proofs of this position were supposed to be both numerous and palpable, in the appearance of animals in various situations, where it seemed to be impossible to account for their production in the ordinary mode of generation.

A variety of experiments were performed upon this subject about the conclusion of the seventeenth and the earlier part of the eighteenth century, in which the Italian naturalists took the lead,[2] and they very

[2] Redi appears to have been among the earliest physiologists, whose experiments on this subject were performed with the due degree of accuracy; he clearly pointed out the source of the ova of many animals, which had been previously ascribed to spontaneous generation. We are indebted to Muller for a very complete account of the infusory animals; see also Haller, El. Phys. xxix. 2. 12. and art. "Animalcule," in Brewster's Encyc. The experiments of Trembley, Saussure, and Spallanzani, have added many important facts to our knowledge respecting the production of these animalcules. It would appear that, after the doctrine of equivocal generation was given up, it was still the opinion of some distinguished naturalists, that the ova of animals and vegetables were convertible into each other. Needham supported this doctrine, founding it upon some experiments of Baron Munchausen (a somewhat inauspicious name), who is said to have produced animals from the seeds of funguses, and funguses from the ova of animals. It is even asserted that Linnæus, for some time, gave credit to these experiments; Spallanzani, Opusc. t. i. p. 165. The same doctrine is maintained by Dr. Grant; see Brewster's Journ. v. viii. p. 110.

satisfactorily proved, that in many cases of what had been conceived to be spontaneous generation, it was possible to point out the means by which the ova of the young animals had been deposited, and to prevent their appearance. Yet still considerable difficulty remains on this subject, and there are many individual instances, which it is difficult to explain, where we are either obliged to confess our ignorance, and wait until some new light be thrown upon the subject, or to suppose that the analogy which we employ so frequently in physiology is here not applicable. The argument against equivocal generation is, however, merely analogical, and therefore can have but a certain degree of strength to whatever extent it be carried; negative experiments on such a subject, although of little avail when put in competition with positive results, yet become important if sufficiently multiplied and varied.

The cases to which I more particularly refer are the various instances of intestinal worms, and still more of the seminal animalcules. The appearance of the former may perhaps be explained upon the supposition, that their germs are contained in our food, and that they are conveyed into the intestinal canal, and developed there, as being the situation specifically adapted for their subsistence. But we cannot extend this explanation to the production of the seminal animalcules; it does not seem possible to conceive that their ova can have entered the blood, passed through the secretory organs, and been finally deposited in the testes, without admitting a series of events, which appear as difficult to comprehend as the actual formation of the bodies in the situation where they are found to exist. Upon the whole, it will be prudent to regard this as one of those mysteries which the present state of our knowledge does not enable us to explain, or even to comprehend.[2]

[2] See the observations of Lamarck, Anim. sans Vert. t. i. p. 178; in this, as well as in other parts of his great work, he advocates the hypothesis of spontaneous generation; he conceives that his first order of animals, "Infusoires nus," are produced in this mode; see remark in p. 432, 3, also t. iii. p. 140, 1, for his account of Intestinal Worms. Cuvier, on the contrary, when speaking of the existence of the various species of entozoa, which are sometimes found even in compact cellular texture, remarks that many persons suppose them to be produced spontaneously, in consequence of the difficulty of accounting for their mode of entrance into these parts. But, he adds, as some of them are known to produce ova, or to bring forth young, it is more probable that they proceed from germs, so small as to be admitted through the most minute passages, or even that "les jeunes animaux où ils vivent en apportent les germes en naissent;" Regne Animal, t. iv. p. 27. M. Virey also decidedly opposes the doctrine of spontaneous generation; Dict. Scien. Med. art. "Generation," t. xviii. p. 10, 2. and Senebier, in his translation of Spallanzani's Opusc. de Phys. speaks of it as having been completely refuted by microscopical observations, Introd. p. lx.

# APPENDIX.

I have collected in this Appendix some of the principal passages in the works of Leeuwenhoek, where he gives an account of his successive discoveries respecting the seminal animalcules; I have thought it desirable to refer to them in this connected form, in order that any one may readily learn what was actually the opinion of Leeuwenhoek on this point. Opera, t. i. p. 49, et seq. in an epistle to Wren, he describes the animalcules as found in the testes of the frog, gives a plate of them, and estimates their thickness at about $\frac{1}{1000}$ of a human hair. P. 1, et seq. 2. ser. in a letter to Grew he describes them as seen in fishes, in a hare, a dog, and a rabbit; it is in this letter, p. 8, that he informs us that Hooke exhibited them to Charles II. a circumstance which is only important, as it proves that Hooke gave full credit to the discovery. Leeuwenhoek gives a very decided opinion concerning the use of these animalcules; he remarks, p. 6. "testiculos nullum alium usum præbere, nisi ut animalcula in illis formata, tamdiu in illis sustententur, donec ad ejaculationem sint idonea." With respect to their size, he remarks, that 10,000 of them may exist in a space not larger than a grain of sand, and that the melt of a cod may contain 15,000,000,000,000,000. P. 24 and p. 25, et seq. in two letters to Hooke, we have a further account of them; in a letter addressed to the R. S.

p. 149, et seq. he attempts to show how the animalcule is converted into a fœtus, and he goes so far as to suppose, p. 163, that they possess different sexes, and thus give rise to the sex of the future animal, an opinion which he supports in a letter to Wren, t. ii. p. 28. In p. 168. he gives the figures of the animalcules from a rabbit. In a letter to Wren, t. ii. p. 26, et seq. he argues against the ovarian hypothesis of generation, as maintained by De Graaf and Harvey, and also in a letter addressed to the R. S. p. 398. and in one to Garden, p. 400. When pressed by his opponents to explain what he conceived to be the use of the ovarium in the mammalia, he replies by the following question, p. 414; "cui enim usui inserviunt nobis cognitæ papillæ in quadrupedis masculinis? Imo etiam cui usui sunt papillæ quas viri in pectore gerunt?" In the 3d vol. we have farther remarks and observations in a letter to Van Zoelen, p. 57, et seq. to which I have already referred; in a letter addressed to the R. S. p. 96, et seq. where he combats some objections of Lister; in an epistle to Sloane, p. 281, et seq.; in one addressed to the R. S. p. 304, et seq., where he again insists upon the extraordinary minuteness of the animalcules; in an epistle to Ld. Somers, p. 367, et seq. where we have figures of the animalcules from a common fowl, which differ a little from some of those previously given. In the 4th vol. we have an epistle to Leibnitz, p. 164, et seq. in which he refers to the objections of Valisneri, also to those of another author, whom he does not name: in the same letter he informs us that Boerhaave assents to the existence of the animalcules. In a second epistle to Leibnitz, p. 206, et seq. he opposes the ovarian hypothesis, and states that he has frequently examined the Fallopian tubes of dogs and rabbits after coition, and has seen the animalcules in this part, but never an ovum. In a third letter to Leibnitz, p. 287, et seq. he reverts to the opinion referred to above, that he could observe a difference of sex in the animalcules, upon which depended the future sex of the fœtus. In this volume we have four letters to Boerhaave, in which he discusses different points connected with the form, size, or state of the animalcules; he conceives that he has been able to detect a difference in them according as they are in a more or less perfect state of growth, p. 281, 305. I have referred to Leeuwenhoek's works in their collected form, rather than to the letters or communications which successively appeared in the Phil. Trans. or other periodical works. Haller, in his Bibl. Anat. t. i. p. 606. . 613, gives an account of all Leeuwenhoek's successive publications; he states that the earliest notice of the animalcules is contained in a paper inserted in the Phil. Trans. No. 142. (called by mistake No. 143) written in 1667. I have subjoined a list of some of the most respectable authors, who have given their full assent to the accuracy of Leeuwenhoek's descriptions, many of them, as it would seem, from personal communication with him, or from their own observations.

Valisneri's account of his own observations, and those of Lancisi, Della Gener. par. 1. cap. 2. Op. t. ii.

p. 105, et seq. tab. 18. fig. 14; Huygens, Diop. prop. 59. et Op. Rel. t. 1. p. 176; Andry, De la Gener. des Vers, t. 1. p. 152, et seq.; Morgagni, Advers. 4. p. 9; Baker on Microscopes, v. 1. ch. 16. p. 152, et seq.; Monro, sec. De Test. in Smellie's Thes. t. ii. p. 373; Boerhaave, Præl. § 651. t. v. p^s. 1. p. 156, 7, and note in p. 159, 0; Haller, El. Phys. xxvii. 2. 3. Bonnet, in his letter to Spallanzani, inserted in "Opusc. de Phys." t. ii. p. 95, et seq. Morgan, an author of some reputation in the middle of the last century, thus expresses himself, "that all generation is from an animalculum pre-existing in semine maris, is so evident in fact, and so well confirmed by experience and observation, that I know now of no learned men, who in the least doubt of it;" Mech. Practice of Phys. p. 281. I quote this passage to show what may be considered as having been the popular opinion when this work was published, 1735.

## CHAPTER XIII.

### OF VISION.

I have now gone through the functions which I have styled contractile or physical, such as depend more immediately upon the contractility of the muscular fibre, the result of which is to produce either a mechanical or a chemical change, and where the action of the nervous system, although frequently called into exercise, is not essential to the effect. We must now proceed to consider the second class of functions, the sensitive, in which the nervous system bears the principal share, being the part primarily affected, and where the change that is produced is not necessarily either of a mechanical or a chemical nature.

The sensitive functions may be naturally divided into two classes; the first, those that depend upon the operation of a physical agent on some part of the nervous system; the second, those that are produced by the action of the different parts of the nervous system upon each other, where no physical power is immediately concerned. As an example of the former, I may instance what are usually termed the external senses, and of the latter, the faculty of sympathy. In both cases a certain change takes place in an appropriate set of nerves, which is attended with consciousness, while there is a corresponding change in the sensorium, through the intervention of which the ultimate effect is produced.

To take the case of the eye; the rays of light pass through its humours to the retina, on which the impression is received: the sensation is transmitted by the optic nerve to the brain, where a perception of sight is produced, and gives rise to all the various actions which depend upon the ideas and feelings that we derive from this sense. Upon the same principle, in the second division of these functions, a certain affection of one part of the nervous system leads to an affection of some other part; this affection is transmitted to the sensorium, and excites there a train of ideas and feelings, which become the immediate cause of various other changes, both mental and corporeal. To the first of these classes we may give the title of the physico-sensitive functions, and to the second that of the simply-sensitive functions.

The physico-sensitive functions may be again subdivided into those where the physical cause consists in some external agent, independent of the system, but which acts directly upon it; and, secondly, where the physical cause is itself necessarily connected with the system, depending upon its previous condition, or some antecedent train of actions. The most important of the first class constitute what are termed the five senses, in which light, the undulations of the air, the effluvia of odorous, those of sapid substances, and the contact of a solid body, are respectively the physical causes employed.

The second sub-division consists of certain perceptions, where the cause producing them, although of an appropriate and specific nature, is not derived from any external agent; such, for example, are the feelings of hunger and thirst, which depend respectively upon certain states of the stomach and fauces, and that which attends the motion of the joints, where the contraction of the muscular fibre produces a certain impression upon the nerves, which is conveyed to the sensorium.

What I have termed the physico-sensitive functions agree not only in the circumstance of their having each of them a physical cause, which is necessary for their production, but also in this cause being of a specific nature, appropriated to each particular function. They have likewise each of them an instrument, constructed for the express purpose of receiving the impression of this physical cause, and being acted upon by it, and which, as well as the cause itself, is exclusively appropriated to each particular function, constituting what are termed the organs of sense. Sight is the appropriate cause of vision, and no organ except the eye can receive the impression of light; the undulations of the air are the appropriate cause of the sensation of sound, while, at the same time, no organ of the body except the ear can receive the impressions of these undulations.

The effects which are thus produced upon the sensorium, through the intervention of the different organs of sense, as they agree in the mode of their production, have received a common appellation. Different terms have been employed for this purpose, but the one which, upon the whole, appears to me the most appropriate, is perceptions of impressions, as this is expressive both of their origin and of their operation.[1]

In giving an account of the external senses, I shall begin with that of vision, as it produces the most extensive class of our perceptions, while, at the same time, we are the best acquainted both with the action of the exciting cause, and with the mechanism of the organ by which it operates. The remarks upon vision will be arranged under three heads; I shall, in the first place, give a brief description of the eye, its structure, the connexion of its different parts with each other, and the uses which they have been supposed to serve. In the second place, I shall offer some observations on the nature and cause of vision, on the mode in which light acts upon the eye, so as to produce the ultimate effect, first upon the retina and subsequently upon the sensorium. Having thus become acquainted with the direct effects of vision, we must, in the third place, inquire into the connexion which subsists between the perceptions of sight and those of the other organs of sense, as well as into the modifications which are produced by

[1] See Reid on the Human Mind, p. 185, and on the Intellectual Powers, Ess. 2. ch. ii. p. 79. . 82. They are denominated by Locke, Ideas of Sensation, Essay, Book 2. passim; by Hume, Impressions, Essays, v. ii. p. 31; by Hartley, Sensations, on Man, Introd. p. 1.

association and sympathy, giving rise to those curious and important affections, which may be termed the acquired perceptions of vision.

## § 1. *Description of the Eye.*[2]

The eye may be regarded as an optical instrument, which consists of three orders of parts. The first are those by which the rays of light are received, and so far modified, as to render them

[2] Among the numerous descriptions of the eye that have been published, the following may be selected, as probably being the result of actual observation. Fallopius, Instit. Anat. Op. t. i. p. 454 . . 6; Fabricius, De Oculo, Op. p. 187, et seq.; Kepler, Paralipomena, cap. 5. p. 158 . . 168; Briggs, Ophthalmographia; Cheselden's Anatomy, ch. 6. p. 290, et seq.; Winslow's Anatomy, sect. 10. art. 2. v. ii. p. 284, et seq.; Porterfield on the Eye, v. i. book 2; Boerhaave, Prælectiones, § 508, et seq. t. iv. p. 44, et seq.; Cowper, Anat. Corp. Hum. tab. 11; Camper, De quibusdam Oculi Part.; Haller, El. Phys. lib. 16, sect. 2; Oper. Min. t. iii. p. 218, et Arter. Oculi Hist. cum tab. in Icon. Anat. Fas. 7; Warner's Description of the Eye; Zinn, Descrip. Anat. Oculi Hum.; et in Comment. Gott. t. iv. p. 192; Sœmmering's elaborate Icon. Oculi Hum. and the transcript of the same by Caldani, pl. 93 . . 5; D. W. Sœmmering, De Oculis Hominis Animaliumque Commentatio; Blumenbach, Instit. Phys. sect. 17. § 255 . . 268; Monro's (sec.) Three Treatises; Bichat, Anat. Descript. t. ii. p. 416, et seq.; Cuvier, Leç. d'Anat. Comp. No. 12. t. ii. p. 264, et seq.; Young's Lect. No. 38. v. ii. p. 447, et seq.; Bell's Anat. v. iii. part 2. book 1. p. 224, et seq.; Monro's (tert.) Elements, part vi. ch. iii. sect. 2. p. 392, et seq.; Travers on the Eye, p. 1 . . 44. To these references may be added the beautiful figures of Mr. Bauer, in Phil. Trans. for 1822, pl. 6 . . 12, in which many points connected with the peculiar anatomy of the organ are admirably illustrated.

subservient to vision; the second are certain productions of the nervous system, which receive the impressions of light, and convey them to the sensorium, while the third are a number of accessory parts, which preserve the eye in a state proper for the performance of its functions, and enable it to execute them in the most perfect manner.

What is termed the globe or ball of the eye is a body of nearly a spherical form, which is composed of three transparent substances, styled humours, enclosed in membranes of suitable strength and thickness to preserve the form of the organ, and to attach it to the neighbouring parts. The vitreous humour constitutes the main bulk of the globe:[3] its consistence is nearly that of the white of the egg, but it seems to be composed of a fine tissue of membranous cells, in which a slightly albuminous fluid is deposited.[4] There is a depression in its anterior part, in

3 Petit informs us, that the weight of the human eye, deprived of the muscles and fat, is 142 grains; the vitreous humour being 104 grains, the aqueous and crystalline each 4 grains, and the membranes 30. In the eye of a boy these numbers were 132, 95, 4, and 29; in an ox, 615 the whole, 360 the vitreous humour, 38 the aqueous humour, the crystalline 52, and the membranes 165; Mem. Acad. pour 1723, p. 38, et seq. and pour 1728, p. 206, et seq. See also Porterfield on the Eye, v. i. p. 132 . . 4; and Haller, El. Phys. xvi. 2. 18. Winteringham made some accurate experiments on the form and dimensions of the eye of the ox; Exper. Inq. Ex. 53. § 1, 2. p. 282 . . 4. In Martin's Phil. Brit. we have measures of the different parts of the eye, v. iii. p. 26, et seq.

4 Porterfield on the Eye, v. i. p. 242 . . 5; Blumenbach, Inst. Physiol. § 264; Bell's Anat. v. iii. p. 314. The humours of the

which is lodged the second of the humours, the crystalline. This is a body of considerable density and firmness, having the form of a double convex lens, which is placed perpendicularly behind the aperture of the pupil, so that all the rays of light which enter the pupil must necessarily pass through the crystalline. By maceration in water, it is found to be composed of laminæ, which progressively increase in density from the circumference to the centre.[5] A large proportion of its solid contents are stated by Berzelius to consist of a peculiar matter, which contains neither albumen, nor jelly, nor muscular fibre, and which, except in the absence of colour, has all the properties of the red particles of the blood.[6]

eye have been successively analyzed by Chenevix, Phil. Trans. for 1803, p. 195, et seq.; Nicholas, Ann. Chim. t. liii. p. 307, et seq. and Berzelius, Med. Chir. Tr. v. iii. p. 253 .. 5. See also the remarks of Dr. Young, Med. Lit. p. 521, 2.

[5] Chenevix found the external part of the crystalline of an ox to have the specific gravity of 1·1940, while the specific gravity of the whole was not more than 1.0765, ubi supra; see also Young's Lect. v. i. p. 448. Winteringham found the specific gravity of the whole crystalline, compared to that of the internal part, to be nearly as 26 to 27; Exper. Inq. p. 239.

[6] Med. Chir. Tr. v. iii. p. 254, 5; this opinion is, however, directly opposed to that of Chenevix. His experiments would lead to the conclusion, that the humours all consist of water, united with different proportions of albumen, the aqueous and vitreous also containing a portion of muriate of soda, which is not found in the crystalline. He states the specific gravity of the three humours of the eye to be, the aqueous 1.0053, the vitreous the same, and the crystalline 1.0765, ubi supra; see also Porterfield on the eye, v. i. p. 232. Monro (sec.) informs us, as the result of experiment, that the crystalline is more refractive than

Before the crystalline is lodged the aqueous humour, consisting almost entirely of water, holding in solution a small quantity of saline matter, with a trace of albumen, being perhaps the only substance to which the term humour strictly applies. The iris is altogether immersed in the aqueous humour, which it divides into two unequal parts or chambers, as they have been styled; the anterior, or the one between the iris and the cornea, being considerably more extensive than the posterior, or that which lies between the iris and the crystalline.[7]

The greatest part of the globe of the eye is inclosed by a strong, dense, opake membrane, termed the

its specific gravity would indicate; its power of refraction is stated to be midway between that of glass and water; Three Treatises, p. 86. An account of all that was known respecting the structure of the crystalline, its composition, figure, dimensions, specific gravity, refractive power, &c. before the time of Haller, may be found amply detailed in his notes to Boerhaave, Prælectiones, § 527, t. iv. p. 92 . . 4; also in El. Phys. xvi. 2. 19 . . 21. The form and structure of the crystalline was a topic which was very minutely investigated by Petit, Mem. Acad. pour 1730, p. 4, et seq.

[7] The comparative size of these chambers has been made the subject of much discussion, principally as affecting the operations which are occasionally performed upon the eye. Petit bestowed much attention upon this subject; he states the comparative sizes to be 2.5 and 1.6; see his papers in Mem. Acad. pour 1723, p. 38, et seq. and pour 1728, p. 289, et seq. Haller also made it the subject of experiment; Notæ ad Boerhaave, Prælect. § 526, t. iv. p. 89, and El. Phys. xvi. 2. 23. See also Porterfield on the Eye, v. i. p. 146, and Bell's Anat. v. iii. p. 309, 0. In Sœmmering's 8th plate we have perpendicular sections of the eye, so as to present a view of the form of these chambers.

sclerotic coat; at the fore-part it is wanting, and the space thus left is occupied by the cornea, a transparent membrane, which, composing a greater part of a smaller sphere, produces more or less prominence of this part of the globe. The cornea possesses a laminated texture, and is provided with a greater number of blood-vessels than the sclerotic. Within the sclerotic lies the choroid coat, a membrane which is considerably thinner, and more vascular. The choroid is also wanting at the fore part, leaving a circular opening, to which is attached the iris or uvea,[8] the colouring ring that surrounds the pupil. The choroid is lined with an expansion of nervous matter, termed the retina, and this is connected with the optic nerve, which passes from the posterior part of the globe to the central portion of the brain. The convexity of the cornea and the density of the different humours are such, that when parallel rays of light fall upon the cornea, and pass through the pupil, they are brought to an exact focus upon the retina.[9]

[8] There has been some uncertainty in the language of physiologists respecting the appropriation of the terms iris and uvea, but the most correct method seems to be to consider the iris as the anterior, and the uvea as the posterior lamina of the ring which surrounds the pupil. It would appear that the muscular fibres, to be described hereafter, are found in the iris alone, while the substance, whatever it be, which gives the colour to the eye, is situated either between these parts, or in the uvea. See Bell's Anat. v. iii. p. 263, note.

[9] For an account of the coats of the eye, see Winslow, Anat. sect. 10. art. 2. § 2, 3; Haller, El. Phys. xvi. 2. 5 . . 10;

The principal refraction of the rays of light takes place when they enter the cornea; it is increased by their passage from the aqueous to the crystalline humour, and continues to increase until they arrive at the centre of the lens; as they pass on to its posterior part, and when they afterwards enter the vitreous humour, the convergence of the rays is somewhat diminished.[1] We are indebted to Kepler for an interesting experiment, in which, by removing a portion of the membrane from the back part of the eye, and covering the opening with oiled silk, we are presented with an inverted picture of the object towards which the eye is directed.[2] In this process the organ acts altogether mechanically, the rays of light being affected by the humours through which

Blumenbach, Instit. Physiol. § 256 . . 263; Bell's Anat. part 2. book 1. ch. ii. v. iii. p. 244, et seq.

[1] The latest, and, we may presume, the most accurate experiments on the comparative refractive powers of the different humours of the eye were performed by Dr. Brewster, in conjunction with the late Dr. Gordon; the refractive power of water being taken at 1·3358, the different parts of the eye were as follows:

| | |
|---|---|
| Aqueous humour | 1·3366 |
| Vitreous ditto | 1·3394 |
| Outer layer of crystalline | 1·3767 |
| Middle ditto ditto | 1·3786 |
| Central part of ditto | 1·3990 |
| Whole of ditto | 1·3839 |

Brewster's Journ. v. i. p. 43.

[2] Paralipomena, p. 177, 8; Dioptrice, Prop. 60; Haller, El. Phys. xvi. 4. 2; Porterfield, Ed. Med. Ess. v. iv. p. 126, 7, and On the Eye, v. i. p. 360.

they pass, precisely in the same manner as if they had been transmitted through a succession of physical media of the same density.

Between the choroid coat and the retina there is a thin stratum of a black viscid substance, which is termed Pigmentum nigrum, and is probably a secretion from the vessels that are dispersed over the surface of the choroid. From the experiments of Dr. Young, it would appear to consist of a mucous substance united to a quantity of carbonaceous matter, upon which its colour depends.[3] Its use has been supposed to be to absorb the superfluous rays of light, that might otherwise oppress the sight or render objects indistinct. This effect is illustrated by what takes place in the eyes of certain classes of animals, which are not provided with the pigmentum nigrum, as well as by the peculiar condition of the eye of the albino. In these cases the organ is unable to bear the strong light of day without experiencing uneasiness; while, at the same time, it can discern objects distinctly by a very small quantity of light.[4] Hence we find, that those animals which seize their prey by night, or whose habits lead them to spend their time principally in darkness, are

[3] Med. Lit. p. 521. See Haller, El. Phys. xvi. 2. 14, for the opinions entertained by himself and his contemporaries concerning this substance.

[4] Blumenbach, Instit. Physiol. § 274; also De Gen. Hum. var. p. 276.

either without this substance or have it of a lighter colour.[5]

It has been customary with anatomists to class the retina among the coats of the eye, but it properly belongs to the second order of parts, those by which the sensation of sight is received and conveyed to the sensorium commune. It is described as being an expansion of the optic nerve: perhaps, however, it would be more correct to speak of it as an expansion of nervous matter connected with the optic nerve, because the parts differ materially in their structure, and we are not able to demonstrate the

[5] Haller, El. Phys. xvi. 2. 12; Monro's Three Treatises, p. 100; Bell's Anat. v. iii. p. 255, 6. A different view of the subject has, however, been lately taken by M. Desmoulins; he observes, that the choroid, in different animals, exhibits every variety of colour and shade, and this without any relation to the perfection of their sight; that it is of the brightest colour in many animals that see with the greatest accuracy in a strong light, and that its use may be to reflect the light from the surface of the choroid to the back part of the retina, and thus increase the effect; Magendie's Journ. t. iv. p. 89, et seq. For an account of the colour of the choroid in various animals, see Cuvier, Leç. d'Anat. Comp. t. ii. p. 402. We have a number of interesting observations in Hunter's Essay, "On the Colour of the Pigmentum Nigrum of the Eye," Anim. Œcon. p. 243 .. 253, in which he points out the connexion which is so generally found to exist between the colour of this part and that of the hair and skin, and the relation which this bears to the other functions and structures. I may remark that Hunter had formed a speculation, concerning the effect produced on vision by a light-coloured pigmentum, nearly similar to that subsequently brought forwards by M. Desmoulins; see p. 252.

actual passage of the one into the other. It consists of a number of fine fibrils of nervous matter, disposed in a reticulated or radiated form, among which are interspersed a minute net-work of blood vessels, so as to be adapted to receive the most delicate impressions of external objects.[6]

From the analogy which it bears to the other parts of the nervous system, and from its connexion with the optic nerve, we cannot doubt that the retina is the part on which the impressions of sight are received, or that it is, what has been termed, the immediate seat of vision. It appears, however, that it is not equally sensitive in all its parts, the centre being the most so, and its power in this respect progressively diminishing as we recede from this point. A curious discovery was made by Marriotte, that the portion of the retina which lies over the commencement of the optic nerve, is altogether insensible to

[6] The extreme vascularity of the part led to an opinion, which was maintained by some eminent anatomists of the last century, that the retina consists entirely of a net-work of vessels; see Albinus, Anat. Acad. lib. 4. cap. 14. Haller, although he gives a very minute account of this part, does not very explicitly state its fibrous texture; El. Phys. xvi. 2. 15. See also, Zinn, De Oculo Hum. cap. 3. § 3. Monro (sec.) says that it is not fibrous, but that it is composed of a uniform layer of cineritious matter; Three Treatises, p. 93. Fontana has given us the magnified figure of the retina of a rabbit; Sur les Poisons, t. ii. pl. 5. fig. 12. We are indebted to Dr Knox for many valuable observations on the minute anatomy of the retina; Edin. Phil. Trans. v. x. p. 232. . 7.

the impression of light.[7] From this discovery he deduced the singular hypothesis, that the choroid, and not the retina, is the immediate seat of vision, arguing that in the part of the eye which is insensible, the retina is present while the choroid is wanting. The fact, as stated by Marriotte, appears to be correct, and as the hypothesis coincided with the doctrine which was insisted upon by the Stahlians, as well as by some other sects of physiologists, that the membranes are among the most sensitive parts of the body, this opinion, improbable as it appears, gained many supporters, but it is so entirely at variance with all our notions of the respective uses of these parts, as to be now altogether discarded.[8]

[7] Marriotte's original account of his discovery is contained in a letter to Pecquet, and is inserted in Phil. Trans. v. iii. No. 35. p. 668, for May 1668, also in Mem. Acad. t. i. p. 68, 9. and p. 102, 3. Pecquet's answer is also given, in which he admits the correctness of the fact, as stated by Marriotte, but argues against the conclusion which he deduces from it. Marriotte defends his hypothesis in Phil. Trans. for 1670, No. 59. p. 1023, et seq. For a full account of the subject and the discussion to which it gave rise, see Haller, Notæ ad Boerhaave, Prælect. § 543. t. iv. p. 128, 9, also El. Phys. xvi. 4. 4, 5 ; Porterfield on the Eye, v. ii. p. 224, et seq ; Priestley on Light and Colours, per. 4. sect. 5. ch. 2. ; Bell's Anat. v. iii. p. 283. . 8.

[8] Marriotte's hypothesis was warmly defended by Le Cat, Traité des Sens, p. 166. . 179. The only advocate of any eminence which it has met with of late years is Priestley, Hist. of Light and Colours, ubi supra, p. 189, et seq. The curious discovery made by Sœmmering, of a foramen in the centre of the retina, has been supposed to throw some light on the experiment

The only conclusion which we can draw from the fact is, that the functions of the nervous matter differ according to its mechanical disposition; that when it is in the form of a thin expansion it is better adapted for receiving the impression of objects, and that when it is condensed into a firm cord, it is more suited to the transmission of impressions to the sensorium commune.

We may presume that the specific and sole purpose of the optic nerve is to convey the visible impressions received by the retina, for it may be presumed, that all the other functions to which the nervous system is subservient are performed by the other nerves, with which the eye is so plentifully furnished.[9] But as is the case generally with nerves

of Marriotte; yet it can scarcely be applicable to it, as the size and position of the insensible spot are said not to be exactly coincident with this peculiar structure, and do not correspond with the entrance of the optic nerve into the retina. For the original account of it, see Sœmmering, "De Foramine Centrali Limbo luteo cincto Retinæ Humanæ." Comment. Gottin. t. xiii. 1795. p. 1, et seq. cum fig., and his Icones Oculi Hum. tab. 5. fig. 4, 5, 6; also Sir E. Home's "Account of the Orifice in the Retina of the Human Eye, discovered by Prof. Sœmmering; to which is added Proofs of this Appearance being extended to the Eyes of other Animals." Phil. Trans. for 1798. p. 332, et seq. pl. 17. Dr. Knox has lately confirmed and extended the observation; Mem. Wern. Soc. v. v. p. 1, et seq. and p. 104, et seq. pl. 4. also Edin. Phil. Trans. v. x. p. 233 . . 6.

9 See Mr. Bell's paper on the appropriate functions of the different nerves that are sent to the eye; Phil. Trans. for 1823, p. 289, et seq. Respecting the use of the optic nerve, and of the other nerves which belong to the organs of the external senses,

which possess specific powers, we do not observe any peculiarity in the structure or fabric of the optic nerve, which could have led us previously to form any conclusion respecting the nature or mode of its action.

The third order of parts, those which may be

we have an opinion advanced by M. Magendie, very different from the one which is generally adopted, but which professes to be the direct result of experiment. By dividing the different nerves in the living subject, he conceives himself to have discovered that the senses of sight, hearing, smell, and taste, are not exercised through the medium of the optic, the auditory, the olfactory, and the gustatory nerves, as they have been named from their supposed offices, but that the impressions, in all these cases, are conveyed to the sensorium by certain branches of the fifth pair, which are distributed in greater or less quantity over the respective organs. With respect to the eye in particular, he found that the division of the fifth pair always produced blindness, although the optic nerve was untouched. The division of the optic nerve also produced blindness, so that this last appears to be essential to the sight, although not capable of producing it without the co-operation of the fifth pair. M. Magendie likewise informs us that all these nerves are insensible to mechanical stimuli, while the 5th pair is exquisitively sensible to them. The experiments and deductions are contained in various papers in M. Magendie's Journ. t. iv. p. 170, et seq. p. 176, et seq. and p. 302, et seq. We have some observations on the comparative anatomy of the olfactory nerves by M. Desmoulins, t. v. p. 21, et seq. and a morbid dissection by M. Serres, t. v. p. 37, et seq. which are supposed to confirm M. Magendie's doctrine. M. Magendie also informs us that he has found the retina in the human subject to be insensible to mechanical stimuli; Journ. ubi supra. This fact is in accordance with the observations which have been lately made upon the specific functions of the different parts of the nervous system.

termed accessory or auxiliary, are very numerous, and are adapted to a variety of useful purposes. One of the most important of these is the iris. Its principal use is to regulate the quantity of light which enters the pupil, and for this purpose it possesses the power of contracting in a bright light, and of expanding when the light is feeble, so as to allow the exact number of rays to fall upon the retina, which is, in all cases, the best suited for distinct vision. It also contributes to distinct vision, especially when we view near objects, by limiting the spherical aberration of the lens; this it effects by excluding the more divergent rays which pass through the cornea, and which, from their falling on the extreme parts of the crystalline, could not be brought to a correct focus on the retina.[1]

Physiologists were, for a long time, unable to explain satisfactorily, either the principle upon which the iris acts, or the mechanism by which the action is effected. It appeared, indeed, to differ so essentially from the ordinary operations of the animal œconomy, that Blumenbach regards it as affording an example of what he terms the vita propria,[2] a phraseology on which I have already had occasion to animadvert. It had, indeed, been stated by Winslow[3] and by Porterfield,[4] that the iris contains muscular fibres, and

[1] Young's Lect. No. 38. v. i. p. 451.

[2] Comment. Gottin. t. i. p. 43, et seq. and Instit. Physiol. § 273. p. 154. Barclay has made some very judicious remarks on this supposed power; On Life, sect. 14. See also Rudolphi's Elem. of Physiol. by How, § 225. v. i. p. 219.

[3] Anatomy, sect. 10. art. 2. § 220. v. ii. p. 287, 8.

[4] On the Eye, v. i. p. 153. and v. ii. p. 117.

the circumstance of its being occasionally under the control of the will, rendered it highly probable that it was a muscular organ. It was not, however, until the investigations of the late Prof. Monro,[5] and more recently of Mr. Bauer,[6] that the actual existence of the muscular fibres was satisfactorily demonstrated; we learn from these observations that there are two sets of fibres, one circular and one radial, the action of which must be respectively to contract and expand the aperture of the pupil.[7]

5 "Three Treatises," On the Eye, ch. 5. sect. 3. p. 110, et seq. tab. 3.

6 Phil. Trans. for 1822. p. 78, 9. pl. 6, 7, 8. See also the observations of Mr. Jacob, in Med. Chir. Tr. v. xii. p. 509 . . 4, pl. 9. (by mistake numbered 10) fig. 1, 2, 3, 4.

7 Mery, who seems to have minutely examined the iris, could not detect the muscular fibres; Mem. Acad. pour 1704, p. 261, et seq. and pour 1710, p. 374, et seq. Zinn does not believe in the existence of the circular fibres, because he could not detect them, and because the functions of the part he conceives do not correspond with what might be supposed to take place from their action; De Moto Uveæ, in Comment. Gottin. Antiq. 1778. t. i. p. 55, et seq.; see also his work, Descrip. Oculi Hum. cap. 2. sect. 3. § 3, in which, although he admits the existence of fibres in the iris, he does not think they are entitled to the appellation of muscular. Blumenbach likewise opposes the doctrine of its muscularity, as we have seen above, and Dr. Knox informs us that he is unable to detect the fibres; Edin. Phil. Trans. v. x. p. 71. We have a further account of Dr. Knox's observations in the Edin. Journ. Med. Scien. v. ii. p. 103 . . 5, the result of which still leads him to doubt of its proper muscularity. Mr. Bell, on the other hand, Anat. v. iii. 265, 6, and M. Magendie, El. Physiol. t. i. p. 61, 2, admit the part to be muscular. The opinions that were entertained on this point before the time of

We learn from an experiment of Fontana's, that when light stimulates the iris, it does not act directly upon the part itself, but upon the retina. His experiments consisted in throwing a small pencil of rays upon the iris, which was found to produce no effect upon it; but when the same pencil was directed to a part of the retina, the contraction of the iris immediately ensued.[8] This effect may be regarded as analogous to what we observe in other parts of the body, where a stimulus being applied to a portion of the nervous system, produces contraction in a set of muscular fibres that are more or less directly connected with it. Although it appears that the iris possesses radial as well as orbicular fibres, which by their contraction, must tend to expand the pupil, yet we may conclude, that the expansion is, in a great measure, produced by the mere relaxation of the circular fibres, when the stimulus of light ceases to act upon them. We are altogether ignorant of the nature of the stimulus which causes the contraction of the radial fibres. It is scarcely probable that

Haller may be found in the notes to Boerhaave, Prælect. § 520. t. iv. p. 72. and in El. Phys. xvi. 2. 10 . . 12; his opinion was against the muscularity of the part; see his experiments in Op. Min. t. i. p. 372 . . 4.

[8] Dei Noti dell' Iride, cap. 1. p. 7, et seq. The same essay is inserted in Journ. Phys. t. x. p. 25, et seq. and p. 85, et seq. Mr. Cooper, however, informs us, that he has met with "several cases of complete gutta serena in both eyes, in which there was the freest dilatation and contraction of the pupil;" Dict. of Surgery, Art. "Cataract," p. 296. M. Magendie informs us that the iris does not contract by the application of a mechanical stimulus; El. Physiol. t. i. p. 73.

light is the agent in this case; perhaps it may be referred to some mechanical condition of the organ into which it is brought by the contraction of the circular fibres.[9]

Among the accessory parts of the eye are certain secretory glands, of which there are two species: the lachrymal gland, by which the tears are secreted, and the sebaceous glands, named after their discoverer Meibomius. The lachrymal gland is situated under the anterior part of the upper eyelid; it is of the conglomerate class, and is provided with a number of excretory ducts, which gradually discharge the fluid over the surface of the cornea. Its office would seem to be to preserve the part in a moist state, to remove the extraneous bodies which may accidentally enter the eye, and to prevent the friction of the lids upon the ball.[1] The glands of Meibomius are situated between the duplicature of the lids and secrete the semi-fluid substance, which has been generally supposed to be of an unctuous nature, but which we are informed by M. Magendie possesses the characters of albumen.[2] In ordinary cases, we may therefore presume, that it is mixed with, or dissolved in the tears,

9 Fontana, in the work referred to above, cap. 2. p. 17, et seq. endeavours to prove, that the expanded state of the iris, and consequent contracted state of the pupil, is its natural condition; one of the principal arguments which he employs is, that the pupil is in the state of extreme contraction during sleep, when all the parts of the body are supposed to be relaxed.

1 Blumenbach, Inst. Physiol. § 268. p. 152, with Dr. Elliotson's note.

2 El. Physiol. t. i. p. 46.

and renders them better adapted for their office of lubricating the cornea. The motion of the lids is so contrived as to diffuse the tears over the surface of the eye, while the superfluous quantity is carried off by the puncta lachrymalia, and conveyed along the ducts into the nostrils.

It is unnecessary to offer any observations upon the palpebræ, the cilia, and the supercilia, farther than to remark, that they are all well adapted for their office of protecting the eye in the different circumstances in which it is placed, and for enabling it to exercise its functions in the most perfect manner.[3]

One of the most elaborate parts of the mechanism of the eye is the system of muscles which are attached to the globe. Of these there are six to each eye; four, named from their form and position, straight, and two oblique. The minute description of these muscles falls under the province of the anatomist. The effect which will be produced by their contraction may be learned by an inspection of the mode in which they connect the globe of the eye with the contiguous parts; it is sufficient to say, that by their means we are enabled to move the eyes in

[3] These parts are fully described by Porterfield, in his Treatise on the Eye, v. i. book 1; by Haller, under the denomination of "Oculi Tutamina," in El. Phys. lib. xvi. sect. 1; and by Magendie, under that of "parties protectrices de l'œil," El. Physiol. t. i. p. 40 . . 7; they are figured by Sœmmering with great minuteness in his Icones Oculi Hum. tab. 2.

all possible directions, with the greatest facility and correctness.[4]

Besides the optic nerve, which I have noticed above as belonging to the second order of parts, the eye and its appendages are plentifully provided with nerves derived from other sources.[5] These serve,

[4] The first correct descriptions of the muscles of the eye were given by Fallopius, in his "Observationes Anatomicæ," Op. t. i. p. 379, 0; by Fabricius, "De Oculo," cap. 11. Op. p. 194, 5. tab. 1. fig. 6, 7, 8; and afterwards by Winslow, Mem. Acad. pour 1721, p. 310, et seq. and Anatomy, sect. 10, art. 2. § 5. A very full account of these muscles, as well as of every other part connected with the eye, may be found in the elaborate treatises of Porterfield, Ed. Med. Ess. v. iii. p. 163 . . 177, and "On the Eye," v. i. p. 79 . . 95. See also Haller, El. Phys. xvi. 2. 24, 5; and Hunter on the Animal Œcon. p. 253 . . 7. We are indebted to Sœmmering for an accurate delineation of these muscles, Icones Oculi Hum. tab. 3, 4; and to Mr. Bell for a minute investigation of the effects which are produced by their contraction; Phil. Trans. for 1823, p. 172, et seq. pl. 21.

[5] It is remarked, that in all animals which are provided with a nervous system, a great proportion of the nervous matter is appropriated to the eye: in fishes, according to Haller, as much as nine-tenths of the nerves are distributed to this organ; El. Phys. xvi. 2. 26. An account of the various sources from which the different parts of the eye and its appendages receive their nerves is contained in § 27 . . 30. See also Mr. Bell's plates of the nerves, No. 1. In Phil. Trans. for 1823, p. 289, et seq. Mr. Bell has applied his ingenious hypothesis of the double office of the nerves to those of the eye, and has pointed out the functions which the different nerves that are sent to this organ respectively perform. We have a good view of the nerves of the eye, as well as of the nerves that are sent to the other organs of sense, in the splendid "Icones Anatomicæ" of Langenbeck, fascic. 2. tab. 2.

some of them, to give the muscles the power of voluntary motion, and others to assist in the various functions which depend, either directly or indirectly, upon the co-operation of the nervous system. The researches of Mr. Bell have enabled us to appropriate, with considerable correctness, the respective offices of the different nerves that are sent to the parts about the eye; while we, at the same time, derive from his remarks a strong confirmation of the opinions which he entertains upon the subject of the nervous system generally, to which I have already had occasion so frequently to refer.

Before we dismiss this part of the subject, it will be proper to make some observations on the use of the crystalline lens. The structure of this body, its situation, and its connexion with the contiguous parts, many of which are, like itself, of a very elaborate organization, would seem to point it out as serving some important purpose in the œconomy of the eye. It was accordingly supposed by many of the older anatomists to be the immediate seat of vision, until Kepler demonstrated its refractive power, and showed, by the experiment related above, that the retina is the part on which the visible impression of objects is received. But it is not probable that the sole use of the crystalline depends upon its refractive power; for, had its place in the eye been occupied by the vitreous humour, and, more especially, had this humour received only a slight increase of density, the rays of light would have been brought to a focus on the retina, without the aid of the crystalline. And we find that even in those eyes, where, in consequence of disease, the crystalline has been artificially extracted, if the operation be successfully performed, and no displacement of the parts be produced, the vision is but little impaired, or at least is rendered nearly perfect by the use of a convex lens. Still it is inconsistent with the views which we entertain of the nature of the animal œconomy to suppose that such an organ should not serve some specific purpose, and accordingly physiologists have assigned three different uses for which this part would seem to be adapted, while, at the same time, they have conceived these points to be necessary for the perfect exercise of the function of vision.[6]

The three objects to which I refer are to correct the spherical aberration, to prevent the unequal refraction of the differently coloured rays, and to assist in the adaptation of the eye to distinct vision at different distances. With respect to the first of these objects, when a number of parallel rays of light pass through a spherical lens of equal density in all its parts, according to the laws of optics, the focus will be imperfect, but, if the lens be of the nature of that of the crystalline, composed of layers gradually increasing in density as we approach its centre, we shall have the rays brought to a proper focus.

6 M. Magendie, in opposition to all other modern physiologists, appears disposed to limit the use of the crystalline to increasing the brightness and clearness of the image by diminishing its size; El. Physiol. t. i. p. 67, 72.

The same remarks, both as to the defects arising from a sphere of uniform density, and the mode of rectifying the defect, apply to what has been termed the Newtonian aberration: the progressively increasing density of the different layers of the crystalline will, as it is supposed, render the eye an achromatic instrument, and thus prevent the confusion of colours which would be produced without this contrivance. Although these remarks are founded upon correct optical principles, it has been questioned, both on theoretical grounds, and by a reference to experiment, how far they will apply to the eye, or at least how far we are able to detect their operation. It has been calculated by Maskeleyne, that no perceptible aberration, depending upon the different refrangibility of the prismatic colours, will take place in the eye [7], and it is stated as a matter of fact, that in

7 Euler advanced the opinion, that the different humours of the eye are so adjusted to each other, as to render it achromatic; Mem. Berlin, pour 1747, p. 279. Maskelyne, however, argues that the reasoning of Euler is not correct, as applied to the actual constitution of this organ. He further calculates the amount of the aberration which would necessarily take place in the eye, and concludes that " the real indistinctness...... will be fourteen or fifteen times less in the eye than in a common refracting telescope, which may be easily allowed to be imperceptible." Phil. Trans. for 1789, p. 256, et seq. Porterfield, on the contrary, supposes that the aberration arising from the different refrangibility of the rays of light is very much more considerable than that depending upon the mere form of the lens; On the Eye, v. i. p. 378, 9. And we have an experiment of Dr. Wollaston's, by which the eye is sensibly proved to be not achromatic; Young's Lect. v. ii. p. 584. We have some judicious observations on this point by Dr. Hall,

those eyes, where the crystalline has been removed, we do not perceive the defects which are supposed to be remedied by its presence.

The third use which has been assigned to the crystalline is to assist in adapting the eye to the distinct vision at different distances. In the natural and perfect state of the organ, those rays only can come to an exact focus on the retina which enter the cornea in a parallel direction, as proceeding from distant objects. When, therefore, we wish to view near objects, we use a voluntary exertion, by which the shape or conformation of the eye is altered.[8] If we accurately attend to our sensations, we shall find that a specific effort is necessary for this purpose, and that a certain length of time is required for its accomplishment. The nature of this power, or the mode in which the change is effected, has been the subject of much ingenious discussion, and has given rise to

in Quart. Journ. v. v. p. 253. See also Haller, El. Phys. xvi. 4. 11; Blumenbach, Inst. Physiol. § 270. p. 153; Young's Lect. v. i. p. 448.

[8] I must remark, that both M. Magendie and Mr. Bell do not admit that the eye possesses this power. M. Magendie founds his opinion upon an observation which he made on the eye of an albino animal; this is so transparent as to enable us to see the picture at the back of the globe, and he informs us that it is equally distinct for near, as for distant objects; Elem. Physiol. t. i. p. 70..3. Mr. Bell, after reviewing all the hypotheses that have been brought forwards to account for the effect, thinks that they are none of them adequate to the purpose, and concludes that much of what has been "attributed to mechanical power is the consequence of attention merely." Anat. part 2. b. 1. ch. 11. v. iii. p. 334, et seq.

many curious experiments. Three methods have been suggested for this purpose; first, by bringing forward the crystalline nearer to the cornea, without altering the form either of the whole eye or of the crystalline itself; secondly, by changing the figure of the globe of the eye, so as to increase the distance between the cornea and the retina; or, thirdly, without altering the general form of the eye, by increasing the sphericity of the crystalline, and thus giving it an increase of refractive power.[9]

The first of these hypotheses appears to have been generally adopted by the earlier physiologists, and is the one which was maintained by most of the contemporaries of Haller. Porterfield endeavoured to prove that the accommodation of the eye is effected by means of a contractile body which is attached to

9 The subject is discussed by Haller, with his usual learning and candour, and the hypotheses of the various writers who had preceded him are briefly detailed in El. Phys. xvi. 4. 20 . . 7. He is himself inclined to adopt the opinion, that the power of seeing distinctly at different distances depends upon an alteration in the size of the pupil, § 27; an idea which was originally brought forward by Delahire, Mem. Acad. t. ix. p. 620, et seq.; but this opinion is generally supposed to have been disproved by Porterfield; Ed. Med. Ess. v. iv. p. 124, et seq. also book 3. ch. 3. v. i. p. 389, et seq. of his elaborate treatise on the eye. Le Roy, however, again advocated Delahire's hypothesis in opposition to the observations of Porterfield; Mem. Acad. pour 1755, p. 594, et seq. This opinion is likewise adopted by Caldani, Instit. Physiol. p. 211 . . 3, principally, as it appears, on the authority of Haller, and, to a certain extent, by Dr. Knox, whose investigations on the structure and action of the eye appear to have been conducted with peculiar accuracy.

the crystalline, and which has the power, when necessary, of bringing it nearer to the cornea;[1] but this opinion is controverted by Haller, who shows that the body is not contractile, and that, even if it were so, it could not produce the effect which is assigned to it. And this seems to be the case with all the speculations of a similar kind, that neither the nature of the individual parts nor the general structure of the eye admit of that action, which would be adequate to bring about the necessary alteration in the relative position of the different parts of the organ.[2]

The second hypothesis, that which supposes the adjustment of the eye to be effected by some cause

[1] Ed. Med. Ess. v. iv. p. 197, et seq. and "On the Eye," v. i. p. 446, et seq.

[2] An hypothesis nearly resembling that of Porterfield has been lately brought forward by Dr. Knox. In the prosecution of his delicate researches into the anatomy of the eye, he conceives that he has discovered the annulus albus, the part which unites the choroid and sclerotic coats, to be muscular, and accordingly terms it the ciliary muscle: from its structure, and especially from its comparative anatomy, he regards it as a principal agent in the adjustment of the eye; Edin. Trans. v. x. p. 52..6, and p. 250..2. Part of the effect he ascribes to the contraction of the pupil, p. 57; see also Edin. Journ. Med. Scien. v. ii. p. 110, 1. There are some judicious remarks by Winteringham, on these supposed motions of the internal parts of the eye, in his Exper. Inq. p. 286..0. Mr. Crampton has announced the discovery of a muscular structure in the eye of the ostrich, the operation of which, it is conceived, must be to alter the convexity of the cornea, and thus assist in the adjustment of the eye; Thomson's Ann. v. i. p. 170, et seq.

producing a change in the form of the globe, has met with many able advocates, and, among others, Prof. Blumenbach.[3] The four straight muscles of the eye, the tendons of which are applied over a part of its surface, are supposed to be the agents in effecting this change of figure. When these muscles contract, it is supposed that they must compress the ball in such a manner, as to cause a certain degree of protrusion of the cornea, and a consequent increase of the distance between this part and the retina.[4]

A series of well contrived experiments on this subject were performed by Sir E. Home, in conjunction with the late Mr. Ramsden, in which they attempted to prove the actual existence of this increased convexity of the cornea, as well as to show, that an eye from which the crystalline had been extracted was capable of adjusting itself to near objects.[5] Could this have been proved, it would have

[3] Inst. Physiol. § 276, p. 155.

[4] This hypothesis was supported by Dr. Hossack; Phil. Trans. for 1794, p. 196, et seq. Monro (sec.) conceives that both the straight and the oblique muscles, and likewise the orbicularis palpebrarum, by their action contribute to lengthen the axis of the eye; Three Treatises, p. 137. In pl. 4. we have a good view of these muscles. Blumenbach supposes that the change depends upon the action of the straight muscles alone. See Dr. Knox's observations on the insufficiency of this hypothesis, in Edin. Trans. v. x. p. 50.

[5] Phil. Trans. for 1794, p. 21, et seq. for 1795, p. 1, et seq. and for 1796, p. 1, et seq. in which he supports his position by various facts in comparative anatomy; and for 1797, p. 1, et seq. where he farther illustrates it by the morbid actions of the

afforded us an unequivocal demonstration of the truth of the hypothesis; but simple as the experiment may appear, and however easy it might have been supposed to obtain satisfactory evidence on the subject, the question respecting the power of the eye after the removal of the crystalline, appears to be scarcely yet decided.[6]

The third hypothesis, that which attributes the power of adjustment to a change of figure in the crystalline itself, may be considered as having originated with Leeuwenhoek, who conceived that he had detected muscular fibres in the lens, which, by their contraction, would render it more convex, and consequently increase its refractive power.[7] Descartes adopted this opinion, but it does not appear that he

muscles. A good view of this discussion is contained in Nicholson's Journ. v. i. 4to. p. 303, et seq.

6 The affirmative is maintained by Haller, El. Phys. xvi. 4. 25. and is supported by many respectable authorities to which he refers; but, without impeaching his general accuracy, it may be presumed that, upon this point, he was not sufficiently informed. Porterfield had previously given a distinct and apparently correct account of a case, in which the removal of the crystalline deprived the eye of the power of accommodation; Edin. Med. Ess. v. iv. p. 182 . . 6. Dr. Knox, however, supposes that the removal of the lens does not destroy the power of accommodation; Edin. Trans. v. x. p. 56; while we have the high authority of Mr. Travers for the opposite opinion; On the Eye, p. 62.

7 Phil. Trans. v. xiv. No. 165. p. 170, et seq. with the accompanying plate; see also his account of the crystalline of a whale; Phil. Trans. v. xxiv. p. 1723, et seq. tab. 1. fig. 5, 6. Speaking of the crystalline on another occasion, he terms it "crystallinum musculum." Opera, v. i. p. 102.

added any new facts in support of it.[8] It has been lately embraced by Dr. Young, and defended by him with his accustomed ingenuity and acuteness.[9] He rests his opinion partly upon the structure of the crystalline, in which he conceives that he has detected the same fibrous appearance which was described by Leeuwenhoek, but more perhaps from his experiments, in which he shows, that the faculty of adjustment is not prevented by having the eye immersed in water, in which situation its refractive power could not be affected by any alteration in the convexity of the cornea. He also maintains that an

[8] In his Dioptr. cap. 3. "De Oculo," § 5, we have the remark, "Humanum Crystallinum esse masculi instar .... qui totius oculi figuram mutare potest." Op. t. ii. pars 2. p. 66. In his Tract. de Homine, it is stated, that by means of the ciliary ligament, the crystalline can be rendered more or less convex, Op. t. iii. p. 75, but in this passage the muscularity is rather implied than expressed. Pemberton, in his inaugural dissertation, published at Leyden in 1719, argued in favour of the muscular structure of the crystalline, and endeavoured to point out, by a series of mathematical propositions, the mode in which the fibres act in producing the requisite change; "De Facultate Oculi, qua ad diversas Rerum conspectarum Distantias se accommodat." In Haller, Disp. Anat. t. vii. par. 2. p. 139, et seq.

[9] Phil. Trans. for 1793, p. 169, et seq. pl. 20. fig. 2, 3. and Lect. v. ii. p. 523, et seq. Phil. Trans. for 1801, p. 53 .. 83. and Lect. v. ii. p. 573, et seq.; also Lect. v. i. p. 450, 1. and Med. Lit. p. 98, 9. Monro (sec.) admits of the fibrous structure of the crystalline, but he conceives that we have no evidence of its muscularity; On Fishes, ch. 11. p. 79; and Three Treatises, On the Eye, ch. 2. sect. 2. p. 85 .. 7. We have a delineation of the actual appearance of the lens in Sœmmering, Icon. Oculi Hum. tab. 5. fig. 16 .. 9.

eye from which the crystalline has been extracted is incapable of adjusting itself to near objects, but upon this question I am inclined to think that the experiments, considered as leading to a negative result, have not been sufficiently numerous to admit of so important an inference.[1] From a general review of all the facts that we possess on the subject, I feel much disposed to coincide in the opinion of Dr. Young, but at the same time I think it would be desirable to repeat the experiments upon a greater number of eyes that have been deprived of the crystalline, before we can regard the question as decided.[2]

[1] See the experiments of Sir E. Home referred to above. We learn from Wells, Phil. Trans. for 1811, p. 381 .. 5, that after the middle period of life, the eye, in its ordinary state, loses the power of adjustment; hence it follows, that in experiments of this kind, the eyes of young persons only should be employed. He found that the effect of belladonna, when applied to the eye, was not only to expand the pupil, but likewise to destroy the power of adjustment, p. 382 .. 4, and 387, 8. It may be presumed, however, that the loss of the power of adjustment, although contemporary with the expansion of the pupil, is not the effect of this expansion, but that it rather depends upon a paralysis produced in some part of the organ. He argues against the hypothesis of the muscular structure of the crystalline being the medium of the adjustment, because he could never produce any appearance of contraction in this part by the application of stimuli, and because he conceives that its physical properties are not suited to the purpose of contraction, p. 390, 1. See also the remarks of Sœmmering in the description of his plates of the Eye, p. 67, 8.

[2] A new mode of accounting for the change of the eye has been recently advanced by Mr. Travers, which may be regarded

The peculiar formation of the eye which produces short-sightedness, where its refractive power is so considerable, as to cause those rays to form a distinct picture on the retina, which enter the cornea in a diverging state, has been supposed to be analogous to the condition to which the organ is brought when we employ a voluntary effort to view near objects. And as this state of the eye is thought to depend upon the cornea being unusually convex,[3] it has been conceived that the same change of figure must be produced by the adjustment to near objects. But however strong the analogy may appear, its force will

as a combination of the first and third hypotheses; he considers "adjustment as a change of figure in the lens," not, however, from a contractile power in the part itself, but in consequence of the lamellæ of which it is composed sliding over each other, when acted upon by external pressure, while upon the removal of this pressure, its elastic nature restores it to its former sphericity. The iris is supposed to be the agent in this process: the pupillary part of this organ Mr. Travers conceives to be a proper sphincter muscle, which, when it contracts and relaxes, will tend, by the intervention of the ciliary processes, to effect a change in the figure of the lens, which will produce a corresponding change in its refractive power, ". . . . by the steadily contracted state of the pupil suited to the nearest extremity of the focal range, they" (the radiated fibrous processes connected with the iris) "will be closed and braced together; and bearing upon the circumference of the crystalline at every point, will necessarily elongate the axis of the lens." On the Eye, p. 62..7.

[3] Porterfield, Ed. Med. Ess. v. iv. p. 128, 9, 229, and Treatise on the Eye, v. ii. p. 36; Smith's Optics, § 89; Haller, El. Phys. xvi. 4. 15..17; Nicholson's Nat. Phil. v. ii. p. 348; Blumenbach, Instit. Physiol. § 275. p. 155; Bell's Anat. v. iii. p. 233.

be entirely destroyed if we admit the correctness of Dr. Young's experiments mentioned above.

With respect to the state of the eye which produces short-sightedness, we have sufficient evidence that it is hereditary. It is, however, rather the tendency to it than the actual mal-conformation which is so, for we find that very young children are seldom, if ever, short-sighted, but that the affection generally commences at the period when they first begin to apply themselves to books. It is much more frequent among the higher than the lower classes of society, a circumstance which depends partly upon the former being more devoted to literary pursuits, and partly upon the too early and frequent use of glasses, by which any natural tendency which the eye might have to assume this form is confirmed, while the efforts are prevented which it would otherwise make to acquire a distinct view of remote objects. There are also certain occupations, which require the eye to be constantly adapted to the view of minute bodies, where this state of the vision almost universally prevails, while a different mode of life is observed to produce a contrary tendency. Daily observation on the eyes of the short-sighted, proves that this defect is generally connected with an obvious projection of the cornea, but this appears not to be universally the case. There are instances in which it seems that the natural state of the eye, or that which it assumes when no voluntary effort is employed, is the one adapted for viewing near objects, and that, upon whatever cause this state depends, the eye permanently retains it, so that it is, in a great measure, deprived of the power of adjustment. This power is likewise, for the most part, lost in eyes of all descriptions as age advances, but here the eye remains permanently adapted to distant vision.[4]

## § 2. *Of the Nature and Cause of Vision.*

With respect to what may be termed the cause of vision, I have little to observe in addition to what has been said on the subject of nervous action generally. We know that when the impression has been received on the retina, it is transmitted by means of the optic nerve to the sensorium commune, an effect which the older physiologists ascribed to the agency of the animal spirits, which has been more lately referred to a vibration propagated along the part, and still more recently to the operation of the electric fluid. So far as the particular case of the eye is concerned, it may perhaps seem more favourable to the doctrine of vibrations, and indeed it was from some observations made upon the sense of vision that the hypothesis was originally formed.[5] As we have its specific cause so entirely under our

[4] These positions are confirmed by an interesting paper of Ware's, in Phil. Trans. for 1813, p. 31, et seq. to which we have, in the same volume, a valuable appendix by Blagden, p. 110, et seq. See also the paper of Wells referred to above.

[5] Newton's Optics, Quær. 12 . . 4. Op. t. iv. p. 220, 1.

control, we are enabled to make our experiments and observations upon it with more precision than on the other external senses, and one important point which we have been enabled to ascertain is, that when an impression is made upon the extremity of a nerve, the effect remains for some time after the cause is removed. There are many facts which prove this to be the case with respect to the action of light upon the eye. If a burning body be rapidly whirled round, it will produce the appearance of a complete circle of fire.[6] Upon the same principle, if the seven prismatic colours be painted upon a card, which is made to spin upon its centre, no individual colour will be seen, but the eye will receive the general sensation of whiteness, from the combined impression of the whole. These effects depend upon the principle, that the eye retains the impression of the object in each particular part of the circle, until it arrives again at the same point, so that the different or successive impressions are all blended together.

There is another very curious series of phenomena, which are somewhat analogous to the above, as far at least as they depend upon the permanency of the effect after the exciting cause is removed. They were first minutely described by

6 Newton's Optics, Quær. 16. Op. t. iv. p. 222; Porterfield on the Eye, v. ii. p. 223; Hartley on Man, v. i. p. 9, 0; Musschenbroek, Elem. Phys. ch. 33. § 998. p. 418. The general principle is clearly stated by Cullen; Physiol. § 48. p. 45.

Buffon, who named them accidental colours,[7] they were afterwards successively examined by Scherffer, Æpinus, and Darwin, and are now known by the name of ocular spectra.[8] If the eye be steadily directed, for some time, to a white spot upon a dark ground, and be then turned aside, we shall perceive a well defined image of the spot, but the effect will be reversed; the spot will now appear dark and the ground white, and the opposite effect will be produced if we view a dark spot upon a white ground. The same kind of alternation takes place between different colours as between different degrees of light; if, for example, we look at a blue object, the eye acquires a yellow spectrum, while a yellow object produces a blue spectrum. In the same manner red and green alternate with each other, and in short every colour has its appropriate spectral colour, the sensation of which is always produced in the eye, when the primary colour has made a sufficiently strong impression upon it. It may be presumed, that a considerable share of what is termed by painters the harmony of colouring and the richness of effect, as exhibited either in pictures, or in the arrangements of drapery and furniture, depend upon this affection of the eye, the brilliancy of the colours

[7] Mem. Acad. pour 1743, p. 147, et seq. They had been previously described by Jurin, but only in an imperfect manner; see his Essay at the end of Smith's Optics, § 260..6. p. 169.

[8] Journ. Phys. t. xxvi. p. 175, 273, et seq.; Do. p. 291, et seq.; Phil. Trans. for 1786, p. 313, et seq.

being much increased by the position in which they stand with respect to each other.

Besides the effect arising from the permanency of the impression after the removal of the exciting cause, there is another principle, to which this peculiar affection of the retina may be partly referred, that a nerve is unable to persevere in the same kind of action beyond a certain period, in consequence of the occurrence of what has been termed exhaustion.[9] The term was originally derived from the hypothesis of the animal spirits, proceeding upon the idea of there being a limited supply of these spirits in the nerve, which, by a too long continuance of the action, was suspended. The hypothesis itself being without foundation, the explanation that is derived from it must necessarily be so likewise, but in whatever manner we may explain it, the fact is one of constant occurrence, and it frequently assists us in determining whether an action is to be originally referred to the operation of the muscles or the nerves.

By combining these two principles or properties of the nervous system, we seem to obtain an easy

9 Darwin classes the spectra under the two heads of direct and reverse, the first depending upon the permanence of the impression, the second upon exhaustion; Phil. Trans. for 1786, p. 313, et seq., and there appears a real foundation for this distinction. Perhaps the phenomena that are described by Dr. Brewster, designated "affections of the retina, as exhibited in its insensibility to indirect impressions, and to the impressions of attenuated light," Journ. of Science, v. iii. p. 280, et seq. may be, in some measure, explained by a reference to the effects of exhaustion and re-action.

method of explaining the various appearances which are presented by the ocular spectra. In the first case, where we have simply the effect of a greater or less degree of illumination, we may naturally ascribe the effect to the exhaustion of those parts of the retina which had been more strongly excited by the greater force of the impression made upon them. And in the same way we may explain the variations of colour that occur in the second case; for we shall find that the spectral colour is, in every instance, that which would result from a union of all the prismatic colours, except the one to which the eye had been previously exposed, and to the action of which it had consequently become more or less insensible.

It is probable that the formation of these spectra in the eye have frequently given rise to a belief in super-natural appearances. In certain diseased states of the nervous system, the retina is more than usually disposed to retain these impressions, so that, for a long time after the exciting cause has been removed, the spectrum will still remain visible.[1] The same causes which tend to weaken the nervous system, frequently produce a similarly debilitating influence over the mental powers, so as to render

[1] Dr. Alderson has made use of this principle in his ingenious "Essay on Apparitions," and it has been since employed in the same way by Ferriar, and by Dr. Hibbert, in their works on the same subject. We are by this means not unfrequently enabled to explain certain supposed super-natural appearances, the evidence of which is too direct for us to doubt of their actual occurrence, without setting aside all human testimony.

them peculiarly susceptible of being affected by superstition and credulity. The surprize which such appearances must occasion to those totally ignorant of their nature, the terror which is often associated with darkness, concurring with the weakened state of the mind and body, must be conceived, in many cases, adequate to produce the effect, without having recourse to the idea of any intentional deception on the part of the individual concerned, or of the miraculous interference of supernatural agency.[2]

Another circumstance which regards the operation of the nervous system, and which has been thought to favour the hypothesis of vibrations, is, that the power of a nerve in transmitting impressions is destroyed by pressure, while, by the removal of the pressure, the part regains its power, provided its structure be not injured. Now, as we have no proof of the existence of any substance being connected with the nerve or attached to it, which can be regarded as the efficient cause of sensation, it would seem that the effect must be referred to the relation of the different parts of the nerve to each other, and this, it is conceived, may be ultimately resolved into a certain kind of motion among the particles, which motion is successively propagated from one to the other, and is counteracted by pressure.

It has been further urged in support of the opinion, that nervous action essentially consists in

[2] The remarks of Dr. Brewster, referred to above, tend to illustrate this subject; Journ. v. iii. p. 290, 1.

vibrations, that besides light, which is the specific and appropriate cause of vision, the sensation of sight may, under certain circumstances, be produced by other causes, which may all of them be ultimately referred to motion. A smart blow on the eye, friction and pressure upon the ball,[3] and electricity, all produce this effect. It is difficult to conceive how a ray of light, mechanical violence, and electricity, can all have the same action upon the eye, and it may be inferred, that the only common principle on which they can operate, is the production of a certain kind of motion in the retina and the optic nerve. Of the nature of this motion, however, either as inferred from experiment or from hypothesis, it is impossible for us to form any conception; the attempt of Hartley to reduce it to a regular system of vibrations does not tend to throw any real light upon its nature, while I conceive that it is clearly disproved by the discovery of Dr. Philip, that the action can be propagated across the interval of a divided nerve.[4]

There is a singular state of vision, which must be noticed in this place, where the eye exercises its function in a perfect manner, as far as respects the form and position of objects, and even the quantity of light that falls upon their different parts, but

[3] Newton's Optics, Quær. 16. Op. t. iv. p. 222. A curious, and, as it would appear, an accurate account of the effect of strong pressure upon the eye-ball is given us by Elliott, in his "Observations on the Senses of Vision and Hearing," p. 2, 3.

[4] See v. i. p. 255. and v. ii. p. 409.

produces only an imperfect conception of colour. It would appear, that in this condition of the organ, there is not properly a confusion of colours, but that there is either a total incapacity of perceiving colour generally, or an insensibility to perceive certain colours, while there is a sufficiently distinct perception of others.

Numerous cases of this kind are upon record,[5] and we have a minute description given us by Mr. Dalton of this peculiar defect, as existing in his own eyes. He informs us, that when he looks at the prismatic spectrum, he can only distinguish three colours, which would appear to be blue, yellow, and purple, while he is incapable of perceiving either the green or the red rays.[6] The cause of this defect is not known; we are not acquainted with any phy-

[5] One of the earliest is in Phil. Trans. for 1777, p. 260, et seq. by Huddart; the person of whom he gives an account, seems to have had a very clear conception of figure, and of light and shade, but probably no idea of colour of any description.

[6] Manch. Mem. v. v. p. 28, et seq. Mr. Dalton ascribes the defect in his vision to one of the humours of his eye being "a coloured medium, so constituted as to absorb red and green rays principally;" p. 42. but, I believe that this explanation is not considered as satisfactory. He gives an account of another case, p. 37 . . 41. We have two cases by Dr. Nicholls, Med. Chir. Tr. v. vii. p. 477, et seq. and v. ix. p. 359, et seq.; and one by Dr. Butter, in Edin. Phil. Journ. v. vi. p. 135, et seq.; he conceives it to be a physiological and not an optical defect, while Dr. Brewster, in his remarks on the case, supposes that it depends upon a want of sensibility in the retina, analogous to the insensibility of the ear to certain sounds. We have also a case by Mr. Harvey, in Edin. Phil. Trans. v. x. p. 253, et seq.; see also two cases in Brewster's Journ. v. x. p. 153, et seq.

sical state of the organ which could have this effect upon the rays of light, nor does it appear, that we have any facts derived from the other senses, which can guide us in our explanation. It has been attributed to a deficiency in the perceptive powers of the eye, similar to what occurs in the ear of those who are incapable of distinguishing musical sounds. But I conceive it would be difficult to show the analogy between the two cases, nor if it were established, would it throw any light upon the nature of the efficient cause.

## § 3. *Acquired Perceptions of Sight.*

My next object must be to give some account of the acquired perceptions of sight, and the associations which are formed between this sense and the other classes of perceptions of impressions. The most important and curious subject for inquiry which here presents itself, respects the means by which we judge of the distance, magnitude, and position of bodies, or how far we are able to connect the visible impressions which we receive by the eye with the actual condition of the objects.[7] With regard to the method by which we judge of distance, it was formerly supposed to depend upon an original law of

[7] We meet with many valuable and judicious observations on this subject in Reid's Treatise on the Mind, ch. 6. sect. 6; particularly as illustrating the position, that our perceptions bear no necessary resemblance to the impressions made on the organs of sense.

the constitution,[8] and to be independent of any knowledge gained through the medium of the external senses. This opinion was attacked by the celebrated Berkeley, in a treatise remarkable for its acuteness and strength of reasoning, in which he clearly demonstrated, that our knowledge on this subject is acquired by experience and association.[9] This conclusion is fully warranted by many circumstances of frequent occurrence, where we fall into the greatest mistakes with respect to the distance of objects, when we form our judgment solely from the visible impression made upon the retina, without attending to the other circumstances which ordinarily direct us in forming our conclusions.[1]

Although Berkeley, in the establishment of his theory, adduced a variety of facts in its favour, still he was not able to bring forwards any decisive experiment, from which he could directly deduce its truth. Fortunately, however, the means of making an experiment of this kind occurred to Cheselden,

[8] When physiologists speak of certain functions or powers as produced by instinct, it may be presumed that they do not essentially differ from those who consider them as depending upon what have been termed laws of the constitution; see Young's Lect. v. i. p. 449; Monro's Three Treatises, c. 6. sect. 3.

[9] "Essay towards a New Theory of Vision." He thus announces the object of the essay in the first paragraph: "My design is to shew the manner, wherein we perceive by sight the distance, magnitude, and situation of objects. Also to consider the difference there is betwixt the *ideas* of sight and touch, and whether there be any *idea* common to both senses." p. 1.

[1] Smith's Optics, § 160. and Remarks, § 311..320.

the result of which very remarkably coincided, at least in the most important particulars, with the doctrine of Berkeley. I refer to the well known case, in which this eminent surgeon operated on the eyes of an individual who was born blind, and whose sight was not restored until he had attained a sufficient age to give a correct account of his feelings, and of the impressions which he received after he had acquired his new sense.[2] It clearly appears that, in the first instance, he had no correct ideas of distance, and we are expressly told that he supposed all objects to touch the eye, until he had learned to correct his visible, by means of his tangible impressions, and thus gradually to acquire more correct notions of the situation of surrounding bodies with respect to his own person.[3]

[2] Phil. Trans. for 1728, No. 402. p. 447, et seq.; also Anat. p. 300, et seq. See remarks by Smith, Optics, § 132 . . 5.

[3] In the present improved state of surgery, instances are not rare in which persons who are born with cataracts have them afterwards removed, so as to acquire the power of vision, yet it will be found upon inquiry, that cases equally adapted for the experiment with that of Cheselden are seldom to be met with. In a great majority of them, although the state of the eye renders it completely useless with respect to all the purposes of life, still it is sensible to the impression of light, and admits of an indistinct perception of objects, from which an imperfect idea of distance is obtained. It generally happens that the cataracts are removed at an earlier age than in Cheselden's case, or that the individual, from the nature of his education, or the state of his mental powers, is not able to give a correct account of his feelings and perceptions. The case that is related by Ware, Phil. Trans. for 1801, p. 382, et seq. also in Nicholson's Journal, v. i. p. 57,

Proceeding then upon the principle, that our ideas of distance are all of the class which I have named acquired perceptions, it remains for us to investigate the circumstances which assist us in forming our judgment respecting them. We shall find that they may be arranged under two heads, some of them depending upon certain states of the eye itself, and others upon various accidents that occur in the appearance of the objects. With respect to distances that are so short as to require the adjustment of the eye in order to obtain distinct vision, it appears that a certain voluntary effort is necessary to produce the desired effect; this effort, whatever may be its

et seq. must either have been one where the cataract had been incomplete, or where the patient, who was only seven years of age, was not fully able to comprehend the nature of the questions which were proposed to him. For if we receive the account literally, as it is given us by the writer, we must conclude, not only that the patient had correct ideas of visible distance, but of the relative position, and even of the shape and colour of objects; ideas which must either be intuitive or have been acquired by experience. The author has, however, unfortunately overlooked these circumstances, and endeavoured to invalidate the force of Cheselden's reasoning. See the remarks of Prof. Stewart on this case in Edin. Trans. v. vii. p. 2 . . 4. In a late case of cataract, which was operated upon by Mr. Wardrop, the observations may be regarded as confirming those of Cheselden, and are so considered by the author; Phil. Trans. for 1826, p. 529, et seq. In the two cases upon which Sir E. Home operated, the patients had certain indistinct ideas of visible form and colour previous to the operation, yet in the one where the vision was the least distinct, in consequence of the greater opacity of the lens, the author considers the results of his experiments as substantially confirming Cheselden's doctrine; Phil. Trans. for 1806, p. 83, et seq.

nature, causes a corresponding sensation, the amount of which we learn by experience to appreciate, and thus, through the medium of association, we acquire the power of estimating the distance with sufficient accuracy.[4]

When objects are placed at only a moderate distance, but such as not to require the adjustment of the eye, when we direct the two eyes to the object, we incline them inwards, as is the case likewise with very short distances, so that what are termed the axes of the eyes, if produced, would make an angle at the object, the angle varying inversely as the distance. Here, as in the former case, we have certain perceptions excited by the muscular efforts necessary to produce a proper inclination of the axes, and these we learn to associate with certain distances.[5] As a proof that this is the mode by which we judge of those distances where the optic axes form an appreciable angle, when the eyes are both directed to the same object, while the effort of adjustment is not perceptible, it has been remarked, that persons who are deprived of the sight of one eye, are incapable of forming a correct judgment in this case.[6]

[4] These cases fall under the remarks of Berkeley in § 16, although he has not entered upon the consideration of the nature of the effect; Essay, p. 17.

[5] This is the case to which the remarks of Berkeley in § 16. particularly apply; Essay, p. 9.

[6] See Reid on the Human Mind, ch. 6. sect. 22, 3; also Magendie, El. Phys. t. i. p. 87, 8.

When we are required to judge of still greater distances, where the object is so remote as that the axes of the two eyes are parallel, we are no longer able to form our opinion from any sensation in the eye itself. In this case we have recourse to a variety of circumstances connected with the appearance of the object; for example, its apparent size, compared with what we know to be its real size, the distinctness with which it is seen, the vividness of its colours, the number of intervening objects, and other similar accidents, all of which obviously depend upon previous experience, and which we are in the habit of associating with different distances, without, in each particular case, investigating the cause on which our judgment is founded.[7]

It is generally admitted that we judge of the magnitude of objects by experience and association. We know that, according to the laws of optics, the farther an object is removed from the eye, the smaller must be its image on the retina. We find, however, that our opinion respecting the magnitude of bodies is quite independent of the size of this image, but that

7 We have an elaborate examination of this subject by Smith; Optics, § 138, and Remarks, § 235 . . 248. See also Haller, El. Phys. 16. 4. 31. Porterfield enumerates six methods which are employed, according to circumstances, in the judgments which we form of the distance of objects; "their apparent magnitude, the vivacity of their colours, the distinction of their smaller parts, the necessary conformation of the eye for seeing distinctly at different distances, the direction of their axes, and the interposition of other objects;" Ed. Med. Ess. v. iv. p. 282; also "On the Eye," v. ii. p. 409.

we deduce our ideas of its size entirely from our supposed knowledge of its distance.[8] We often commit the most singular mistakes respecting the size of bodies, when we are ignorant of their distance from us, and, more particularly, when we are prevented from correcting our mistakes respecting the distance by the peculiar situation in which the body is placed. The arts of landscape and architectural painting, and, still more remarkably, the science of perspective, depend entirely upon the principle, that we judge of the size of bodies by their distance. If the artist is able to convey to our minds a correct conception of the position in which the different objects are supposed to stand with respect to each other, we immediately conceive of them as presenting the size that they actually possess, without any relation to the space which they occupy upon the canvass.

The third problem which we proposed to investigate, the means by which we judge of the position of bodies, is one that has been supposed more difficult to solve. We know, both from the laws of optics and from the experiment of Kepler mentioned above, that when the rays of light pass through the eye, and are brought to a focus upon the retina, the image is reversed, yet we form a conception of it as existing in its natural position. The question has then been asked, why do the reversed images give a correct

[8] Berkeley's Essay, § 55 .. 64, p. 60 .. 71, et alibi. There are many correct and judicious remarks on the means by which we judge of the distance and magnitude of objects in Hartsoeker's Essai de Dioptrique, art. 13 .. 7, p. 85 .. 8.

perception? When we speak of two points in space, as being one above the other, or one to the right of the other, do we mean to express that there is some natural and necessary connexion between these points and their visible position, depending on the structure of the eye, or on any innate or intuitive perception, or do we acquire our knowledge of visible position, like that of distance and magnitude, by the gradual influence of experience and association? In the case of a blind man, suddenly restored to sight, as in that of Cheselden, would he perceive objects in their erect position, or would he conceive them to be reversed? Berkeley, in conformity with his system, extends his hypothesis to visible position, as well as to distance and magnitude, and supposes that our perceptions respecting it are acquired by experience.[9] The blind man, according to his doctrine, would have no conception of the relative position of the two points, until he had exercised his touch, or had learned from some other source, that one of them was more distant from the surface of the earth than the other, and thus associated his visible with his tangible perceptions.

Porterfield supports the contrary opinion, and endeavours to prove, that there are certain ideas of position implanted in the mind, independently of experience, or of any association with the touch, and

9 Essay, § 88..100, p. 103..118. Haller, El. Phys. 16. 4. 7. agrees with Berkeley; yet he seems to consider it as a difficulty. Smith also refers it to association with the touch; Optics, § 135, 6.

which necessarily directs us in forming our conclusion respecting the relative situation of objects.[1] A similar doctrine is maintained by Reid, who, like Porterfield, lays down certain positions, which he conceives to be original laws of the constitution, and what is a stronger ground, he endeavours to show that we have no evidence of any case in which objects appeared reversed, while both the eye itself and the nerve connected with it were in a sound state.[2]

If I were required to give a direct answer to the question under discussion, I should feel disposed to decide in favour of Reid's opinion, principally from the considerations mentioned above. It may be farther remarked, that Cheselden's case, although perhaps not unequivocal, favours this view of the subject; for it is not probable that a person so intelligent as his patient appears to have been, and who was able to give so full and clear an account of his sensations, would not have been aware of the inverted position of objects, and of their gradually assuming the erect position, had he been obliged to correct his ideas on this point by the operation of experience. The effect that is produced by applying pressure to the ball of the eye seems also in favour of the opinion of Reid; for we find that, upon whatever part the pressure

[1] On the Eye, v. ii. p. 329, 0; and Ed. Med. Ess. v. iv. p. 129, 0.

[2] On the Human Mind, ch. 6, sect. 11, 2. His general proposition is, that we see objects in "the direction of the right line that passes from the picture of the object upon the retina to the centre of the eye." p. 169.

be applied, we have the impression of an obscure circle of light precisely on the opposite side of the eye.

But I conceive that the discussion concerning the supposed want of correspondence between the mental perception and the picture upon the retina, is founded altogether upon an incorrect view of the subject. It seems to proceed upon the principle, that in receiving the impressions of sight, we ourselves view the image on the retina, whereas all that we know is, that the impression is in some way conveyed by the optic nerve to the brain, and constitutes a perception; but we are totally ignorant of the process by which this is effected, nor do we see the nature of the connexion which subsists between the two events.[3] There seems, therefore, no reason

[3] See Young's Lectures, v. i. p. 449. Reid has very satisfactorily shown that our perceptions do not bear any necessary resemblance to the impressions that are made upon the organs of sense, from which they are derived; On the Human Mind, sect. 6. A curious question, which, I conceive, may be referred to this part of our subject, has been lately made the topic of investigation by Dr. Wollaston, the cause of the apparent direction of the eyes of a portrait. By a series of plates, in which, while the eyes remain unchanged, the lower parts of the face are altered, it would appear evident that our conception of the direction of the eyes is, in a great measure, derived from the disposition of the other features, proving that we form these conceptions more by association than by the absolute state of the eye itself; Phil. Trans. for 1824, p. 247, et seq. pl. 9 . . 11. Probably, however, a part of the effect depends upon the small scale on which the drawings are made; were they painted the size of life, I conceive that they would exhibit a very distorted appearance.

why the inversion of the image should lead to the conception of an inverted object rather than the contrary, and hence the question that has been so frequently asked, why do we not see objects inverted? may be answered by asking in return, why should we expect this to be the case? The problem that was proposed by Berkeley, respecting the means by which we acquire our ideas of visible position, is of a more general nature, and one that is highly deserving of our attention;[4] but I conceive that we are scarcely yet in possession of any facts or arguments which can lead to a satisfactory solution of it.[5]

[4] Berkeley distinctly states, that when the blind man first acquired his sight, he "would not think, that any thing which he saw was high or low, erect or inverted." § 95. p. 112. This reasoning proceeds upon the principle, that he would have no conception of visible position, until it was gradually acquired through the medium of the touch; see also § 115 . . 9. p. 134 . . 140; this, it may be observed, is a totally different state from the conception of an inverted object.

[5] Mr. Bell has endeavoured to prove, that we judge of the position of objects by the feelings attendant upon the motion of the muscles of the eye. "When an object is seen," he says, "we enjoy two senses; there is an impression upon the retina; but we receive also the idea of position or relation, which it is not the office of the retina to give. It is by the consciousness of the degree of effort put upon the voluntary muscles, that we know the relative position of an object to ourselves." Phil. Trans. for 1823, p. 178. He illustrates and endeavours to prove his doctrine by a series of experiments, in which, after obtaining an ocular spectrum in the eye, he found that the apparent position of the spectrum followed the motion of the ball, as long as this motion was affected by the contraction of the muscles, but that when the motion of the ball was produced by pressure with the

I must now offer some remarks upon a subject which has given rise to much discussion and to numerous experiments, and upon which it appears that we are still unable to form any decisive opinion; the cause of single vision with two eyes. When the eyes are both of them directed to an object, a separate image is formed upon each of the retinæ, yet the mind forms the conception of only one object. The same question here presents itself as in the former

finger, the association no longer existing, the spectrum did not appear to move; p. 178 . . 0. Mr. Bell's experiments, and the hypothesis which is derived from them, have been controverted by Dr. Brewster, who alleges that, according to the known laws of optics, the apparent motion of the spectrum, when the eye ball is pressed aside by the finger, should be much less considerable than Mr. Bell has supposed it to be, and that this small motion of the spectrum may actually be observed; Edin. Journ. Scien. v. ii. p. 1, et seq. I will not venture to decide upon this point; I am aware of the delicacy of the experiment, and of the great skill and sagacity of Dr. Brewster in investigations of this nature; but I may be allowed to state, that in repeating the experiments, as I conceived with the necessary precautions, my results appeared to agree with Mr. Bell's. But allowing the correctness of Dr. Brewster's observations with regard to the effect produced upon the spectrum by pressing aside the ball of the eye, I still do not perceive that it will influence our conclusion, that in the *ordinary* actions of the organ, our judgment of the relative position of external objects is much influenced by associations formed with the contraction of the muscles of the orbit. The circumstances mentioned by Dr. Brewster, viz. the spectrum following the motion of the head, or that of the whole body, when either the head alone or the whole body is moved, prove no more than that the motion of the muscles of the eye is not the only source whence we derive our ideas of visible position or of visible motion.

case, is there any thing in the nature of vision, or in the constitution of the eye, which causes the object to appear single, or does the effect depend upon association and experience? Are two distinct impressions actually conveyed to the mind, or is there in reality only one perception received by the sensorium?

The opinions that have been formed on this point may be arranged under four heads. It has been maintained by some physiologists, that although a separate impression is made upon each retina, yet in consequence of the conjunction of the optic nerves, these impressions become united, and as it were amalgamated, before they arrive at the sensorium commune, so as to produce only one perception.[6] An idea of this kind seems to have been generally adopted by the older physiologists, derived partly from the fact of our being conscious of only one impression, and partly from the apparent union of the optic nerves in their passage from the retina to the brain, the use of which it was otherwise difficult to explain. I have already had occasion to offer some remarks on the nature of the connexion which exists

[6] This opinion may be considered as sanctioned, to a certain extent, by the authority of Newton; Optics, Quær. 15. Opera, t. iv. p. 221; and was the one supported by Briggs; Nov. Vision. Theor. p. 17 .. 31. The tendency of Kepler's reasoning on this subject appears to be, that when the two retinæ are similarly affected, we cannot distinguish between the two impressions, and therefore conceive of them as constituting only a single impression; Diop. Remarks upon Prop. 62.

between the optic nerves.[7] The subject has since been farther investigated by Dr. Wollaston, who conceives that what he terms a semi-decussation of them takes place, a portion of the fibres of each nerve crossing at the part where they come into contact, and passing on to the opposite side of the brain.[8] The pathological facts from which Dr. Wollaston derived his opinion, prove that a certain consent or sympathy exists between the functions of the retinæ; yet it may be doubted how far it will apply to the explanation of the case now under consideration.

A second opinion that has been maintained on the subject is, that we do not actually receive the perception of the two impressions at the same time, but

[7] Vol. 1. p. 251, 2. For an account of the opinions previously entertained upon this subject, see Porterfield, Ed. Med. Ess. v. iii. p. 196 . . 207, and On the Eye, v. i. p. 189, et seq. B. 2. ch. 9; Boerhaave, Præl. not. ad § 516. t. iv. p. 62, 3; Haller, El. Phys. 16. 2. 2.

[8] Phil. Trans. for 1824, p. 222, et seq. Future observations must determine how far the anatomical facts that have been brought forwards in support of the distinct course of the two optic nerves can be reconciled with the pathological arguments; see Briggs, Nov. Vis. Theor. p. 10, 1; Porterfield on the Eye, v. i. p. 191, 2; Cheselden's Anat. p. 294, 5; Zinn, Descr. Oculi Hum. cap. 9. § 2. We have, however, equally or even more powerful evidence brought forward by Sœmmering in favour of the decussation; De Decussatione Nerv. Opt. in Ludwig, Script. Neur. t. i. p. 127, et seq. The experiments and observations of M. Magendie would tend to the opinion that there is a complete decussation; El. Phys. t. i. p. 63. Mr. Twining has adduced various cases of disease in the optic nerves, from which he argues against their union or demi-decussation: Brewster's Journ. v. ix. p. 143, et seq.

that vision consists in a rapid alternation of the eyes, according as the attention is directed to one or other of them by accidental circumstances. This hypothesis was embraced by Dutours, who attempted to prove it by experiment,[9] and it is the one to which Haller inclines;[1] but it is supposed to be entirely overthrown by an observation of Jurin's, that when we direct both the eyes to an object, we see it with more vividness than when viewed by one alone. This increased vividness he found to be a constant quantity, which, in a sound eye of the ordinary degree of power, he estimated at one-thirteenth of the whole effect.[2] The experiment of Dutours, to which I refer, consisted in directing the sight of the two eyes through two tubes, to the ends of which two glasses of different colours are respectively attached; in this case we have not a perception compounded of the two colours, but we see first one and then the other, or sometimes one appears to be placed over the other, or to be seen with more vividness, but they always remain more or less distinct.

These two hypotheses, although they properly come under our consideration in this place, must be regarded, strictly speaking, as not offering any solu-

[9] Mem. présentées à l'Acad. t. iii. p. 514, et seq. and t. iv. p. 499, et seq.

[1] El. Phys. 16. 4. 10; he seems disposed to refer it, in part at least, to the principle, that the mind is unable to distinguish between two perfectly similar impressions of one kind, whether on the nerves of the eye, the ear, or any other of the organs of sense.

[2] Smith's Optics; remarks, § 697; Porterfield, On the Eye, v. i. p. 71, et seq.

tion of the proposed question, but rather as showing, that the difficulty which was supposed to attach to it does not really exist. The two remaining hypotheses, however, proceed upon the idea that the impressions are both of them separately conveyed to the brain, but that they produce there only one perception. According to the first of these, the effect depends upon some law of the constitution, or some general principle of vision, which enables us to see the object single, independent of any mental impression; while, according to the other, the single perception is not supposed to take place in the first instance, but to be the gradual result of habit and association.

One of the first writers who entered upon the discussion of this question is Porterfield; he endeavoured, by an elaborate and learned investigation of the laws of vision, to show that, from the natural constitution of the organ, we always see objects in their proper situation, and that therefore, as each eye must see the object in the same place, we can have no conception of more than one object.[3] Reid has satisfactorily shown that Porterfield's reasoning is fallacious, and has pointed out various circumstances which are in direct opposition to his conclusion.[4] He, however, adopted the opinion of Porterfield, that single vision is the result of a natural law of the constitution, but he explains it upon a different principle. He endeavours to show that whenever

3 Ed. Med. Ess. v. iii. p. 208, et seq.; On the Eye, v. ii. p. 279, et seq.

4 On the Mind, ch. 6. sect. 10.

the impressions of objects are received upon what he terms corresponding points of the retinæ, such points are similarly situated with respect to their centres, and that, in this case, a single perception is necessarily excited.[5] It is admitted that Reid supports his position by many plausible arguments, but Wells has proved, by an ingenious train of experiments, that it does not hold good in all cases, and that it cannot therefore be considered as a general law of the constitution.[6] He likewise remarks, with great justice, that Reid's hypothesis is not strictly con formable to the anatomical conformation of the eye, the crystalline not being situated exactly in the centre of the organ;[7] and moreover, that it is contrary to the analogy of the general structure of the body, according to which these corresponding points should be both of them within, or both of them without the centres of the retinæ, not as Reid supposes, both of them on the same side of the centres. The experiment of Dutours, which was mentioned above, has also been supposed to be adverse to Reid's hypothesis. If we look through a single tube, to the end of which both a blue and a yellow glass are attached, we perceive a green colour. Now it has been argued, that if the corresponding points

[5] Ch. 6. sect. 13. It is necessary to remark, that Reid uses the term corresponding points physiologically; see p. 285. Smith, on the contrary, employs it anatomically, to designate points that are similarly situated with respect to the centres.

[6] Essay on Single Vision, p. 18, . 32. p. 382, et seq.

[7] Essay, p. 21, . 4.

of the retinæ have a natural sympathy with each other, when we look through two tubes, one of which has a yellow and the other a blue glass, the impressions ought to become united and produce the perception of green; but this is never the case.[8]

The hypothesis which is advanced by Wells to account for single vision, although differing from that of Reid, may be placed in the same class, as supposing it to be derived from a law of the constitution, and to be independent of any mental operation. He performed a series of experiments from which he deduced the conclusion, that objects appear single where they are seen in the direction of the optic axes, and as it appears that this single vision is occasionally produced under circumstances different from those in which the eye is ordinarily placed, it seems to follow, that it cannot be the result of habit or association.[9]

The last hypothesis is the one which supposes, that we naturally see objects double, but that, finding by experience, that one object only exists, we learn to disregard the actual perception conveyed to the mind, and conceive of the object as single. The

8 Wells's Essay; p. 45. note. It would appear that Reid, notwithstanding his habitual candour, was so far influenced by hypothesis, as to affirm that this composition of colours actually takes place; On the Mind, p. 203. Scherffer conceived that he produced a violet colour by looking through a blue and a red glass; Journ. Phys. t. xxvi. p. 284; but the change in this case was not sufficiently decisive to warrant the general conclusion.

9 Essay on Single Vision, part 2. p. 34.. 62.

principal writer who has supported this doctrine is Smith. He proceeds upon the principle of the Berkeleyan theory, extending it to the case of single vision, to which it appears not to have been applied by its author.[1] But notwithstanding the clearness with which it is stated by Smith,[2] I conceive that it is successfully combated both by Reid[3] and by Wells.[4] It is asserted that there is no instance on record, where any one ever acquired the power of single vision, when the optic axes were not similarly directed. It is moreover urged against Smith's doctrine, that in Cheselden's case, and others of a similar kind, double vision has never been observed to occur, while it is remarked, that in infants, and even in blind persons, the eyes always move together, unless from some mechanical or morbid cause, which obviously affects the action of the muscles, or deranges the general functions of the organ.

The effect of delirium and of intoxication have been adduced in favour of Smith's hypothesis; for it is said that in these cases, where the usual train of associations is interrupted, we have double vision produced. But to this it may be answered, that we have here not a mental, but a physical defect, for if we examine the state of the eye in these instances

[1] Reid, indeed, affirms that Berkeley directly maintains this opinion; On the Human Mind, p. 332; but, I believe, it will not be found in his Essay, although it may be supposed to be a necessary consequence from his general principles.

[2] Optics, § 137.

[3] On the Mind, ch. 6. sect. 17.

[4] Essay on Single Vision, p. 9.. 18.

we shall observe, that the eyes do not move in a parallel direction, and that, consequently, the impressions are not made upon corresponding points in the retinæ. Upon the same principle, double vision is always produced, when, from accident or disease, the eyes are prevented from moving in concert, and, however long the irregular motion is continued, it is found that the defect of vision remains, and that we never acquire the power of conceiving the impressions to be single, until the physical defect of the eye be remedied.[5]

In considering this question, we are naturally led to inquire into the cause of the tendency which we observe in the eyes to move in the same direction; is this a natural propensity, or is it acquired by habit? Smith, in conformity with his general principle, argues in favour of the latter opinion; he remarks, that when both eyes are directed to the same object, we see it more distinctly than when viewed by one, and, finding this to be the case, we insensibly acquire the custom of moving the eyes together.[6]

5 Cheselden, indeed, mentions an instance, where a person, in consequence of an injury, had the eyes distorted, and consequently experienced double vision, who afterwards gradually acquired the power of seeing objects single, although the distortion was not removed; Anat. p. 295, 6. But it may be questioned, whether in this case the sight of the distorted eye was not so far impaired as that the patient ceased to attend to the impression. I must remark, however, that Camper admits the correctness of Cheselden's observation, and advocates the hypothesis of Smith; De Visu, in Haller, Disp. Anat. t. iv. p. 243.

6 Optics, § 137. Porterfield also conceives that it depends upon habit, and, in proof of his opinion, remarks, that the eye-lids

The contrary doctrine is adopted by Reid,[7] and I conceive that it is sanctioned by experience; for it would appear that where the organ is sound, and there is no mal-conformation of the neighbouring parts, the eyes will be found to have a natural tendency to move in the same direction, and that this parallelism of motion is observed in the eyes of very young children, and even of blind persons, or of those who have the sight of only one eye, in which cases we cannot suppose that it has been acquired by any operation of habit or association.[8]

Now if the muscles of the eye are so constituted and so connected with the nervous system, that in their natural state they have a tendency to place the eyes in such a position with respect to each other, as that the impressions of an object are formed upon corresponding parts of the two retinæ, it would seem to follow as a probable inference, that some farther purpose was to be obtained, and that there must be a natural sympathy or connexion between the corresponding parts of the retinæ, which, without any mental effort, produces only a single perception. Notwithstanding, therefore, the anatomical argu-

and other neighbouring parts have the same tendency to move in corresponding directions, a circumstance which, I apprehend, is rather unfavourable to his opinion; Edin. Med. Ess. v. iii. p. 255, also On the Eye, v. i. p. 118, and v. ii. p. 326.

7 On the Mind, ch. 6. sect. 10.

8 Dr. Wollaston supposes that the parallel motion of the eyes is connected with the partial union of the optic nerves; Phil. Trans. for 1824, p. 229.

ments that have been adduced against Reid's doctrine of corresponding points, I am disposed to regard it as not without foundation, and that even if this cannot be maintained, that there are stronger and more direct objections against the hypothesis, which accounts for single vision upon the principle of habit and association.

It may be proper to notice in this place that peculiar state of the eyes which produces squinting, as it has been supposed to throw some light upon the theory of single vision. In the individuals who are the subjects of this defect, the eyes do not move in the same direction, and it was supposed by many of the older physiologists, that it depended upon a want of correspondence between the retinæ; and that, in order to produce the same effect upon each of them, it was necessary that the impressions should be made upon different parts of the surfaces.[9] There are two points to be ascertained before we can form a correct judgment concerning the cause of squinting; do the persons who squint use both their eyes at the same time? and, if so, do they see objects double? To both these questions it seems that we must answer in the negative, as we find that, when they look attentively at an object, they never use more than one eye. The immediate cause of the other eye not being directed to the object, or rather

9 Delahire supposed that squinting depends upon the most sensible parts of the two retinæ not being similarly situated with respect to their centres; see his treatise, Accidens de la Vue, § 10, in Mem. Acad. t. ix. p. 530, et seq.

being drawn away from it, appears to depend upon its vision being imperfect,[1] so that if it were directed to the object together with the sound eye, it would produce a confused impression, and it is to prevent this defect that the habit of turning the eye aside is unconsciously acquired. This view of the subject was proposed by Buffon, and our subsequent observations seem to justify his opinion.[2] Hence we perceive that the idea which was formerly entertained respecting the cause of squinting, as depending upon a want of correspondence between the different parts of the two retinæ, is without foundation, and that consequently it throws no light upon the nature of single vision.

[1] A case which is related by Darwin, in Phil. Trans. for 1778, p. 86, et seq. seems however to prove that there are occasional exceptions to this general principle.

[2] Mem. Acad. pour 1743, p. 231, et seq. Jurin had previously refuted Delahire's hypothesis referred to above; Essay attached to Smith's Optics, § 178 . . 194; he ascribes it to a habit acquired early in life of directing only one eye to the object. Porterfield, who considers in detail the phenomena and causes of squinting, enumerates six different circumstances, by which he conceives it to be produced; one of these is that assigned by Buffon; Edin. Med. Essays, v. iii. p. 237, et seq. Dutours proposed a modification of this hypothesis; he supposes that one of the retinæ is, in these cases, more sensible to light than the other, and is consequently turned away from the object; Mem. Présent. à l'Acad. t. vi. p. 470, et seq. Reid, On the Mind, sect. 16; Priestley, On Vision, per. 6, sect. 12. ch. 3; Sir Ev. Home, Phil. Trans. for 1797, p. 12 . . 8; and Mr. Bell, Anat. v. iv. p. 456, et seq. adopt the opinion of Buffon.

# CHAPTER XIV.

## OF HEARING.

After the sense of sight, that of hearing will next claim our attention, both in consequence of its real importance in the various concerns of life, and of the elaborate structure of the organ by which it is exercised. We have also a tolerably correct knowledge of the nature of its specific cause, of the mode in which the ear receives the impressions of sound, and of the manner in which they act upon it. We have, however, a much less perfect idea of the use of the different parts of the ear than of the eye, we have less command over the cause of sound, when we attempt to make experiments upon it, and we are also less able to obtain a perfect knowledge of the acquired perceptions of hearing. In this chapter I shall first give some account of the structure and functions of the ear, and shall afterwards make some remarks upon the acquired perceptions of hearing.

### § 1. *Account of the Structure and Functions of the Ear.*

Sound[1] is excited by the vibration or oscillation of the particles of certain bodies, which, from this

[1] For an account of the production of sound, its transmission from one body to another, and the various modifications which it

circumstance, are termed sonorous. They are of different kinds, and are found in all the three mechanical states in which bodies exist, of solid, fluid, and aeriform. These vibrations are capable of being transmitted from one body to another, either of the same or of different kinds, and are increased or diminished according to the nature of the body by which they are successively received. The air of the atmosphere is the medium by which sound is, in most cases, conveyed to the ear, although we find that both fluids and solids are, under certain circumstances, capable of transmitting it, and probably even with greater force and velocity.[2] If a gun be fired at sea, and the ear be, at the same time, immersed in the water, we receive two impressions of

experiences, it will be sufficient to refer to the learned and elaborate work of Dr. Young; Lect. v. i. No. 31. . 4. An ample list of references is contained in v. ii. p. 264, et seq. The authors who have treated on the "Ear and Hearing," are enumerated in p. 271, 2.

[2] It appears, indeed, that in the ear, the medium by which the undulations of the air are ultimately conveyed to the auditory nerve, is probably a fluid; as it is well known to anatomists, that the internal cavities of the organ, which we presume to be the seat of the perceptions of sound, are filled with a substance of this description. Its existence seems to have been first ascertained by Schellhammer; it was distinctly recognized by Valsalva and others, but the subject was so much elucidated by the researches of Cotunni, that his name is generally attached to the receptacles in which it is lodged; see his treatise De Aquæductibus Auris Humanæ internæ Anatomica Dissertatio; also Meckel's dissertation De Labyrinthi Auris Contentis, where we find the subject amply considered, as well as various other parts of the anatomy of the ear.

sound, the first being the one which is conveyed by the water, in consequence of this body being a better conductor of sound than the air. In the same way, if a loud sound be produced at one extremity of a long series of metallic rods, and the ear be placed at the other extremity, two sounds will be heard, the first of which is conveyed along the metal, and the second through the air.

Sound, like light, is capable of being reflected from a body at a definite angle, and concentrated into a focus, although in a less precise manner. Upon this principle it is that echoes are produced, and that the vibrations which constitute sound are increased by speaking-trumpets, domes, whispering-galleries, &c., which may be regarded as analogous in their operation to convex lenses or mirrors. In the same way sounds are increased by hearing-trumpets, and we presume that the cartilaginous folds of the external ear have a similar effect in receiving sounds, and transmitting them to the internal parts of the organ.

The ear may be considered as consisting of three orders of parts. The first is composed of the external ear, consisting of a cartilaginous body of a peculiar form, which is attached to the integuments of the head, and receives the undulations of sound, and of a tube, called the meatus externus, which conveys the undulations to the second order of parts. These consist of a cavity in the temporal bone, named the tympanum, or drum of the ear, which contains the minutely organized bodies that serve to modify the vibrations of sound, as well as the nervous expansion, which is to be regarded as the immediate seat of the sensation, analogous to the retina of the eye. Lastly, we have the Eustachian tube, a passage which extends from the posterior part of the tympanum into the fauces.[3] The use of the external

[3] The first complete account of the structure of the ear was given by Duverney, in the year 1683; the work contains a number of plates; see Mem. Acad. t. i. p. 256..9. In the following year a valuable treatise on the ear was published by Schelhammer. Valsalva's treatise, De Aure Humana, is regarded as one of the most accurate productions of the anatomists of the seventeenth century. It not only comprehends a very full account of all that was known respecting the ear at the time of its publication, in 1704, but contains many original observations, and is accompanied with a number of engravings, which, although not executed in a style of great elegance, are well adapted to illustrate the text. Eustachius's letter, De Auditus Organis, written in 1562, contains an account of the tube which bears his name; Opusc. Anat. p. 138..0. Fallopius, in his Observ. Anat. p. 364..6, briefly describes the ear, and Fabricius more copiously in his treatise, De Aure, Op. p. 249, et seq. To these we may add the following works as deserving our attention: Perrault, On the Organ of Hearing, in Mem. Acad. t. i. p. 158..161; Winslow, Anat. sect. 10. art. 4. v. ii. p. 312, et seq.; Boerhaave, Prælect. § 547..565, t. iv. p. 139..201, cum notis; Haller, El. Phys. lib. xv. in the three sections of which are considered the structure of the organ, the theory of sound, and the sense of hearing; Cassebohm's Five Treatises, accompanied with numerous figures; Martin's Description of the Ear, in his Philos. Brit. v. ii. p. 219..4; Sabatier, On the Internal Ear, in his Anatomie, t. ii. p. 127..148; Boyer on the same, in his Anatomie, t. iv. p. 136..169; Scarpa, Disquisitiones de Auditu et Olfactu; Monro, sec. in his Three Treatises; Bichat, Anat. Des. De l'Oreille et de ses Dependances, t. ii. p. 472,

parts of the ear in receiving the vibrations of the air is more peculiarly exemplified in some of the inferior animals, in whom it is of considerable size, and is furnished with muscles, which are under the control of the will, and by which its orifice is turned towards the sounding body.[4] By this means the impressions of sound are received in their full force, while the elastic nature of the part tends to increase the vibrations, and to convey them in the most advantageous manner to the tympanum.

et seq.; Caldani, Icon. Anat. pl. 96..0, several of these are original; Saunders, On the Ear, the three first chapters, with the plates; Bell's Anat. v. iii. part 2. book 2. ch. iv. p. 399, et seq.; Young's Lect. pl. 25. fig. 349..351; and the elaborate plates of Sœmmering, Icones Org. Aud. Hum. For the comparative anatomy of the organ, see Cuvier, Leç. d'Anat. Comp. No. 13. t. ii. p. 446, et seq.; and Pohl, Expositio Anat. Org. Aud. per Classes Anim.

[4] In man the auricle is seldom moveable; it has, however, its appropriate muscles, and there are certain individuals who possess a degree of voluntary power over it; Haller, El. Phys. xv. 1. 4. It would seem to be one of those parts, in which an organ, that is eminently useful in some tribes of animals, in others is of little importance, and is therefore imperfectly developed. We are informed, however, by Sœmmering, in his essay, On the Comparative Anatomy of the European and the Negro, that "savages can move their ears at pleasure, and possess the sense of hearing in great perfection." Appendix to White, On the Gradation in Man, p. cxliii. True external ears are only found among the mammiferous quadrupeds, and in some of these they are wanting. For figures of this part, see Cowper, Anat. Corp. Hum. tab. 12. fig. 1; Albinus, Acad. Annot. lib. 6. tab. 4, and his work on the Muscles, tab. 11. fig. 3, 4, 5; Sœmmering's Icones, tab. 1.

The tympanum of the ear, and the parts connected with it, exhibit a minute organization of various structures, osseous, membraneous, and nervous; all of which we conclude serve some specific purpose in the modification of the vibrations, the reception of them by the sensitive part, and the transmission of them, by means of the auditory nerve, to the sensorium commune. The small bones or ossicles of the ear are of a very peculiar form and elaborate structure. There are also many singularly-shaped canals, excavated in the temporal bone, which communicate with the ear; but of the specific use of these parts it may be said that nothing is certainly known.[5] These canals are lined with membranes, and there is a membrane of considerable size, called the membrana tympani, which is stretched across the cavity of the ear, so as entirely to separate the meatus externus from the internal parts of the organ. It is upon this membrane that the chain of ossicles is disposed, being attached one to the other, and con-

[5] Blumenbach differs from most of the modern anatomists in supposing that the number of ossicles is three only, the small lenticular bone being generally absent; Inst. Phys. § 248. p. 143. We have a view of the minute muscles belonging to the ossicles in Albinus's great work, tab. 11, fig. 3, 4, 5. M. Magendie has pointed out some circumstances in the arrangement of these muscles, which appear not to have been previously noticed; Journ. Physiol. t. i. p. 341, et seq. Mr. Chevalier has lately published some minute observations on the ligaments of the ossicles; Med. Chir. Tr. v. xiii. p. 61, et seq. He states the particulars of a case in which the loss of these bones did not destroy the sense of hearing, p. 68, note.

nected with the orifices of the canals, so as to render it probable, that the vibrations of sound are first received upon the membrana tympani, are communicated by this part to the ossicles, and by them to the bony canals.[6] The auditory nerve, which receives the vibrations of the ossicles and canals, and which constitutes the immediate seat of the sense of hearing, is expanded upon the surface of these canals, or on the parts immediately contiguous to them.

We are so far acquainted with the nature of sound, and with the physical properties of the minute parts of the ear, as to enable us to assign their general use, but we do not seem to be warranted in carrying our explanation beyond this point. The use of the membrana tympani has been made a particular object of inquiry: its situation in the centre of the organ, and its connexion with the other parts, as well as its size and structure, seeming to mark it out as serving some peculiarly important purpose in the action of the organ. Sir E. Home had an opportunity of examining the membrana tympani of an elephant, and was able to detect a muscular structure in it, by which it would possess the faculty of contracting or relaxing, according to circumstances,

[6] Scarpa, who devoted much attention to the minute anatomy of the internal parts of the ear, has described a membrane attached to the orifice of the fenestra rotunda, to which he has given the name of tympanum secundarium; this he conceives to be of considerable importance in the functions of the organ, and especially in those cases where the membrana tympani is destroyed or injured; De Struct. Fenest. Rotund. &c. cap. 1, 2. in Roemer, Dilectus, v. 1, p. 1, et seq.

and from the delicacy of its organization he was led to conjecture, that it might be the part of the ear which was appropriated to the reception of musical sounds.[7] But this hypothesis was overthrown by a case which occurred to Sir A. Cooper, in which a patient, who had the membrane of one ear entirely destroyed, and of the other considerably injured, still retained his power of perceiving musical sounds unimpaired.[8] There are, indeed, certain facts in

[7] Phil. Trans. for 1800, p. 1, et seq. This paper contains a number of observations on the minute structure of the several parts of the ear, their relation to each other, and the nature of the mechanism by which they contract or relax the membrana tympani. The conclusion which the author deduces is, that "the difference between a musical ear and one which is too imperfect to distinguish the different notes in music, would appear to arise entirely from the greater or less nicety with which the muscle of the malleus renders the membrane capable of being truly adjusted." p. 12. He had afterwards an opportunity of examining the membrana tympani of the elephant in a more perfect state, from which he confirmed the general accuracy of his former observations on its muscularity, at the same time that he showed that the dispositions of its fibres differed from that of the human organ; Phil. Trans. for 1823, p. 23, et seq. pl. *3 .. 5*. From the remarks in this paper, it may be inferred, that the author still maintains his former opinion respecting the specific use of the part. Sir E. Home found also that the membrana tympani of the whale is muscular, but that the fibres are not disposed in the same manner as in the elephant; Phil. Trans. for 1812, p. 83, et seq.

[8] Phil. Trans. for 1800, p. 151, et seq. and for 1801, p. 435, et seq. Cheselden was aware that the integrity of this membrane was not necessary for the perfect accuracy of the sense of hearing; Anat. p. 305, 6. See also Haller, El. Phys. xv. 3. 2.

comparative anatomy, which would lead us to suppose, that some of the bony canals are the immediate organs for receiving the impression of musical sounds, if we are to suppose that there is any part specifically adapted for this purpose. It must be confessed, however, that we have little more than mere conjecture to guide us in our attempts to investigate the uses of the different parts that are connected with the tympanum.[9]

[9] Speaking of the ossicles and the other minute parts of the internal ear, M. Magendie remarks, " . . . . on ignore absolument la part que prend à l'audition chacune des parties de l'oreille interne." Elem. Physiol. t. i. p. 121. We have a paper by Vicq-d'Azyr, On the Ears of Birds, compared with those of the mammalia ; the circumstances the most worthy of notice are, that the cochlea and three of the ossicles are wanting, from which we may infer, that these parts are not necessary for the distinct perception of sound ; Mem. Acad. pour 1778, p. 381, et seq. See also Cuvier, Leç. d'Anat. Comp. t. ii. p. 505, and Scarpa's work referred to above, cap. 5. § 10, p. 80, 1. He conceives that the mechanism of the internal parts of the ear in birds indicates the tympanum secundarium to be essential to the most perfect state of the sense of hearing ; § 29, 0. p. 96 . . 8. Dr. Young remarks, " the use of the semi-circular canals has never been satisfactorily explained : they seem, however, to be very capable of assisting in the estimation of the acuteness or pitch of a sound, by receiving its impression at their opposite ends, and occasioning a recurrence of similar effects at different points of their length, according to the different character of the sound ; while the greater or less pressure of the stapes must serve to moderate the tension of the fluid within the vestibule, which serves to convey the impression. The cochlea seems to be pretty evidently a micrometer of sound ;" Med. Lit. p. 98 ; Lect. v. i. p. 387, 8. On the use of the cochlea we have a treatise in two

There are many pathological facts which prove that the integrity of the Eustachian tube is essential to the perfect function of the ear. When, from any cause, this passage becomes closed or obstructed, the hearing is very materially impaired, while it is restored by removing the obstruction. It is, perhaps, not very easy to ascertain in what mode it acts, but it may be concluded, that the proper vibration of the membrana tympani is, in some way, connected with the state of the air in the tube, as it is observed, in examining the ears of different kinds of animals, that the membrane and the tube are always found together, or that one of them never exists in the absence of the other.

The ear is provided with an elaborate structure of muscles, some of which are connected with the external part, and although seldom capable of being used in the human species, are constantly employed by some of the inferior animals. Besides the muscular fibres which are disposed over the membrana tympani, there are delicate muscles attached to the ossicles, which seem intended for the motion of these

parts by Brendel, in Haller, Disput. Anat. t. iv. p. 399, et seq. and p. 405, et seq. entitled, De Auditu in Apice Cochliæ. Sir A. Carlisle has published an elaborate paper on the structure and use of the stapes; Phil. Trans. for 1805, p. 198, et seq. His conclusion is, that "it is designed to press on the fluid contained in the labyrinth by that action, which it receives from the stapedeus muscle, and the hinge-like connexion of the straight side of its basis with the fenestra vestibuli; the ultimate effect of which is an increase of the tension of the membrane crossing the fenestra cochliæ." p. 206.

bodies; we are, however, unable to explain the purpose which is served by their motions, or the effect which these motions will have upon the sense of hearing.

There are two sets of nerves appropriated to the ear, one for the immediate purpose of receiving the impression of sound, and the other for the general purposes of the nervous influence. The first of these is termed the portio mollis of the seventh pair of the cerebral nerves; its ultimate filaments are dispersed over the internal parts of the organ, and more especially through the bony canals which communicate with the tympanum, constituting, as we may presume, the immediate seat of the sense of hearing, analogous to the retina of the eye.[1] The general nerves of the ear are derived from the fifth

[1] For an account of this nerve I may refer to Meckel's (Ph. Fr.) Dissertatio de Labyrinthi Auris Contentis, § 22 . . 4. p. 37 . . 42; to Scarpa's Anat. Disquis. de Auditu et Olfactu, tab. 8. fig. 2. for the distribution of the acoustic nerve through the cochlea; to Sœmmering's Icones Organi Auditus Humani; to his treatise, De Basi Encephali, § 76 . . 0; and to Mr. Bell's Anat. v. iii. p. 437 . . 9. Hunter remarks, concerning the distribution of the nerves in the ears of fishes; "the nerves of the ear pass outwards from the brain, and appear to terminate at once on the external surface of the enlarged part of the semicircular tubes described above. They do not appear to pass through these tubes so as to get on the inside, as is supposed to be the case in quadrupeds; I should therefore very much suspect, that the lining of the tubes in quadrupeds is not nerve, but a kind of internal periosteum." Phil. Trans. for 1782, p. 383. and Anim. Œcon. p. 84. Hunter's idea does not appear to have been confirmed.

pair; they are principally dispersed over the muscular parts, and give the ear the vital properties which preserve it in a due state for executing its various functions.[2]

## § 2. *Acquired Perceptions of the Ear.*

The acquired perceptions of the ear are less numerous and less distinct than those of the eye. This is partly in consequence of the vibrations which constitute sound being less completely under our control, and partly from their physical effects being less understood than those of light. There seems, however, to be sufficient cause to believe, that blind persons judge of the distance, magnitude, and position of objects entirely by experience and association, and it is often very remarkable to observe what precision they acquire in this respect, without any assistance from the sight, the sense which, under ordinary

[2] It would be impossible to describe the complicated ramifications of the fifth pair of nerves in this place. I shall only remark concerning it, that whereas the specific sensibilities of the organs of sense are generally connected with a specific nerve, the fifth pair gives them their general sensibility, and serves to connect them with the other parts of the system. We have an elaborate account of the anatomy of this nerve in Meckel's treatise, "De Quinto Pare Nervorum Cerebri;" its origin is minutely described by Wrisberg, in his "Observationes Anatomicæ de Quinto Pare Nervorum Encephali." But on this subject, as well as on every thing that respects the anatomical structure of the basis of the brain and the connexion of its parts, it will be sufficient to consult the learned work of Sœmmering, "De Basi Encephali," with its engravings and numerous references.

circumstances, we almost exclusively employ on such occasions.[3]

With respect to what may be termed audible ideas of distance, they are gained by comparing the strength of the impression with a previous knowledge of the space which exists between the ear and the sounding body. The audible ideas of magnitude are principally concerned in acquiring a knowledge of the size of apartments, which blind persons are often able to estimate with considerable correctness.[4] This knowledge they acquire by attending to the force of the reverberation which is produced from the walls, and it depends upon their comparing the effect thus produced upon the ear in the case under consideration, with their previous experience in similar circumstances.

It was formerly supposed that we acquire our knowledge of the position of sounding bodies from the direction in which the vibrations enter the ear. But it was correctly remarked by Mr. Gough, that

3 M. Magendie has given us an interesting account of a boy, who, after having been completely deaf until the age of nine, by means of an operation, acquired the perfect use of the ears. Among the most remarkable circumstances of the case, we may notice the difficulty which he had in obtaining a knowledge of the position of sounding bodies, and still more of imitating articulate sounds; it was only after many unsuccessful trials that he could accomplish this object, and even after an interval of some months, his powers in this respect were very limited; Journ. t. v. p. 223, et seq.

4 A remarkable example of this kind is mentioned by Darwin as having occurred in the person of Fielding; Zoon. v. ii. p. 487.

from the formation both of the external ear and of the meatus auditorius, before the undulations of the air can arrive at the tympanum, they must suffer many successive reflections, which would entirely alter their original direction. He endeavoured to account for the effect by supposing that the bones of the skull, in the neighbourhood of the ear, are sensible to the vibrations of sound, and that we form our judgment from the portion of the head which immediately receives these vibrations, or which feels them the most powerfully, and still more, by comparing the effect produced upon the two sides of the head, or upon the parts surrounding the two ears.[5] Upon this principle we may explain the well known fact, that those persons who have lost the use of one of the ears, are less able to judge correctly of the position of sounding bodies, analogous to what we observe in those who have lost the sight of one of the eyes.

Audible impressions are of two kinds; those which produce the mere perception of sound, and those which give rise to what are termed musical tones. This difference is supposed to depend upon the vibrations in the latter case bearing a regular relation to each other, while simple sounds possess no regularity of this kind. When a body is uniform in its texture and its figure, the vibrations of its different parts will be isochronous, so that they will all coincide and leave distinct intervals between them, thus

[5] Manchester Mem. v. v. p. 622, et seq.

constituting musical tones; while, on the contrary, when the body is not uniform, the oscillations will be proportionally irregular, and we shall have mere sound produced, composed of an assemblage of vibrations bearing no relation to each other.

All kinds of elastic bodies, when of the proper form, are capable of producing musical tones. What we commonly employ for this purpose are metallic wires and membranes of various forms, constituting the basis of stringed instruments, and air confined in tubes, producing wind instruments. Sounds, both simple and musical, differ from each other as they are strong or weak; but musical sounds have, besides this, a specific difference, independent of their strength, by which they become what is styled acute or grave, high or low; a difference which is supposed to depend upon the rapidity of the vibrations, those which are the most rapid giving rise to the acute or high tones.

The mental emotions which are associated with certain musical tones are very powerful, and often evidently depend upon education, habit, or some accidental circumstance; but there can be no doubt that certain combinations of sound are naturally grateful to the ear, while others, on the contrary, are harsh and unpleasant. The combinations which are the most agreeable are those in which the number of vibrations that occur in a given time bear a certain geometrical relation to each other. These combinations of tones give rise to what we term harmony, while the succession of tones constitute tune. The science of harmony, or that which treats of the relation which musical tones bear to each other, forms a very important department of mechanical philosophy, while the proper adjustment of the different tones to each other constitutes one of the most refined and elegant of the polite arts. Both of these may be considered as ultimately depending upon the same principle, and capacity in the ear, or in the part of the sensorium connected with it, for perceiving the relation between musical tones. This would seem to be a distinct faculty from the mere sensibility to sound. We frequently observe persons, whose power of hearing is very acute, and who are yet totally devoid of what is termed a musical ear; while, on the contrary, deaf persons often possess the most delicate perception of musical tones.

We are quite ignorant on what this faculty depends, or what it is that constitutes a musical ear. It has been conjectured that the perception of musical sounds may depend upon the vibrations of some particular parts connected with the tympanum, as for example, upon the cochlea, or some of the bony canals, because we find that these parts are the most elaborately formed in certain animals, which have been thought to possess a delicate perception of musical tones, whereas in animals that have acute perceptions of sound generally, but no power of discriminating between different tones, although the organ of hearing is capacious, these parts appear to be less developed. But it is doubtful whether the facts are sufficiently ascertained to support the hypothesis, and we have no account of the comparative state of the ears in different individuals of the same species, which can assist us in the inquiry.[6]

[6] Dr. Wollaston has made us acquainted with a series of curious facts respecting a peculiarity in certain ears, which seem to have no defect in their general capacity of receiving sound, or in the perception of musical sounds in particular, which yet are insensible to very acute sounds. This insensibility commences when the vibrations have arrived at a certain degree of rapidity, beyond which all sounds are inaudible to the ears so constituted. Thus we are informed that certain individuals are incapable of hearing the chirp of the grasshopper, others the cry of the bat, and one case is mentioned where the note of the sparrow was not heard; Phil. Trans. for 1820, p. 306, et seq. With respect to the limit to the perception of these acute sounds, the author remarks: "The chirping of the sparrow will vary somewhat in its pitch, but seems to be about four octaves above E in the middle of the piano-forte. The note of the bat may be stated at a full octave higher than the sparrow, and I believe that some insects may reach as far as one octave more; for these are sounds decidedly higher than that of a small pipe one-fourth of an inch in length, which cannot be far from six octaves above the middle E. But since this pipe is at the limit of my own hearing, I cannot judge how much the note to which I allude might exceed it in acuteness, as my knowledge of the existence of this sound is derived wholly from some young friends who were present, and heard a chirping, when I was not aware of any sound. I suppose it to have been the cry of some species of gryllus, and I imagine it to differ from the gryllus campestris, because I have often heard the cry of that insect perfectly." p. 312.

## CHAPTER XV.

### OF TOUCH, TASTE, AND SMELL.

The senses of sight and hearing bear a considerable analogy to each other, with respect to the mode in which they receive the impressions of their exciting causes. The agents which affect the retina and the expansion of the portio mollis, are of a totally different nature from the primary causes of the impressions, and are connected with them solely by means of experience and association. But in the other external senses, those of touch, taste, and smell, the immediate exciting cause is either the body itself, or a certain portion of it which is directly applied to the sensitive organ. In gaining our conceptions of the form, taste, and odour of bodies, although we are assisted by comparing the different classes of perceptions with each other, yet, if the organ be in a sound state, we are not liable to the same kind of errors as in those of sight and hearing. We accordingly find, that one great use of touch is to correct the impressions that we derive from the other senses, and that when we have it in our power to apply the organ of touch to a body under examination, it is had recourse to, as the mode which enables us to acquire the most satisfactory information respecting it.[1]

[1] Blumenbach, Instit. Physiol. § 230, p. 133.

## § 1. *Of Touch.*

In popular language, the sense of touch is supposed to receive every impression which is not derived from the other four senses, sight, hearing, smell, or taste; an inaccuracy which probably arose from the term feeling being frequently used as synonimous with touch. Strictly speaking, however, the word touch should be applied to the sense of resistance alone, a sensation which is sufficiently specific, and may be easily distinguished from all other sensations.[2]

The sense of touch, even when restricted to the idea of resistance, is very much more extended in its seat than any of the other senses. Every portion of the external surface, and perhaps even of the internal surfaces, appears to be capable of receiving the impression of resistance; yet there are certain parts which possess a peculiar degree of delicacy in this respect. In man this is the case with the points of the fingers, while in some of the inferior animals the sensibility to touch appears to reside principally in the lips and the tongue. The greater sensibility of these parts is to be attributed, in some measure, to the delicacy of their cutis, to the quantity of blood

[2] Haller notices the distinction, but only in a general way; El. Phys. 12. 1. 1, 2. Blumenbach, however, is disposed to employ the word in its more extended sense; Inst. Physiol. § 227..9, p. 133. M. Magendie's distinction of "tact" and "toucher," may be considered as equivalent to the one that I have proposed; El. Phys. t. i. p. 146.

distributed over their surface, and to their being plentifully supplied with nerves; but we may presume that it depends in part upon the effect of habit, because we find, that certain individuals, who are without hands, have acquired a nearly equal degree of sensibility in the toes, or some other parts of the body.[3]

It is frequently asserted, that the touch is the most certain of all the senses, and the one which corrects the errors into which we are led by the others, especially by the sight and the hearing.[4] This is, to a certain extent, true; for the organ of touch, when it acts, is brought into contact with the body producing the impression, whereas, in the case of the eyes and the ears, we only receive the impression of something that is emitted from the body, or of

[3] Blumenbach, § 231, 2. p. 134. See Malpighi, in Manget, Bibl. Anat. t. i. p. 26, et seq. Grew supposes that he had discovered a peculiar organization in the lines with which the fingers and hands are marked, which was connected with the peculiar sensibility of these parts; but the observation has not been confirmed; Phil. Trans. for 1684, v. xiv. No. 159. p. 566, 7. fig. 1. On the anatomical structure of the parts of the surface which are endowed with the most delicate sensibility, see Riet, De Organo Tactus, in Haller, Disput. Anat. t. iv. p. 1, et seq.; Ruysch, Theat. Anat. 3. tab. 4. fig. 1. and 7. tab. 2. fig. 5. and Cowper, Anat. Corp. Hum. tab. 4. fig. 1 . . 5. for the nervous papillæ of the lips, after removing the epidermis; Albinus, Anat. Acad. lib. i. tab. 1. fig. 1 . . 11. for the integuments, and lib. 3. tab. 4. for the papillæ of the glans penis and mamma; and Caldani, Icones Anatom. tab. 90.

[4] This was a favourite opinion of Buffon, and one on which he expatiated with much eloquence; Nat. Hist. v. iii. p. 294, . 301.

some medium that has been affected by it. Yet, if the perceptions gained by the sense of touch are little subject to error, it must be confessed, on the other hand, that they are very limited, and that our knowledge would be confined within a very narrow range, were we to acquire no ideas but through this sense.[5]

The relation which the touch bears to the other senses, especially to that of sight, is a point that has given rise to much discussion among physiologists and metaphysicians. It has been asked, are our ideas of distance and extension gained by the touch alone, or by the touch associated with the sight? It has likewise been asked, how far we are able to acquire an idea of figure, as distinct from mere extension, by the sight alone, without the aid of the touch?[6]

[5] The form and structure of the human hand enables us to acquire a degree of accuracy in our tangible ideas, much superior to that of any other animal, but this depends merely upon the mechanical convenience of the part. There is a well known case of a female in the country, entirely without either upper or lower extremities, who has supplied the defect of hands by the tongue and lips, combined with the motions of the muscles of the neck.

[6] The celebrated problem which was proposed to Locke by Molyneux refers to this point; whether a blind man, suddenly restored to sight, could distinguish between a globe and a cube which were placed before him, by his sight alone; Molyneux supposes that he could not, and Locke assents to the opinion; Essay, book 2. ch. 9. § 8. Smith agrees with Locke; Optics, § 132; but Jurin offers some very powerful considerations in favour of the opposite opinion; Remarks on Smith, § 161 . . 170. See also the observations of Winteringham, in his Exper. Inq. p. 259.

Notwithstanding the number of celebrated men who have investigated these questions, we are perhaps still unable to give more than a conjectural answer to them; we may, however, venture to assert that the idea of distance and of figure which could be gained by the sight alone must be very imperfect, and frequently altogether incorrect.

We have an ingenious speculation of Condillac, which was intended to elucidate this subject. He supposes a being to be formed resembling the human, in its organs and physical powers, but in the first instance without a nervous system. He then conceives it to be endowed with the single sense of touch, and examines what ideas would be conveyed to it by the surrounding objects, through the medium of tangible impressions alone. He afterwards gives it the other senses in succession, and in each case inquires what would be the result of the successive additions, and how the perceptions would be gradually brought into that state in which they are possessed by a perfectly organized being.

In blind persons the sense of touch supplies many of the impressions which, under ordinary circumstances, are produced by the sight. They are, however, very materially aided by the sense of hearing, more especially in what regards their communication with their fellow-creatures; this sense, through the intervention of speech, being the one which we employ in the common intercourses of society. But it occasionally happens, that we meet with persons who are deprived of the senses both of sight and of hearing, and yet, as far as we can judge, possess the full power of receiving the perceptions of external objects, were they provided with the necessary instruments for acquiring the impressions of them.

An extremely interesting case of this description lately occurred in Scotland, where a man was born blind and deaf, yet whose mental powers appear to have been naturally perfect. He was fortunately surrounded by kind and intelligent relatives, so as to enjoy every advantage of which his bodily situation admitted, for obtaining information by means of the other external senses, and we are indebted to Mr. Wardrop and to Prof. Stewart,[7] for a minute account of the state of his understanding, and of the portion of knowledge which he was enabled to acquire. His conceptions of external objects, most of what may be termed his general or abstract ideas, were principally derived from the touch, and it is not a little curious to observe, with what perseverance he pursued his investigation of the various objects that were presented to him. He appeared to possess a peculiar delicacy of touch, and still more of smell; and by means of these senses alone he acquired a knowledge of the nature and presence of surrounding bodies,

[7] We have a very ample account of Mr. Wardrop's Memoir in Edin. Med. Journ. v. ix. p. 473, et seq. Prof. Stewart's narrative of the case, accompanied by a variety of details from Dr. Gordon and others, is contained in the Edin. Phil. Trans. v. vii. p. 1, et seq.; the same is inserted in the 3d vol. of his "Elements," p. 401, et seq. See also a paper by Dr. Gordon, Ed. Trans. v. iii. p. 129, et seq. and remarks in Ed. Rev. v. xx. p. 462, et seq.

which would previously have been thought impossible. The general result of the observations that were made upon this person warrants the conclusion of Locke and of Berkeley, that our acquired perceptions are originally derived from impressions made on the external senses, and that when we abstract or generalize our ideas, we do it by comparing and combining the knowledge we have derived from this source.[8]

## § 2. *Senses of Smell and of Taste.*

The senses of smell and of taste are analogous to that of touch in the circumstance of the body itself, which produces the impression, being immediately applied to the organ; and indeed they may, to a certain extent, be regarded as modifications of the latter sense. In the human species they are subservient rather to our gratification and enjoyment than to our existence, but in the inferior animals they seem to be the means by which they receive many of those instinctive ideas that are immediately necessary for the support of the individual and the continuance of the species.

The immediate organ of smell appears to be the

[8] Dr. Hibbert has given us an interesting account of another case of an adult who was born blind and deaf; but, in addition to these privations, there was reason to suppose that he was naturally very defective in his intellectual powers, while no attempts had been made to obviate or remove this deficiency; Edin. Phil. Journ. v. i. p. 171, et seq.

mucous membrane, named Schneiderian, from the anatomist who first accurately described it,[9] which lines the internal parts of the nostrils, and more particularly the turbinated bones. The same membrane, although differing a little in its structure, is continued over all the parts which communicate with the nostrils, especially the various cavities or sinuses that are in the contiguous bones. We may presume that the sense of smell is exercised, to a certain extent, by the whole of this membrane, although some parts of it possess a more delicate sensibility.

The Schneiderian membrane is supplied very plentifully with blood vessels and with nerves; the latter, as is always the case with the organs of sense, being derived from at least two distinct sources, from the first pair, termed the olfactory, and from certain branches of the fifth pair.[1] The olfactory are supposed to be the nerves which are the proper seat of the

[9] See his treatise, De Osse Cribriformi et Sensu ac Organo Odoratus; also, De Catarrhis, lib. 1. sect. 2. ch. 1. p. 149, et seq.; Haller, El. Phys. xiv. 1. 13. We have views of the anatomical relations of the organ of smell in Haller, Icones Anat. fas. 4. tab. 2. Tabulæ Narium internarum; in Santorini, tab. 4. exhibiting the interior of the nose, mouth, and pharynx; in Scarpa, Anat. Annot. lib. 2; in Sœmmering, Icon. Organ. Hum. Olfactus; and in Caldani, Icon. Anat. pl. 101, 2.

[1] Haller, El. Phys. xiv. 1. 18, 19. and xiv. 3, 4. We have an account of the structure and disposition of the first pair of nerves by Hunter; Anim. Œcon. p. 263 . . 5; by Scarpa, Anat. Annot. pl. 1, 2. to the second book; by Sœmmering, De Basi Encephali, § 21 . . 30. and Icon. Org. Olfactus, tab. 2. fig. 3, 4, and tab. 3. fig. 1; by Vicq-d'Azyr, Planches, No. 16 . . 20, 27; and by Metzger, Nervorum primi Paris Historia.

sense of smell,[2] while the branches of the 5th pair serve for the general purposes of the nervous influence.[3] These branches of the 5th pair form a part

[2] Many of the earlier anatomists did not admit this body to be a nerve, but supposed it to be an excretory organ appropriated to the brain; this hypothesis is defended by Diemerbroek, Anat. lib. 3. c. 8. p. 603; his work was published in 1672. Vieussens, who published his "Neurographia" in 1716, supposes it to be the proper nerve of smell; lib. 3. c. 2. p. 163, 4.

[3] M. Magendie has, however, attempted to demonstrate by experiment, that this appropriation of the office of these nerves is incorrect; he conceives that the sense of smell does not depend upon the olfactory nerves, as they have been usually denominated, but upon the branches of the fifth pair, that are distributed over the pituitary membrane of the nostril. As these branches, at the same time, seem to impart to this membrane its general sensitive power, it is not evident, upon this view of the subject what is the use of the first pair of nerves; El. Physiol. t. i. p. 132 . . 4; Journ. t. iv. p. 170, et seq. p. 302, et seq. and t. v. p. 21, et seq. See also Lond. Med. Journ. v. lii. p. 82. We have, on the other hand, a number of judicious observations upon M. Magendie's doctrine by M. Eschricht, the object of which is to prove, that the first and fifth pair of nerves bear the same relation to the organ of smell, that the optic and the auditory nerves bear to the branches of the fifth, as respectively distributed to the eye and the ear; Magendie's Journ. v. vi. p. 339, et seq. M. Dumeril has endeavoured to prove, that fish have no proper organ of smell, but that the part which is usually supposed to be for the purpose of receiving odours, constitutes their organ of taste. The author rests his hypothesis partly upon the assumption, that odours, being essentially of a volatile or gaseous nature, cannot exist in fluids, and partly upon the anatomical structure of the mouth in fishes, and of the nerves which are sent to it. The correctness of the first position may, I think, be reasonably doubted, and with respect to the distribution of the nerves of the part, it may be fairly objected, that their connexions and relations

of the system of nerves, which has been ingeniously developed by Mr. Bell, under the title of respiratory, and consequently, we may presume, that it is upon these that the irritation is produced which excites sneezing. There is a peculiarity in the termination of the olfactory nerve, that it does not, like the optic and the auditory nerves, terminate in a filamentous texture, but is reduced to a pulpy substance, which is, as it were, incorporated with the mucous membrane for which it is destined.

It has been remarked that the organ of smell is very imperfectly developed at birth, and it would appear, that the sensations connected with it are proportionally feeble and indistinct. It is also observed, that in the inferior animals, those in which the organ is of the greatest size and the most elaborate structure, have the sense of smell the most acute, and we are informed, that the same may be observed in the different varieties of the human species, especially in certain tribes among the Africans and the aboriginal Americans.[4]

The sense of taste is seated in the tongue and fauces, and is probably extended even to the gullet, but the most delicate sensibility appears to reside

to each other, are too intricate and numerous to warrant us in drawing conclusions of so much importance as those which M. Dumeril attempts to establish. We have a translation of his paper in Nicholson's Journ. v. xxix. p. 344, et seq. taken from Mag. Encyc. Sept. 1807.

4 Blumenbach, in his first Decas, remarks upon the ninth skull, that of an American Indian, "Olfactus officina amplissima." p. 24.

in the tongue, and more especially in the extreme part of it. The tongue, like other acutely sensitive organs, is plentifully provided with nervous filaments, and with blood vessels, and it has a peculiar papillary structure, which we presume is adapted for receiving the impressions of sapid bodies.[5] Like the other organs of sense, its nerves are derived from different sources, according to the purposes which they are intended to serve. Those that are destined to receive the impressions of taste, are derived from the 5th pair, while the nerves that serve for the motion of the part, or for the general purposes of the nervous influence, proceed from the 8th and 9th pairs.[6]

The senses of smell and of taste are in many respects very intimately connected with each other. They are both of them excited in the same manner by the application of odorous and of sapid particles respectively to the nerves which are appropriated to receive these

[5] Sœmmering, Icon. Organ. Gust. Hum.; for the papillary structure of the tongue, see Morgagni, Advers. Anatom. No. 1. tab. 1. and Ruysch, Thes. Anatom. No. 1. tab. 4. fig. 6. p. 35, 6. Op. t. ii.

[6] Monro, on the Nervous System, tab. 26; Blumenbach, Instit. Physiol. § 238. p. 137; Bell's Anat. vol. iii. p. 161, 2; Meckel, de Quinto Pare Nerv. Cereb. § 100, 1. This affords an exception to the remark in p. 153. respecting the office of the fifth pair. We have an elaborate description of the ninth pair of nerves by Boehmer, in which he gives a full account of its distribution, its connexion with the other nerves, and the various opinions that had been entertained respecting its use; Ludwig, Scrip. Neur. t. i. p. 279, et seq. cum tab. See also Caldani, Icon. Anat. pl. 103, 4. where many of the figures are original.

impressions. In a variety of instances the same substances excite the impressions both of taste and of smell, and although these impressions are generally supposed to be sufficiently specific and distinct from each other, yet a close attention to their nature, and to the manner in which they are produced, leads us to conclude, that in our ordinary conceptions, we frequently confound them with each other. This appears to be more particularly the case with the taste; many of the impressions which are referred to the tongue, and are supposed to result from the application of sapid particles to it, being really produced by odorous effluvia affecting the nerves of the nose.[7] Some physiologists have carried the idea so far as to deny altogether the separate existence of the impressions of taste, conceiving that the tongue and palate are only sensible to resistance, and that there is nothing specific in the nature of their action. This opinion has been supported by experiments, which, it must be acknowledged, reduce the operation of the nerves of taste into much narrower limits than had been formerly assigned to them. Still, however, I think there can be little doubt that the nerves of the tongue are capable of receiving impressions, which cannot be referred either to those of mere touch, or to any other of the primary sources of our perceptions, and that, whereas, in a great number of instances, we have ideas of smell that are unaccompanied by those

7 We have some judicious observations on the relation which subsists between these senses in Caldani, Instit. Physiol. cap. 17. and 18. p. 159, et seq.

of taste, so we have certain ideas of taste that are unconnected with those of smell.[8]

The senses of smell and of taste can scarcely be said to give rise to any of our acquired perceptions, but they present us with many remarkable examples of the effects of habit and association. We may presume that certain odours, and more especially certain flavours, are naturally more agreeable than others, but we find these original tastes to be so much modified by custom and by the various usages of society, that our acquired tastes generally become much more powerful and more difficult to eradicate, than those that are natural to us.

The effect that is produced upon the organs of smell and of taste by the frequent exercise of them is worthy of remark, the first being conspicuous among uncivilized people, and the latter in the most highly refined states of society. The fact is the more worthy of our notice, as in this case there is nothing in the mechanism of the organs, which can

[8] Chevreul classes substances according as they affect both the smell and the taste, the smell only, the taste only, or produce the mere sensation of touch in the tongue; Magendie's Journ. t. iv. p. 127, et seq. and Mem. du Muséum, t. x. p. 439, et seq. Our conceptions of taste are very indistinct, and the terms which we employ to discriminate them are vague and general. Grew made an ingenious attempt to define, with more accuracy, the various flavours of vegetables; Anat. of Plants, § 29. p. 13, 4; but the subject has been since scarcely attended to. Bellini's treatise, De Organo Gustus, contains an account of the doctrines of the mechanical physiologists respecting the mode in which sapid bodies affect the tongue.

render us more dexterous in the use of them, so that this greater acuteness of the sense must be entirely acquired by more minutely attending to our perceptions. By this means we may either actually increase the force of the perceptions, or without increasing their force, we may be rendered more sensible to its operation.

## § 3. *Sensations of Heat and Cold, &c.*

I have already remarked that, besides the organs of what are usually termed the five external senses, there are other parts of the body capable of receiving impressions, which are of a specific nature, and which give rise to perceptions that are specifically distinct from those which have been described above. Some of the most remarkable of these are the sensations of heat and cold, those which attend muscular motion, those of hunger and thirst, and those of the sexual organs.

The sensations of heat and cold[9] are referred, in a great measure, to the surface of the body, and would seem naturally to be felt in nearly an equal degree by every part of it. A considerable difference is, however, produced in certain portions of the skin, from their being habitually more or less exposed to changes of temperature, and there may probably be an original difference in consequence of the number

9 M. Magendie regards these as a species of what he calls "tact;" El. Phys. t. i. p. 150.

of nerves that are sent to a part, the texture of its integuments, or the quantity of blood which is transmitted to it. Whatever hypothesis we may adopt respecting the nature of heat, or its relation to cold, we find, as a matter of fact, that any considerable elevation or depression of temperature causes the sensation in question, and that, in a certain degree, the force of the sensation depends upon the quantity of heat which has been added to, or removed from the body. But we have sufficient proof that the sensations of heat and cold are not in exact proportion to the degrees of heat and cold that are applied, but that they rather depend upon the difference between the previous temperature of the body and that to which it is afterwards subjected.

There are, however, many states of the constitution in which the sensations of heat and cold do not correspond, either with the actual temperature, or the alteration which it experiences. In many morbid conditions of the body the sensations of heat and cold afford no indication of the real temperature, while the actual temperature is occasionally affected without affording us a corresponding change in our sensations. Mental impressions of various kinds have the effect of almost instantaneously increasing or diminishing both the actual temperature and the sensation of it, while we observe that the effects do not bear any exact proportion to each other. In the sensations of heat and cold, as in all the other classes of our sensations, we find that the influence of habit is very powerful, but it would appear that the associations formed with impressions of temperature are not very numerous or very important.

The seat of these sensations is the same with that of the touch, and like this, may be referred to the nerves that are distributed over the cutis.[1] Yet, it appears probable that the two sets of impressions are received by different nerves, because we find that the sensibility to resistance and to temperature are not in proportion to each other, either as existing in different individuals, or in the different parts of the same individual. We have occasional opportunities of observing this difference in certain morbid conditions of the body still more remarkably than in the healthy state. A case of this kind is related by Darwin,[2] and they are not of very unfrequent occurrence, where the general sensibility of the surface is much impaired, while it still retains the impression of temperature, and the reverse, so as to show a want of correspondence between these two powers, which, it may be concluded, could only take place in consequence of their being exercised by different organs. It must, however, be acknowledged, on the other hand, that we are not able to observe any thing

[1] Mr. Bell remarks, that the muscles are comparatively insensible to the impressions of temperature; Phil. Trans. for 1826, p. 175.

[2] Zoonomia, v. i. p. 122. See also the account of Dr. Vieusseux's case, as drawn up by himself with much minuteness; although many of the symptoms are difficult to explain, they clearly point out the difference between the sensations of touch and of temperature; Med. Chir. Tr. v. ii. p. 221,. 3.

in the structure or disposition of the nerves that are dispersed over the surface, which would lead us to infer that their different parts were destined for the exercise of different functions.

A little reflection upon the nature of our own feelings will teach us that the sensations which attend the motion of the limbs are of a specific kind, and are completely different from the sensations of touch strictly so called.[3] These are ultimately to be referred to the contraction of the muscles, this act, as it appears, producing certain impressions which are conveyed to the sensorium commune, and excite corresponding perceptions. These perceptions we learn to associate with peculiar contractions, and in this way we acquire a knowledge of the motions to which these contractions give rise. The acquired perceptions thus obtained we employ in most of the ordinary actions of life; in all the motions both of the limbs and of the trunk, we learn to proportion the effort to the degree of effect which we wish to produce, and thus we gain the habit of performing all the necessary actions, without being conscious of the mental process which is necessary for this purpose. The skill which certain individuals acquire in the mechanical part of music, as well as the great dex-

[3] Mr. Bell's researches on the office of the different parts of the nervous system admirably illustrate the doctrine which is laid down in the text, by proving that there is a set of nerves appropriated to the sensitive faculty of the muscles, distinct from those which give them the power of motion; Phil. Trans. for 1826, p. 163, et seq.

terity of rope-dancers, tumblers, and jugglers, depends in a great measure, upon their accurate perceptions of the contractions of the muscles,[4] a faculty which in this, as in all other analogous cases, is to be referred, in some degree, to an original delicacy in the nerves of the part, and in some degree to custom and education.

We may presume that it is the perceptions which attend the contractions of the muscles which the blind, in many cases, substitute for visible perceptions, and even in those who can see, it is probable that these contractile perceptions, as they may be termed, are connected with visible perceptions, and aid us in judging of the form of bodies, and of many of their mechanical properties. The ideas of tangible extension are principally gained by moving the hand over the surface of the body, in which act the muscles connected with the elbow and shoulder are called into operation. And, upon the same principle, in ascertaining the thickness of a body, we employ the muscles that connect the thumb with the fingers, and thus in each case we receive sensations which are different from those of mere resistance, such as would be produced by the simple pressure of a hard body upon a portion of the cutis.

The sensations of hunger and thirst, and of the sexes, are styled appetites, and consist in uneasy

[4] Mr. Bell, in his remarks on what he terms "the nervous circle which connects the voluntary muscles with the brain," Phil. Trans. for 1826, p. 163, et seq., points out the necessity of perceptive nerves in the voluntary muscles.

feelings, which seem to be produced by a peculiar condition of certain secreting organs, which uneasiness is removed by a change in the condition of these organs.[5] I have had occasion to make some remarks upon the immediate cause of hunger and thirst in the chapter on digestion; they probably depend upon the state of the mucous membrane which lines the stomach and fauces, which is removed by the reception of food and drink. With respect to its relation to the other sensations, every one who attends with accuracy to his own feelings, must be conscious that hunger no more resembles pressure upon the body, than sight resembles a mechanical force applied to the eye-ball.

The associations connected with the peculiar sensations of the stomach are of considerable importance in the relations of life. In all nations, as well the most barbarous as the most civilized, the usual intercourses of society are accompanied by the taking of food, and it may be conceived that the enjoyment derived from this source forms a permanent connexion with the benevolent feelings and promotes their operation. None of the senses exhibit in a more remarkable degree the effect of habit; and there can be little doubt that the great majority of individuals are influenced much more by this principle, than by the actual calls of hunger, in the recep-

[5] We may consider the appetites as perceptions, necessarily connected with the internal organization of the body, and impelling to certain actions, independent of external circumstances; see remarks in Harris's Philos. Arrang. p. 417.

tion of their food, both as to the times of taking it and the nature of the food employed. The accidental associations of the stomach, as well as those of the palate, are often very remarkable, and these are so connected together, that we often experience the sensation of nausea from bodies, which possess no property that can excite vomiting, except that it has a taste similar to some other body which has an emetic effect. We are likewise well acquainted with numerous instances, where vomiting is produced by mere mental affections, unaccompanied by any sensible impression.

The sexual feelings seem to be immediately produced by the presence of the seminal secretion in the vesiculæ seminales, or some of the contiguous parts, and are removed by its discharge. The sensations which depend upon the appetites are some of the most violent to which the animal frame is subject, and exercise a very powerful influence over the actions of the individual. They give rise to many of our strongest and most durable associations, and are the immediate origin of the most important connexions of social life. They exist with very different degrees of force in different individuals, partly, as it would seem, from a difference in the original constitution, and partly, from the influence of custom and external circumstances.

## § 4. *General Remarks on the Perceptions of Impressions.*

When we take a general view of the different classes of the perceptions of impressions, and compare them with each other, notwithstanding their diversity, we perceive many points of resemblance and analogy. As I have already remarked, they have each their specific cause, although the distinct nature of the cause is not, in all cases, equally apparent. The emanations from luminous bodies, the undulations of the air, the effluvia of odorous substances, and the particles of sapid food, are sufficiently specific, and have each of them an organ for their specific operation. This, however, is not so obviously the case with the sense of touch, either as to its cause, or the organ by which it is exercised; the cause being merely the sense of resistance, by whatever body produced, while the nerves which exercise this sense are diffused over the whole surface, so as to lead to the conclusion, that the skin may be the proper organ of touch.

The sensations of temperature have an obvious external cause of a sufficiently specific nature, but we are ignorant of their specific organ, and it remains a question that we are unable to decide, whether there are certain nerves especially appropriated to the impressions of temperature. The sensations of hunger and thirst, and of the sexes, are to be regarded in a different light from those of temperature; we have here an appropriate organ, but it may be doubted whether we have a specific agent, unless we consider the secretions of the parts as such. In the sensations which attend muscular contraction, the will may be considered the agent, while the whole of the muscular system and the nerves of voluntary motion may be regarded as the appropriate instruments.

We have now been considering the nature of sensation, from whatever cause excited, as an effect produced in some part of the body by a certain agent, this effect being transmitted by the nerves of the part to the brain, where it constitutes a perception. According to this view of the subject, sensation would appear to exist in the nerves, and perception in the brain, or at least these are the agents by which the faculties are respectively exercised.[6] But although this may appear the most consistent view of the subject, still it leaves some points unexplained; one of these respects the manner in which we acquire our perceptions of simple pleasure and pain, and refer them to their proper seat.

These sensations must be regarded as distinct from any that have yet fallen under our consideration, and they would appear to be excited by a different process. In every part of the body which is provided with nerves, we possess a degree of feeling

[6] This is substantially the doctrine of Parry, Pathology, § 578, although it appears to differ from it, in consequence of the distinction which I have thought it necessary to make between sensation and perception.

which seems to be independent of external impression; and all such parts, from the operation of various circumstances, both internal and external, become the seat of pain. It would appear that the immediate cause of pain may be resolved into whatever tends to derange the structure or action of the part, every kind of mechanical injury, excessive stimulation, or a morbid condition of any of its constituents.

It may be questioned, whether the physical feelings of pleasure are to be considered, like those of pain, as capable of being received by all the sensitive parts of the body, or whether they are not rather of a more specific nature, and confined to particular organs and structures. In many cases our feelings of pleasure are intimately connected with mental emotions; the mere removal of pain is not unfrequently regarded as a positive enjoyment, and it may be remarked, that where we have distinct perceptions of physical pleasure, they may be generally referred to certain organs connected with the appetites or the external senses.

The perceptions of pain, like those that are derived from the impressions of external objects, are seated in the sensorium commune. This is proved, both by those cases in which the feelings of pain are interrupted by dividing or compressing the nerve, and by those in which the irritation of a nerve produces a conception of pain in a part which no longer exists. We, however, always refer the pain to the part of the body upon which the injury has been inflicted. With respect to pains in the external parts of the body, we have no difficulty in explaining the means by which we refer them to their proper seat, but it is not so easy to account for the way in which it is accomplished with regard to the internal parts, where the seat of the injury is concealed from our view. Do we in these cases judge of the seat of the pain by association and experience, or is there any thing in the structure of the organs which enables us, in the first instance, to form our judgment?

To a certain extent, the knowledge which we possess may be referred to the effect of habit and association. When we receive an impression on any part of the body, from some obvious external cause, which produces a certain sensation in the part, if we again feel the same sensation, we refer it to the same seat. This may apply to all the external parts of the body; and, with respect to the organs of sense, we may conceive that we shall be guided by the specific nature of the agent or of the effect. But this throws no light upon the means by which we are enabled to distinguish the seat of local impressions upon internal organs, especially when these arise from constitutional causes. For example, when disease occurs in the stomach, we have a corresponding sensation in the part, the effect being the direct result of local action, unconnected with any mental operation. In these cases, I am disposed to believe that the ganglia are concerned, and that we are to search for their use, in part at least, as constituting secondary centres of perception to which the action of the nerves is transmitted, and where the painful feeling is actually experienced.[7]

Although the express object of physiology is to give an account of the system in its natural and healthy state, yet we are enabled to derive occasional assistance in our researches, from a knowledge of the diseased actions of the body, which constitutes the science of pathology, as we hence obtain a clearer conception of the modes in which the various parts of the animal frame are connected with each other. On this account it may be proper to notice an important feature in the animal œconomy, one which materially affects its operations and regulates its motions, which may be characterized by the term of self-adjustment. Exposed as the body is, at all times, to a variety of external agents, differing from each other both in their direct and their indirect effects, it was necessary that there should be some kind of corresponding change in the machine, to prevent the irregularities that might otherwise arise in its action. Now we shall find that the different vital functions are so adapted to each other, that their respective defects or excesses are compensated by the extraordinary action of some other function, which extraordinary action is the necessary result of the previous irregularity. It was from observing a number of examples of this kind

7 The different circumstances mentioned by Hartley, prop. 32., although they are all of them more or less employed in ascertaining the seat of internal pains, do not appear to me sufficient to account for the effect.

that a pathological hypothesis was formed, which has long been a favourite doctrine of the schools of medicine, according to which all these trains of actions are referred to the operation of a specific principle, which has been named the vis medicatrix naturæ. But we may venture to affirm, that there is no foundation for this mode of reasoning, as these trains of actions can be referred to no one physical principle, and only agree in their final cause. They resemble each other only in exhibiting examples of the admirable order which pervades all parts of the universe, and which we observe as well in the inanimate, as in the animated parts of creation.

There is a peculiar operation, which is confined to the living body, which tends to preserve the machine in its proper order, and to regulate its motion, which has been styled re-action.[8] This more nearly approaches to what may be regarded as a specific principle, and may perhaps be considered as a mode of self-adjustment, which operates in all cases upon the same substances, and by the intervention of the same

[8] For a variety of valuable observations on this affection of the living system, I shall refer to the Pathology of the late Dr. Parry, a work containing many profound and sagacious remarks on the actions of the animal œconomy and their connexion with each other; § 581, et seq. These sections contain many curious illustrations of the self-adjusting operations. Some important observations on the power of re-action will be found in an essay of Dr. M. Hall's, On the Effects of the Loss of Blood, in which the author offers a salutary caution to the inexperienced or unobserving practitioner against the improvident use of the lancet; Med. Chir. Trans. v. xiii. p. 121, et seq.

functions. If the action of a vital part be, by any cause, diminished, provided the defect be within certain limits only, the diminution of action becomes the immediate cause of an increase of power in the part, by which it is enabled to overcome the obstacle and restore the balance of the system. This capacity of re-action appears to reside both in the contractile and the sensitive parts, and is one of the most efficient means which is employed by the physician for restoring the functions to their state of healthy action, when this has, by any means, become deranged.

## CHAPTER XVI.

### OF THE CONNEXION OF THE PHYSICAL AND THE INTELLECTUAL FACULTIES.

I have now taken a view of the first division of the nervous functions, the physico-sensitive; we must next proceed to consider those that I have termed simply sensitive, such as depend upon the action of the different parts of the nervous system, or of the different nervous functions, on each other. The purely intellectual functions do not properly fall within the province of the physiologist; yet they are frequently so much connected with the sensitive functions, that it will be proper to make some remarks upon the nature of the relation which they bear to each other, and of the influence which the mental powers exercise over those of the body.[1]

Although I endeavoured in the last chapter to restrict the meaning of the word "touch" to what appears to be its correct sense, yet I took occasion to remark, that the exciting causes of all the external senses act by what may be regarded as a species of touch. The rays of light strike the retina of the

[1] Gregory enumerates a knowledge of "the laws of union between the mind and the body, and the mutual influence which they have upon each other," as one of the necessary parts of the education of a physician, and it is evidently no less essential to the study of physiology; Duties of a Phys. p. 93.

eye; the undulations of the air, which constitute sound, communicate their motions to the interior of the ear; the sense of smell is produced by particles emitted from the odorous body, and carried by the air to the nose; while taste is immediately caused by the contact of the sapid body. The ultimate cause of perception is, however, unknown; nothing but experience could teach us that rays of light entering the eye would excite ideas of vision, or that undulations of air would impress the ear with ideas of sound, and we are unable to say why the reverse operations might not have taken place; yet we are sufficiently convinced of the fact by uniform experience. It is upon our ignorance of the connexion which exists between the cause of sensation and the effect produced, that Berkeley founded his celebrated doctrine of the non-existence of matter. The object of his hypothesis is to shew, that the ordinary conception of material particles, which are endowed with a variety of properties, so as to give rise to the phenomena which constitute what we term the material world, is without proof; that the intervention of these particles to the production of perception is not necessary, and that all of which we have any actual knowledge is the existence of certain perceptions, which are the immediate operation of the Deity upon the sentient principle.[2] The full compre-

[2] Berkeley's Principles of Human Knowledge, in which he argues against the commonly received opinion respecting the existence of matter, exhibits the same kind of acute and profound spirit of research which forms so distinguished a feature of the

hension of this subject will probably always elude the grasp of the human faculties; but it does not fall within our province to discuss its merits. The object of physiology does not consist in refined speculations upon the essence of matter, but in observing the changes which are produced upon the living body, and the connexion which these changes bear to each other.[3]

"Theory of Vision." But the subject, unlike the former, is not capable of an appeal to observation and experiment. He has, however, the merit of showing that the opinion which he combats is founded rather upon authority than upon any demonstration of its truth, and that if we admit it, we do so more from its being a convenient method of expressing our conceptions, than from a conviction of its correctness. We accordingly find, that all the attempts that have been made to refute Berkeley's hypothesis have consisted rather in an appeal to popular feeling than in strict philosophical deduction. Even Reid, after arguing with considerable acuteness and efficacy against the ideal hypothesis, deserts his vantage ground, and calls in the aid of certain principles, the existence of which is at least as questionable as that of any part of the system which he combats. It may be remarked, that the fundamental position upon which Berkeley's theory rests, is clearly stated in the writings of Malebranche, although it does not appear in what degree Berkeley derived his doctrine from this source. There is, however, a degree of similarity, as well in the character as in the writings of these philosophers, which renders it not improbable that Berkeley must have been acquainted with the "Search after Truth."

3 It has always, I confess, appeared to me, that the dread with which it has been so much the habit of most of the modern metaphysicians to view the speculations of Berkeley, as leading to sceptical opinions with regard to the important topics of religion and morals, is entirely without foundation. I should as soon expect that a student of physiology would doubt whether he

Since the publication of Locke's Essay on the Human Understanding, it has been generally admitted, that our ideas are primarily derived from impressions made upon the senses.[4] This great philosopher[5] arranged the objects of thought in two great

possessed the power of voluntary motion, because he was not acquainted with the mode in which the will acts upon the muscular fibre, as that a person of common understanding would have his faith and conduct affected by a perusal of Berkeley's works. A great portion therefore of the zeal which was manifested by Reid and his associates, in overthrowing the Berkeleian hypothesis, I regard as altogether unnecessary. At the same time, I think, that the principle which Reid has so clearly laid down, that the mind perceives the impressions of external objects themselves, and not the mere ideas of these impressions, is the obvious and direct expression of the fact.

4 Prof. Stewart observes, that "impressions made on our senses by external objects, furnish the occasions on which the mind, by the laws of its constitution, is led to perceive the qualities of the external world, and to exert all the different modifications of thought of which it is capable." Elements, sect. 4. v. i. p. 99.

5 I cannot mention the great name of Locke, without expressing my admiration of his writings and my veneration for his character. On this subject I shall indulge my readers with a quotation from the Quarterly Review, in which the merits of this philosopher are justly estimated. "There is scarcely one event of our lives, to which we look back with more lively recollection, than to the period when we first read the essay upon the Human Understanding. It still remains in our memory, like an era in the history of our thoughts, from which we seem to date a sort of revolution in the very constitution of our knowledge. For it is not with a view to opinions that the writings of Locke are to be studied; but rather for the sake of witnessing the operation of his mind. There runs through his essay such a vein of precise

divisions, which he termed ideas of sensation and ideas of reflection. The first, comprehending the knowledge which we immediately derive from the impressions of external objects; the second, the ideas which are produced by the operation of the mind upon the materials which it had already acquired, from the impressions made upon the senses. According to the nomenclature which has been employed in this work, the first may be termed perceptive ideas, the second intellectual ideas.[6]

There is no question in the whole circle of the

and admirable reflection; he places his thoughts, right or wrong, in so clear a light; distinguishes and discards all trifling and merely verbal disputes; makes us understand ourselves so unequivocally, in the words which we employ, and in the subjects upon which we are meditating; that we know not any work that could be named in which the exercise of thinking may be so safely taken. This is never so strongly felt as when we come to his writings, fresh from the pages of some modern metaphysician. It is like changing the smoky atmosphere of a city for some pure and mountain air; the mind feels as if it were inhaling health from the very thoughts which it breathes; so much singleness and directness and integrity is there about all his opinions; such a contempt for paradox, such superiority to all the little tricks by which the common-place thoughts of common-place minds are trimmed out in the present day; and decked, if we may so express ourselves, in the mere cast off clothes of real learning and physiology." v. xxvi. p. 487. See also Enfield's History of Philosophy, vol. ii. p. 538, 9. Locke effected for metaphysical science what Haller did for physiology; he showed us the limit of our knowledge, and pointed out the subjects which were the best suited for future inquiry.

6 These terms nearly coincide with Hartley's ideas of sensation and intellectual ideas; On Man, Introd. v. i. p. ii.

sciences, which has been more the subject of disputation, than what respects the connexion between the nervous system and the intellectual faculties. The doctrine which is the most commonly received is, that the mental powers, although connected with the brain, are ultimately to be referred to something independent of matter, while, on the contrary, certain philosophers of great acuteness have maintained that the mind, or that part of our frame which thinks and reasons, is necessarily connected with matter, can never exist but in conjunction with it, and that thought is no more than a property of a peculiar kind of material existence.

It has unfortunately happened that this subject, which is one of great interest and curiosity, has seldom been viewed with that philosophical spirit, which should always direct our investigations, and by which alone we can expect to arrive at truth. It is admitted that certain errors may be so interwoven with our accustomed associations, on topics connected with morals and religion, as to render it doubtful, on some occasions, how far we ought to attempt their removal; but if this concession be made, on the one hand, it is incumbent upon us, on the other, not to inflame the prejudices which may exist on these topics, but to use our endeavours to correct all undue excitement, and thus to bring the mind into that tranquil state, which may enable it to receive truth without the fear of injury. In this spirit of candour and conciliation, I propose to make a few remarks upon this celebrated question.

It is argued by the materialist, that different kinds of matter possess different and specific kinds of properties; some bodies are hard, others elastic, some are endowed with what we term life, others are destitute of it. Living substances again have their distinguishing properties; the muscles are contractile, and the brain is sensitive. These properties of the muscles and of the brain are supposed to be necessarily attached to the respective substances, and to be incapable of existing without them; we can have no contraction without the muscular fibre, and no sensation without nervous matter. But besides sensation, a certain part of the nervous system, the brain possesses another set of properties, peculiar to itself, which have been termed mental, and which collectively are supposed to constitute mind. Mind, therefore, is a faculty, or set of faculties, belonging to the brain, in the same way that contraction is the property of the muscle. It is argued, that we can have no conception of the existence of mind, except as attached to the brain, that it is derived from external impressions acting upon the brain, through the intervention of the organs of sense; that it is more or less perfect, according to the perfection of the brain and its appendages; that it is co-existent with this organ and partakes of its diseases.[7]

7 As this question has been very clearly stated by Mr. Belsham, I shall insert the positions which he has advanced in favour of materialism, in what respects the physiological considerations. "When there is no organization, as far as our observation extends, there is no perception. Wherever such an organic struc-

To this it may be replied, that, although different bodies have different and specific properties, yet that these properties have all a relation to each other, so that their operation may be referred to certain general principles. When a muscle is stimulated to contraction, although we do not see how the stimulus acts upon the fibre, yet we can trace the effect of its contraction, and can observe the relation which it bears to the cause. We cannot explain how light produces vision, but we can point out the mechanical laws by which it operates, and we can experimentally prove that the impression on the retina is the primary cause of the sensation which is transmitted by the optic nerves to the sensorium commune.

But no analogy of this kind can be detected with respect to mind. Even admitting the first step in the process to be beyond our comprehension, we might expect, as in the case of the muscle, and of the eye, to be able to follow up the succession of changes,

ture as the brain exists, perception exists. Where this organization is imperfect, perception is imperfect. Where the organization is sound, vigorous, and healthy, perception is proportionably vigorous and clear. Where the organization is impaired, perception is enfeebled and obscured. And when the organization ceases, perception appears to cease." Elements, p. 333. I feel reluctant to involve the great truths of theology in our physiological discussions, but, I think I may, without impropriety, repeat the remark of Dr. Prichard; "The whole universe displays the most striking proofs of the existence and operation of intellect or mind, in a state separate from organization, and under conditions which preclude all reference to organization." On the Nervous System, p. 52, 3.

and to shew their physical connexion with each other and with the brain. Nothing, however, of this kind can be accomplished.[8] We observe, indeed, a certain correspondence between the development and integrity of the brain, and the perfection of the intellect; but this is explained upon the principle, that whatever be the primary cause of mind, the brain is the organ by which it is manifested, and that, consequently, the proper condition of the organ is as essential, as that of the faculty by which it is directed.

The controversy, therefore, as far as it is not merely verbal, appears to turn principally upon the two following considerations: first, do the phenomena of mind bear such a resemblance to those which are usually ascribed to matter, as to justify us in placing them in the same class, and attributing them to the same organ? And, secondly, is the relation between the condition of the brain and the state of the intellect, such as to indicate that a necessary connexion exists between them.

Our reply to the first of these questions may be founded upon the same kind of reasoning which we employ in the subdivision of what are ordinarily termed the physical properties of matter. There are certain phenomena, which, from their analogy or resemblance, we class together under the denomination of mechanical, referring them all to the operation

[8] On this subject the remarks of Mr. William Belsham appears to me to be very much in point, I would almost say conclusive; Essays, No. 13. v. i. p. 312, et seq.

of an assumed general principle, which we style gravitation. We have, in like manner, another set of phenomena, which in consequence, as we suppose, of their differing essentially from the former, while they resemble each other, we refer to another assumed principle, chemical affinity. We conceive these to be different, because we cannot refer their operation to the same general laws, and because we do not perceive a change in the one to be accompanied by a corresponding change in the other. Upon the same grounds, therefore, that we conceive ourselves justified in supposing gravitation to be a property different from chemical affinity, I should maintain that mental are essentially dissimilar from physical phenomena, and that we must consequently reply to the proposed question in the negative.[9]

It may be admitted, indeed, that the point in discussion is, in some measure, a question of degree, and one which, as it cannot be subjected to the test of experiment, must always remain a matter of mere

9 It may be asserted, that all the physical changes to which bodies are subject, constituting what we term the properties of matter, may be ultimately resolved into certain modifications of attraction and repulsion; and, I conceive, it may with equal confidence be asserted, that the phenomena of attraction and repulsion are in no degree applicable to the operations of the mind. We should be less liable to erroneous conceptions on this subject if we were to follow the suggestion of Locke, book ii. ch. xiii. § 17..9, and, discarding all hypothetical language, were content to speak of the properties merely of matter and of mind, without considering them as attached to any substance or substratum, of which we are entirely ignorant.

opinion. It may be farther urged against the immaterialist, that his decision is founded upon our ignorance; that (to pursue the analogy of the subdivision of the physical powers) in the same way that we have discovered galvanism and magnetism to be modes of electricity, so future discoveries may assimilate mind to matter, demonstrate their necessary connexion with each other, and shew their points of analogy and resemblance. I will not presume to prescribe limits to our discoveries, either in physical or in metaphysical philosophy, but it may be fairly argued that, until such discoveries are made, or until we have some indication of their probability, we are impeding the progress of knowledge, by assuming a possible occurrence as the basis of an hypothesis, and that the cause of truth and knowledge is more effectually served, by arranging phenomena according to their actually ascertained differences, than by attempting to generalize possible, or even imaginary resemblances.

The materialists have, however, seldom gone so far as to assert, that they could point out any real resemblance between the properties that are referred to matter and mind respectively, under the titles of physical and intellectual. But they allege, that the division rests entirely upon the definition which is applied to the former. The immaterialists, it is said, assume a series of properties, which they style physical, and which they restrict to what they denominate matter; but it is maintained that this division is altogether arbitrary, that we are entirely unacquainted with the actual properties of matter, and that, for any thing which can be proved to the contrary, thought is as much entitled to this appellation as hardness or extension. I admit the imperfection of our knowledge, and the narrow limits by which the powers of our comprehension are bounded; but it must be observed that, if we take this view of the subject, we resolve the whole into a mere verbal dispute, respecting the definition of terms; and it will then remain for us to inquire, which of these statements is the most in accordance with our usual conceptions on the subject, and which is the best calculated to afford us accurate ideas respecting the point under discussion.[1]

[1] The most powerful argument of the modern materialists, is derived from our ignorance of the actual nature of matter, and the consequent impossibility of giving a correct definition of it. This is clearly expressed by Priestley, in his Disquisitions, sect. 1, 2; and in his Correspondence with Price; see particularly, On the Nature of Matter, p. 243 . . 256. It is very forcibly urged by Cooper; Tracts, p. 266 . . 286; and must be admitted to be of considerable weight. But, were we to act strictly upon this principle, we should abstain from all controversy upon the subject, and at once confess that it was one concerning which all discussion was vain and useless. What I contend for is, that, regarding this question in the same manner with other philosophical questions, and applying our terms with the same degree of accuracy, and with the same restrictions as in the other analogous cases, we cannot consider the properties of mind and of matter as belonging to the same class, or as referrible to the same agent. Porterfield remarks, that "sense, perception, and thinking, cannot possibly be a mode of motion or figure, nor of any other property or power of matter;" On the Eye, v. ii. p. 215. and, unless we change, not only our technical definition of matter, but

The second question which was proposed proceeds more upon physiological considerations, and is one in which we are able to appeal more directly to the evidence of facts than in the former case. Now these facts, when duly considered, will, I conceive, lead us to a similar conclusion. When we inquire whether the relation between the condition of the brain and the state of the intellect is such as to indicate that a necessary connexion exists between them, we apply to the anatomist, and obtain from his investigations the only data which can enable us to form our decision.

It would be in vain for me to attempt to give even an abstract of the great number of observations which have been made on this subject; but the result of the whole appears to me to shew, that the greatest disproportion exists between the derangements of the brain and those of the mental powers. After the most complete state of insanity, it is often difficult to detect the smallest alteration in the

our conception of its nature, I conceive we must assent to the position. As connected with this topic, I shall recommend to my readers the perusal of the seventh section of Dr. Barclay's Inquiry, a work replete with information respecting the opinions that have been formed by others upon some of the most abstruse points in physiology, and enriched with many original remarks, indicative of a well furnished and capacious mind. In a volume comprising so many intricate discussions, it is impossible that any one who exercises the invaluable and inalienable right of private judgment should in all cases assent to the opinions of the writer; yet I feel myself called upon to acknowledge, not merely the high gratification, but the great advantage which I have derived from its careful and attentive perusal.

structure of the brain; whereas, on the contrary, the brain has not unfrequently been found very considerably disorganized, when no defect had been previously observed in the intellect.[2]

It may indeed be said, that in both these cases we derive our conclusions from insufficient data; that there may, in the first instance, be some slight, although essential, change in the physical state of the brain, and that, in the second, notwithstanding its apparent derangement, still some certain portion may remain unchanged, which is the immediate organ of intellect. But this objection itself favours the opinion, that the relation which the mind bears to the brain is totally different from that which the other functions bear to their organs, and it is from this want of resemblance that I conceive myself warranted in drawing the inference, that mind is not a property of the brain, in the same way that contractility is a property of the muscle, or sensibility of the nerve. Beyond this point I do not presume to extend my inquiry; the nature of the connexion between

[2] In proof of this position, it will be unnecessary to do more than quote the following passage from Pinel: "Il faut convenir cependant que dans d'autres cerveaux d'alienés on ne trouve aucune de ces lesions physiques, aucune alteration dans la structure organique de ces parties, et, ce qui est encore plus decisif, c'est qu'on les remarque quelquefois dans d'autres cas differens, et à la suite de certaines maladies entièrement étrangeres à l'alienation mentale, comme l'epilepsie, l'apoplexie, les convulsions, les fievres ataxiques." Traité sur l'Alienation Mentale, p. 453.—Dr. Burrows, in his late valuable work, has given a full account of the observations of modern anatomists on the state of the brain in insanity; Commentaries, p. 58, et seq.

matter and mind, or the mode in which they act upon each other, is at present completely unknown, nor do I think that we are in possession of any method of investigation by which it is probable that any additional light will be thrown upon the inquiry.[3]

I have stated that impressions made upon the external senses, and carried to the brain, produce perceptions, and that these constitute the origin of all our knowledge of the properties and qualities of bodies. We are ignorant of the nature of the process by which this train of actions is produced, but it appears certain, that some permanent change is left

[3] It were well if on this, as on most other points in which the conduct of the understanding is concerned, we were to follow the sage counsel of Locke, and not attach too much importance to questions that are beyond our power ever to decide upon, or which, after long discussion, are finally resolved into mere verbal controversies. See the Introduction to his Essay, especially, § 2, 7. Before I dismiss the subject, I may remark, that the doctrine of materialism has been proposed under two forms or species, which differ very essentially from each other. The one which I regard as the least incorrect, is that which was supported by Priestley, and which rests principally upon the alleged impossibility of defining matter, or of drawing a distinction between the supposed properties of matter and spirit. The other species of materialism is that in which the phenomena of mind are conceived to depend upon some peculiar substance, which is thought to be of a more refined nature than that which enters into the composition of the body generally; such as the materia vitæ, electricity, or the imaginary ethereal fluid, which has borne so conspicuous a part in both our physical and our metaphysical hypotheses. By a very singular inconsistency, we find that some of those who have been the most warm opposers of what we may term the nominal materialism, have been the advocates of this more gross and palpable form of the doctrine.

in the brain, because when a perception has once existed with sufficient strength, a state nearly resembling it may be produced without the repetition of the exciting cause. The state thus produced constitutes an idea.[4]

Although every one, who reflects upon his own feel-

4 This is, strictly speaking, a perceptive idea. I think it would be convenient to restrict the term idea to an object of thought, as proceeding, either directly or indirectly, from external impressions, and to apply the term conception to that state which is induced by the presence of the body which causes the impression. I am, indeed, aware, that in so doing, I am acting in opposition to the great authority of Prof. Stewart, who defines conception to be "that power of mind, which enables it to form a notion of an absent object of perception;" Elem. ch. iii. v. i. p. 133; but this difference almost unavoidably follows from the mode in which I have thought it necessary to use the term perception. Hume considers "ideas" to be synonymous with "thoughts;" Essays, v. ii. p. 31. Helvetius, whose language on this subject is generally correct, still farther restricts the term idea to what I have styled intellectual, corresponding to Locke's ideas of reflection; Sur l'Esprit, t. i. p. 68. I think that a degree of ambiguity has been produced in the writings of some of the modern metaphysicians, by the manner in which they have employed the words simple and compound ideas; the former being appropriated to those that originate in perception, the latter in intellectual operations; Hartley, Introd. p. ii. But I conceive that the circumstance of their being perceptive or intellectual has no necessary relation to their condition as being simple or compound. Many perceptive ideas are compounded, i. e. we receive perceptions which are themselves composed of more simple perceptions, and which are impressed upon the mind without any decomposition or analysis. I do not mean by this remark to enter upon the discussion, how far our ideas are, strictly speaking, capable of being compounded, generalized, or abstracted; I only

ings, must be aware of the difference between an original perception and its idea,[5] yet it is not easy to

maintain, that the same kind of combination takes place in the one case as in the other. Locke did not make the distinction to which I have referred above; he has complex ideas of sensation, and simple ideas of reflection; Essay, b. 2. ch. 2 . . 12.

5 That state of the mind, in which ideas are mistaken for perceptions, may be regarded as the most unequivocal indication of insanity, as distinct from fatuity, on the one hand, or mere obstinate caprice, on the other. As far as we are to be guided in our conclusions by pathological and anatomical considerations, I should maintain, that in insanity the disease of the mind is frequently independent of, or antecedent to, that of the brain. In delirium we may conceive that the primary disease is in the brain, while in fatuity the brain would appear to be imperfect and incapable of performing its functions. There is also another state, in which the brain is morbidly affected, so as to convey false perceptions to the mind, but where the mind itself is sound, and where the individual is consequently aware of the fallacy of his perceptions. A case of this kind is detailed with great minuteness by Nicolai of Berlin, who was himself the subject of it; Nicholson's Journ. v. vi. p. 161, et seq. see also v. xv. p. 288, et seq. Many accounts of spectres, and [other supposed miraculous sights and sounds, are to be referred to corresponding changes actually produced upon the brain, although considerably modified by the imagination and credulity of the narrators. This point is very ingeniously elucidated by Dr. Alderson, in his Essay on Apparitions; Ferriar, who has subsequently written on the same subject, has not sufficiently distinguished between three sets of causes, all of which may contribute to the result; the peculiar condition af the brain, the mental indisposition under which the individual laboured, and certain natural phenomena, which, without any disease, either of the brain or of the understanding, were not understood, and were therefore referred to some supernatural agency. Cases of this description, where there are false perceptions accompanied by a conviction of their failure, must, I apprehend, furnish an insur-

determine precisely in what this difference consists. Hume, who makes this the express subject of inquiry, only observes, that the idea is less vivid and distinct than the primary perception,[6] an observation which does not afford any adequate solution of the difficulty. But the connexion between perception and ideas, however produced, is one of which we cannot doubt, and it is no less certain, that in some way or other, they are connected through the intervention of the nervous system.

mountable objection to Reid's fundamental position with regard to the mode by which we acquire our knowledge of the external world. On this subject see Parry's Pathology, § 775..782, where we meet with many valuable observations, but I think that the author has incorrectly classed this affection as a variety of insanity, to which it is no more entitled, than a false perception proceeding from a morbid state of the nerve of the finger, or any other part of the body. We have a still later work on the Philosophy of Apparitions, by Dr. Hibbert; it contains much curious information and many valuable remarks upon the different morbid conditions, both of the body and of the mind, which may be supposed to give rise to these appearances; but I think that in some of his metaphysical speculations he is too refined, and that his views may be characterized rather as ingenious, than as correctly deduced from the acknowledged laws of the animal œconomy. A doctrine not very different from the above is broached by Hobbes in his Treatise on Human Nature, p. 102; a treatise which contains much acuteness of remark, delivered in a quaint and antiquated style.

[6] Essays, v. ii. p. 30, 1.

## APPENDIX.

The conclusion which Dr. Hibbert draws from his inquiry is, "that apparitions are nothing more than morbid symptoms, which are indicative of intense excitement of the renovated feelings of the mind. p. 375. I am disposed to dissent from that definition on two grounds; first, I think that these phenomena are not always the result of intense excitement; and, secondly, that they are not simply renovated feelings, i. e. a repetition of previous perceptions. With respect to the first point, I should prefer considering them as morbid symptoms, depending upon some irregularity of the natural functions, and which, according to circumstances, may consist either in an excess or a defect of the ordinary actions of the system. And with respect to the second, there are, I conceive, in most cases of this description, new perceptions produced, which had never previously existed. Consequently the part of the nervous system, whether the brain or the nerve, which is affected, is brought into a state which it had never before experienced.

I am induced to form this conclusion partly from the circumstance of having been myself the subject of this disease. Without going into a minute detail of the case, I shall merely state, that I was labouring under a fever, attended with symptoms of general debility, especially of the nervous system, and with a severe pain of the head, which was confined to a small spot situated above the right temple. After having passed a sleepless night, and being reduced to a state of considerable exhaustion, I first perceived figures presenting themselves before me, which I immediately recognised as similar to those described by Nicolai, and upon which, as I was free from delirium, and as they were visible for about three days and nights with little intermission, I was able to make my observations. There were two circumstances which appeared to me very remarkable; first, that the spectral appearances always followed the motion of the eyes, and secondly, that the objects which were the best defined, and remained the longest visible, were such as I had no recollection of ever having previously seen. For about twenty-four hours I had constantly before me a human figure, the features and dress of which were as distinctly visible as that of any real existence, and of which, after an interval of many years, I still retain the most lively impression, yet neither at the time nor since, have I been able to discover any person, whom I had previously seen, that resembled it.

During one part of this disease, after the disappearance of this stationary phantom, I had a very singular and amusing imagery presented to me. It appeared as if a number of objects, principally human faces or figures, on a small scale, were placed before me, and gradually removed, like a succession of medallions. They were all of the same size, and appeared to be all situated at the same distance from the face. After one had been seen for a few minutes, it become fainter, and then another, which was more vivid, seemed to be laid upon it, or substituted in its place, which, in its turn, was superseded by a new appearance. During all this succession of scenery, I do not recollect that, in a single instance, I saw any object with which I had been previously acquainted, nor, as far as I am aware, were the representations of any of those objects, with which my mind was the most occupied at other times, presented to me; they appeared to be invariably new creations, or at least new combinations, of which I could not trace the original materials.

The circumstance which I at the time considered to be so extraordinary, that the motion of the spectres followed that of the eye, has been also observed by Dr. Brewster, and must therefore be regarded as not so anomalous a fact as I, at the time, considered it to be.[1] Dr. Hibbert supposes this motion of the spectres to prove that the retina itself is the seat of these morbid impressions, and this is the conclusion which naturally presents itself to the mind on the first view of the subject.[2] But perhaps upon further reflection, we may find reason to doubt of the correctness of this opinion. In the first place we have no independent evidence of the eye itself being affected in these cases, while there is every reason to suppose that the brain is the primary seat of the disease. And, in the next place, if we inquire why the mental spectres appear to occupy any definite portion

[1] Journ. of Science, v. ii. p. 8, 9.

[2] Phil. of Apparitions, p. 249 , , 1.

of space, why they seem to be on the right side or on the left side, why they appear at the distance of five, ten, or twenty feet, I apprehend that the answer must be, that our judgment is directed by associations previously formed with states of the brain, which, to a certain extent, resemble the present morbid condition. I have, in a former chapter, stated the circumstances which enable us to judge of the visible position and magnitude of objects, and we may suppose that our ideas of visible motion are derived from associations of an analogous kind. I think it would tend to illustrate this subject, if the patient were directed to observe whether the spectres appear to follow the eye, when the balls are fixed, while the whole of the head is turned round, and likewise what occurs when a degree of vertigo is induced. These observations it did not occur to me to make at the time, nor do I find that they have been made by others.

## CHAPTER XVII.

### OF ASSOCIATION, HABIT, IMITATION, SYMPATHY, INSTINCT, AND IMAGINATION.

Of those classes of vital operations, which I have designated as intermediate between the mental and the physical powers, and as originating, or consisting in the joint operation of both parts of our frame, I shall select the following, as more particularly deserving our consideration, in consequence of the extensive influence which they exert over the animal œconomy; association, habit, imitation, sympathy, instinct, imagination, and volition; upon each of these I shall proceed to offer a few remarks in succession.[1]

[1] It has been a favourite object with some metaphysicians, and among others with Reid, to establish what they conceive to be a complete analogy between the mind and the body, by ascribing to the former a variety of distinct faculties or functions. But this attempt is, I am inclined to believe, at least premature. With respect to the body, we observe that different trains of actions are performed by different organs, and we thence style them different faculties. But we have no independent proof of this being the case with respect to the mind; and, as to the nature of the actions themselves, although it is true that memory and judgment, for example, are different from each other, yet it may be remarked, that as far as we can form any opinion on such subjects, they seem rather to be varieties of the same kind of power, than powers of a totally different nature. See Locke's Essay, b. 2. ch. xxi. § 20. Stewart follows the plan of Reid, and indeed seems to attach more importance to it than Reid himself. Hartley speaks of the

## § 1. *Association.*

When two or more impressions of any kind have been made upon the nervous system, and repeated together for a sufficient number of times, they become associated; so that if one of them only be produced, it will call up the idea of the others.[2] This operation of the animal œconomy is too obvious to

faculties of the mind, but it does not appear that he employs the term in the strict sense in which it is used by Reid and Stewart; On Man, v. i. p. 3.

The faculties enumerated by Hartley are memory, imagination, understanding, affection, and will. Of these the memory and understanding are less the objects of physiological consideration. Reid enumerates among the intellectual faculties, the powers which are immediately derived from our external senses, memory, conception, the power of analyzing and compounding, judgment, reason, taste, moral perception, and consciousness; On the Intellectual Powers, p. 76. Some of these, however, upon Reid's own principles, I conceive, are not to be considered as distinct faculties, but as complex feelings, produced by the joint operation of more simple processes, connected by association. Prof. Stewart, in his Elements, considers in succession, as distinct powers or faculties of the mind, perception, attention, conception, abstraction, association, memory, imagination, and reasoning. Cooper maintains "that all the phenomena of thought may be comprised under perception, recollection, judgment, and volition." Tracts, p. 273.

[2] Hartley's general theorem is as follows: "If any sensation A, idea B, or muscular motion C, be associated for a sufficient number of times with any other sensation D, idea E, or muscular motion F, it will, at last, excite d, the simple idea belonging to the sensation D, the very idea E, or the very muscular motion F." On Man, prop. 20, v. i. p. 102.

have been overlooked by the most casual observer, and frequent allusion is made to it, as well by the philosophers as by the poets and orators of antiquity. It was correctly described by Locke, who appears to have been the first writer that had a clear conception of its importance in the regulation of our thoughts and actions.[3] Since his time it has formed a prominent feature in all our metaphysical systems, and Hartley made it the basis of his theory, forming, as it were, the connecting link between all the vital ope-

[3] Locke, after remarking that "some of our ideas have a natural connexion and correspondence one with another," goes on to state, that "there is another connexion of ideas wholly owing to chance or custom: ideas, that in themselves are not all of kin, come to be so united in some men's minds, that it is very hard to separate them; they always keep in company, and the one no sooner at any time comes into the understanding, but its associate appears with it; and if they are more than two which are thus united, the whole gang, always inseparable, show themselves together." Essay, b. 2. ch. 33. § 5. v. i. p. 420. In the remaining part of the chapter he points out the influence which the association of ideas possesses over many of our principles of action and modes of thinking. The germ of Locke's doctrine may, indeed, be found in Hobbes; he says, "when a man thinketh on any thing whatever, his next thought after is not altogether so casual as it seems to be;" and, referring our ideas to "motion within us," he supposes that the first motion has some effect in bringing on the second; Treatise on Human Nature, p. 104. Berkeley also describes the fact with his usual clearness and brevity of expression; he says, "that one idea may suggest another to the mind, it will suffice that they have been observed to go together, without any demonstration of the necessity of their co-existence, or without so much as knowing what it is that makes them so to co-exist." New Theory of Vision, p. 16.

rations, both physical and intellectual.[4] Adam Smith employed his usual aptness of illustration in describing its influence upon our ideas of beauty, with respect both to objects of taste, and to our sense of moral propriety,[5] and it has been made use of by Darwin to explain many of the most complicated functions of the animal œconomy, as well in its state of health as of disease; but although we shall find its operation to be very extensive, I conceive that both Hartley and Darwin have considerably exaggerated its influence.

Association manifests itself in various ways. Perceptions may be associated with perceptive ideas and with intellectual ideas, and the ideas of each species may be associated with each other, or with ideas of the other species; perceptions may be associated with mechanical actions, and, conversely, mechanical actions with perceptions; mechanical actions may be associated with each other and also with ideas.[6] Examples of all these varieties will readily

[4] Hartley informs us in his preface, that Gay had, a few years before, published a treatise, in which he "asserted the possibility of deducing all our intellectual pleasures and pains from association." This must have been about the year 1730.

[5] Theory of Moral Sentiments, part 5. v. ii. p. 1, et seq.

[6] The different classes of associations are given in some detail by Mr. Belsham in his Elements; ch. 3. sect. 1, 2. p. 22..35; he follows the system of Hartley with very little deviation. Hume conceives that all our associations may be traced to the three principles of resemblance, contiguity in time and place, and cause and effect; Essays, v. ii. p. 73; to these, I presume, we ought to add analogy. Darwin justly remarks, that the general

suggest themselves to the mind, but those with which the physiologist is the most immediately concerned are the associations which take place with muscular contractions.

Darwin lays it down as a law of the animal œconomy, that all animal motions which have occurred at the same time, or in immediate succession, become so connected, that when one of them is re-produced the other has a tendency to accompany or succeed it. Many of those trains of action, which are the most commonly employed in the ordinary concerns of life, are connected together by association; and although, in the first instance, there is, as far as we can perceive, no necessary connexion between the individual actions, and they might even have been associated by mere accident, yet if the conjunction be sufficiently repeated, an association is formed, which can never afterwards be broken. The force of association is so powerful, and its effects are so universal, that it is often difficult to decide, whether any particular actions are connected together by association, or by some other principle of the constitution.

This difficulty may sometimes be resolved, by inquiring into the cause of these complex actions, and observing how they were originally acquired. A

direction of our inquiries and the nature of the information which we store up in the mind, depend very much upon the kind of association which we the most frequently employ; Zoon. v. i. p. 49 . . 3. Priestley's Hartley, Introd. Essays, No. 2. contains " a general view of the doctrine of the association of ideas," which may be perused with advantage.

number of muscles in different parts of the body are employed in progressive motions of various kinds, for example, in walking. We alternately contract the different muscles of the lower extremities; by an effort of the back and loins, we throw the weight of the trunk, alternately, from side to side, and we generally move the arms at the same time, to assist in balancing the body, and preserving its perpendicularity. All this series of complicated motions is so connected together, that it would require a powerful exertion of the will to perform them in a different order, and they proceed with almost as much regularity as the motion of the heart, or any other over which the will has no control. Yet the act of walking is one which can only be acquired by long practice; and we have reason to suppose, that if a person, born without arms or legs, could be afterwards furnished with them when advanced in life, he would be as incapable of walking as of playing upon an instrument of music. There are, on the contrary, other actions strictly voluntary, and which also require the co-operation of many muscles, but which seem to have some necessary connexion with each other. Of this description is swallowing. The muscles of the mouth, lips, cheeks, tongue, throat, and neck, are all concerned; and they are made to succeed each other in such an order, as to produce one of the most beautiful and complicated mechanical actions of which the human body is capable; yet we observe that the infant, immediately after birth, swallows the mother's milk with as much facility as it ever afterwards acquires. This train of actions, therefore, cannot be connected by association.

It hence becomes a curious subject of inquiry, whether we can point out any circumstance which can enable us to discover on what the difference in these two cases essentially depends. What we naturally look to, as the probable means of solving this difficulty, is the nervous system; and we are led to inquire, whether there be any thing in the disposition of the nerves, the source whence they are derived, or the connexion which they have with each other, which can throw any light upon the subject. I think we may venture to assert, that our present knowledge will not afford us any satisfactory solution of the difficulty; but so much has been accomplished in this department of physiology by Mr. Bell, that we may expect to obtain considerable assistance from this source in our future investigations. In the mean time, we seem to be warranted in concluding, that where the association is formed between muscular parts, all of which are entirely under the control of the will, as, for example, between the muscles of locomotion, we may refer the effect to the principle of association; but that in proportion as the trains of actions are less under the control of the will, they seem to be referrible to the effect of association, combined with that of mere nervous connexion.

## § 2. *Habit.*

A principle in the animal œconomy, which is nearly allied to association, and which produces equally powerful effects, is habit.[7] It may be defined, a peculiar state of the mind or body, induced by the frequent repetition of the same act. Habit is proverbially styled a second nature, and there are numerous instances which prove the truth of the remark, for there is scarcely any impression however disagreeable, or any mode of life however repugnant to our feelings, to which by habit we do not become reconciled.[8] The operation of habit has been supposed,

[7] Reid divides what he terms the active powers of man into the three classes of mechanical, animal, and rational, and makes instinct and habit to compose the first class; Essays 3. ch. 1. p. 97, et seq.; but I acknowledge that I do not perceive the propriety either of the denomination or of the restriction. He defines habit, "a facility of doing a thing, acquired by having done it frequently." Ubi supra, p. 117.

[8] Custom and habit are frequently considered as synonimous terms, but, strictly speaking, we must regard the latter as the effect of the former. "Custom is the frequent repetition of the same act; habit is the effect of such repetition." Taylor's Synonyms, p. 52. Cullen points out this distinction; but, when he enters upon a detail of the effects of custom on the animal functions, he frequently violates his own definition. The remarks which he makes are, however, very judicious, and show the powerful effect of this principle upon some parts of the system which might seem to be the least connected with its vital powers. He describes its effects: "1. On the simple solids; 2. On the organs of sense; 3. On the moving power; 4. On the whole nervous power; 5. On the system of blood vessels." Mat. Med. p. 21,, 31.

by some metaphysicians, to be confined to the voluntary actions, or, at least, to such as were originally so, although they may have subsequently become involuntary. But this limitation appears to be incorrect; the perceptions, the intellectual operations, and even the physical functions, are all of them considerably influenced, and, in some instances, even completely reversed by the effects of habit.

But, although the operation of habit is to be observed perhaps equally in the mental and in the physical part of our frame, yet the course of our inquiries will naturally lead us to regard it more as it respects the bodily constitution. And whether we contemplate it as affecting the functions, both intellectual and sensitive, or the operation of external agents upon the system, we shall find its influence to be almost universal. In the accommodation to circumstances, which is the result of habitual action, we may perceive the operation of what I have termed the principle of self-adjustment; but, in this case, the effect being brought about slowly or almost imperceptibly, the action is less observable, and the successive steps of the operation are often not to be detected. Some of the most remarkable instances of the changes induced by habit are in the nutritive functions, where the digestive organs must have experienced a complete alteration in their mode of acting upon bodies, so as to render those substances digestible, and even salutary, which were originally inert and indigestible. In this case we are led to conclude, that the secretions of the alimentary canal must have experienced such an alteration, as to adapt them to the new substances upon which they are destined to act, while a no less remarkable alteration must have taken place in the nervous system connected with these parts.

The effects of habit on the sensitive functions, where the nervous system is more directly concerned, are no less remarkable. There appears to be no assignable limit to the alteration which may be produced, both as to the degree and even the very nature of the effect. We become insensible to the most powerful agents, and we acquire artificial perceptions, which are frequently the most opposite to those that seem the natural result of impressions made upon the external senses.

The effects of habit are peculiarly observable in those operations which recur only after certain intervals, such as taking food or going to rest. When the usual period arrives, we experience the sensation of hunger, or become oppressed with drowsiness, not because the stomach is entirely empty, or the powers completely exhausted; for if by any accident the meal be deferred, or the inclination to sleep be powerfully resisted, the hunger and drowsiness leave us, and some hours may elapse before we again perceive the effects.[8] It is extremely difficult to assign any cause for these periodical accessions of habitual feelings; but it may be observed, that there is a

[8] The temporary cessation and subsequent renewal of these feelings may, to a certain extent, be ascribed to re-action, as far as respects the stomach; but the period of their recurrence, and the regularity of their return, depends upon habit.

tendency in the human constitution to go through a certain train of actions in the space of the diurnal revolution of twenty-four hours. This is observable in the state of the pulse, and in many of the secretions, and it may be presumed that the influence of habit is confined to this limit, for it is not probable that, by any volition, we could so far change our modes of life, as to go through the usual routine in a shorter time, or extend it to a much longer, for example, in eighteen hours on the one hand, or to thirty on the other.[9]

One of the most remarkable effects of habit is to blunt or diminish sensations of all kinds, so that not only do disagreeable impressions cease to be so, but even pain, if not too violent, becomes comparatively

9 Although I think it can scarcely be doubted, that there is a natural tendency in the animal œconomy to undergo a certain succession of actions in the diurnal period, it is extremely difficult to determine, how far it should be assigned to this cause, and how far to habit. The alternation of day and night obviously directs most of the occupations of life, and the influence of this alternation is impressed upon us, in various ways, from our earliest infancy. The Esquimeaux tribe, that was discovered by Captain Ross, in the N.E. part of Baffin's Bay, who, for nearly eight months of the year, are not subject to these alternations, and who are, at the same time, unconnected with the inhabitants of other countries, seemed to afford an excellent opportunity of throwing some light upon this point, by ascertaining how far their customs are founded upon the observance of the diurnal period. The information that was obtained is unfortunately imperfect, but as far as it goes, it favours the idea, that they have no regular periods for taking food or rest; see Capt. Sabine's narrative in Quart. Journ. v. vii. p. 80.

indifferent. On the contrary, many circumstances, originally indifferent, acquire by habit a kind of connexion with the animal œconomy, which makes them almost essential to the continuance of its functions. The effect of repetition upon the intellectual operations is very different from that upon the nervous functions, for while these are diminished by it, the former are rendered more acute.[1]

## § 3. *Imitation.*

The next principle which I shall notice is imitation. Perhaps, strictly speaking, imitation ought to be regarded as a complex action, rather than as a distinct principle, yet it seems to depend upon a peculiar state, which is not very easily referrible to any more general effect, and which, from whatever cause it originates, produces very important operations. There is a tendency to, or capacity for, imitation naturally existing in the constitution, for one of the first symptoms of intellect that we perceive in

[1] Bichat, Sur la Vie &c. art. 5. p. 29, et seq. His remarks on the effect of habit upon the sensitive, or, as he terms them, the animal functions, are generally correct; but I cannot agree in his observation that the organic functions are "constamment soustraits à l'empire de l'habitude." He continues: "La circulation, la respiration, l'exhalation, la nutrition, les secretions ne sont jamais modifieés par elle." p. 35. I conceive that a sufficiently long course of habitual action will considerably modify, perhaps every one of these, certainly the two last. The author, indeed, in the next paragraph, in a great measure, retracts his assertions.

children is their attempt to imitate the actions of those around them. This has usually been regarded as an ultimate fact, a circumstance, the reality of which we cannot doubt, but the causes of which we are unable to explain.[2]

We may speculate so far as to assume, that it is more easy to imitate an action which is impressed upon our senses, than to invent a new one, and that when an action has been once performed, the repetition of it is more easy than the original performance, and would seem even to be attended with a certain degree of pleasurable sensation. But although the physical cause of imitation is obscure, its final cause is obvious and important. By imitation we learn the use of speech, or the power of uttering articulate sounds, and when aided by association, of comprehending them when uttered by others. Hence we acquire the rudiments of all our future education, and profit by the knowledge of those who have preceded us.

I have already had occasion to offer some remarks upon the voice and speech, as produced by the contraction of certain muscles attached to the glottis,

[2] Reid observes, "Another thing in the nature of man, which I take to be partly, though not wholly instinctive, is his proneness to imitation." On the Active Powers, p. 111. A theory, if we may so term it, of imitation, is formed by Darwin, and is elaborated with his usual dexterity. Zoon. sect. 22. § 3, but, I think, I may venture to assert, that there is no one position on which the theory is built, for which he has adduced any substantial proof, and that the whole rests upon a series of analogies that are indefinite and inapplicable.

and to the tongue and lips respectively.[3] The problem which now remains for us to solve is, in what manner, or by what medium of communication, we are enabled to become acquainted with the actions of these muscles, and thus to imitate them, when some of the parts are entirely concealed from our view, and the rest we know, rather from anatomical examination, and from minutely investigating the operation, for the purpose of experiment, than from our ordinarily noticing their action. We acquire our ideas of the tone of the voice entirely by the ear; and that, in the case of speech, we derive our knowledge principally from the same sense, is proved by comparing the state of the blind with that of the deaf, in respect to their capacity for uttering articulate sounds. In the former case no deficiency is perceived; often, indeed, they possess a remarkable accuracy in this faculty, in consequence of the attention being almost exclusively confined to audible impressions, whereas, on the contrary, in the deaf, it is only after a long and tedious process, that they are able to acquire a very imperfect power of articulation.

The fact then would appear to be, that certain contractions of the muscles of the glottis, and of the parts connected with the mouth, enable us to produce vocal and articulate sounds, but that the changes immediately connected with these contractions are either concealed from our view, or not observed by

[3] Vol. ii. p. 213, . 7.

us, so that the only intimation which we obtain of their existence is conveyed to us by the ear. Without knowing how the change is actually effected, we, by an act of volition, produce the same change in the larynx or mouth, and thus produce the same sound. It has been conjectured, that we learn by repeated trials what peculiar sensation in the muscles of the organs is excited by their contraction, and the consequent emission of certain sounds, and that when we wish to reproduce the same sound, we begin by re-producing the same sensation through the intervention of the muscles.[4] But this supposition only removes a part of the difficulty, even supposing this experimental process to have been gone through, of which, I conceive, we have no proof, and are certainly altogether unconscious.

## § 4. *Sympathy.*

One of the most distinguishing peculiarities of the animal frame, unlike any thing that we behold in inanimate nature, is the connexion subsisting between different parts, which we term sympathy.[5] There is

4 Hartley on Man, ch. 1. sect. 3. prop. 21. v. i. p. 107 . . 9.

5 Darwin, in treating of the effects of sympathy, as is too frequently the case, has involved the subject in a series of metaphysical subtleties; Zoon. sect. 31. § 1. v. i. p. 441, et seq. Parry defines sympathy as follows: "When, from a cause immediately acting on one part, so as to produce sensation or motion, either or both of these effects is produced on another part, that second effect is called sympathy. Thus, in inflammation of the

scarcely any action which we perform, or any part that is moved or affected, but the motion or affection influences other parts, besides those primarily acted upon. In some cases this evidently depends upon mere contiguity, in others we can trace a direct vascular or nervous communication, and it may be frequently referred to association. But there are instances where none of these causes seem to be applicable, where the parts are distant from each other, where there has been no repetition of the actions, so that they cannot have acquired an association with each other, and where there seems to be no direct communication through the medium either of the vessels or of the nerves.

But, although in such cases, we perceive nothing in the physical disposition of the parts which can explain this sympathetic connexion, I am disposed to think, that it must be referred to the operation of the nervous system. One important use of this system is to unite all the several parts and functions of the animal machine into one connected whole, each portion of which may, to a certain extent, feel the impressions that are made upon every other portion.

But although we may suppose that the sympathetic actions are thus connected with each other,

liver, a pain is sometimes felt on the top of the right shoulder." Pathology, § 607. p. 259. He points out four species of sympathies; 1. Sensation producing sensation; 2. Sensation producing motion; 3. Motion producing motion; 4. Motion producing sensation. We must be careful not to confound the effect of sympathy with those of association; but by ascertaining the mode in which they were originally produced, it will be, in most cases, not difficult to discriminate between them.

by what may be termed an indirect operation of the nervous influence, it will still remain for us to inquire in what way this connexion is effected. And here two important questions present themselves; is the influence, in such cases, conveyed by a certain set of nerves only, for example, by those belonging to what has been termed the sympathetic system, or by the non-symmetrical nerves of Mr. Bell? and, secondly, is the intervention of the sensorium necessary? To the first of these questions we may, I believe, without hesitation, answer in the negative, because we have many cases of obvious sympathetic action, in parts where these nerves do not exist, and yet, as we can conceive of no other medium of connexion except by nerves, we must refer it to the operation of those of another description. With respect to the other point, whether the intervention of the sensorium commune be necessary, I may remark, that this subject was fully discussed by the physiologists of the last century, and more especially by Whytt. His phraseology is often, as I conceive, incorrect, in consequence of its resemblance to the Stahlian hypothesis, but the facts which he adduces are of such a nature as, I think, to prove that the co-operation of the brain is essential in those actions which we refer to the operation of sympathy.[6]

[6] See particularly his treatise, On Vital and Involuntary Motions, sect. 11. entitled, "On the share which the mind has in producing the vital and other voluntary motions of animals." Works, p. 140, et seq.

After having stated in the text what, I believe, may be regarded as the ascertained fact, we may, in the notes, indulge in a little

Among the affections which are ordinarily termed sympathetic, I may mention the general uniformity in the motion of the two eyes; the secretion of milk by the mamma, consequent upon parturition: the convulsive contraction of the diaphragm which produces sneezing, as caused by the irritation of the nerves belonging to the mucous membrane of the nostrils; pain of the head occasioned by a certain condition of the stomach; imperfect vision from a morbid state of the intestinal canal; vomiting from the irritation of a biliary calculus in the duct of the liver; and a variety of other affections, the occurrence of which would never have been predicted or suspected, but which are well ascertained matters of fact. It must be admitted, that in these cases, we do not perceive any peculiar or especial nervous connexion, which might seem necessary to account for the phenomena, but we are so well acquainted with the nature of nervous action as to justify the con-

speculation on the subject. I have divided the nerves into three classes, the simply sensitive, the perceptive, and the motive; the first of these, as far as we know, transmit their influence in both directions, and without any regard to a central point of union. In the second, the influence passes from the extremities to the centre, and in the third, from the centre to the extremities. Now, in the production of sympathy, a perception is transmitted by a nerve of the second class to the brain, and a consequent change is propagated from this organ by a nerve of the third order; if motion be the ultimate result, we may suppose the operation to produce no farther effect, but if it be a perception which ensues, we may conceive a motion, possibly in the capillary arteries, to have been produced; this re-acts upon a nerve of the second order, which conveys the perception to the brain.

jecture that it is the immediate agent in these operations.

I have been speaking of sympathy as affecting different parts of the same body, but its operation is more wonderful as affecting different individuals. Besides the mental impressions of a sympathetic kind, which, like other complex intellectual processes, depend upon a number of associations, originating from various causes, there is a kind of physical sympathy, by which, from observing pain or suffering in another, the body becomes actually affected in a similar manner. This subject has been ingeniously illustrated by Adam Smith, and forms the basis of his beautiful, although perhaps fanciful theory of morals. He observes, that the source of our compassion for the sufferings of others arises from the faculty which we possess, not only of imagining ourselves in the situation of the sufferer, but of actually being affected, to a certain extent, with the same painful sensation. "When," says this writer, "we see a stroke aimed and just ready to fall upon the leg or arm of another person, we naturally shrink and draw back our own leg or our own arm, and when it does fall, we feel it in some measure, and we are hurt by it as well the sufferer." [7]

7 Theory of Moral Sentiments, v. i. p. 4. See the same idea expanded in Stewart's Elements, ch. 2. sect. 1. v. iii. p. 153, et seq. On Sympathetic Imitation. The remarks of this writer are always interesting and elegant, but, it must be acknowledged, that they are frequently diffuse, and deficient in that correct precision, which is so desirable in metaphysical discussions.

To this cause may be attributed the tendency to fainting, which many individuals experience at the sight of blood, or from being present at a severe surgical operation. A still more remarkable example of this transferred sympathy occurs in some kinds of convulsive diseases, where the sight of the patient will excite a similar disease in the spectator. Were it not digressing from the proper subject of physiology, I might here mention the effects of different kinds of fanaticism, very remarkable instances of which are upon record, where violent motions having taken place in certain individuals, have been propagated, apparently in an irresistible manner, among all their followers. Examples of this kind are not uncommon even in our own age and country, and whatever we may think of the principles of those who encourage them, or of the state of mind by which they are produced, we can have no doubt of their reality as the result of sympathetic impression.[8]

It is, I conceive, very difficult to explain these phenomena, or to refer them to any more general principle. It may be said that they depend upon a species of imitation, but the imitation, if it be so considered, is essentially different from the ordinary

[8] A very remarkable train of facts, which may be referred to this source, as examples of sympathetic impressions propagated through a number of individuals, and affecting both the mental and the corporeal functions, is related in the Edin. Med. Journ. v. iii. p. 434, et seq. See also an account of Epidemic Convulsions, in the Isle of Anglesea, related by Dr. Haygarth, in his Essay on the Imagination, § 2. p. 47, et seq.

kind, as being involuntary. Upon the whole, I am disposed to regard this class of actions as specific, and not explicable by any of the powers which are generally admitted, as regulating the operations of the living body.

## § 5. *Instinct.*

A principle of great importance in the animal œconomy, both as regards the individual, and the relation which subsists between different individuals, is instinct. It may be defined[9] a capacity for performing by means of the voluntary organs, certain actions which conduce to some useful purpose, but of which purpose the animal is itself ignorant.[1] It is

9 A more concise definition has been proposed; spontaneous impulse to certain actions not accompanied by intelligence. Reid defines it, "a natural blind impulse to certain actions, without having any end in view, without deliberation, and very often without any conception of what we do." On the Active Powers, Ess. 3. ch. 2. p. 103. I may remark, that the chapter on instinct, although, like every other part of Reid's works, deserving of an attentive perusal, is upon the whole vague and indeterminate, Cabanis describes instinct to consist in determinations made by the animal, independent of its volition; Rapports, t. i. p. 86. He afterwards defines it more precisely, "Le produit des excitations dont les stimulus s'appliquent à l'interieur." p. 137. We have a further and more detailed account of it in t. ii. p. 388, et seq.; but in this, as in other parts of his work, he confounds the action of the organic functions with the effects of instinct.

1 M. Magendie applies the term instinct more generally to "des penchants, des inclinations, des besoins, au moyen desquels ils sont continuellement excités et même forcés à remplir les intentions de la nature." El. Phys. t. i. p. 207. According to

well illustrated by the example, so frequently cited, of the mode in which birds proceed in building their nests. If a bird be taken from its parent, soon after being hatched, and be confined in a cage, so as to have no communication with other birds, before it lays its eggs, it will prepare its nest with as much skill as if it had been brought up with individuals of the same species, and had practised the building of nests for a number of successive seasons.[2]

The motive which directs the bird, and the skill which it displays, cannot have been derived either from tradition, imitation, or reason, nor can the effect depend upon any direct impression made upon

this definition, all the natural appetites are included under the class of instincts, for some of which at least, although the capacity be implanted in the constitution, the exercise of them depends upon the knowledge which we acquire by education or the intercourses of society. What he afterwards describes under the title of social instincts are many of them complex feelings, arising principally from various modes of association. I remarked above, that Cabanis does not sufficiently distinguish between automatic and instinctive actions; Reid has likewise fallen into this error, ubi supra, p. 103, et seq., and it seems to be the case even with Parry; Pathol. § 620..2. But it is sufficiently easy to make the distinction, if we bear in mind, that the latter are performed without the direct application of a stimulus, and through the intervention of the voluntary organs, although not, strictly speaking, by an effort of volition. M. Virey unequivocally refers the action of the vital functions, even of the absorbents, to instinct; art, "Instinct," in Dict. Scien. Med. t. xxv. p. 377.

[2] The case of the newly born lamb, which was adduced by Galen, as an illustration of the power of instinct, is equally remarkable; see Young's Lectures, v. i. p. 449, 0. and Parry's Pathol. 620, 1.

the nerves or muscles. We must here suppose that a particular state of the brain exists, similar to what, in human beings, is gradually induced by reason or instruction, which prompts to a train of actions, as far as we know, connected together only as they tend to one ultimate object. When an animal for the first time receives those feelings which induce it to prepare for its young, it can have no conception of the events which are to follow; it can form no idea of the nature of its offspring, of its wants, nor, in short, of any thing connected with it. We must therefore suppose, that a part of its œconomy consists in having certain impressions made upon the brain at certain periods, corresponding to the time of laying the eggs, which lead to the same effect, as if these impressions had resulted from causes which induce analogous actions in the human species.

The above is one of the most complicated cases of instinct; there are some where the effect appears to be the result of a direct impression upon a nerve or an organ of sense, and when the impression is followed by a certain action, similar to what, in a human being, we should attribute to association. To this may be referred the natural dislike or antipathy which animals experience to certain articles of food, which are not suited to their digestive organs. It would appear that, in these cases, they are principally guided by the smell, for there are remarkable instances, where this sense seems to direct them, even in opposition to the most palpable evidence of some of the other senses. There is a species of Stapelia, which has exactly the odour of putrid flesh, and it is observed that the carrion fly lays its eggs on the flower, no doubt under the instinctive impression that it thus provides a suitable lodgment for its young.[3]

There is a series of anatomical facts, connected with this subject, which seems to demonstrate that instinct is, in its essential nature, a different principle from reason. By comparing the faculties of different classes of animals, we find that these two powers generally exist in a kind of inverse ratio to each other; the more perfectly organized animals possessing a larger share of reason, and those that are less so being more directed by instinct. Now by observing the nervous system of these animals respec-

[3] The remarks of Cuvier appear to me so appropiàte, that I shall present them to my readers at some length. "Il existe dans un grand nombre d'animaux une faculté differente de l'intelligence; c'est celle qu'on nomme *instinct.* Elle leur fait produire de certaines actions necessaires à la conversation de l'espece, mais souvent tout à fait etrangeres aux besoins apparens des individus, souvent aussi tres compliquées, et qui, pour être attribuées à l'intelligence, supposerairent une prevoyance et des connaissances infiniment superieures à celles qu'on peut admettre dans les especes qui les executent. Ces actions, produites par l'instinct, ne sont point non plus l'effet de l'imitation, car les individus qui les pratiquent ne les ont souvent jamais vu faire à d'autres; elles ne sont point en proportion avec l'intelligence ordinaire, mais deviennent plus singulieres, plus savantes, plus disinteressées, à mesure que les animaux appartiennent à des classes moins elevées, et, dans tout le reste, plus stupides. Elles sont si bien la proprieté de l'espece, que tous les individus les exercent de la même maniere sans y rien perfectionner." Regne Animal, t. iv, p. 53.

tively, we find that there is a gradation in the comparative size of the brain and nerves, which corresponds to the state of their faculties. In Man, where reason exists in the greatest degree, and where instinct holds a subordinate place, the brain is the largest, in comparison to the rest of the nervous system. In quadrupeds and birds the size of the brain decreases, while that of the spinal marrow and nerves increases; this comparative scale goes on through the amphibia and fish, until we arrive at some of the insect tribes, which, although they possess a variety of organs and many elaborate functions, yet have very small and imperfect brains. And we observe that the faculties of reason and instinct bear a respective ratio to the comparative size of the brain and nerves. In quadrupeds we have very decisive proof of the operation of instinct, although still with an evident portion of reason; in cold-blooded animals instinct very much predominates, and to this faculty we shall probably, upon mature reflection, refer many of the varied operations of the insect tribes, their variety and perfection depending rather upon the variety and perfection of their organs of sense and motion, than upon the nature of the principle which directs the actions.

The operation of instinct, as observable in the inferior animals, is so remarkable, that the existence of this principle in them has seldom been doubted. It has, however, been called in question by Darwin; he argues that the effects of instinct should be always uniform, and proceed precisely in the same track, as it is a kind of blind impulse, impressed upon the animals, which is exactly the reverse of reason. But he remarks, that in the actions usually called instinctive, as the building of a nest, we discover symptoms of reason; we see the bird adapting itself to circumstances, both in the position and choice of its materials. If it cannot procure the substance which similar birds employ, it endeavours to get something like it, and if it cannot build the nest exactly in the proper situation, it searches out for one resembling it.[4] To this argument it may be replied, first, that animals possess a certain share of reason, and it does not follow that this is to be extinguished by instinct; it is more probable that they will co-operate to the same end, and will each supply the deficiencies of the other. And, in the second place, there is no ground for supposing, that instinct consists in this blind impulse to certain specific actions; it seems rather to depend upon a state of mind impressed on the animal, which may lead it to accomplish the action in the

[4] Zoon. sect. 16. v. i. p. 135, et seq. He remarks, that "all those actions of men or animals, that are attended with consciousness, and seem neither to have been directed by their appetites, taught by their experience, or deduced from observation or tradition, have been referred to the power of instinct." p. 136. This definition is defective from its not, on the one hand, excluding the operation of a direct external stimulus, and on the other, from its not including the final cause of the action. Brown's remarks on Darwin's objections to our ordinary conceptions of the operation of instinct are very judicious and satisfactory; Remarks on the Zoonomia, sect. 9. p. 263, et seq. He defines it, "pre-disposition to certain actions, when certain sensations exist." p. 265.

best manner that is within its power. But there is one circumstance, which I regard as an unanswerable proof of the existence of instinct; that there are many animals, whose whole duration is only for a short space of time, at a certain period of the year; they can therefore never see their parents, and of course derive no benefit from the experience of their predecessors. Yet in their habits they exactly resemble them, and have gone on so, precisely in the same track, for hundreds of generations. This applies to several of the insect tribes, whose habits are often extremely curious, and exhibit much of what might be called ingenuity and contrivance, did not its uniformity prove it to be instinctive.

It has been a subject of contention among metaphysicians whether man possesses any thing which can properly be called instinct, or whether those actions which, at first view, appear to be of this description, are not more properly to be referred to other sources. Reid, and most of the Scotch writers, have supposed that we possess a great variety of principles that are innate, or at least originate from the nature of our constitution, independent of any external circumstances. The disciples of Locke, on the contrary, have very much diminished the number of these original qualities, and have endeavoured to account for the effects, by the operation of various agents, directed by association, sympathy, imitation, and some other of the principles which have been described above. That these innate principles have been multiplied to an unnecessary, or even a ridiculous excess, seems to be now generally allowed, and also that some cases, which at first view appear the most complicated, have been resolved into other faculties, seems equally probable; yet I am disposed to think, that there are certain actions, which are the most conveniently explained by admitting the existence of instinct. And this appears to be agreeable to the analogy of nature. The actions of brutes are directed by a large share of instinct, mixed with only a small portion of reason; those of man by a greater proportion of reason, but not without some admixture of instinct.[5]

## § 6. *Imagination.*

The imagination is a faculty of a purely intellectual nature,[6] yet its effects upon the body are so remarkable, that it will be proper to take some notice of them in this place. When the mind is stored with ideas, either obtained from the perception of external objects, or from the operation of its own powers, it possesses the faculty of combining these ideas in various forms, and of disposing them in new trains, different from those in which they were origi-

[5] Prof. Stewart correctly observes, that in infancy "existence is preserved by instincts, which afterwards disappear when they are no longer necessary." Elements, sect. 8. p. 270.

[6] According to Prof. Stewart, the imagination is not a distinct power of the mind, like attention, conception, or abstraction; Elem. p. 478. The remark may be metaphysically correct, but the effects of the imagination are sufficiently distinct to warrant our considering them in a separate section.

nally received.[7] This constitutes the imagination, which thus becomes the source of a new set of feelings, often more powerful than those immediately derived from the direct impressions of external objects.[8] It belongs to the moralist and the poet to trace the effects of the imagination upon the passions and the feelings, but its influence upon the corporeal functions strictly falls under the cognizance of the physiologist. Many facts clearly prove that the imagination can affect, not only the nervous system, which might be supposed the more immediate subject of its operation, but that it can act upon the circulation, the respiration, the digestion, and, in short, that it is one of the most important agents in the animal œconomy. In medical practice, its effects are the subject of daily observation, and present at one time the most powerful obstacle, and at another the most active assistant, to the exertions of the physician. The history of medicine abounds with examples of its influence, and the greatest sagacity is requisite to

7 Darwin employs the term imagination in a somewhat different sense; he does not think it necessary that the ideas should be combined in an order different from that in which they were originally received; he appears to resolve it nearly into recollection or memory; Zoon. v. i. p. 43, 130. Indeed, very much the same idea was entertained by Hobbes; he defines imagination, "conception remaining, and by little and little decaying." Treatise on Human Nature, p. 4; he speaks of imagination and conception as very nearly synonymous terms.

8 Montaigne's Essay on the Force of the Imagination, b. 1. ch. 20, affords a curious and amusing specimen of the combination of sagacity and credulity.

distinguish between the physical effect of remedies and their power over the imagination. Instances are daily occurring of remedies being announced, under some secret or mysterious form, which accomplish the most remarkable cures, attested by unexceptionable evidence. The composition of these remedies is generally, after some time, made known, but it may be asserted, that there is scarcely a single instance on record, in which the same beneficial effects have resulted after the discovery. An account of the influence of the imagination, as connected with medicine, would afford a melancholy detail of the weaknesses and follies of human nature. The powers of witchcraft were universally acknowledged, the most ridiculous and disgusting compounds were sanctioned by colleges and universities, not much more than a century ago, and in our own times we have seen the general assent which has been given to animal magnetism, and the metallic tractors.

A remarkable series of facts on this last subject was published a few years ago by Dr. Haygarth. While the delusion was at its height, he determined to ascertain how far the effects ascribed to this instrument could be accounted for by the powers of the imagination. He accordingly provided himself with bits of wood, formed like the tractors, and with much assumed pomp and solemnity, he applied them to a number of patients, whose minds were prepared for something extraordinary. He not only used them in nervous diseases, where the cure is often equivocal, and may be ascribed merely to fancy or caprice, but he employed them in cases apparently of the most opposite nature. The effects were astonishing: obstinate pains of the limbs were suddenly cured, joints that had been long immovable were restored to motion, and, in short, except the renewal of lost parts, or the change of mechanical structure, nothing seemed beyond their power to accomplish,[9] Had we the imagination at all times under our controul, we might dispense with a large part of the materia medica. Undoubtedly, upon this principle, we must explain many of the pretended miracles of ignorant ages and nations. The facts are true, but the inference from them is false. In some cases the vulgar were imposed upon by designing impostors, but not unfrequently we may conclude, that both parties were equally the dupes of their credulity.

If we admit the justice of these remarks, it will induce us to advance a step farther in our investigation, and to ask, whether the imagination may not actually produce a state of the system, which shall constitute a specific disease. It has been observed by medical writers, that a disease has been unusually prevalent at a particular period, when there has appeared no external cause to which this increased prevalence could be reasonably assigned, while at the same time, from certain circumstances, the disease in question has been more than ordinarily the object of attention. That this might be the case with diseases of the nervous system, is sufficiently intelligible, as in

9 Of the Imagination, as a cause and a cure of disorders, &c.

these cases the functions of the parts are often much affected, without their experiencing any change in their organization. But it has not been confined to these affections; diseases have been produced in parts that are only remotely connected with the nerves, and where the change must ultimately consist in an altered action of the arterial capillaries, and perhaps of the absorbents. Although in such cases it is necessary to obtain very direct evidence of the fact, and to search in all directions for more obvious causes, yet it does not seem impossible that such a change may be effected upon the corporeal organs, through the medium of the mental emotions.

## APPENDIX.

SINCE the preceding chapter was written, I have been much gratified by the perusal of Dr. Alison's elaborate essay on Sympathy, published in the second volume of the Edin. Med. Chir. Trans. p. 165, et seq. His definition of physiological sympathy, although perhaps somewhat longer than necessary, I conceive to be correct and appropriate. The term sympathy, he remarks, "is correctly applied to all combinations or successions of the vital phenomena presented by different parts of the body, which we observe so generally, that we judge them not to be accidental, which are independent of the will, and not owing to any necessary dependence on one another, which we can refer to other ascertained laws of the animal œconomy, of the living actions of the parts thus simultaneously or successively affected." p. 166. He divides the effects of this principle into those that produce sensations merely, and those that produce actions, but it is to the latter of these that his observations are exclusively directed, although, I may remark, that most of his statements will apply equally to both of these cases.

The cause of these actions, or the nature of the connexion which subsists between the primary and the consequential change, is fully considered by the author. He gives many reasons for the opinion, which was maintained by some of the most eminent physiologists of the last century, that we can discover no direct anatomical connexion, sufficient to explain the phenomena; while he endeavours to prove that there is, in all cases, an indirect connexion, through the intervention of the brain, or according to Whytt, by means of a mental sensation. The exposition of this doctrine is given at considerable length, and it is shown, that some of the late experiments of Dr. Philip serve very materially to establish its correctness. The following quotations contain a summary of the reasoning employed in the first part of this paper. "It is quite obvious, that the instances now given of irritations of different and distant parts, producing the same sympathetic action when they excite the same sensation,—and still more the instances of different irritations of the same parts, producing totally different effects of this kind, when they excite different sensations, are nearly incompatible with the supposition of sympathies depending on certain definite nervous connexions," p. 185: and again; "I have now stated the arguments which appear to me the most convincing, in regard to the two principles formerly laid down; *first*, that the sympathetic actions we have considered, in the natural state, are to be ascribed to the influence of certain mental sensations; and, *secondly*, that the effect of these sensations, in producing them, cannot be explained on the anatomical principle of connexions among the nerves of the sympathizing parts." p. 189.

The latter portion of the essay is principally devoted to the consideration of Mr. Bell's hypothesis respecting the nervous system, the relation of its different parts to each other, and to the actions of the animal œconomy in general. It would be impossible for me to follow Dr. Alison through all his reasoning, but I shall state, as the result produced upon my mind, after an attentive perusal of the whole, that he is successful in showing that the appropriation of what Mr. Bell terms the respiratory nerves, to those muscular contractions which are directly or indirectly connected with the function of respiration, is not strictly correct, and that the anatomical connexion between these nerves is not sufficient to explain the sympathy which exists between the different organs that are concerned in the exercise of this function. I think it due to Dr. Alison to remark, that in criticizing the opinions of Mr. Bell, he pays a very high tribute to his merits as an anatomist and physiologist, and that his observations are delivered in that candid and philosophical spirit, which indicates a mind bent rather on the attainment of truth, than on the mere establishing of a particular set of opinions.

Dr. Alison remarks upon a passage in my first volume, where I have spoken of the respiratory nerves of Mr. Bell as not under the control of the will, and as not capable of producing perceptions. The inaccuracy of this passage, as I conceive, depends principally upon my supposing Mr. Bell's system of respiratory nerves to be exactly coincident with his non-symmetrical nerves, but which, as Dr. Alison states, is not the case; p. 193. These latter, however, I am still disposed to consider as not under the control of the will, and as not conveying perceptions; but as belonging to what I have termed the simply sensitive, such as serve for the purpose of connecting the system into one whole by the transmission of the nervous influence. That I regard perception as a necessary requisite in the exercise of respiration, is evident from various parts of my seventh chapter, but this I conceive to be derived from branches of the symmetrical nerves, with which the organs of this function are furnished.

A paper was read to the Royal Society in April last, by Dr. W. Philip, on the functions of the nervous system, in which he dwells at some length on the connexion between this system and the respiratory muscles; the conclusion which he draws is, that they do not, in any respect, differ from the muscles of voluntary motion, in their relations to their other functions.

## CHAPTER XVIII.

### OF VOLITION,[1] AND THE PASSIONS.

### § 1. *Nature of Volition.*

I HAVE frequently had occasion to remark upon the connexion between muscular contraction and the

[1] If we may be allowed, in any case, to consider the mind as possessed of distinct faculties, volition would appear to be the power which is the most essentially different from the other mental operations. These seem all to depend upon a certain combination or relation of ideas to each other, influenced, more or less, by the intervention of perceptions from the impressions of external objects. But in the exercise of volition the process proceeds in the inverse direction; it originates in the mind, is transferred to the brain and nerves, and from these to the muscles or organs of sense. I have referred above to the opinion of Locke respecting the futility, or even impropriety, of attempting to ascribe the mental operations to distinct faculties. He, however, remarks upon the nature of volition, as essentially different from that of thought, and seems disposed to resolve all the mental operations into modifications of these two powers; Essay, b. 2. ch. vi. v. i. p. 104, 5. The same principle forms the basis of Reid's division into the intellectual and active powers; Intell. Powers, p. 67. If we were to indulge in any farther speculations on the subject, we might propose the division of the mental operations into three classes, to be referred to perception, volition, and intellect, which, for the convenience of language, might be denominated distinct faculties. Perception consists in the power of receiving impressions from external objects, an operation which must proceed from the extremities of the nervous system towards its centre; volition is the re-action of the mind upon external objects, where the operation is transmitted in a contrary direction; while

will; on this circumstance is founded the division of muscular motions into the two great classes of voluntary and involuntary. Voluntary motion may be regarded as one of the most important effects produced by the re-action of the nervous system upon the muscles, and as being that power which more immediately connects us with the external world.

Volition, or the act of the mind which constitutes the will,[2] is excited by a variety of causes, partly depending upon direct perceptions of pleasure and pain, and partly upon associated feelings; but in all cases volition, where it leads to an active exertion, is preceded by a motive. The mere act of volition, like all the other mental faculties, is directly connected

in intellect, our ideas only are concerned, without the intervention of external objects, and where probably the brain acts as the sole intermedium, without the co-operation of the nerves. I conceive it would not be difficult to arrange all the mental operations as species of these three genera; but it may be questioned, whether such a technical arrangement would throw any light upon the subject, or in any respect advance our knowledge, either of the nature of the mind or of its connexion with the body.

[2] Locke defines volition to be "an act of the mind directing its thought to the production of any action, and thereby exerting its power to produce it." Essay, b. 2. ch. xxi. § 28. According to Hartley, "The will is that state of mind, which is immediately previous to, and causes, those express acts of memory, fancy, and bodily motion, which are termed voluntary." On Man, Introd. p. iii. According to Reid, volition is "the determination of the mind to do or not to do something which we conceive to be in our power." On the Active Powers, p. 60. Volition, as appears by these definitions, applies both to the physical and to the intellectual functions, but the object of this treatise will lead me to consider it principally as connected with the former class.

with the brain, while the exercise of volition requires the co-operation of the brain, nerves, and muscles. Whatever volition is formed in the mind, it cannot be carried into effect unless the nerve and the muscle be in a sound state. The manner in which this singular process is accomplished is very much concealed from our view. All that we certainly know is, that the mind forms a volition; this is accompanied with a consciousness of power, and immediately the effect is produced. For example, we will to move the arm in a certain direction, and, provided the nerves that connect the arm with the sensorium commune and the muscles of the part be in a sound state, the arm is immediately moved.

It becomes an interesting object of inquiry, what takes places in this process, what are the intermediate links in the chain of actions between the feeling in the mind and the motion of the limb. The act of volition induces a certain state of the brain; this is in some way propagated through the nerves, these again act upon the muscles in some peculiar manner, and, lastly, the muscular fibres are shortened, and thus move the joint in the required direction. That the will originates in the brain, in the same sense that our other intellectual feelings arise there, we can have no reasonable doubt, although, as forming a part of the mind, we may conclude that something besides the mere modifications of matter is concerned in the operation. There are abundant facts that prove the nerves to be the media through which the will acts, and indeed, according to the view of the subject which has been taken in the former part of this work, we are led to conclude, that the chief use of the nerves which are distributed to the muscles is to place these muscles under the control of the will. But although we may feel no doubt of the reality of these three changes; first, that of the brain; secondly, that of the nerve; and lastly, that of the muscles as induced by the nerves, we are totally ignorant of the nature of the two first, and equally so of the manner in which the three are connected together.

The only attempt at explanation which deserves to be noticed is that of Hartley, and this perhaps more from the general respectability of the author, than from the merit of the hypothesis itself. He refers all the mechanical changes in the nervous system to vibrations among its particles, and in this way accounts for the permanency of the impression produced by external objects, as well as by the operation of the intellectual powers themselves.[3] Upon this hypothesis, we may remark, that it assumes the position, that the changes in the nervous system are effected by the intervention of motion; but I think it

[3] On Man; prop. 4. v. i. p. 11, 2. It is not a little curious to observe the confident strain in which even so acute a metaphysician as Cooper speaks of the hypothesis of Hartley. After having resolved all the phenomena of thought into perception, recollection, judgment, and volition, he goes on to say: "Of these the three latter are demonstrably modes of motion. Hartley has proved it; and I again repeat, what I have observed on a former occasion, that it is inexcusable in the present day to attempt the discussion of the phenomena termed mental, without adopting or confuting his system." Tracts, p. 273.

may be asserted, that not one of the characteristics of motion can be recognized in any of these operations, and that not a single circumstance can be adduced, which affords any decisive evidence of its existence.

The only direct argument that has been brought forward, either by Hartley himself, or by any of his followers, is the acknowledged fact, that when an impression has been made upon the nervous system by an external agent, the effect remains for a certain length of time after the cause is withdrawn; that it is then gradually diminished, and that a permanent change is finally produced.[4] This has been conceived to bear an analogy to the vibratory or oscillatory motions between the particles of bodies; but the analogy is at least of very doubtful application, and is not supported by the phenomena. For if we are in any degree to reason upon mechanical principles, we should conceive that a substance capable of such extreme delicacy in its vibratory action, as the medullary matter of the brain and nerves, must be eminently elastic; and that, consequently, when the action ceased, its particles would be restored to their former relative position. And even admitting the hypothesis in its fullest extent, with all its array of propositions and corollaries, I do not perceive that it throws the smallest light upon the nature of the

[4] Hartley on Man, prop. 3..5. v. i. p. 9..34; the whole of his first chapter should, however, be read, in order to obtain a complete view of his hypothesis. See also Priestley's Hartley, Essay 1, and Belsham's Elem. ch. iii. sect. 4. p. 38..44.

connexion between the different parts of the operation that we have been contemplating. It neither shows how the consciousness of power can affect the brain, how this idea can be conveyed to the muscular fibre, nor how it can cause the muscle to contract.

This account of the process of voluntary motion must render it evident, that what we will to perform is merely the ultimate effect, because we are unconscious or even ignorant of the train of causes. The exact objects of volition may be classed in two divisions, under the title of immediate and remote; the first consisting principally of the formation of certain vocal and articulate sounds, or certain motions of the joints, as producing voice, speech, and loco-motion; the second, of those actions which we conceive to be within our power, but where we think only of the end to be obtained, without attending to the mechanical means. These two may be illustrated by what takes place in acquiring an art or accomplishment. In learning a language, for example, we begin by imitating the pronunciation of the words, and use a direct effort to put the organs of speech in the proper form. By degrees, however, we become familiar with this part of the operation, and think only of the words that are to be employed, or even the meaning that is to be conveyed by them. In learning music, we begin by imitating particular motions of the fingers, but at length the fingers are disregarded, and we only consider what sounds will follow from certain notes, without thinking of the mechanical way in which the notes are produced. Both these kinds of motions, however, may be said to be voluntary, because they are both brought about through the medium of the will, although in the latter case the motion is not the direct object of volition.

I have stated that a consciousness of power enters into our feelings of volition, and we must inquire in what this immediately consists, or in what way our sense of power is exercised.[5] The power which attends our volitions is absolutely directed to the contraction of certain muscles, but these are not the objects of our will, because we are frequently unconscious of the contractions. When we wish to effect a particular motion of a muscle, we induce a certain state of feeling, which we know by experience has been previously associated with the same muscular action. This feeling we appear to be able to repeat at pleasure, and to it succeeds the desired motion; our idea therefore of power consists in the recollection of the feelings which accompany our motions. But here, as on so many former occasions, although we are able to trace back certain successive steps in the order of actions, we see no connexion between

5 Reid has offered some strictures upon the account of power which is given by Locke; On the Active Powers, ch. ii. p. 22, et seq. Many of the remarks I conceive to be just, but I apprehend that the more important error into which this acute philosopher has fallen, consists in his considering power to be represented by a simple idea. The objections of Hume against the existence of the idea of power depend, I think, principally upon his supposing that it must be a simple idea; Inquiry, sect. 7. Essays, v. ii. p. 77, et seq.

them, and are quite at a loss to determine why they are united by the relation of cause and effect.

With respect to the nature of the power of volition, or the capacity which the mind possesses of producing at pleasure certain changes both physical and intellectual, we are altogether unable to refer it to any more general principle. It may be regarded as the most completely mysterious operation to which our frame is incident, and as one which, in all its parts, is the most remote from any of the effects which we ordinarily ascribe to matter. And even were we to admit the material hypothesis, and to take it in connexion with the Hartleian doctrine of vibration, still we gain no actual information upon the subject. We merely assume a series of positions, of none of which we have any direct evidence, and which possess no more than a verbal connexion with each other, without any actual analogy or resemblance. Upon the same principle, therefore, that I have acted on former occasions, I object to all such attempts at explanation, as being not merely futile, but decidedly objectionable.

The second class of muscular motions are the involuntary, or those which are produced by something acting upon the muscle, independently of the will. These have been styled by Hartley automatic,[6] but the term is not appropriate, for we

[6] He says that he calls these motions "automatic," from their resemblance to machines, whose principle of motion is in themselves; Introd. p. iii. But this, it may be remarked, is merely a technical resemblance, there being no real similarity between the

suppose the existence of an external cause as much in these, as in voluntary motions; the essential distinction between them consists in the relation which they bear to volition. Among the involuntary motions may be classed the contraction of the heart and perhaps of the diaphragm:* almost all the muscular fibres, that are spread over expanded membranes, act independently of the will, as well as those that are attached to vessels of all descriptions, such as the capillary arteries and the absorbents. All muscles are subject to involuntary motions in certain diseased states of the body, and there are some which partake of the two modes of action; but, for the most part, each kind of motion belongs exclusively to its appropriate muscle.

Before volition can be exerted, it is necessary that a motive exist in the mind, and hence it follows that, strictly speaking, there can be no voluntary motions in new-born infants. One of the first actions that is performed after birth is swallowing, but in this case there can be no exercise of volition, as there is no conception of the nature of the action, and in short no mental feeling of any kind in existence. This I consider as a clear case of the operation of instinct; where a series of actions is performed, so as to accomplish an important object in the animal œconomy, which is attended with

principle of action in the two cases. Parry does not appear to have distinguished between voluntary and involuntary motions with his usual accuracy, nor to have correctly marked their relation to each other; Pathol. § 608..0.

* See the remarks of Dr. Philip as referred to in p. 243.

consciousness, and is effected by the muscles that are under the control of the will, but where the individual is ignorant of the end in view, and employs no mental process for its production. But in the adult swallowing is a voluntary act, although generally of the remote species; however, it appears that motions, which are involuntary in the first instance, become afterwards voluntary. Hartley ascribes this change to association. He supposes that when an involuntary action has been frequently performed, we connect or associate together the idea of the motion with the sensation which precedes it, and that, having it in our power to re-produce our sensations at pleasure, we learn to re-produce the motions connected with these feelings, whenever we conceive them to be necessary for our enjoyment or existence.[7] In this way it is that Hartley supposes we acquire the use of speech. The infant is led to utter a variety of sounds in consequence of direct impressions made upon the organs. These sounds become associated, from various causes, with other perceptions; and, according to the usual operation of associated feelings, the sounds call up the ideas that are connected with them.

All muscular motions are, therefore, in the first instance, involuntary; some of them continue so during life, while there are others over which we gradually acquire a voluntary power. It may be asked, what is the cause of this difference? Is there any circum-

[7] On Man, v. i. p. 107..9.

stance in the structure or organization of the muscles of involuntary motion different from that of the muscles which become subject to the will? We might previously suppose that some difference does exist, because we find, with a few exceptions, that the corresponding muscles in different individuals agree in the relation which they bear to the will. In a sound state of the body the muscles subservient to speech and loco-motion are completely voluntary, the muscles that belong to the circulation are involuntary, while there are others that are of an intermediate nature. Now this difference appears to depend in great measure, if not altogether, upon the source whence the muscles derive their nerves. The nerves which place a muscle under the control of the will are derived immediately from some part of the brain or spinal cord, and we may generally observe a proportion between the degree of voluntary motion and the quantity of nerves with which a part is furnished. The other office of the nerves, that by which all the parts of the system are connected together into one whole, and endowed with mere sensation, seems to depend more upon the nerves that proceed from the ganglia, and it is probably to the ganglia that the perceptions of the internal organs are always in the first instance referred.

We may then conclude, that there is something in the original structure of the part which places it under the control of the will. It appears, however, that the voluntary motions are at first involuntary; by degrees the will acquires its power over them, and that they become again involuntary, at least the connexion is of that kind which I have named remotely voluntary, and which exists without our consciousness: Hartley calls these motions secondarily automatic.[8] With respect to those muscles over which the will never acquires any power, we have reason to suppose that, under ordinary circumstances, they receive the impression of their appropriate stimuli upon the fibre itself, without the intervention of the nerve. When this is clearly ascertained to be the case, it constitutes an obvious point of discrimination between the voluntary and the involuntary muscles, and affords an evident reason why the latter must, at all times, continue to be involuntary.

## § 2. *Account of the Passions.*

Among the powers which serve as the connecting media between the physical and the intellectual parts of our frame are the various passions or affections. The passions are generally regarded as exclusively

[8] On Man, v. i. p. 108, 9. He considers the act of swallowing to be an example of this transition of automatic into voluntary and of voluntary into secondarily automatic motions; p. 117. Prof. Stewart endeavours to prove, that motions which are once voluntary always remain so, and that our not being conscious of the act of volition is owing merely to our not attending to them; Elem. ch. ii. v. i. p. 112, et seq. It appears to me to be rather a question of words than an actual difference in the conception of the facts; but perhaps the term remotely voluntary, which I have adopted, may express the fact, without involving any controversy respecting theory.

belonging to the department of morals or metaphysics; yet it will appear, upon examination, that they are nearly related to our corporeal organization; that they in a considerable degree depend upon it, and have a material influence over it.

The impressions received by the senses and conveyed to the brain, the common centre of all our perceptions, uniting there with the ideas that had been previously acquired by the understanding, may be regarded as the origin of our passions. It would appear, therefore, that in all cases they may be ultimately referred to the wish to obtain some good, or to avoid some evil, either real or supposed, and may, consequently be regarded as modes or modifications of volition.[9] The organs of sense will be the proper

9 Locke, without entering into minute details, merely considers the passions generally, "as modes of pleasure and pain;" Essay, book 2 ch. 21. v. i. p. 215..0. Hartley still farther illustrates this principle, and also shows in how great a degree they are influenced by association; On Man, prop. 89. v. i. p. 368..373. Hume's "Dissertation on the Passions" contains many ingenious observations on the mode in which they are called into operation, the connexion which they have with each other, and the relation which they bear to our ideas, both perceptive and intellectual; Essays, v. ii. p. 184..221. A considerable portion of Reid's treatise "On the Active Powers," is devoted to the subject of the passions; see particularly, Essay 3. ch. 6. p. 180, et seq.; it displays the usual excellencies and defects of his writings: the style is clear, and the illustrations generally appropriate; but it is diffuse, and he manifests a zeal and pertinacity for his peculiar doctrines, which not unfrequently degenerate into uncharitableness and prejudice. Cogan's "Philosophical Treatise" contains many useful remarks on the distinction between the different passions, although, I think, in some cases, rather too technical and refined.

inlets of the passions; but the external senses themselves are only affected by them in a remote or secondary manner, while we shall find that certain of the physical functions are placed more immediately under their influence.[1] A perception received by the eye or the ear, combined with some previous idea of danger, excites the passion of fear. But the effects of fear are especially manifested upon the heart and arteries; the pulse becomes irregular, throbbing violently or being nearly suspended, according to the degree of the emotion or the mental feeling immediately connected with it.[2] The extent to which this action may proceed is absolutely indefinite; we have numerous examples, in which the effects produced upon the circulation by mental excitement have remained during life, and to such an excess has this excitement been occasionally carried, as to have caused instant dissolution.

[1] Grove defines a passion to be "any emotion of the soul (mind) which affects the body, and is affected by it." Works, v. iv. p. 228. Cogan devotes a section to the "medical influence of the passions," in which he details their effects upon the physical functions generally; Treatise, part 2. c. iii. sect. 1. p. 278, et seq.

[2] In Mr. Bell's elegant treatise "On the Anatomy and Philosophy of Expression," we meet with many valuable observations on what may be termed the physiology of the passions, or the mode in which certain organs of the body serve to express certain mental emotions. It would appear that the nerves which he styles respiratory are the primary agents, and that they transmit the impressions made upon the nervous system to the muscles of the face and the neighbouring parts, by means of which the ultimate effect is produced; Essays, No. 1 . . 6, passim.

It is not the circulation alone which is affected by the passions, nor do they act merely by increasing or diminishing the vital energy of the whole system. Particular organs seem to feel the effect of particular mental emotions; fear and joy act upon the heart, surprise appears more especially to affect the respiration, and grief the digestive organs. We shall find a clear indication of this connexion in our common forms of speech, which must have been derived from observation and generally recognized, before they could have become incorporated with our language. The paleness of fear, the breathlessness of surprise, and the bowels of compassion, are phrases sanctioned by the custom of different ages and nations.[3]

It was probably from dwelling upon considerations of this description, that Bichat was led to form what appears so singular a conclusion, that the organic functions are the primary seat or origin of the passions.[4] Nothing, however, appears to me more clear, than that the ordinary conception on this subject is the correct one; that the passions are, in the first instance, mental operations, and of course connected, like other operations of this kind, with the nervous system. They originate, according to circumstances, either from impressions made by external objects, from certain internal feelings, or from ideas; all of these distinct sources being more or less

3 Parry mentions many instances where certain mental emotions produce a peculiar effect upon certain secretions; Pathol. § 666.

4 Sur la Vie et la Mort, par. 1. art. 6. § 1..3. p. 36..50.

combined and connected together; but the passions, when formed, have their seat in the nervous system, and through this it is that they exert their influence over the various parts of our frame.

This view of the subject will lead us to the conclusion, that the passions are, to a certain extent, innate, or that different individuals, placed under the same circumstances, will exhibit different passions, depending upon a difference in their physical constitution. This difference may be referred either to a difference in the organization of the individual, by which certain organs are disposed to receive particular impressions in preference to others, or merely to a greater or less delicacy of the nervous system generally, by which the same impressions are more or less acutely perceived. According to the degree in which the passions depend upon physical causes, in the same proportion must they be regarded as under the influence of the corporeal organization; but, I conceive, that there is none of them in which this combination of the two sets of causes cannot be traced.

The opposite doctrine has, indeed, been defended by some ingenious metaphysicians, and particularly by Helvetius. He contends, that every individual is originally formed with an equal capacity for receiving the impression of external objects or of internal sensations; but that, from the effect of education, or of various incidental circumstances, we acquire the power of attending more or less minutely to our perceptions; and that, from the same cause, they make a greater or less impression upon the mind, and consequently become associated, in various ways, and with various degrees of force, with our mental powers.[5] Hence, according to this hypothesis, the varieties which we observe in the passions of individuals may be referred, in a great measure, to the accuracy with which they attend to and recollect their sensations; this circumstance being itself, in the first instance, the result of accident or of some extraneous cause, apparently slight, and afterwards entirely overlooked.

But I apprehend that this opinion is inconsistent, no less with actual fact than with correct hypothesis. Those who have been much in the habit of observing children, can scarcely have failed to notice a difference in their passions and dispositions, showing itself at the earliest period in which they give any indication of perception or intellect, where there has been the greatest similarity in the modes of life and in the acquired habits. No one can doubt that there is an original difference in the form of the body, in the strength of the limbs, in their capacity for action, and in the perfection of the organs of sense; yet these are all subject to be much modified and affected by external causes. In the same way, it is reasonable to suppose, that there is a provision in our frame for an original difference in the mental

5 This principle forms the leading subject of his treatise "De l'Homme:" the mode in which he reasons may be learned by perusing the first eight chapters of his first section; Œuvres, t. iii. p. 24, et seq.

powers, which is either fostered or counteracted by the force of education and the general habits of life. What we term disposition or character, may be regarded as a compound of the passions and the understanding. The latter we conceive to be composed of the ideas which the mind originally acquires from external objects; and if we suppose the former to depend, in a considerable degree, upon original constitution, we shall be at no loss to account for the actual condition of human nature, exhibiting strong marks of a native bias, yet influenced in various modes by accidental impressions.

Both metaphysicians and physiologists have been in the habit of arranging the passions into two great divisions, under the denomination of exciting and depressing, according as they are supposed to operate in stimulating or depressing the vital powers.[6] Of the organic functions, the one which is the most affected is the circulation, and perhaps it is on this alone that we can conceive their action to be primarily exerted. No one can doubt that anger increases the action of the heart, or that in fear the blood is not transferred with the usual force through the different parts of the sanguiferous system. For the most part, however, it would appear more probable

[6] This division of the passions into exciting and depressing does not correspond to the two great causes to which the origin of the passions has been referred,—the desire of procuring pleasure and of avoiding pain; this is more connected with moral considerations, while the former principally regards their physiological effects.

that the exciting and depressing effects of the passions are produced through the intervention of the nervous than of the sanguiferous system; and that, according to the nature of the action upon this part of our frame, we are to look for the effects which respectively produce the two classes of mental emotions.

But, although we may conclude that the passions act in the first instance upon the nervous system, and secondarily upon the circulation; yet there are various circumstances which lead us to conclude, that the difference in their action is something more than that of degree, and that, as I remarked above, particular organs are specifically affected by particular passions. While, therefore, we perceive, on the one hand, that organization materially influences the passions, so it would appear, on the contrary, that the passions affect the functions of the organs, and it is not unreasonable to conclude, that they may ultimately affect the structure of the organs themselves. If a violent emotion of grief, or the indulgence of a fit of anger produces a temporary derangement of the stomach, we may suppose that the long continued exercise of these feelings, or their frequent recurrence, may so alter the actions of the parts, as permanently to injure their functions, and finally to affect their structure. Here again we find the testimony of common observation in favour of our speculation. We may conclude, that proverbial aphorisms are, for the most part, founded in truth, and we shall perceive it confirmed in the connexion which subsists between a cheerful disposition and a tendency to fatness. In the same manner, expressions which are generally regarded as entirely metaphorical, will often be found to originate in a simple matter of fact. A sour disposition is probably at some times the effect, and at other times the cause, of an acid state of the stomach; and a man, who is said to possess a warm heart, will often indicate a higher degree of animal temperature, than one who is characterized by coldness of disposition and moderation of feeling. In all such cases it is difficult to discriminate between the effects of external circumstances and of internal structure and organization; but the facts appear to be most easily and satisfactorily explained, by supposing that they depend upon the joint operation of both these causes.

## CHAPTER XIX.

### OF CRANIOSCOPY AND PHYSIOGNOMY.

The view which I have taken of the connexion that subsists between the physical structure of the nervous system and the mental faculties, naturally brings me to a subject which has of late attracted a considerable degree of attention among anatomists and physiologists—the dependence of the character and disposition upon the peculiar shape and organization of the brain. Certain facts, which seemed to favour this opinion, had been long noticed; persons of observation were in the habit of associating the idea of superior intellect with a capacious and prominent forehead, while the contrary form was equally conceived to indicate a deficiency of the mental powers. The inspection of the skulls of the insane, and still more of idiots, seemed to prove, that the perversion or deficiency of their faculties was connected with a peculiar form of the head,[7] and it was thought that a kind of analogy might be traced through the lower animals, which favoured the same conclusion. When the sculptors of antiquity formed

7 Lavater gives us the outline of the features of a number of idiots, which, it will be admitted, are very characteristic of the defective state of their mental faculties; Essays, by Holcroft, v. ii. p. 280. See also the plates in Pinel, Sur l'Alienation Mentale; and in Gall and Spurzheim's Anatomy, No. 19, 20.

the statues of their gods or heroes, to which they were desirous of imparting the character of high intelligence, they endeavoured to accomplish this by giving a peculiar form to the head; and many expressions, employed in the languages of various ages and nations, show that an opinion of this kind has been commonly adopted. But it was embraced in this general way rather as a speculation, countenanced by a few casual observations, than as the correct deduction of a number of well ascertained facts, which were capable of acquiring a philosophical character, and of forming a distinct department of philosophical science.

### § 1. *Nature and Object of Cranioscopy.*

The subject was first placed in this point of view by Drs. Gall and Spurzheim, who, in consequence of their accurate dissection of the brain, and their mode of separating its different parts from each other, were led to conjecture, that these parts were appropriated to distinct mental faculties. Dr. Gall had previously devoted himself to an examination of the natural indications of character which are exhibited by individuals, and had convinced himself, that the varieties which we observe in this respect are to be regarded as in a great measure innate. Proceeding upon this principle, and assuming that the brain is the organ through the intervention of which the mental faculties are exercised, he conceived it to be not improbable, that a physical difference in the form and structure of the brain might be detected, corresponding to these differences in the native character and dispositions. Partly, as it would appear, from his idea of the anatomical structure of the brain, in what regards the relation of its different parts to each other, and partly from a pre-conceived hypothesis, he fixed upon the external convolutions of the cerebrum and cerebellum, as the respective seats of the individual faculties; and proceeding upon the supposition, that the size of an organ must be a measure of the capacity which it possesses of exercising its appropriate functions, he deduced the principle, which lies at the foundation of the new doctrine, that the character and disposition are necessarily connected with the respective size of the convolutions of the brain. It is farther assumed, that the size of the convolutions may be ascertained by an examination of the form of the cranium, the peculiar shape of which, as it differs in different individuals of the same species, is conceived to be, in a great measure, determined by that of the brain; as we find, in other organs of the body, that the hard parts are frequently moulded by the growth of the softer parts that are contiguous to them. Hence we derive the practice of cranioscopy,[8] or the art by which we endeavour to discover the nature and extent of the mental faculties, by ascertaining the form of the skull.

The arguments which have been urged in favour

[8] I may remark, that this subject, which was originally brought before the public under the appellation of cranioscopy, has been lately styled phrenology; but as the first of these terms appears to me the most appropriate and descriptive, I shall continue to employ it.

of the science of cranioscopy are partly anatomical and partly physiological. In the first place it is said, that the brain exhibits a very elaborate structure, and a very complicated organization, and it is therefore reasonable to conclude, that its different parts must be subservient to the exercise of different functions. Secondly, both metaphysicians and physiologists have been in the habit of referring all the impressions which we receive through the intervention of the nerves to some central part of the brain, but the great diversity of opinion which exists respecting the part which ought to be regarded as this common centre, affords us at least a strong presumption of its non-existence, while, on the contrary, if we suppose that there actually is such a central spot, we are at a loss to assign any use to the remainder of the brain. Thirdly, we are in possession of a number of observations upon the partial loss of the mental faculties, in consequence of disease or injury of the brain; and although we are not able to trace out the connexion between the situation of the injury received and the defect of the mental powers, yet it favours the opinion that these faculties are distributed over the different parts of which the brain is constituted. Fourthly, the analogy of the nerves that are connected with the external organs of sense is adduced by the cranioscopists in favour of their doctrine. Each of these nerves, in conveying their respective impressions, must exercise a different office, and in the same way, the different convolutions of the brain are supposed to be the organs of the respective mental functions. Fifthly, it is argued that the state of the brain, in regard to its perfection and full development, corresponds to the state of the mental faculties at the different periods of life, and also to their degree of perfection among the inferior animals, so as to indicate a necessary connexion between these circumstances. Sixthly, the brains of different individuals actually differ in the proportionate form and size of their parts, and it is therefore reasonable to presume, that this may be the cause of the difference which is admitted to exist in the faculties of different individuals. Seventhly, the exercise of the mental powers, like those of the physical functions, is attended with fatigue; but it is found by experience, that the fatigue only extends to that particular power which has been exercised; it may, therefore, be presumed that its action is confined to a certain portion of the brain only. Eighthly, proceeding upon the principle, that the dispositions and mental faculties are, to a certain extent, innate; and, observing that they exist in different individuals in different proportions, it follows that they must be attached to different organs.

The above appears to me to exhibit a fair statement of the nature of the arguments which have been employed, to prove the antecedent probability of the doctrine of cranioscopy. But its advocates are aware, that its merits must principally rest upon the degree in which it is found to correspond with well ascertained facts and correct observation, and with the power which it actually affords us of acquiring a knowledge of the character and disposition of individuals by an examination of the skull. It is therefore by an appeal to experience, that the supporters of cranioscopy, and Dr. Spurzheim in particular, attempt to establish their opinion, and they have accordingly brought forward a number of facts of this description, which are supposed to form a sufficiently firm basis for their system. They consist of the results which were obtained by examining the heads of the various individuals of all ages, ranks, and conditions, minutely noticing the deviations from the average form, especially with regard to the size and situation of the eminences or protuberances which they exhibited. The examination has also been extended to the inferior animals, and the same principles have been applied to their skulls, both as to what respects their general form, and the proportionate size of their individual parts, whether indicating a generic or an individual difference.

In estimating the value of these arguments, I shall arrange them in two divisions, as they relate to general considerations of probability, or as they depend more upon particular facts. And with respect to the first point, I think it will be admitted that there is none of them which possesses more than an indirect application to the question under discussion. Admitting that the perfect organization of the brain is a necessary intermedium for the exercise of the mental powers, we may conclude, that every part of this organ must have a necessary connexion with the exercise of these powers, as every part of the eye and the ear has a reference to the production of vision and of sound. In consequence of our knowledge of the physical laws of light and of the undulations of the air, we are enabled to trace out the mode in which the several parts of the eye and of the ear co-operate to produce the ultimate effect. Had we the same knowledge of the mode in which the mind operates upon the brain, we should probably have it in our power to detect the same kind of co-operation of all its parts and structures to the production of perception and thought. But on this point we are in total ignorance, and therefore, although we may go so far as to assert, that a perfect brain, in a cer ain sense, is essential to a perfect mind, we are unable to say in what way it is so.

The only anatomical argument which is of so tangible a nature as to allow of any thing approaching to direct deduction, is derived from a consideration of the degree in which an injury of the brain produces a corresponding injury of the mental powers. Upon this point I have already stated my opinion, and I have only to add, that while the connexion is not of that nature which indicates the relation of cause and effect, so I should be still less disposed to allow, that the facts which we possess are of that distinct and direct nature, which can enable us to connect particular injuries of the brain with corresponding injuries of particular faculties.

The position, that the size of an organ is an indication of the degree of its power or capacity, a position which may be regarded as almost the fundamental principle on which the whole doctrine rests, is in direct contradiction to fact. To revert to the case of the eye; it may be asserted that the perfection of this organ, either when considered with respect to the different species of animals, or to the different individuals of the same species, does not bear the least relation to its size, but depends entirely upon the nature of its organization, and, except in those cases where the exercise of an organ is connected with mechanical force, as in muscular contraction, bulk has no relation to the perfection of a part.

The analogy that has been so much insisted upon, of the power which the several organs of the body possess of exercising their appropriate functions, will, I apprehend, be found upon examination to be inapplicable to the case of the brain. We perceive that the eye is especially adapted for receiving the impressions of sight, and we can explain the mode in which it acts upon the rays of light. We know, on the other hand, that the ear is not adapted for receiving the impressions of vision, nor of being affected by the rays of light, and we hence conclude, that the ear exercises a different faculty from the eye. But as far as the argument would apply to the brain, we must consider it as a single organ, although composed of various parts, and the different mental powers as modes or species of the same faculty. And proceeding upon the same principles in this case, as with respect to the organs of sense, we should consider the brain, taken in the whole extent, as the organ of mind, confessing our ignorance of the particular use of its minute parts, or of the manner in which its powers are affected or modified.

And even were it proved, as a general principle, that distinct parts of the brain were appropriated to distinct mental functions, we may still be permitted to doubt, whether the cranioscopists have been fortunate in their division and appropriation of the functions which are supposed to possess these distinct localities. If we consider the subject theoretically, we might presume, that there would be a separate organ corresponding to each of the external senses, as the impressions are themselves distinct in their nature, and might be supposed to require some different modification of the nervous matter for their perception. And again, with respect to the intellectual powers, there are some which appear so distinct from the others, that we might apply to them the same mode of reasoning, and suppose it probable that they might possess their appropriate organs. The faculty of memory might be supposed to require a different modification of the nervous power from that of the imagination ; and this again from that of abstraction or volition. But we do not observe any classification or division of this kind in the faculties that are enumerated by Dr. Spurzheim or his disciples. Some of them are complex feelings, resulting from the union of primary perceptions with ideas; others appear to be a combination of ideas only ; some may be regarded as the obvious result of association ; and others again as the effect of association operating through the intervention of education, or of the accidental circumstances in which the individual has been placed.[9]

And, with respect to what may be regarded as the practical application of the art or science of cranioscopy, it may be objected, that the convolutions of the cerebrum are not what one should expect to be the seat of the ultimate operations of the organ. They are not the part in which we behold that elaborate and complicated structure, the existence of which has been supposed to form so powerful an argument in favour of the doctrine, while this view of the subject still leaves unexplained the uses of the more minutely organized parts, that are situated in the interior of the brain. And, farther, were we to admit the position, that the convolutions of the brain are the seat of the mental faculties, it would be necessary to establish two points, before we could

9 In Dr. Spurzheim's "Anatomy of the Brain," we have the situation of the different organs delineated in pl. 5, 6, 7; the number enumerated is thirty-five, and their denominations are as follows:—Amativeness, philoprogenitiveness, inhabitiveness, adhesiveness, combativeness, destructiveness, secretiveness, acquisitiveness, constructiveness, self-esteem, love of approbation, cautiousness, benevolence, veneration, firmness, consciousness, life, marvellousness, ideality, mirthfulness, imitation, individuality, configuration, size, weight and resistance, colouring, locality, calculation, order, eventuality, time, melody, language, comparison, causality. This work contains the last account of Dr. Spurzheim's peculiar views respecting the structure of the brain, the relation of its different parts to each other, and the mode in which they are the most advantageously detached and exposed to view. It is accompanied by a number of expressive engravings.

employ them as indications of these respective powers; first, that the convolutions of all brains occupy corresponding situations with respect to the cranium, or are exactly opposed to the same portion of its internal surface;[1] and, secondly, that the cranium is, in all its parts, of a uniform thickness, so as to afford us, by its external surface, the means of acquiring an accurate knowledge of the convolutions that are subjoined to it.

But, although I conceive that the above considerations are not without their weight, and that, upon an impartial review of the subject, they are such as may at least counteract the hypothetical arguments that have been advanced in favour of cranioscopy, I am disposed to agree, with what appears to be the principle of the most intelligent advocates of the doctrine, that the question can only be decided by an appeal to facts. These facts are of two kinds, although exactly coinciding in their object. We must obtain skulls that are marked by some peculiarity of form and shape, and must then endeavour to learn what was the natural character of the subject; or we may take the cases of those who have shown some decided peculiarity of disposition and character, and may examine the figure of their skulls. A sufficient number of these observations, carefully made and impartially recorded, cannot fail to decide the question, whether there be any ground for the doctrine of the appropriation of the different parts of the brain to distinct faculties, and, more particularly, whether

[1] We are informed by Dr. Craigie, in his valuable manual of "Pathological Anatomy," that this is certainly not the case; p. 306, 7.

we have it in our power to ascertain their seat by an external examination of the cranium. On this point I must give it as the conviction of my mind, that the facts hitherto adduced are altogether inadequate to the end proposed, that they are frequently of doubtful authority and of incorrect application, and that nothing but the love of novelty, and the eagerness with which the mind embraces whatever promises to open a new avenue to the acquisition of knowledge, could have led men of talents and information to place any confidence in them.

In offering thus freely my objections to the doctrine of cranioscopy, I have thought it proper to abstain from certain topics, which have been generally urged against it, since I consider them to be, in a great measure, the offspring of bigotry and illiberality. If, on the one hand, its advocates have been hasty and credulous, it must be admitted, on the other hand, that its opponents have too frequently been harsh and uncandid. But its principles are too widely disseminated, and have taken too deep root in the public mind, to be repressed by mere authority or counteracted by ridicule; they must be put to the test of experiment, and by this standard alone will their merits be ultimately appreciated.[1]

[1] I have subjoined a list of some of the principal works that have appeared in illustration of the doctrine, commencing with those that were published by Drs. Gall and Spurzheim themselves. Gall, Cranologie; Gall et Spurzheim, Recherches sur le Système Nerveux; Gall et Spurzheim, Anatomie et Physiologie du Système Nerveux; the latter accompanied by a series of

## § 2. *Nature and Object of Physiognomy.*

Nearly allied to the science of cranioscopy is that of physiognomy, but differing from it in this respect, that the former professes to judge of the character by the shape of the head, while the latter principally makes use of the form of the features and the general aspect of the countenance. Physiognomy is a science of very early date, and was strongly insisted on by many of the ancients, but among the moderns it was little cultivated by men of talents, until the publication of the work of Lavater. It must be admitted,

beautiful engravings; the Physiognomical System of Drs. Gall and Spurzheim; Spurzheim's Examination of the Objections made to his Doctrine; Spurzheim, Essai Philosophique sur la Nature Morale et Intellectuelle de l'Homme; Spurzheim's Anatomy of the Brain, by Willis. One of the earliest accounts of the doctrine is in the Edin. Med. Journ. v. ii. p. 354, et seq.; this article contains a critique on various treatises by Bischoffe, Walter, and Hufeland; Forster's Sketch of Gall and Spurzheim's System; Combe's Essays on Phrenology; this work, of which several successive editions have appeared, may be regarded as the most elaborate and spirited defence of the system; M'Kenzie's Illustrations of Phrenology. Although a number of strictures have, at various times, been published on the doctrine of cranioscopy, they have appeared in the form of detached essays or articles in the periodical journals; and, 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. I must, however, except from this censure the article "Cranioscopy," in the Suppl. to the Edin. Encyc. v. iii. p. 419, et seq. by Dr. Roget, which is truly characteristic of the cultivated and candid mind of its author.

as a matter of fact, that there are few persons of any reflection, or of any knowledge of human nature, who do not almost involuntarily exercise their judgment on the physiognomy of every new face that is presented to them. Without regard to any physiological speculation or controverted opinions, we, as it were, instinctively attach the idea of a certain disposition to a certain countenance, and regard one set of features as an index of wit and another of stupidity. Nor does this idea want the support of plausible hypothesis. The great instruments of expressing the human passions and feelings are the muscles of the face, and when any passion is strongly marked and frequently repeated, the muscles acquire a tendency to maintain this position even when the corresponding feeling ceases to exist. And farther, by the frequent and powerful contraction of certain muscles, the shape of the neighbouring parts may be affected, the tendons may be permanently extended or contracted, and even the bones of the face may be somewhat altered in their form. The science of physiognomy affords indeed much scope for fancy, and, it must be acknowledged, that the peculiar genius of Lavater was not the best adapted to reduce it to the strict rules of induction. His character was marked by enthusiasm rather than by judgment, and although he was very assiduous in the collection of facts, he was deficient in the power of arranging them, and drawing from them any general principles.[2]

[2] The biographical memoir of Lavater, which is prefixed to Holcroft's translation of his Essays, and which is principally taken

He informs us, that in the prosecution of his inquiry, he was influenced by a kind of mystical feeling, which he is unable to describe, and in the formation of his system he constantly appeals to a species of instinctive impression, rather than to any principles of correct reasoning.[3] The basis of his hypothesis, if it may be so called, rests upon a fanciful division of the face into three regions, the upper part being that of the intellectual life, the middle of the moral, and the lower part of the animal life; these are supposed to be analogous to the head, chest, and abdomen, and are respectively the seats of three corresponding classes of faculties. He frequently appeals to the common experience of mankind in proof of the truth of his doctrine, and he maintains that no one who does not possess what he terms "physiognomical sensation" can become an adept in his art. "Whoever," he says, "does not discover in Haller the energetic contemplative look and most refined taste, the deep reasoner in Locke, and the witty satyrist in Voltaire, even at the first glance, can never become a physiognomist."[4]

The positions which he labours to prove, as the foundation of the science, are, that "there is a certain correspondence of internal power and sensation with external form and figure," that every part of the face

from the account of his son-in-law, Gessner, shows him to have been enthusiastic, credulous, zealous, and sincere.

[3] See particularly § 2, entitled "A Word concerning the Author;" also § 5, "Of the Truth of Physiognomy;" also v. ii. § 3, p. 14, et seq. in Holcroft's Trans.

[4] Essays, by Holcroft, v. i. p. 118.

is to be regarded as the organ of a peculiar and appropriate sensation or passion, that it is by studying the lineaments of the countenance, and the changes which they experience, as depending upon the passions and mental emotions, that we are to obtain a knowledge of the character and disposition of the individual, and that, for this purpose, we must compare the shape and relation of the different parts of the countenance with the particular traits of his character. The basis of the forehead he seems to regard as the part of the head which gives the most correct indication of the intellectual powers, but he conceives that the general form of the lower part of the face, as well as the outline of the skull,[5] may assist us in our examination. Although, as we have seen, he rests his doctrine so much upon an appeal to general experience and to popular feeling, yet he enters into a minute detail respecting the form of all the different parts of the face, the forehead, the eyes, the eyebrows, the nose, mouth, lips, teeth, and chin; and endeavours to point out the relation which they ought to bear to each other and to the whole countenance. Considering Lavater's work as a great collection of features and

[5] V. ii. p. 205 . . 241. He devotes a considerable degree of attention to the form of the skull, and indicates the mode in which we are enabled to judge by its means of the nature of the character. His remarks are accompanied by a number of outline drawings, but, as is commonly the case, without giving any specific rules for their application. His observations are directed to the general form of the bones of the head and face, and we find nothing in them which relates to the protuberances of the different parts of the skull, which forms the basis of Dr. Gall's system.

countenances, it may be styled a valuable repository of facts;[6] but every one must perceive that his inferences are frequently not sanctioned by the premises, and that his judgment is often warped by prejudice.

The object, whether real or imaginary, of the sciences of cranioscopy and physiognomy is to distinguish between the mental faculties or dispositions of different individuals. Whatever may be our opinion respecting the origin of these differences, whether innate or acquired, and whatever may be our means of ascertaining them, no one can doubt of their existence, even at a very early period of life. What may be called the mechanism of the human mind (an expression which is employed without intending to convey any theory respecting the ultimate cause of the intellectual phenomena) ought to form a very principal object of attention with the moralist and the public instructor, and more particularly with those engaged in the education of youth.

[6] Independently of any literary merit, Hunter's translation of Lavater's Essays, embellished by Holloway's engravings, constitutes a beautiful specimen of English art.

## CHAPTER XX.

### OF VARIETIES AND TEMPERAMENTS.

The physical part of the human frame exhibits no less decisive marks of original differences in its organization than the mental. When these differences consist in obvious external characters, which attach to whole nations, or to large communities, they are called varieties; when they belong to a certain number of individuals, and are more connected with internal constitution, they are styled temperaments; and when the peculiarities exist in one individual only, idiosyncracies.

### § 1. *Of the Varieties of the Human Species.*

To determine the number of varieties into which the human race ought to be arranged, and to point out the precise features by which each of them is characterized, may be conceived to belong more properly to the province of natural history; but the cause of these varieties is a subject which strictly falls under the cognizance of the physiologist, and upon which I shall therefore proceed to offer a few observations.

And, in the first place, it will be proper to point out some of the more remarkable circumstances by which man is distinguished from all other animals, as we shall, by this means, be better able to appreciate the nature and extent of the differences between the different tribes of the human race. These circumstances are arranged by Blumenbach under the five heads of external form, internal organization, functions, mental qualities, and diseases. Among the more prominent of these are the erect posture; the peculiar form and construction of the anterior and posterior extremities, as connected with their respective uses of prehension and loco-motion; the more elaborate structure of the hands, and especially the size and position of the thumb;[1] the more general action of the digestive organs, so as to constitute man what has been termed omnivorous; his power of accommodating himself to all climates; his slow growth, long infancy, and late puberty; certain sexual peculiarities; the faculties of reason and invention; and lastly, the power of uttering and understanding articulate sounds.[2]

[1] Some writers, as for example, Darwin, have gone so far as to attribute a great part of the superiority of man to the position of the thumb with respect to the fingers, which enables him to grasp and handle objects with more dexterity and minuteness; Zoon. v. i. sect. 16. p. 143, 4; the remark is not wholly unfounded, but it is pushed to an extravagant length. See also Helvetius, De l'Esprit, t. i. p. 60. . 2, on the various circumstances in the structure and physical functions of animals (even those that the most nearly resemble man) which contribute to prevent their progressive improvement.

[2] Blumenbach de Gen. Hum. var. nat. sect. 1. Mr. Lawrence has very amply and satisfactorily pointed out the characteristics, both anatomical and physiological, which distinguish man from all

These various circumstances, and others of less moment, which I have omitted to enumerate, are amply sufficient to establish the position, that man is so far removed from all other animals, both in his form and his functions, as to entitle him to be regarded as a distinct species. But when we take a survey of the whole human race, although we find that they agree in their general form and structure, and exhibit a general similarity in their functions, we observe, on the other hand, that there are many points in which they differ very materially among themselves, and that these differences are transmitted from one generation to another, so as to prove that they are not the effect

other animals; he has ably exposed the exaggerations and errors into which some authors of considerable celebrity have fallen, when they have attempted to approximate certain varieties of the human race to the inferior animals; Lect. sect. 1. p. 134..242. The principal circumstances mentioned by Mr. Lawrence are smoothness of the skin, and absence of natural means of defence; erect stature, with various anatomical points necessarily connected with it; possession of two hands and their perfect structure; great proportion of the cranium to the face; structure and relation of the jaws; structure and position of the teeth; development of the cerebral hemispheres; proportion of the brain to the nerves; greater number and superiority of the mental faculties; speech; capability of inhabiting all climates, and subsisting on all kinds of food; slow growth, long infancy, late puberty, and certain sexual peculiarities. Camper has pointed out, with much clearness, the leading circumstances in which the anatomical structure of man differs from that of other animals; "Deux Discours sur l'Analogie qu'il y a entre la Structure du Corps Humain et celle des Quadrupedes," &c. in Œuvres, t. iii. p. 527, et seq. with the accompanying plates.

either of external circumstances or of mere fortuitous causes.

Naturalists have differed with respect to the number of varieties into which the human race is to be divided; but the division of Blumenbach is the one which is the most commonly adopted, and which appears to be founded upon the most correct observation. He fixes the number of varieties at five; the Caucasian, so named from its supposed origin in the western part of Asia, the Mongolian, the Æthiopian, the aboriginal American, and the Malay. The Caucasian he regards as the standard or type of the rest; this together with the Mongolian, and the Æthiopian, forming the three most distinct varieties, while the American may be regarded as intermediate between the Caucasian and the Mongolian, and the Malay between the Caucasian and the Æthiopian.[3]

[3] De Gen. Hum. var. nat. sect. 3. See also Lawrence's Lectures, p. 326, 7, and plates Nos. 1..5, taken from Blumenbach, and his 10th chapter, where will be found a collection of very valuable facts, and a copious list of references. Dr. Prichard, in his interesting and valuable "Researches into the physical History of Man," minutely examines the different circumstances which are pointed out by Blumenbach, as forming the characteristics of his five varieties, and in consequence of the number of exceptions which are to be met with, he is disposed to reduce them to three. From the form of the upper part of the head, especially from its breadth, as viewed posteriorly and vertically, he designates them by the terms mesobregmate, stenobregmate, and platybregmate: these nearly coincide with the three principal varieties of Blumenbach, the Caucasian, the Mongolian, and the Æthiopic, vol. i. p. 173, 4. Other physiologists, especially among the French, who are generally disposed to multiply divi-

Their present distribution over the face of the globe would appear to coincide nearly with what it has been ever since we were in possession of any adequate description of the different parts of its surface. The Caucasian inhabits the whole of Europe, except the northern parts of Sweden, Norway, and Russia, the west of Asia as far as the Oby, the Caspian sea, and the Ganges, together with the north, and even a portion of the interior of Africa. The Mongols are spread over the central and eastern parts of Asia, with the exception of the Peninsula of Malacca; they also stretch along the whole of the arctic regions, from Russia and Lapland, to Greenland, and the northern parts of America, as far as Behring's Straits. The Æthiopic variety inhabits the greatest part of the central, and the whole of the southern parts of Africa, and is also found dispersed over some of the Oriental Islands. The Malays, however, constitute the greatest part of the inhabitants of these islands, as well as of Malacca and the islands of the Pacific Ocean; while the whole of America, except the northern extremity, is the

sions in all branches of science, have added to the number of Blumenbach. A late writer, M. Bory de St. Vincent, in his treatise entitled, "L'Homme," has extended them to fifteen; t. i. p. 82, 3; see also Brewster's Journ. v. v. p. 39..1. In the third vol. of Buffon's Nat. Hist. sect. 9. p. 302, et seq., we have many interesting observations on this subject, delivered in the animated style which characterizes the works of this author. See also remarks by Lacepede, art. "Homme," in Dict. Scien. Nat. t. xxi. p. 382..392; et Virey, Histoire Naturelle du Genre Humain, in his first section, t. i. p. 119, et seq.

native seat of what are termed the American Indians.

An interesting question here presents itself, whether these different varieties, as they have been termed, the European and the African for example, are derived from one common stock, the present differences being merely the result of circumstances, operating on them through a long course of ages, or whether they have sprung from different parents, each possessed of the characters of their respective descendants. In technical language are they varieties only of the same species, or are we to regard them as distinct species.[4]

[4] Many persons are disposed to regard this discussion as altogether useless, or even to denounce it as impious, alleging that the question is decided by the account given us in the commencement of the book of Genesis, of the creation of the human race. But I conceive it to be a legitimate object of inquiry. We do not find that the writer of this book lays claim to any superhuman source of information with respect to natural phenomena, while the whole tenor of his work seems to show, that on such topics, he adopted the opinions which were current among his contemporaries. We may respect the feeling which produces this zeal in the cause of religion, but we must lament the indiscreet mode in which it is exercised. It might have been expected, at least in a protestant country, that the example of Galileo would have proved to us the danger of identifying the truth of our theological creed with the correctness of our philosophical speculations, and that the liberty which we are compelled to allow to the astronomer, might have been extended to the inquirers into the other departments of natural knowledge. But some recent examples show us that this period is not yet arrived. "Well, indeed, it is for us," to borrow the expressive language of a writer in the Quarterly Review, "that the cause of revela-

Although the division of the objects of natural history into the different gradations which constitute what is termed a scientific arrangement is too often altogether arbitrary, with regard to the case now before us, we are indebted to Blumenbach for an accurate conception of the object of the inquiry. According to his definition, animals may be considered as belonging to the same species, when they agree so far in their form and habits, as that those points in which they differ may be referred solely to the effect of what he terms degeneration;[5] while, on the contrary, when these differences cannot be referred to any source of degeneration, they must be considered as belonging to different species.[6] The causes of de-

tion does not depend upon questions such as these: for it is remarkable that in every instance the controversy has ended in a gradual surrender of those very points, which were at one time represented as involving the vital interests of religion. Truth, it is certain, cannot be opposed to truth. How inconsiderate a risk then do these advocates run, who declare that the whole cause is at issue in a single dispute, and that the substance of our faith hangs upon a thread, upon the literal interpretation of some word or phrase, against which fresh arguments are springing up from day to day." v. xxix. p. 163.

[5] I may remark, that the term degeneration is not used by Blumenbach in its popular sense, as being equivalent to deterioration, but is employed, in conformity with its derivative meaning, to signify any deviation which takes place from the primary type, or original condition of the species.

[6] On the constitution of a species we have the following remarks in Dr. Prichard's "Researches;" "..the term *species* must be solely applied to those collections of individuals which so resemble each other, that by referring merely to the known and well ascertained operation of physical causes, all the differences

generation which he points out are temperature, climate, modes of life, diet, and some other circumstances of an analogous nature, and he examines at length the operation which these several circumstances may be supposed to have had in altering or modifying the human form and constitution.[7]

between them may be accounted for, so as to present no obstacle to our regarding them as the offspring of one stock, or, which is the same thing, of races precisely resembling each other." v. i. p. 92. Dr. Fleming defines the word species to be "a term universally employed to characterize a group, consisting of individuals possessing the greatest number of common properties, and producing, without constraint, a fertile progeny." Phil. of Zool. v. ii. p. 148. Cuvier observes, on this subject, "On est obligé d'admettre certaines forms, qui se sont perpetuées depuis l'origine des choses, sans exceder ces limites; et tous les êtres appartenans à l'une de ces formes constituent ce que l'on appelle une *espèce;* ses varietés sont des subdivisions accidentelles de l'espèce." Regne Animal, t. iv. p. 19.

It might seem that this question is decided by the criterion that was proposed by Hunter and other eminent naturalists, whether the offspring be prolific. It is sufficiently proved, that in most cases where the experiment has been tried, a hybrid produced from parents who are admitted to be of different species, is unproductive. This consideration certainly affords a strong presumption in favour of the common origin of mankind; but it appears that we are not yet in possession of a sufficient variety of facts to allow us to draw the general conclusion; see Prichard's Research. v. i. p. 95 .. 8; Lawrence's Lect. p. 265 .. 9. Hunter's "Observations to show that the wolf, jackall, and dog, are all of one species," even if we allow them to be conclusive as to the point for which the experiments were instituted, can scarcely be considered as bearing upon the general question; Anim. Œcon. p. 143, et seq. Dr. Edwards has lately published an essay, in which these various circumstances are well illustrated, "Des Caracteres Physiologiques des Races Humains."

7 De Gen. Hum. var. nat. § 23. p. 66, 7.

Upon the first view of the subject, we might be tempted to pronounce, without hesitation, that we are acquainted with no natural causes which possess sufficient power to effect so great a metamorphosis as we actually find to exist. It is also alleged, that as far as we can judge, since the first records of history, the same differences have existed that we observe in the present day.[8] Nor do we find those shades and gradations which might have been expected, were the varieties the mere result of external causes operating on the human body, which necessarily act upon different individuals in different degrees. Besides, there are instances where tribes, belonging to different varieties, have for a long space of time lived in the same country, and under the influence of the same circumstances, yet where no approach to a common nature has been observed to take place, but each has retained its peculiar traits.[9] It must be admitted

[8] This was remarkably exemplified in the Egyptian tomb, for an exact copy of which we are indebted to the skill and perseverance of the enterprising traveller Belzoni. Among the figures that were painted on the walls, the difference between the negro and the Arab was as clearly marked as at the present day. Respecting the exact age of this interesting record of antiquity, I do not profess to give an opinion; but there can be no doubt that it is sufficiently remote for the purpose of my argument.

[9] This appears to be the case with the inhabitants of some of the Oriental Islands, where we have the Aboriginal Malays, with a mixture of the Chinese and the negroes; the two latter, as it would appear, having been settled there for some centuries, but each retaining all their distinctive characters. In those countries the climate and the state of society produce a considerable similarity in the habits of the different classes of people, so that

that the different circumstances referred to above may have considerable power over the vital functions; and indeed it is, in a great measure, to their combined operation that we must ascribe the changes which are produced in the inferior animals by what is termed domestication. Yet, I conceive, that if we carefully notice the facts that fall under our observation, we shall scarcely be able to point out that decided and unequivocal influence, which might be supposed necessary to produce the effects that are actually perceived.

One of the most obvious marks of distinction between the different varieties of the human race is the colour of the skin; and as Blumenbach assumes the white variety to be the standard or type of the species,[1] it is necessary, in order to establish the hypothesis, to show how any of the supposed causes of degeneration can produce the black colour of the Æthiopian. He accordingly attempts to account for this colour by supposing, that the heat of the climate gives rise to an excessive secretion of bile, and that, in consequence of the connexion which there is between the action of the liver and the skin, an accumulation of carbonaceous matter takes place in the cutaneous vessels, and that this process being

all of them are exposed to nearly the same physical and moral influences.

[1] Blumenbach assumes the Caucasian as the type of the rest, partly from its possessing the specific characters of man in the most marked degree, and partly in consequence of the form of the head being intermediate between the other varieties.

continued for a succession of ages, the black colour of the skin becomes habitual.[2] But upon this hypothesis we may remark, that although the inhabitants of colder climates, when they pass into the torrid zone, are frequently affected with bilious diseases, and consequently acquire a sallow complexion, this is merely the effect of disease; and as we do not find the natives of these countries to be liable to such affections, we are not entitled to account for the peculiar colour of the skin upon this principle. Besides, we do not find that the children of those who have acquired this sallow complexion, by a residence in warm climates, provided they are themselves healthy, and are born

[2] De Gen. Hum. var. nat. § 44, 5. p. 122, et seq. The following positions contain the fundamental parts of the hypothesis: "Causam equidem proximam adusti aut fusci coloris externorum cutis integumentorum, in abundante carbonaceo corporis humani elemento quærendam censeo, quod cum hydrogenio per corium excernitur, oxygenii vero atmospherici accessu præcipitatum, Malpighiano muco infigitur." p 124, 5. "In universum autem carbonaceum istud elementum maxime in atrabiliis prævalere videtur; manifeste etiam officinæ bilis cum integumentis .... consensus." p. 126. "Tum autem ingens climatum in hepatis actionem potentia, utpote quæ intratropicos cœli ardore mirum quantum excitatur et augetur." p. 126... "Æthiopes vero indigenæ diutissime jam et per longas generationum series climatis istius actioni obnoxii fuere,.." p. 127. An hypothesis very similar to Blumenbach's is adopted by Dr. S. S. Smith, of New Jersey: he remarks, "it appears that the complexion in any climate will be changed towards black, in proportion to the degree of heat in the atmosphere, and to the quantity of bile in the skin." Essay on the Variety of Complexion, &c. p. 30. See the observations of Prichard, v. ii. p. 528..0.

in the temperate zone, inherit, in any degree, the complexion of the parents.[3]

Blumenbach is disposed to refer the dark colour of the Æthiopian, in some measure also, to the direct effect of exposure to the sun's rays, conceiving it to be analogous to that browning or tanning of the skin which takes place, from the same cause, among Europeans. But I conceive, that this eminent physiologist has, in this case, been misled by a false analogy. There is, in fact, no relation between these two affections: their seat is different; the one being in the epidermis, the other in the more vascular part of the cutis. The former is a temporary or transient effect, which is, in a great measure, removed when the immediate exciting cause is withdrawn; while, as

[3] Dr. Prichard enters at length into the consideration of the structure of the parts on which the variety of colour depends, and of the different circumstances connected with it; Researches, ch. iii. sect. 2, 3, 4. p. 131 . . 156. He reduces the varieties of colour to three only, which he technically names the melanic, the albino, and the xanthous. The second of these terms is employed in its ordinary acceptation, while by the first and third the author designates the great mass of the human race, without reference to their situation on the face of the globe or their other peculiarities. The first includes all the dark complexions, from the negro to the European brunet; the latter, "all those individuals who have light brown, auburn, yellow, or red hair." I am, however, disposed to think, that the more popular arrangement of the shades of the skin into white, black, yellow, copper-coloured, and tawny, corresponding to the five varieties of Blumenbach, will be found more discriminative, and more applicable to our physiological investigations.

far as we can perceive, the colour of the negro is not in the least changed by the removal into a colder climate, and is transmitted unimpaired to his posterity.[4]

[4] The best authenticated narratives of travellers prove to us, that although, as a general fact, the Æthiopic variety is found in the hottest regions, yet that there is not an exact proportion between the heat of the climate and the blackness of the skin; see Blum. de Gen. Hum. var. § 43. p. 121, 2. This was particularly noticed by Humboldt, as applicable to the different parts of the American continent, where the natives all belong to the same variety, and are therefore a proper subject of comparison. In the same way we find, from the narratives of Cooke and other navigators, that the inhabitants of Otaheite, and of others of the Pacific Islands, that are situated not far from the Equator, are fairer than the generality of the Malays, and it also appears that the Chinese are fairer than the Esquimaux, both of them being derived from the Mongolian stock; Elliotson's Blumenbach, p. 412, with the note. We have a copious collection of facts on this subject, accompanied by many judicious observations, in Dr. Prichard's Researches, v. i. books 3, 4, and 5. His general conclusion, as expressed in the following paragraph, appears to me to be authorized by the premises: "The influence of the climate on the colour and organization of mankind is another inquiry, which the history of the great insular races might be expected to elucidate. With respect to the Polynesian tribes, it has been remarked by Mr. Marsden and Mr. Crawford, that the heat of climate seems to have no connexion with the darkness of complexion. The fairest nations are, in most instances, those situated nearest to the equator. If we inquire into the history of the Papua and Australatian tribes, with relation to this point, we shall find that the complexion does not become regularly lighter as we recede from the intertropical clime; for the people of Van Dieman's land, who are the most distant from the equator, are black; but we observe, that the occasional deviation to light hues

With respect to the other circumstances, in which the different tribes of the human species differ from each other, such as the form of the bones and of the soft parts; and even, in some cases, a difference in the structure of certain organs, we are still less able to conceive how they can have been produced by any of the external causes which may be supposed to operate upon the living body. The influence of temperature and of climate generally, of food, and of the occupations and habits of life, has been frequently made the subject of inquiry both by medical and by physiological writers, but without our being able to arrive at any very precise results. It would seem, however, to be pretty clearly established, that the same animal, when suffered to live at large in different countries, acquires different characters, and we can often perceive

chiefly displays itself in temperate regions, as in New Holland, among the tribes in the neighbourhood of Port Jackson." p. 489, 0. We have many interesting remarks on the colour of the human species in Mr. Lawrence's Lect. ch. ii. p. 271, et seq. See also Dr. Prichard's remarks on the complexion of the various tribes of what he terms the Indo-European nations, v. ii. p. 204, 5; also further remarks on the connexion between the darkness of the complexion and the heat of the climate in v. ii. p. 531, 2. On this and other analogous topics, we find much interesting matter in Forster's observations made in his voyage with Cook, ch. vi. p. 212, et seq. Camper, however, in an essay written expressly on this topic, "De l'Origine et de la Couleur des Negres," adopts the popular opinion, that the blackness of their skin depends upon the direct effects of climate, and that the brownness occasioned by exposure to the sun's rays, is transmitted to the offspring, and becomes increased by a number of successive generations; Œuvres, t. ii. p. 451, et seq.

that the character which it has acquired is peculiarly well adapted for its new situation. We are, however, for the most part, altogether unable to assign any probable cause for this alteration, and we refer it to the effects of climate and diet, merely because we know of no others which can be supposed to operate.[5] One of the most remarkable examples of the influence of external circumstances, upon both the physical and the intellectual powers, is the production of what is termed cretinism in certain parts of Switzerland. It consists in a state of mental imbecility, combined with, and probably depending upon, a mal-conformation of the bones of the head; it appears to be generated by something peculiar to the atmosphere of confined valleys, and does not seem to be hereditary.[6]

These various considerations afford a powerful argument in favour of an original difference in the varieties of the human race,[7] but there are others, equally or perhaps more powerful, which lead to the opposite conclusion. The analogy of the inferior animals strongly supports the doctrine of the com-

[5] Prichard's Research. b. 9. ch. i. sect. 7. "On the relation of particular varieties of the human species to climate," contains many valuable facts and judicious remarks.

[6] Saussure, Voyages dans les Alps, ch. 47. § 1050..6. et alibi; Reeve on Cretinism, in Phil. Trans. for 1808. p. 111, et seq.

[7] The difficulty of assigning the common origin to mankind is forcibly, although perhaps insidiously, urged by Kames, in the preliminary discourse to his Sketches of the History of Man; v. i. p. 3, et seq.

mon origin of mankind. The different kinds of dogs, for example, which exhibit so many forms, sizes, and colours, are supposed to have all proceeded from one source, yet they remain as permanently distinct from each other as the European and the negro, and are apparently as little affected by external circumstances.[8]

This formation of varieties, which afterwards become permanent, seems to depend upon some natural tendency in the animal constitution, which it is not easy to explain, but of which we often meet with curious illustrations. A remarkable instance was lately related by Sir A. Carlisle, with respect to an American family, where a female had two thumbs on each hand, and six toes on each foot. She married and had several children, who, in their turn, became parents, and at the present time a considerable number of her descendants possess the supernumerary thumbs and toes.[9] A similar series of facts has occurred in the family of the individual who obtained the name of the Porcupine Man, one of whose descendants, in the third generation, was

[8] It is scarcely necessary to remark, that the question respecting the origin of the dog itself, whether it be a primary species, or derived from the wolf or jackall, does not affect the point under discussion; no one imagines that all the existing varieties of the dog were produced by the union of a number of different primary species.

[9] Phil. Trans. for 1814. p. 94, et seq. It appears that this peculiarity has now gone to the fourth generation, and has been propagated both by the male and female parents.

lately exhibited in this metropolis, possessing exactly the peculiarities of his grandfather.[1]

We may, upon this principle, partly explain the mode in which the varieties of the human race were orginally produced; but we can scarcely suppose that it was the sole principle which was called into action; it is more probable that the effect produced is the result of the co-operation of many causes, than of any single accidental occurrence. And this is more analogous to the changes that take place among the inferior animals, where we see the remarkable effects of domestication in producing these changes. The dog, in its wild state, always exhibits nearly the same characters; it is covered with hair of the same colour, its ears and tail are of the same shape, its limbs of the same form, and it manifests the same

[1] The origin of this family peculiarity is satisfactorily ascertained in the account which is given by Machin, in the Phil. Trans. No. 424, p. 299. who describes the boy, Ed. Lambert, then fourteen years of age. In the Phil. Trans. for 1755. p. 21, et seq. we have a further account by Baker, of the same individual, then forty years of age, and the father of six children, all with the prominencies on the epidermis like himself. In 1802, Tilesius published a description of one of these children, then an adult, with engravings; and in the year 1821, an individual of the third generation was publicly exhibited in this metropolis, whose skin exactly resembled the original description in the Phil. Trans. and the plates of Tilesius. We have an account by Mr. Humphries, of the origin of a new variety of sheep, which lately occurred in America, and which offers a series of facts precisely analogous to the above; Phil. Trans. for 1813. p. 88, et seq. See Prichard's Resear. v. ii. p. 550. In Brewster's Journ. v. 8. p. 24, 5, we have an account of a native of Ava, entirely covered with hair, who had a daughter in the same state.

powers and instincts. Yet, into what numerous varieties do we behold it transformed, when it becomes the guard and companion of man. Its size and disposition varying from the formidable mastiff to the puny lap-dog; its hair all colours, sometimes short and smooth, at other times long and curled, the shape of its face, ears, and tail, exhibiting every shade of difference. These differences we account for from the joint operation of the two principles which have been described above; the production of what we term accidental varieties, similar to the supernumerary fingers and toes of the American family, or the protuberances of the porcupine man, and the more gradual and continuous action of domestication, by which an equally remarkable change is brought about, and is transmitted to the descendants of the animals so changed. In many of the inferior animals we can distinctly perceive the most unequivocal proofs of the operation of both these causes; and with respect to the first, at least, we have equally clear evidence that it applies to the human race. With respect to the second, we are not able to adduce any facts of so direct a nature as applicable to man, but still we have sufficient evidence, that the effect of refinement and a high state of civilization on the human race, is analogous to that of domestication on the inferior animals.[2] In those coun-

[2] Dr. Prichard enters into a minute examination of the state of the different varieties of the human species, and compares the general laws of the animal œconomy as they are manifested in each of them. His conclusion is, that " it does not appear,

tries where the difference of habits between the higher and the lower classes exists in the greatest degree, and where, from moral and political causes, they are kept the most distinct, an obvious difference may be observed in their form and organization, although they both of them belong to the same variety.[3]

If then we admit the common origin of mankind, we may inquire, whether any of the varieties, as they now exist on the earth, is similar to the first created pair, and if so, which of them it is which bears this resemblance. In the prosecution of this inquiry we may derive some faint light from historical records, and some perhaps still fainter from analogy,

from a review of the principal facts in physiology, as they have been traced among the different races of men, that these races are distinguished from each other by any of those broad outlines, which generally, perhaps uniformly, separate particular species of animals. The great laws of the animal œconomy are the same in their operation on all. There are deviations in some respects, but these deviations are not greater than the common degree of variety in constitution which occurs within the limits of the same family." Researches, v. i. p. 125. We have a very judicious recapitulation of the arguments upon which this opinion is founded in the second chapter of the ninth book, v. ii. p. 584, et seq.

[3] This is particularly observable among the inhabitants of Hindostan, where, in consequence of the division into castes, the same condition of life, and the same occupation are continued, without any change, through many successive generations. The superior orders, who are employed as artisans, are of a decidedly lighter complexion than the agriculturists; in many of the Pacific islands the same difference exists between the different classes as in Hindostan.

but we are left almost entirely to the uncertain guidance of conjecture. Now we may remark, that it is more probable, that the changes induced upon mankind, have been in consequence of a progress from a state of barbarism to one of refinement, than the reverse; and hence we are led to regard that variety to be the primary one, which, through all the vicissitudes of human affairs, has remained in the most degraded state, and which, in its structure, differs the most from that variety, which has uniformly enjoyed the greatest degree of civilization. Upon this principle we must regard the Æthiopian as the type of the original pair, from which have sprung the Mongolian, the Malay, the aboriginal American, and lastly, the Caucasian, which we are entitled to regard as the most perfect specimen of the human race.[4]

As it appears to have been among the Egyptians that the first great progress was made in the arts of

4 Dr. Prichard conceives, that what he terms the melanic variety, "may be looked upon as the natural and original complexion of the human race." Researches, v. i. p. 139. We are told by Dr. S. S. Smith, that the negro population of North America is gradually acquiring a lighter hue, and that the peculiarities of their features are likewise diminishing, p. 91, 2. et alibi; see also Prichard, v. ii. p. 365, 6. Hunter remarks, that in the inferior animals, the alteration which is produced in them by what may be termed civilization, by shelter from the inclemencies of the seasons, by nutritive food, cleanliness, and other circumstances of this description, consists in changing their colour from a darker to a lighter shade; Observ. on the Anim. Œcon. p. 244.

civilization, it has become an interesting subject of inquiry, to what race or variety this people ought to be referred. There are certain passages in the writings of the ancients, which seem to prove, that individuals possessed of the negro character existed in Egypt, and that a dark complexion was generally regarded as characteristic of its inhabitants. We may presume, however, that at an early period, earlier than that to which our historical records extend, the original character had been considerably modified, or perhaps entirely changed, so that the form and complexion of the Egyptians more nearly resembled that of some of the Asiatic tribes of the Caucasian variety than any belonging to the Æthiopian race.

The inquiry has been pursued with much learning and industry by M. Cuvier, principally by the examination of the skulls of mummies, and the result appears to warrant the conclusion that, as far as regards the form of the skull, the ancient Egyptians resembled the Caucasian variety, at the period of their highest advance in civilization.[5] Dr. Prichard has examined this question, with his accustomed ingenuity and accurate research, and concludes that the ancient Egyptians did not resemble the negroes as they exist in the western parts of Africa, where their peculiar traits are the most strongly marked; but that they partook, in certain respects, of the African countenance and complexion:[6] the opinion of Blumenbach

[5] Lawrence's Lect. p. 339..348.

[6] Researches, ch. v. sect. 9. p. 316, et seq.

may be considered as not essentially different from Dr. Prichard's.[7] We have not sufficient data to enable us to determine, whether the original negro race was gradually metamorphosed into the state which is indicated by the existing remains, or whether the change was effected by some political revolution.

It has been a favourite object with many naturalists to establish a regular gradation among the different classes of animals, so as to form the whole into one chain; the contiguous links of which are closely connected with each other, and carry us on from the least perfectly organized to that which is the most so, by almost insensible degrees. This has been applied to the human race; and it appears that, if we arrange the skulls of the different varieties according to the forms, the most perfectly characterized European will stand at one end of the scale, and the African at the other; while the intermediate space will be filled up with the Mongolian, the American, and the Malay. It also appears that, if we continue the scale to some of the inferior animals, the gradation proceeds with a certain degree of regularity, indeed so much so, that in some species of the simiæ, the skull resembles the Æthiopian, nearly as much as the Æthiopian resembles the European. The Grecian sculptors were

7 Phil. Trans. for 1794, p. 177, et seq. We have an account of a minute examination that was made by Dr. Granville of a female mummy: from accurate measurements of the different parts, especially of the skull and the pelvis, it appeared to correspond with the most perfect specimens of the Caucasian variety; Phil. Trans. for 1825, p. 279..1.

so sensible of this effect, that without any reference to, or probably knowledge of the fact, as far as regards the different varieties of the human race, in the heads which they formed to represent the gods, they exaggerated the characteristic trait of the Caucasian skull, and by bringing forwards still farther the upper part of the head, they gave to the countenance an expression of superior intellect, which is always associated with the peculiar configuration of the head.

Camper endeavoured to establish a method of ascertaining the exact proportions which the different parts of the head bear to each other in different individuals, from which we might derive an indication of the state of the intellectual faculties. It consisted in drawing a horizontal line through the meatus auditorius, and another line along the profile of the face, so as to touch the most projecting parts of the forehead and the upper jaw. These two lines, by their intersection, make what he terms the facial angle, the size of which is increased by the prominence of the forehead and the recession of the jaw.[8] We cannot hesitate to admit the correctness of Camper's observations, and we can scarcely refuse our assent to the conclusion that he deduces from them. M. Cuvier, however, who has given a correct summary of Camper's dissertation, conceives that the method

[8] Dissertation sur les differences des traits du visage, pars 1. ch. iii. p. 34, et seq. In the fifth chap. p. 51, et seq. we have the results of his measurement of different skulls, illustrated by a series of engravings; tab. 1, 2.

is imperfect, as affording a view of the form of the head in one direction only. He conceives that we obtain a more correct idea of the relation between the cranium and the face, by viewing the head vertically, and we find that, by this method, we obtain the same gradation of form from the European to the Ourang-outang, through the Mongolian and the Æthiopic varieties.[9]

Blumenbach has formed a most extensive and well authenticated collection of skulls, procured from different parts of the world, by which the characteristic forms of the different varieties are fully established. The most important points in which the heads of the Caucasian and the Æthiopian differ from each other are, that the former is more round and altogether more capacious, the forehead is broader and more prominent, and the upper part of the head generally is larger in proportion to the face.[1] There are likewise other parts of the body, in which the negro differs anatomically from the European, occurring both in the bones, the muscles, and the soft textures. They are of a kind which cannot be fairly attributed to any thing in the habits or modes of

9 Leçons d'Anat. Comp. No. 8. art. 1. t. ii. p. 1 . . 12. He estimates the facial angle of the European at 80°, and remarks, that the angle of the negro, on the one hand, is 70°, while that of the antique statues, on the other, is 90°, p. 7. We have some judicious remarks upon these measurements in the seventh and eighth of Mr. Bell's Essays on the Anatomy of Expression; also in the fifth sect. of Dr. Prichard's Researches.

1 Blumenbach, Decades collectionis suæ craniorum, cum tabuli also De Gen. Hum. var. nat. p. xxii. , . xxxiv. tab. 1, 2.

life of the individual, but appear, like the colour of the skin and the texture of the hair, to be transmitted from one generation to another, independent of external circumstances. These differences, although considerable and permanent, appear to be exactly analogous to those which occur among the inferior animals of the same species; and therefore, although we are unable to account for their production, they do not oppose the conclusion, that the whole of the human race are derived from one pair.[2]

So far we proceed upon the basis of fact; but we enter upon more doubtful ground, when we inquire whether there be any innate difference of intellect or general character connected with these variations of the external form. The data by which this question is to be decided lie equally open to the judgment of every one; yet our opinions have been so biassed by considerations of a collateral nature, connected with our moral and political speculations, as to have led to the most opposite conclusions. But such considerations, however important in themselves, should not interfere with the pursuit of truth in our scientific researches. In the present instance, I conceive

[2] We have a judicious summary of the facts, as far as regards the form of the bones, in Dr. Gibson's dissertation, "De Forma Ossium Gentilitia;" and on the general differences of structure and organization in White, "On the Gradation of Animals," a treatise which contains many curious observations, although I conceive that the author has failed in establishing his fundamental position. Spix's interesting and beautiful work, entitled, "Cephalogenesis," may be referred to in this place, as indirectly connected with this subject.

that both the evidence of historic testimony and the deductions from anatomy and physiology will lead to the same conclusion, that the Æthiopian is naturally inferior to the European in his moral and intellectual powers. It may be conjectured, that while the other varieties of the human race have had, from various causes, their organization improved and their faculties elevated, the African has remained stationary, and nearly resembles, at the present day, the state of man at his first creation.[3]

[3] The remark of Hume on this subject appears to me to be the just deduction from the accumulated and unvarying experience of ages; Essays, v. i. p. 21, note M. p. 512. We have, indeed, some rare instances brought forward of negroes who have made a certain proficiency in the liberal arts and sciences. But the actual advance in these cases is inconsiderable, and the admiration with which it is received is a strong confirmation of the truth of the doctrine which is maintained in the text. Those authors who endeavour, upon such a foundation, to establish the equality of the intellect of the negro, might, upon the same principle, argue that the ass is as large as the horse, because an instance may be adduced in which an unusually large ass has exceeded the size of a small horse. We have some judicious remarks by Mr. Lawrence, upon the permanent intellectual superiority of the Caucasian variety, in the eighth chapter of his "Lectures." A very full, and, we may conclude, a very correct account of the anatomical differences between the body of the European and the negro is contained in Sœmmering's treatise, written expressly on this subject; we have a copious abstract of it appended to White's work on the Gradation of Animals. There are two observations made by Sœmmering, which bear immediately upon the present question; that in the negro the size of the skull bears a smaller proportion to the face and organs of sense than it does in the European and that the brain is smaller

## § 2. *Of Temperaments.*

A temperament may be defined a peculiar state of the system, depending on the relation between its different capacities and functions, by which it acquires a tendency to certain actions. I have frequently had occasion to remark, how accurately the different powers are all adjusted to each other, so as to produce one harmonious whole. If the disproportion be too great, disease ensues; but there are many gradations, compatible with health, where yet this disproportion is very observable. A human body, in its most perfect state, should have a certain degree of contractility of the muscular fibre, and a certain degree of sensibility of the nerves. The digestive organs ought to prepare a certain quantity of chyle, and from this a due supply of blood ought to be elaborated; the lungs should be sufficiently capacious to act upon the blood, and all the other functions should proceed in their proper course, so as to form the due balance of the whole of the system.

The ancients paid considerable attention to the subject of temperaments, and pointed out various

in proportion to the aggregate of the nerves which proceed from it to the organs of sense. In both these respects the negro recedes from the characteristic features of the human race. An observation of an analogous kind was made many years ago by Daubenton, that the position of the head on the spinal column differs in man from that in all other animals, and that the peculiarity exists in a less degree in the negro; Mem. Acad. pour 1764, p. 568, et seq.

peculiarities in the constitution and actions of the living body, which have been seen so far to coincide with general observation, that their nomenclature has continued in pretty general use, even to the present day, although the hypothesis on which it was founded is universally discarded. They described four temperaments, corresponding to the four qualities of Hippocrates—hot, cold, moist, and dry: these were supposed to give the specific characters to the four ingredients of which the blood was thought to be composed—the red part, the phlegm, the yellow, and the black bile respectively; and hence were derived the names of the sanguine, the phlegmatic, the choleric, and the melancholic temperaments, as indicating an excess of each of these substances.[4] After the revival of letters this division was adopted in its essential parts by the most eminent physiologists: Stahl ingeniously adapted it to the modern doctrines of the humoral pathology;[5] and even Boerhaave, although he increased the number of temperaments to eight, and relinquished the erroneous opinions of Hippocrates and Galen respecting the constitution of the blood, yet he still derived the characters of his temperaments from the principles of the humoral

4 The doctrine of Hippocrates on this subject will be found in his treatise "De Natura Hominis;" Op. a Fœsio, t. i. p. 224, et seq. and that of Galen in his two books, "De Elementis" and "De Temperamentis."

5 Theor. Med. Vera; sect. de Temp. p. 232. He very elegantly describes the state both of the corporeal and the mental powers, as connected with these supposed conditions of the fluids.

pathology, and supposed them to be formed merely by different combinations of the four cardinal qualities.[6]

Haller appears to have been the first who decidedly opposed the ancient doctrine, not only by showing that there was no foundation for the varieties of the temperaments in the peculiar nature of the fluids, but by substituting in their place the vital actions of the system. But his ideas, although to a certain extent correct, are to be regarded rather as an indication of the plan to be pursued, than as comprehending a complete view of the subject.[7] Darwin proceeded upon the principle of Haller, in endeavouring to establish the temperaments upon the vital actions of the system; and in conformity to the hypothesis which he adopted, of reducing these actions to the four heads of irritation, sensation, volition, and association, he formed four temperaments, in which these qualities were conceived respectively to prevail.[8]

Perhaps, upon the whole, we may find it convenient to revert to the arrangement of the ancients,

6 The eight temperaments of Boerhaave are respectively denominated warm, cold, moist, dry, bilious, sanguine, phlegmatic, and atrabilious; Instit. Med. § 889..896.

7 El. Phys. v. 4. 1..6.

8 Zoonomia, v. i. sect. 31. p. 354..0. He defines a temperament, "a permanent disposition to certain classes of diseases;" but this, I conceive, is restricting it within too narrow limits; it ought to be extended to the ordinary, as well as to the morbid state of the system.

which appears to have a real foundation in nature,[9] although on this, as on other occasions, the father of medicine blended false theory with correct observation. If to the four temperaments of Hippocrates we add, after the example of Gregory,[1] a fifth, the nervous temperament, and bestow new appellations upon the other four, we shall have the leading varieties of the constitution under the denominations of the nervous, the sanguine, the tonic, the relaxed, and the muscular temperaments.[2]

The different states of the system may be conceived to depend partly upon a difference in the original conformation of the body, and partly upon a difference in its powers and functions. The nervous temperament obviously owes its peculiarities principally to the sensibility of the nerves existing in an

[9] Cullen admits the four temperaments of Hippocrates, and remarks concerning them, that it is probable they were first founded upon observation, and afterwards adapted to the theory of the ancients, since we find " they have a real existence. " Lect. on Mat. Med. p. 18. Dr. Prichard remarks that, " This division of temperaments is by no means a fanciful distinction." Researches, p. 169. He restricts the number to four, and designates them by the original names.

[1] Conspectus, v. i. p. 517. . 3.

[2] We have a long article on this subject in the Dict. Scien. Med. by Hallé and Thillaye, t. lvi. p. 458, et seq.; it is, like most other parts of that work, very diffuse, and the list of references very miscellaneous. We have many good remarks on temperaments in Cabanis' Rapports; see particularly t. i. p. 54 . . 64, and 6e Mem. t. i. p. 404, et seq. on the influence of the temperaments upon the formation of the ideas and the moral affections.

undue proportion to the contractility of the muscles. The sanguine temperament would appear to depend chiefly upon the organization of the body, and the nature of its composition; the vessels are capacious and the solids distensible, the proportion of the fluids is large, and all the actions, which depend especially upon chemical changes, seem to proceed with an unusual degree of facility. We have, therefore, much activity, but the strength is soon exhausted, while the functions are all disposed to excessive action, and are liable to be deranged from slight causes. The tonic temperament is perhaps the one which must be regarded as the most perfect state of the human frame, that in which the different powers are the most nicely balanced, and where we have the greatest capacity for action, combined with the greatest strength of resistance. The body is spare, but hardy; capable of long-continued exertion, rather than any great degree of physical strength, while the mind is firm and ardent, and exhibits that happy combination of genius and industry, which gives rise to the best directed efforts of human intellect. In the relaxed temperament we have the capacious and distensible fabric of the sanguine constitution, but with a deficiency of the vital powers, and an imperfect development of the functions. The nervous and muscular systems are feeble in their operations, and the various processes, both chemical and mechanical, are of course imperfectly performed. The muscular temperament is that of mere physical strength; there is a great share of contractile power, with a defect of nervous energy;

the body is capable of great exertion, but the functions are with difficulty excited into action, while the perceptions are blunt, and deficient both in strength and accuracy. The state of the mind corresponds to that of the body; the feelings are not easily roused, but when the mind is once excited, it obstinately retains the impression, and perseveres in its object with unshaken resolution.[3]

It is admitted that few individuals possess these characteristics in an extreme degree; and even where they have been the most strongly marked by nature, education, climate, habits, and many other causes, may modify them in various ways. They are also capable of being combined together, by which intermediate shades are produced; so that it is often difficult to determine under which temperament many individuals, as we see them in society, ought to be classed. We are, however, warranted in asserting, that different temperaments actually exist, that these differences are innate, and that they attach both to the corporeal and to the mental part of our frame.

[3] In Hunter's translation of Lavater, v. i. p. 254, and v. ii. p. 93, we have descriptions of the temperaments, illustrated by very characteristic engravings.

## CHAPTER XXI.

### OF SLEEP AND DREAMING.

THE physiology of the human frame, as I have hitherto described it, supposes that all the functions proceed in a regular course, without interruption or deviation. But this we are aware is not the case. Even under the most favourable circumstances, the body is of very limited duration; many years elapse before its powers acquire their perfect state, and after a short period they show symptoms of decline. And besides this, which may be regarded as the regular process, we are subject to innumerable irregularities, which give rise to diseases of various kinds, that either accelerate the decay of the system or destroy some of its functions.

The doctrine of diseased action forms the science of pathology, and does not come within my province; but the functions are subject to one kind of partial interruption, which, as it does not constitute disease, will fall under our consideration. I refer to the phenomena of sleep.

In the language of poetry, sleep and death have been compared to each other; and could we conceive of a human being, created in the full possession of all his powers, we might imagine, that when he was first seized with an irresistible inclination to sleep, it would appear like the commencement of dissolution

The resemblance is, however, chiefly apparent. Sleep is a state in which all the vital functions retain their full activity,[1] and which is absolutely necessary to the support of our existence. In considering the nature of the phenomena of sleep, two subjects of inquiry especially present themselves; first, in what does the state of sleep differ from that of waking, or in what does sleep essentially consist? And, secondly, what physical change takes place in the brain or nervous system, which can be supposed to be the efficient cause of sleep? In connexion with these topics, I shall make some remarks upon the nature and cause of dreams.

## § 1. *State of the System during Sleep.*

In order to ascertain what is the condition of the system during sleep, it will be proper to examine what takes place in the approach to this state.[2] The first indications are a general languor and an incapacity for exertion of any kind, either mental or

[1] It may, indeed, be questioned whether this be literally the case: in very profound sleep it is probable that all the vital motions are diminished to a certain degree, although none of them are affected so far as to interfere with the due exercise of the functions to which they are subservient; see Haller, El. Phys. xvii. 3. 3. and Blumenbach, Inst. Phys. by Elliotson, sect. 20. § 320. p. 177. This latter physiologist has, however, gone much too far, when he defines sleep to be a "function, by which the intercourse between the mind and body is suspended." § 318.

[2] See Haller, ubi supra.

corporeal. The impressions made by external objects are scarcely perceived, and our voluntary actions are performed with difficulty. After some time the eyelids close, the muscles of voluntary motion are relaxed, and we become insensible to what is passing around us. In the mean time the vital functions continue their operations nearly in their usual manner; the heart beats, the muscles of respiration act, and the glands continue to produce their respective secretions. Nor does the mind become inactive, although it no longer preserves its connexion with external objects. The ideas often flow with perhaps greater rapidity than in our waking hours, while imagination, memory, association, and many of the passions seem to exist with peculiar vivacity. These observations upon the state of the system at the approach of sleep, lead us to conclude, that it consists especially in two circumstances; in the suspension of certain of the functions, which act through the medium of the nervous system, and of the power of receiving impressions by the external senses.

Of the sensitive functions which are suspended during sleep the most important is that of volition. We find that all the muscles of voluntary motion lose their power, and it is upon this circumstance that the complete relaxation depends. Darwin [3] and Stewart,[4] who have offered many ingenious observations upon sleep, regard the suspension of the

[3] Zoonomia, v. i. sect. 18. The remarks of Brown on this part of Darwin's works are sensible and correct; sect. 11.

[4] Elements, v. i. c. v. pt. 1. sect. 5. p. 327, et seq.

power of the will[5] and the absence of impressions on the external senses, as the essential characteristics of sleep. But at the same time that the exercise of the external senses and of voluntary motion is either altogether or nearly suspended, the body appears to retain its susceptibility to the usual internal stimuli; and thus not only the vital functions, as was before observed, proceed nearly in their accustomed manner,[6] but we are sensible to the feelings of pain and uneasiness of various kinds.

## § 2. *Nature and Cause of Dreams.*

THIS idea of the state of the system during sleep will assist us in explaining the curious phenomena of dreams. Dreams consist of a succession of ideas, that pass through the mind, with various degrees of rapidity and vividness, without any regard to congruity or consistence. They may generally be traced to some actual occurrence; but these are so

[5] Prof. Stewart, indeed, supposes that the will is not actually suspended during sleep, but that it loses its influence over those faculties which are subject to it during our waking state; Elem. p. 330, 1. But I confess that I do not perceive any essential difference between these two cases, while the considerations which he offers in order to prove his opinion, only show that the suspension is not complete.

[6] This remark, as will afterwards appear, is to be received with certain limitations; in profound sleep, the action of some of the physical functions is certainly diminished; its effects on animal temperature is noticed by Dr. Edwards, "De l'Influence," &c. p. 473.

perverted and mixed up with imaginary transactions, as to produce the most strange and singular combinations. Dreams differ from our waking thoughts principally in the following circumstances. They are often more vivid, so that we mistake our ideas for perceptions, and conceive that what is only passing through our minds is the representation of what actually exists. In dreams we have little or no conception of time and place, and sometimes we crowd a long series of events into a few moments, and fancy ourselves conveyed to any distance with the most perfect facility. We perpetually fall into the grossest inconsistencies; we suppose persons to be both living and dead at the same time, imagine ourselves to be in two places at once, and we even confound our notions of personal identity. Our passions and feelings, which are occasionally strongly excited during sleep, bear no proportion to the cause which produces them ; we are overwhelmed with joy or with grief, without knowing why, and we are even sometimes aware of the unreasonableness of our transports, without being able to check them. It is observed, that surprize is seldom experienced in dreams, and that we pass through all the adventures of an oriental romance, without being conscious of their singularity.

These circumstances, which are among the chief characteristics of dreams, are all explicable upon the principles stated above, that in sleep, the senses are not capable of receiving external impressions; that, although many of the mental powers retain their activity, the exercise of volition is suspended.[7] While we remain in a state of rest, our waking thoughts are directed, in a great measure, by association, but at every moment our senses convey to us the impression of external objects, and, by the agency of the will, we are perpetually directing the train of our ideas into some channel different from that into which it would flow of its own accord. However vivid any of our ideas may be, still we never mistake them for perceptions, and thus we immediately become sensible of the difference between them. Our notions of time and space are affected by the events that are passing around us, and indeed essentially depend upon the comparison which we establish between these events and our internal feelings. From these causes, we may explain why our dreams are formed of such a farrago of inconsistent ideas, and why we so seldom experience any surprize at the unnatural combinations that are formed in them. And in the same manner, from the suspension of volition, and consequently from not comparing our ideas with each other, but suffering them to proceed with their natural impetuosity, we may deduce the reason why our passions and exertions are so often disproportioned to the causes exciting them.

[7] Dr. Carmichael endeavours to explain the phenomena of dreams by assuming the existence of separate organs for the different faculties, and by supposing that some of these are liable to be in the state of sleep while others are not so, the different organs of sense being likewise incident to the same irregularity; Trans. of the College of Phys. in Ireland, v. ii. p. 48, et seq.

Dreams then appear to consist of a long train of associated ideas, seldom interrupted, as our waking thoughts are, by the intervention of external impressions, or by the voluntary efforts which we make to alter the course of our ideas, by comparing them together, dismissing some and introducing others at pleasure. The commencement of the associated train seems often to depend upon some feeling excited in a part of the body, or upon an impression made upon an organ of sense; but it is frequently difficult to account for the direction which our ideas afterwards assume. Although we sometimes dream of those events, which have most fully occupied our minds during our waking hours, this is not always the case; on the contrary, our dreams often turn upon the most trifling occurrences, or upon circumstances which had been totally forgotten. This may be explained upon the principles that have been stated above. The mind may have been steadily chained down to one set of ideas for many hours, yet just as we are falling asleep, we may experience a sensation in some part of the body, which calls up a new train of ideas, that retains possession of the mind, and completely excludes the former. Prof. Stewart relates a case which very aptly illustrates the manner in which an impression made upon the body during sleep calls up a train of associated ideas, and thus produces a dream. A gentleman who, during his travels, had ascended a volcano, having occasion, in consequence of indisposition, to apply a bottle of hot water to his feet when he went to bed, dreamed that he was making a journey to the top of Mount

Ætna, and that he found the heat of the ground almost insupportable.[8]

When the impression made upon the body becomes very powerful, we are generally awaked by it; but it sometimes happens that a great degree of pain or uneasiness is excited, and yet that sleep continues. In this case, we fall into one of those painful dreams to which the name of incubus or night-mare has been given. We feel the uneasiness acutely, and our dream is composed of some association that has been formed with this uneasiness, or one of a similar kind; we are even aware of our situation, and know, probably from former experience, that, could we speak or move, our painful dream would be interrupted. It must be confessed, that there is something obscure, both in the cause producing incubus, and in the relative condition of the physical and mental powers. It would seem as if the mind and body were in different, or even in contrary states, the body in the most profound repose, and the mind peculiarly active, and this disproportion existing in such a degree as to constitute something approaching to disease.

There is a peculiar kind of dreaming, which sometimes occurs, called somnambulism or sleep-walking, where the body is still more incapable of receiving impressions than in ordinary sleep, and yet the will has a certain degree of power over the organs of speech and voluntary motion. The individual walks about his apartment, utters sentences, and performs some of his usual occupations, yet he remains so soundly

8 Elem. v. i. p. 335.

asleep, that it is impossible to awake him without employing a considerable degree of violence. The state of the organs of sense in somnambulism is singular, and almost incomprehensible. At the same time that it is difficult to produce any effect upon them by the usual stimuli, they appear to be sensible to certain actions, but these are exercised in a very limited manner only, and at the pleasure of the individual. Thus there are well attested accounts of somnambulists, who have procured the implements for writing, and have actually transcribed a copy of verses, in such a manner as to prove that they must have used their sight; yet they were, at the same time, incapable of perceiving the brightest light when held close to their eyes. A circumstance in which the state of mind in somnambulism differs very much from that in common dreams is, that the train of ideas is always intently fixed upon one object. It is also remarked, that, notwithstanding the vividness of the imagination, and the firm possession which it acquires of the mind, when the person awakes spontaneously, or is forcibly roused from his sleep, he has frequently no recollection of what has happened to him. The state of somnambulism, in some respects, resembles what has been called a trance, a condition of the body, which, if it ever exists, has probably been much exaggerated by the credulity or superstition of the narrators.[9]

9 Darwin's Zoon. v. i. sect. 19. § 2. Prichard on the Nervous System, ch. 12. p. 399, et seq.

## § 3. *Cause of Sleep.*

We must now proceed to the next branch of the inquiry, what is the proximate cause of sleep, or what is the state of the brain and nerves which immediately precedes it? The hypotheses on this subject, although numerous, are not very satisfactory, and Professor Stewart expressly declares, that the investigation is beyond the reach of the human faculties.[1] We may readily admit, that it still remains involved in much obscurity, but it appears to be a legitimate object of inquiry, and one which is not more beyond our grasp than the other functions of the nervous system. The common opinion among the earlier physiologists was, that sleep depends upon the exhaustion of the animal spirits or nervous fluid,[2] but it is sufficient to remark, that the existence of the nervous fluid itself is quite a gratuitous

[1] Elements, v. i. p. 327.

[2] This opinion is elegantly detailed by Willis, in his treatise "De Anima Brutorum;" Opera, t. ii. p. 128, et seq. Boerhaave supposed that the proximate cause of sleep consisted in a deficiency of animal spirits being carried to the brain, but that this deficiency might arise either from the exhaustion of the spirits, or from the pressure of the blood upon the brain not permitting the spirits to be conveyed to it; Prælect. § 593 . . 5. t. iv. p. 254, 5. The section "De Somno," although in many parts not correct, according to our present opinions, affords a good specimen of Boerhaave's perspicuous method of analyzing and abstracting the more obscure parts of the animal œconomy; § 590 . . 600.

supposition. Haller,[3] Hartley,[4] and many of the most eminent physiologists of a later date, have conceived that sleep depends upon an accumulation of blood or other fluids in the vessels of the head, pressing upon the brain, and thus impeding its functions. This opinion derives some plausibility from the effects of pressure arising from various morbid causes, which brings on a lethargic state, that finally ends in an abolition of the faculties. The well known case of the Parisian beggar has been often cited in support of this hypothesis. He had a perforation in the skull, by which a portion of the brain was left exposed. When this part was pressed upon, it produced a state of drowsiness, and this might be increased by increasing the pressure, until at length he became completely apoplectic.

In opposition to this hypothesis, it may be observed, that the state produced by pressure upon the brain, although it resembles sleep in the partial abolition of the faculties, yet it differs from it in some essential particulars, and it would be difficult to comprehend how some of the circumstances, which are known to promote sleep, can act in producing an accumulation of blood in the vessels of the brain. Besides, it seems to confound the natural with the morbid state of the system, for all those cases in which sopor is produced,

[3] Although this may be regarded as the fair inference from Haller's observations, I must remark, that he states his opinion with certain limitations, and with his usual circumspection; El. Phys. xvii. 3. 9.

[4] On Man, Prop. 7. p. 45, . 8.

by pressure upon the brain, are regarded as indications of some of the most dangerous diseases, whereas the state of sleep is a regular process of the animal œconomy, which cannot be supposed, either the cause or the effect of any morbid action.[5]

The following remarks, which are principally deduced from the speculations of Cullen,[6] may tend to throw some light upon the subject. We are led to regard the different functions of the animal œconomy as producing their ultimate effect by a kind of

[5] Blumenbach conceives the proximate cause of sleep to consist in a diminished or impeded flow of arterial blood to the brain, a conclusion which he deduces, partly from a consideration of the remote causes of sleep, and partly from the effects which are known to be produced upon the functions of the brain by the abstraction of this fluid; Instit. Physiol. by Elliotson, § 322. But, upon this hypothesis, it may be remarked, in the first place, that it would be very difficult to show how some of the remote causes of sleep can produce the effect which is contemplated; and, secondly, that sleep, which is a natural and salutary process, has any real resemblance or analogy with the morbid state which is produced by a deficiency of arterial blood.

[6] Physiol. § 124 . . 133. In the following observations, I have not employed the phraseology of Cullen, which is encumbered with the speculation of the nervous fluid; but the existence of this fluid is not essential to the hypothesis. Bichat's theory of sleep is essentially the same with Cullen's; he lays it down as a general law of the animal functions, that they have all alternations of action and repose; this intermission, if long continued, and especially if extended to any number of these functions, constitutes sleep; for the most part, the action of the animal functions is partially suspended, and it takes place in some of them only, and in proportion to the degree so is the sleep more or less profound; Sur la Vie, &c. art. 4. § 3. p. 27 . . 9.

mutual action and re-action, one serving, as it were, to counterbalance another, so as to form an harmonious result from the combined operation of the whole. In this way the sources of expenditure are adjusted to those of supply, and we shall always find that there is some method of providing for the regulation of any excess or defect that may take place. Many facts lead us to conclude it to be a general law of the nervous system, that it is incapable of acting, for any length of time, without being exhausted, and requiring an alternation of repose. This applies equally to the organs of sense, to the muscles that are under the control of the will, and to the intellectual powers. Now, during our waking hours, a variety of actions are going on, which tend to produce this exhaustion, and sleep is the period when the nervous functions are recruited.[7]

7 That sleep is an affection of the functions that depend upon the nervous system, is proved by the fact, that those animals require the most sleep whose nervous system, and especially the central part of it, exists in the greatest perfection. We have some judicious observations on the causes and effects of sleep in Reeve's Essay on Torpidity, p. 136..146. Magendie's remarks on sleep are contained in his Elem. Phys. t. ii. p. 460..5; his opinion respecting the relation of the nervous system are somewhat different from those stated above. The article "Sleep," in Rees's Cyclopædia, may be perused with advantage. Dr. Carmichael, in the essay to which I referred above, after remarking that sleep depends upon something more than mere rest after fatigue, conceives that it is essentially connected with the process of assimilation, and particularly with the deposition of new matter in the brain; Trans. of Irish College, v. ii. p. 48, et seq. His first position is correct, but I must acknowledge that I see no

If we examine into the nature of the causes which tend to produce sleep, it may serve to illustrate this doctrine of the alternate process of exhaustion and reparation. These causes may be arranged under two heads; those which diminish the nervous sensibility, and those which prevent the sensibility from being excited into action. The most important of the first class of causes is fatigue, both of body and of mind, by which the nervous power is suspended, and the brain rendered insensible to the accustomed stimuli. The action of opium and of other narcotics may be referred to this head; inasmuch as, by diminishing sensibility, they render the brain incapable of receiving impressions. In the same manner, we may explain the effect of those causes which prevent the blood from experiencing the proper change in its passage through the lungs; such as the inhalation of carbonic acid. When death is produced by suffocation from fixed air, a profound sleep first comes on; and, if the process be not too rapid, the functions are gradually abolished, without pain or uneasiness of any kind.

The second set of causes that produce sleep, are those which act by preventing the sensibility from being excited. Every function requires for its continuance a certain force of impression or stimulating power, without which all action would cease. Could

evidence for the truth of the speculation on which he builds his hypothesis of the proximate cause of sleep, and still less his mode of accounting for the partial state of repose of the mental powers.

we withdraw from the system every thing which stimulates the muscles and the nerves, we should no longer have either motion or sensation. If, therefore, all external impressions are carefully removed, and the mind is prevented from dwelling upon its own ideas, sleep generally ensues. But it is obvious that the effect will take place with more ease and certainty, when the nervous energy has been previously diminished by the first set of causes; as it will, in this case, require a greater degree of stimulating power to produce the same effect. Hence the most favourable combination of circumstances for producing sleep are previous fatigue, not carried to excess, freedom from pain, absence of light and noise, a regulated temperature, a posture in which all the muscles are relaxed, and a tranquil state of the mind. Perhaps the state of the mind is the most important circumstance; for it is repeatedly found, that an interesting train of thought will completely banish sleep, when every thing else seems favourable for its approach. It is observed, that persons in a rude and savage state of society, those that are engaged in mere manual occupations, and young children, fall asleep immediately when the body is at rest. It is upon this principle that we account for the effect of any monotonous noise in producing sleep; as the humming of bees, the murmur of a fountain, or the reading of an uninteresting discourse. "If we examine this class of sounds," Prof. Stewart observes, "we shall find that it consists wholly of such as are fitted to withdraw the attention of the mind from its own thoughts, and are, at the same time, not sufficiently interesting to engage its attention to themselves."[8]

There is a state of the system which has been thought to be allied to sleep, but which is related to it rather in appearance than in reality; this is reverie. In the most profound reverie, as in sleep, external objects make but little impression upon the senses, or, at least, if the impression be made, the perception is not excited in the brain. But reverie differs from sleep in one essential particular, that in the former the faculty of volition is in its full exercise, and indeed it is upon the activity of this principle that the abstraction from surrounding objects essentially depends. The power of directing the thoughts at pleasure, of dwelling upon certain ideas, of excluding others, of preventing the intrusion of external impressions, and of turning the attention immediately from one object to another, constitutes the most perfect state of the human intellect, and one which enables the mind to reach the highest departments of science and philosophy. But, according to the usual order of nature, advantages are balanced by corresponding inconveniences, and what is most anxiously sought after, and appears the most worthy object of desire, is often attended with some necessary evil. The state of the mental faculties,

[8] Elements, p. 329. I may remark, that the whole of sect. 5. p. 327, et seq. deserves an attentive perusal. Haller gives us some useful observations on the phenomena and remote causes of sleep, although combined with incorrect hypothesis; El. Phys. xvii. 3. 4. . 8. See also Blumenbach's Instit. Physiol. by Elliotson, § 321.

which has been described, is peculiarly liable to induce a habit of abstraction from the impression of external objects, which constitutes reverie, and which, if it be permitted to go beyond certain limits, disorders the functions both of body and mind, the body becomes languid and inactive, and the mind falls into derangement.[9]

[9] See Parry's Pathol. § 650. We have many just and philosophical observations on reverie in Darwin; Zoon. v. i. sect. 19; yet, in one point, he appears to me to be incorrect. He ascribes the state of reverie, either to the sensations of pleasure, or to the efforts of volition being so powerful, as to render us insensible to the ordinary impressions of external objects. But, I conceive, it is the latter case only which constitutes reverie; the former is merely to be resolved into the general principle, that a more powerful impression always renders us, to a certain extent, incapable of attending to a weaker. Perhaps, indeed, what I regard as an inaccuracy in Darwin, may depend rather upon his phraseology than upon his actual conception of the subject; as when he speaks of sensations of pleasure, it may be that he means to express the ideas of these sensations, which so exclusively occupy the mind, and are forcibly retained there. That this is the case may be conjectured from his "Definition or Character of complete Reverie;" § 10. I must remark, that in the nomenclature of Darwin, the terms irritation and sensation are nearly equivalent to what I have denominated respectively nervous action and perception. We have occasionally very singular examples of long protracted sleep, where no other obvious derangement exists in the animal œconomy; a remarkable example of the kind is narrated in the Quart. Journ. v. i. p. 121.

## CHAPTER XXII.

### OF THE DECLINE AND DISSOLUTION OF THE SYSTEM.

I HAVE endeavoured to show, in the last chapter, that the final cause of sleep is to afford rest to the nervous system, by which its functions may be recruited, after the expenditure of power, which necessarily occurs during our waking hours. It has, in like manner, been shown, in former parts of this work, that all the components of which the body is composed, as well those which serve for the exercise of the contractile, as of the sensitive functions, receive a regular supply of matter, for the purpose of repairing the losses that are continually going forwards, from the different actions that have been described. Provided the due proportion of rest be obtained, and an adequate supply of matter be afforded, the process of reparation nearly keeps pace with that of expenditure, but still, under the most favourable circumstances, a gradual tendency to ultimate decay is engrafted in our system; and although we may escape the shocks of disease, and the various accidents to which we are at all times exposed, still age makes its gradual advances, and brings with it the inevitable destruction of our corporeal frame. This process may be observed in all our organs, and will be found to affect every function. I shall briefly trace its operations as they manifest themselves in the different parts of the animal œconomy, beginning with the membranes, the bones, the muscles, and the nervous matter; then proceeding to the functions which depend upon contractility; and, lastly, those which are more connected with the exercise of the sensitive and intellectual faculties.[1]

## § 1. *Changes in the Structure and Functions of the Body.*

The natural progress from youth to age is strongly marked, both in the texture and the composition of membrane. In infancy it is soft and relaxed, and contains a large proportion of jelly and water; but as age advances the jelly gradually disappears, or is much diminished, and it loses a considerable portion of its water. For some time its elasticity seems to be increased, with the increase of solid matter, but it gradually acquires a greater degree of firmness,

[1] These progressive changes are elegantly detailed by Boerhaave, Prælect. § 434 .. 480, t. iii. p. 291 .. 374, and Aphor. No. 55. and 128, with Van Sweiten's copious commentary; by Haller, in the thirtieth book of the El. Phys.; and by Blumenbach, Inst. Physiol. sect. 44. A number of important observations on this subject are contained in Bichat's treatise "On Life and Death," a treatise which, I think, displays more marks of original genius than his longer and more elaborate performances. It is in the first part of this work, that his remarks on the natural progress of the system to dissolution will be found; see particularly art. 10. p. 108, et seq.; the second part refers to the cause of death when produced by violence, or by any morbid action.

which not only renders it less flexible and less extensible, but finally less elastic.[2] This change is to be observed in all those parts, which principally consist of membrane, as the ligaments, the cartilages, and the tendons, and more especially, in the vessels of all kinds. As affecting the blood vessels, it was made the subject of an interesting train of experiments by Winteringham. He not only established the fact, that this change takes place in the vascular system generally, but he found that it took place at a different rate in the arteries and in the veins, its progress being more rapid in the former than in the latter,[3] and observations of a similar kind were made by Haller.[4]

The muscles undergo a change in their state as age advances, partly in consequence of the change in the state of the membrane, which enters so largely into their composition, and partly in consequence of the alteration of the fibres themselves. They become generally less contractile, while those that serve for the voluntary motions are less under the control of

[2] Haller, El. Phys. xxx. 1. 12.

[3] Experimental enquiry; see especially ex. 6, 7, 8, 9, 10, 11. and remarks, p. 31, 35 . . 7. Winteringham's work may be regarded as a very valuable series of statical experiments, performed, as it appears, with great minuteness, and leading to many important results. The direct conclusions that he draws from them are, in most cases, the legitimate deduction from the facts, but the indirect conclusions are frequently incorrect, in consequence of his viewing the actions of the animal œconomy too much as mere mechanical operations.

[4] Ubi supra; also xxx. 2. 1 . . 4, and xxx. 3. 1.

the will, or are less able to execute its commands. In some instances, parts that are originally muscular become tendinous, the muscular fibres being gradually absorbed, and tendinous matter deposited in their room. In some cases, the muscular, or other soft parts, become rigid, from a quantity of bony matter being deposited in them.

A very obvious alteration is induced upon the bones; they contain a considerably greater proportion of phosphate of lime, and are thus rendered harder and more brittle; some parts, which are tendinous or cartilaginous in infancy, become gradually converted into bone as age advances, and from this cause the body becomes less moveable in its different parts, the motion of the joints is diminished, and the contraction of the muscles is impeded.[5]

The circulation is affected by the change which takes place in the relation between the arteries and the veins, the arteries being rendered less distensible and less contractile; a larger proportion of blood is therefore deposited in the veins, which consequently become overloaded and distended. Another, and perhaps a still more important change in the state of the sanguiferous system depends upon the smaller proportion of fluids generally, and of blood in particular, which exists in the body as age advances. Every one of the textures, membrane, bone, muscular and nervous matter, and consequently, all the various

[5] The mechanical form of the bones in the fœtal state was made the subject of a separate work by Albinus; Icon. Oss. Fœtus.

organs, contain a greater proportion of fluids in the early periods of life; and, under ordinary circumstances, we may perceive a regular gradation from a more fluid to a more solid consistence, until, as it would appear, the quantity of solid matter becomes incompatible with due performance of the functions.

From both these causes there will be a diminution in the relative quantity of the arterial blood, a circumstance which must materially affect all those operations which are more directly connected with the circulation. A less quantity of blood will pass through the pulmonary vessels, so as to give less opportunity for it to be acted upon by the air; all the secretions therefore which are furnished from the arterial blood will be diminished in quantity or deteriorated in their quality. Hence the digestion will be impaired, and thus the supply of materials will be cut off for the immediate support of life. The changes that have been described are so connected together, that any one deficiency obviously induces a derangement of the whole system. Thus the diminished quantity of arterial blood decreases the velocity of the heart, and by causing it to propel its contents with less vigour, the blood stagnates in some of the minute vessels, and thus lays the foundation for still further derangement. Among these is a diminution of temperature, which is occasioned by several concurring causes, and which again, in its turn, increases the evil, by not supporting the contractility of the muscular fibre.

The changes that take place in the nervous system are no less remarkable than those in the muscular. The composition of the brain is altered; it is gradually rendered firmer, and, as in other parts of the body, the quantity of blood transmitted to it is considerably diminished. The sensitive functions all of them experience a corresponding change; the different organs of sense become less adapted for receiving the impressions of external objects, the nerves transmit them less readily to the sensorium commune, and the perceptions which they excite there are less vivid.

The state of the intellectual faculties undergoes, at the same time, a gradual progress, which bears a relation to these alterations in the physical powers. In infancy the brain seems principally adapted to receive those impressions which are connected with the contractile functions. During childhood the mental powers gradually unfold themselves, and the different faculties rise up in succession, according to the operation of external objects, modified by the peculiar organization and innate propensities of the individual. In youth the impressions upon the senses are the most vivid, there is a greater rapidity of conception, and from this cause there is a greater tendency to form powerful associations. This, therefore, is the season of fancy and imagination, while the power which the mind possesses of associating its ideas with facility gives strength to the memory, and enables us to retain the knowledge which is acquired. As age advances, impressions on the organs of sense produce less effect, new associations are less easily acquired, and in general the mental faculties have more difficulty in undergoing any kind of alteration. While former habits are retained with greater force, and while old associations are recollected with peculiar correctness, recent events are forgotten, and new modes of life are with difficulty had recourse to. At length the powers of the system entirely fail; the external senses become callous to the impression of surrounding objects, and the mental faculties become irregular and uncertain in their operation. Although the decay of the physical and of the intellectual powers does not always proceed with equal rapidity, yet the respite which may be granted to either is not of long continuance.

### § 2. *Causes of Dissolution.*

The successive stages of growth, maturity, and decline, are necessarily connected with our constitution, and must therefore depend upon some invariable law of the animal œconomy. The changes which take place in the constitution and structure of the body, as described above, may be considered as sufficient causes of its necessary and inevitable tendency to decay. Physiologists have not, however, been satisfied with this general conception of the subject, but have endeavoured to point out more minutely the intimate nature of the alterations which were observed to take place, and thus to discover what may be technically termed the proximate cause of natural death. Boerhaave is the first writer whose speculations on this subject were so far matured, or deduced from correct observations, as to entitle them to any detailed examination. His hypothesis was founded, in a great measure, upon mechanical principles, in connexion with the doctrines of the humoral pathology. It proceeded upon the idea, that the motion of the fluids through the vessels, and the friction which must be thus necessarily produced, will tend to destroy the texture of the parts; at the same time all the fluids of the body are gradually converted from a bland into an acrimonious state, while the elements of which they are composed are transformed from those which constitute the fluids, to such as enter into the composition of solid substances. He, however, principally insists upon the physical changes in the vessels, upon their partial obliteration, upon the increasing rigidity of their texture, and upon the greater proportion which the solids generally bear to the fluids.

The opinions of Boerhaave were, most of them, adopted by Haller, and were considerably amplified and supported by new facts and arguments. He also made a very important addition to the hypothesis, by introducing the consideration of the vital properties of the system, which had before been scarcely taken into account. Haller supposes, that not only the texture of the body is rendered more solid and less flexible and elastic, but that the powers of contractility and sensibility are essentially diminished, and that the physical change which the body experiences is derived in part from the diminution of these powers, as well as of that of the organs through which they operate.[6] Although it might be very difficult to decide absolutely upon the question, in what degree the structure and functions of the body primarily influence each other, or to adduce any absolute proof, that the functions may be deranged without any derangement of the organs, yet, I conceive it to be more agreeable to the general analogy of the animal œconomy to suppose that this may be the case, than that the powers of vitality, in all instances, bear an exact ratio to the condition of the organs.

This view of the subject was adopted by Cullen, who, according to his usual custom, has compressed into a short compass an elegant summary of his doctrine. He proceeds upon the three principles, that there is a different distribution of the blood in the different periods of life, that the vessels offer a greater resistance to the entrance and transmission of the fluids as age advances, and that the excitability of the system is gradually diminished. In youth the quantity of blood is the most considerable; the arterial system is always in a state of over-distension, and, from the greater contractility and sensibility of the system, has a tendency to increased action. On this depends the growth of the body; the functions are all in an active state, a large quantity of blood is formed, and this is deposited by the arteries in the different glands or organs of secretion, from which the materials of the body are composed. This addi-

[6] El. Phys. xxx. 3. 3, entitled, "Vis insita et nervosa minuitur."

tion of new matter, and the force of the circulation, distend the different parts and add to their bulk. After some time the addition of matter, and the degree of extension, resist the further continuance of the process, and the power of the arteries is so balanced to the condition of the system as to enable it to retain its present state.[7]

But this balance is soon destroyed by the diminished action, both of the muscular fibre and of the nervous matter, in consequence partly of the decline of their powers, and partly from the diminution in the quantity of arterial blood that is sent to them. At the same time the veins being more distensible than the arteries, and having experienced less alteration in their texture, and partaking also less of the vital actions of the system, the blood is more disposed to accumulate in them. There are many facts in pathology which appear to countenance Cullen's hypothesis of the arterial plethora in youth, and the venous plethora in old age, and it seems likewise to coincide with the state of things, which might be expected to ensue, from the actions of a system so arranged and so organized. From calculations that have been made, combined with anatomical observations, it is found that the growth of the heart does not keep pace with that of the san-

[7] Many parts of Cullen's hypothesis may be regarded as deduced from, or at least as confirmed by, the experiments of Winteringham; this was particularly the case with the change in the distribution of the blood, and in the relative strength of the arteries and veins; see Exp. Inq. p. 29, 0. 32, 35 . . 7. 187, 8.

guiferous system generally,[8] while, at the same time, we learn from the experiments of Winteringham, that the arteries become firmer, and would consequently require a greater force to preserve them in the same state of distension. The veins, being the less active part of the circulating system, and being chiefly of use as reservoirs to contain the blood and suffer it to return to the heart, after it has performed all its functions, and is reduced to what may be regarded as an inert state, thus become surcharged with blood, and it appears, as a matter of fact, that their relative capacity is increased.[9]

The deficiency of the force of the arterial circulation necessarily produces another effect on the body, to which we have already alluded, and which must materially assist in explaining the changes that take place in the functions. When the blood is propelled with less force than ordinary, the deficiency will be first experienced in the minute or capillary branches of the arteries, and these are in fact found to be much diminished, and many of them to be even entirely obliterated. Anatomists are well acquainted with this circumstance, and have frequently occasion to observe, that certain parts of the body, which are vascular in youth, as age advances become entirely solid.[1]

[8] Cullen's Physiol. § 298. p. 249.

[9] Winteringham's Inquiry, p. 29, 0.

[1] The gradual obliteration of the capillary arteries, during the progress of life, was a subject to which Ruysch particularly attended, and which, in consequence of the minuteness of his in-

I have remarked, in a former part of this work, that all the changes which the blood experiences, and all the actions which it produces, take place in the capillaries of the arteries, and that the large trunks are to be regarded principally as tubes, by which the blood is conveyed to the extremities. The diminution of these capillaries must obviously and directly impair the functions of every part of the system, and will explain the diminished activity of both the mental and the corporeal powers.

Another physical cause of decay in the system is a want of due correspondence or co-operation between the different functions, and especially between those of assimilation and of absorption. I have formerly explained the nature of that constant change which is going forwards in the system, by which the particles of matter are undergoing a kind of circulation, the old ones being removed by the absorbents, and new ones deposited in their place by the secreting arteries.[2] It sometimes happens, that these two sets of

jections, he proved to take place to a degree that had not been previously suspected; Advers. Decas 2. pars 4. Op. t. ii.; Haller, El. Phys. xxx. 1. 12. The dexterity of the modern anatomists has made us familiar with an occurrence which was formerly regarded with much astonishment, or even with a degree of scepticism.

[2] It is from this circumstance that Haller styles the body, "Machina, quæ se ipsum et destruit et instaurat." El. Phys. xxx. 2. 6.

> Nostra quoque ipsorum semper, requieque sine ulla,
> Corpora vertuntur: nec, quod fuimusve, sumusve,
> Cras erimus. Ovid, Metam. xv. 214..6.

actions do not correspond, or proceed with a due relation to each other; at one time secretion goes on too rapidly, and the body becomes bulky, while at another time an opposite state produces emaciation. These are not indeed to be regarded as constituting disease, when existing within certain limits, but when excessive they lead to derangement of the machine, and ultimately prove inconsistent with health. And besides an excess or defect in the quantity of action, the functions occasionally seem to acquire a wrong direction; in some cases an unnatural formation of adipose matter takes place, which oppresses and impedes many of the operations of the system, and in other instances still more serious evils arise, from the deposition of bone in membranous parts, especially when they are connected with the vital functions, as, for example, in the valves of the heart or the large arteries.[3]

From the combined operation of all these causes, we may easily conceive how all the functions will be liable to become deranged, and that this derangement is not merely the effect of accident, but is the natural progress of the constitution, and the inevitable fate of animal existence.[4] After the view which I have

[3] Haller treats of this cause of decay in El. Phys. xxx. 3, 7; but many of the facts which he adduces, of solid concretions in the soft parts, are of a different kind from the bony indurations incident to old age; they are morbid calculous depositions.

[4] What we may style natural death is a very rare occurrence, so that, although the process which I have described may be supposed to be always going forwards in every individual, yet it is to a very few that it is granted to reach its termination. Haller

taken of the laws of the animal œconomy, and an investigation of its wonderful mechanism, with all its adjustments and its contrivances, it may appear remarkable, that so admirable a structure should be intended to last for so short a space of time; and we might be tempted to regret, that what is so beautiful should not be more permanent. But the present state of things appears to be the general order of nature with respect to all organized bodies. Perpetual change is an essential quality of their constitution, and this system of change is experienced not only in the component parts of each individual, but extends to the individuals themselves. We have found that it does not depend upon any accidental circumstances or any partial imperfection, but that it is interwoven with our nature; and that as maturity succeeds the period of growth, so is maturity necessarily succeeded by dissolution.

estimates the average probability of human life, under the circumstances in which mankind are ordinarily placed, and deduces the conclusion, that only one individual in about 15,000 reaches the 100th year; El. Phys. xxx. 3. 15. There is, however, reason to suppose that, in this country at least, the average length of human life is increased during the last thirty or forty years.

# APPENDIX.

## APENDIX TO CHAPTER I.

### *Dr. M. Edwards on the Elementary Structure of the Body.*

A SERIES of very interesting microscopical observations have been made in Paris, upon the elementary structure of the different animal textures, by Dr. Milne Edwards: they are contained in two essays, the first of hich was published in 1823,[1] but was not known in this country until after the publication of my first volume; the second appeared at the close of the last year.[2] Dr. Edwards examined in succession all the principal textures which enter into the composition of the body, the cellular substance, the membranes, tendons, muscular fibre, nervous matter, the skin, and the coats of the blood vessels. When the cellular substance is examined by a powerful lens, it appears, as Fontana had described it, to consist of cylinders; but by using still higher magnifying

[1] Memoire sur la Structure élémentaire des principaux Tissus organiques des Animaux.

[2] Recherches microscopiques sur la Structure intime des Tissus organiques des Animaux; in Ann. des Scien. Nat. pour Decem· 1826. t. ix. p. 362, et seq.

powers, these cylinders are resolved into rows of globules, which are all of the same size, 1-300th of a millimetre, or about 1-7500th of an inch in diameter. These rows of globules are separated from each other, and lie in various directions, crossing and interlacing, some of them being straight, others bent, and some twisted, forming imperfect strata, which are connected with each other by a kind of irregular net-work. The membranes of the different classes of the vertebrated animals all seemed to be similarly constructed, being composed of these irregular interlacing rows of globules, which are all of the same size; and he found precisely the same structure in the serous and mucous membranes. Dr. Edwards next examined the muscular fibre in its recent state, and found this, like the membranous fibre, to be composed of globules, likewise of 1-300th of a millimetre in diameter. They differed, however, in their disposition, being formed in straight lines, which were nearly parallel to each other, each line being separable from those that were contiguous to it, while the globules of which it was composed still remained connected together; as the author says, "au lieu d'être réunis en séries irrégulièrement disposées, ils decrivaient toujours des lignes à peu près parallèles entre elles." [3] The size of these globules, and of the fibres which they form, would appear to be the same in all animals in whom a muscular texture can be discovered; not, as was supposed by Muys and others, of different sizes in different animals, or even in the

[3] P. 14.

same animal at different ages. The tendons, the aponeuroses, and the parts of the skin, were all of them ultimately to be resolved into the same-sized globules formed into lines, differing only somewhat in the direction or parallelism of the lines. The observations on the middle coat of the arteries are important as affecting the question of their muscularity; Dr. Edwards examined a portion of this coat, as procured from a human aorta, and found it to be composed of the same kind of globules, "disposés par rangées plus ou moins longues qui se portent toutes dans la même direction, et forment enfin des lignes légèrement ondulées, comme celles qu'on observe dans le tissu fibreux."[4] I may remark, however, that it is not the aorta, or the large arterial trunks, but the capillary arteries, which are conceived to be the seat of their contractile power. Dr. Edwards's examination of the nervous matter afforded him similar results; it appeared to be composed of lines of globules, of the same size with those that form the membranes and the muscles, but, as it would appear, holding an intermediate place between these two substances as to the regularity of their disposition, and having a fatty matter interposed between the rows of globules. Dr. Edwards's observations differ from Mr. Bauer's in one essential respect; Dr. Edwards conceiving the cerebral globules to be all of the same size, while Mr. Bauer supposes that they exist of various sizes.[5] It is probable that the fatty matter which Dr. Edwards observed between the

[4] P. 16. [5] Phil. Trans. for 1821, p. 28.

fibres is the same substance which Mr. Bauer, or rather Sir E. Home, describes as a species of jelly, and which he supposes to be the immediate agent in the communication of sensation and volition.[6] Dr. Edwards's account of this substance is as follows: "Si on écrase la masse médullaire, on aperçoit, outre les globules primitifs, des globules ou gouttelettes dont la forme et la volume varient, et qu'on reconnaît facilement pour être de la graisse."[7]

I shall give, in the words of the author, the general conclusion that he deduces from his observations. "En effect, .... des corpuscles spheriques, du diamètre de 1-300 de millimétre, constituent, par leur assemblage, tous les tissus organiques précédement énumérés, quelles que soient du reste les propriétés de ces parties, et les fonctions aux quelles elles sont destinées."[8]

Dr. Edwards's paper in the Ann. des Scien. Natur. may be regarded principally as giving his sanction to his former observations, respecting the elementary composition of the textures of the animal body. He again remarks, that they may all be resolved into globules of the same size, the 1-300th of a millimétre, and adds, "On voit donc que la structure intime des divers tissus qui composent les animaux présente partout des charactères analogues, et que la forme globulaire est toujours celle qu'affectent les élémens organiques de ces parties."[9] The most important of the additional observations is what respects the struc-

[6] Phil. Trans. for 1821, p. 32. [7] P. 19.
[8] P. 20. [9] T. ix. p. 390.

ture of the secretory organs, which is thus described; "partout on n'aperçoit que des corpuscles globuleux, agglomérés d'une maniere confuse, et constituant ainsi par leur assemblage le parenchyme de l'organe."[1] He thinks that the globules of milk, of pus, and the nucleus of the particles of the blood, when deprived of their coloured envelope, are of the same size with these globules which compose the fibres of all the soft parts of the body, and which are therefore entitled to the appellation of elementary. Dr. Edwards, however, informs us, that the globules of the mollusca are considerably larger, and that their external surface presents a tuberculated appearance, indicating that it is composed of a series of smaller globules. And we may remark that the coloured envelope which surrounds the central nucleus of the red particles of the blood, must either afford an exception to the globular composition, or must at least be composed of globules very much smaller than those which have been described above; and the same remark will apply to the serum. Indeed, Dr. Edwards, in the conclusion of his essay, may be regarded as giving up the principle of his hypothesis, for he remarks, "Ces globules que l'on peut appeler elementaires, sont peut-être formés, à leur tour, d'autres corpuscles plus petits . . ."[2] With respect to the observations, considered as matters of fact, I do not presume to give any decided opinion, but I think it is but justice to state, that they are related in a clear simple manner, and that the papers are written with an

[1] P. 201. [2] P. 390

air of candour and good faith, which is well calculated to inspire oonfidence, both in the veracity and in the accuracy of the author.

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## *M. Dutrochet on the Elementary Structure of the Body.*

Not long after the publication of the first of the above works, a treatise appeared from the pen of M. Dutrochet, on the same subject with Dr. Edwards's, and, like them, containing a number of microscopical observations, which lead to very important conclusions respecting the elementary structure of the animal body.[3] M. Dutrochet began with the examition of the structure of vegetables, the account of which occupies the first four sections of his work; but these, as being only indirectly connected with the subject more immediately under consideration, I shall pass by, and proceed to the fifth, which is entitled, "Observations sur le structure intime des systèmes nerveux et musculaires, et sur le mechanisme de la contraction chez les animaux."[4] He begins by remarking, that former physiologists had ascertained the nervous matter to be composed of globules, or, as he terms them, "corpuscules globuleux," and par-

3 Recherches anatomiques et physiologiques sur la Structure intime des Animaux et Vegetaux.

4 P. 163.

ticularly refers to the observations of Dr. M. Edwards. He, however, advances a step farther than Dr. Edwards, who simply announced them to be globules, and describes them as "des cellules d'une excessive petitesse, lesquels contiennent une substance médullaire ou nerveux;" thus, as it appears, conceiving of the elementary globule of Dr. Edwards, as a body containing other matter, which, if globular, must be composed of particles very much more minute. This structure, we are informed, is very obvious in the ganglia which surround the œsophagus of some of the mollusca, where we can distinctly perceive globular cells, to the interior parietes of which are attached globular corpuscles, these corpuscles being themselves cells filled with medullary or nervous matter.[5] The nerves of a frog are found to be provided with these globular corpuscles; but here they appear to be attached externally to transparent fibres, the fibres being tubes filled with a fluid, which fluid is conceived to perform some important office in the functions of the nervous system.[6] These same globular corpuscles are dispersed irregularly through the substance of polypi, and are supposed to constitute their nervous system.[7] With respect to the structure of nerves, as distinguished from that of the brain, M. Dutrochet contends that the elementary fibres which enter into their composition, are not composed simply of rows of globules,

[5] P. 166. [6] P. 168. pl. 2. fig. 22.
[7] P. 170. pl. 2. fig. 29.

according to the opinion of Dr. Edwards, "mais que ce sont des cylindres d'une substance diaphane dont la surface est hérissé de corpuscules globuleux, lesquels tantôt sont en contact et placés à la file, tantôt sont séparés les uns des autres. Comme ils couvrent toute la surface du cylindre, on est porté, dans l'observation microscopiques, à croire qu'ils le composent interieurement." [8] Upon this difference between the structure of the brain and the nerves, the one being principally destined for the production of nervous power, and the other for its transmission, or, as it is termed, for nervimotion, at the same time that the former is principally composed of nervous corpuscles and the latter of nervous fibres, the author builds an hypothesis of the respective uses of these two structures. And he farther conjectures, that as in vegetables, "la nervimotion est transmise par l'intermediaire du liquid séveux," so in animals these nervous fibres must be tubes filled with a peculiar fluid, and that it is through the intervention of this fluid that the transmission of the *nervimotion* is effected.[9]

M. Dutrochet's account of the ultimate structure of the muscular fibre is less hypothetical; he agrees with the latest observers, that they are composed of straight rows of globules, to which he proposes to give the name of muscular articulated corpuscles, and we have a good deal of speculative reasoning upon the nature of muscular contraction, or the

[8] P. 169. [9] P. 170.

change which the fibre experiences when it shortens itself. He remarks, that the globules which compose the different structures of the invertebrated animals are considerably larger than those of vertebrated animals; that the former appear to consist of cells that contain other globules still smaller; and he hence infers, that the globules of vertebrated animals are likewise cellular, and contain other series of still smaller globules. The above constitute only a few of the numerous observations and hypotheses that are contained in M. Dutrochet's essay. With respect to the observations and the deductions that are made from them, I shall only remark, that in proportion as they recede more from the ordinary opinions that are entertained upon these topics, so do they require a proportionably greater weight of evidence for their establishment. In some points his system agrees with that of Dr. Edwards, but in many essential circumstances it differs widely from it. M. Dutrochet aims at a much more minute development of the system of organized bodies than Dr. Edwards, and must have required a much more powerful instrument for this purpose. He informs us that he employed in his researches the single microscope, which, he remarks, "seul peut procurer une vision très nette et très distincte." The little experience that I have myself had on this subject induces me to coincide in this remark, when the examination of very minute objects is concerned.

## *Dr. Goring's Observations on the Texture of Hair.*

In the account of hair, v. i. p. 91, I remarked, that the bristled texture which it had been supposed by some anatomists to possess, had not been detected by means of the microscope; it appears, however, that this has been accomplished by Dr. Goring. In the last number of the Quarterly Journal, p. 433, 4, we have the following description. "A hair viewed on a dark ground...... is seen to be indented with teeth somewhat resembling those of a coarse round rasp, but extremely irregular and rugged; these incline all in one direction, like those of a common file, viz. from the origin of the hair towards its extremity......" Dr. Goring appears not to have been aware that the existence of this peculiar texture had been announced by Bichat. I may remark, that I had an opportunity of viewing the human hair, and the hair of various kinds of animals, in the microscope of Mr. Bauer, but was not able to detect these appendages.

## APPENDIX TO CHAPTER III.

### *MM. Prevost and Dumas on Muscular Contraction.*

In an appendix to Dr. Edwards's work, "De l'Influence des Agens Physiques sur la Vie," to which I have so frequently had occasion to refer, we have a new hypothesis of muscular contraction by MM. Prevost and Dumas, in which the phenomena are attempted to be explained by the operation of electricity. After some preliminary observations on the nature of electricity in general, upon its two modifications, as developed by the common machine and by the pile respectively, they remark, that the effect of its mechanical action upon organized bodies is to separate their particles from each other. If an electric spark be passed through a drop of blood, we shall observe the molecules of which the blood is composed, "prendre à l'instant un aspect framboisé," which is ascribed to the partial separation of its elementary globules. Upon the same principle, if an electric discharge be transmitted through a fluid containing infusory or spermatic animalcules, the animalcules are immediately destroyed, in consequence, as is supposed, of "l'ecartement forcé qu'eprouvent les globules organiques dont le tissu se trouvait composé." Hence they argue, that those parts of the animal frame will suffer the most from the electric fluid, which are the best adapted for transmitting it. This position is perhaps of a somewhat doubtful aspect; but the nerves appear to be the organs which actually receive the greatest injury when the animal is destroyed by lightning, or by artificial electrical discharges, and this injury is supposed to consist in their mechanical disorganization. The blood is known not to coagulate in animals that are killed by lightning; on this fact our authors reason as follows: "Or, comme il semble suffisament demontré aujourd'hui que la coagulation du sang resulte d'une attraction moleculaire entre les globules qu'il renferme, on conçoit qu'elle devient necessairement nulle lorsque, par l'effet du passage du fluide electrique, cette attraction se change en repulsion."[8]

After these general observations on the operation of electricity upon the animal frame, we proceed to consider its application to the phenomena of muscular contraction. The authors describe the mechanical structure of the muscle as consisting of bundles of fibres, which, by the aid of the microscope, may be ultimately resolved into single fibres, that in their quiescent state lie in right lines parallel to each other, but which, when viewed under the influence of a stimulus, in a state of contraction, "se flechissent tout-à-coup en zigzag, et presentent un grand nombre d'ondulations regulieres." When the stimulus is withdrawn the fibres again become straight, and upon its re-application they resume the angular appearance. Upon minutely attending to the form and situation of these undulations, it was found that they constantly took place in the same spot on the surface of

[8] P. 541.

the fibre, and hence we conclude that they depend upon some fixed and permanent cause connected with their structure. Now the cause assigned is the presence of the minute nervous filaments, which, partly from the result of observation, and partly as deduced from hypothesis, are supposed to intersect the muscular fibres at right angles. As the whole speculation is founded upon the mechanical relation which subsists between the muscular and the nervous filaments, I shall quote the description of it which is given by the authors. In speaking of the mode in which the nerves enter into the muscles, they remark, "Tantôt ce sont deux troncs nerveux paralleles aux fibres du muscle, qui cheminent à quelque distance l'un de l'autre, et se transmittent mutuellement de petits filets qu'on voit passer au travers de l'espece musculaire que les separe, en la coupant à angle droit. Tantôt le tronc nerveux est deja lui même perpendiculaire aux fibres du muscle, et les filets qu'il fournit s'epanoissent en conservant cette direction, parcourent l'organe, et reviennent sur eux mêmes en forme d'anse."[9]

It is then assumed that the nerves are better conductors of the electric fluid than the muscles, and by employing the principle which has been lately established respecting this agent, that when it traverses conductors in the same direction, it causes them to be attracted towards each other, it is conceived that we have the necessary conditions for forming a theory of muscular contraction. The electric fluid passing through the parallel nervous filaments, esta-

9 P. 566.

blishes an attraction between them, which attraction necessarily throws the muscular fibres across which they pass into the angular form, which it actually exhibits in the microscope. To establish the hypothesis, it remains to prove, that electricity is concerned in all muscular contractions, and this the authors endeavour to do, by attempting to show, that in such cases there is actually an evolution of electricity, as indicated by the usual instruments. This, indeed, they candidly admit they have not been able to accomplish in all cases, but the minuteness of the quantity is considered a sufficient reason for the failure.

The subject was shortly afterwards taken up by Dr. Edwards himself, in a paper published in the "Annales des Sciences Naturelles," for May, 1825. He begins by remarking, that MM. Prevost and Dumas had shown, that in the three ordinary modes by which muscular contraction is produced, the application of chemical acids, of heat, or of mechanical pressure, electricity is developed, but still it does not follow, that electricity is the cause of the contraction. In prosecuting his inquiries the author found that the electricity might be evolved from the nerve merely by a certain mode of contact. "La procédé consiste à toucher un nerf, comme on touche un barreau d'acier pour l'aimanter, en faisant passer l'excitateur sur une certaine étendue de sa surface." He farther found that a metallic body was not necessary for the production of the effect, but that, "il suffisait de toucher de la maniere que j'ai decrite avec un corps solide quelconque." It now becomes a question, whether the action exhibited by the muscles depended upon some unknown intermedium, or whether it was simply the consequence of the electricity which is developed by the mere contact of the two bodies; and Dr. Edwards endeavoured to determine this point, by observing the different effects which were produced upon the muscles according as the nerves were placed upon conducting or non-conducting substances; and after varying the circumstances of the experiment, and noticing the results in all their relations, he draws the following conclusion: "Le fait sur lequel j'ai desiré attirer l'attention par cette note, consiste en ce que, toutes choses egales d'ailleurs, les contractions produites par le contact d'une corps solide et d'un nerf sans arc galvanique, sont diminuées ou abolies, si ce nerf, au lieu d'etre isolé, communique avec un bon conducteur; d'ou il paraitrait resulter que les contractions sont dues à l'electricité produite par le contact du nerf et du muscle."

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## *M. Dutrochet on the primary Cause of Animal Motion.*

We are indebted to M. Dutrochet for a second treatise, no less interesting and ingenious than the one of which I have given some account above: the object of this latter work is to investigate the primary cause of animal motion.[1] In this, as in the former, he commences by an inquiry into the laws of

[1] L'agent immediat du mouvement vital.

vegetable physiology, and then applies the same principle to explain the phenomena of animal life. He conceives that he has discovered the mode by which the sap is distributed through vegetables, and has been enabled, by this discovery, to explain "le mechanism secret du mouvement vital." The first section is on the course of the sap and the cause of its progressive motion. These two points he makes the subject of minute observation and inquiry; he examines the opinions of preceding writers and experimentalists, more especially those of Knight, Mirbel, and Decandolle; freely, but candidly, criticizes their hypotheses, and fairly states the grounds upon which he differs from them. According to M. Dutrochet, the sap ascends through the vessels named by Mirbel "fausses trachées," the *tubes lymphatiques* of Decandolle, and is carried by them to the leaves, where it undergoes certain changes, analogous, as it would appear, to those effected in animals both by the lungs and the stomach. The tracheæ, or spiral vessels, are supposed to perform a different office from the lymphatic tubes; they do not serve for the conveyance of the sap, but contain a certain "liquid vivifiant," which receives its specific vital properties, not by the process of oxygenation, but by an analogous operation performed by light, which is termed "insolation."[2] To proceed, however, to the more immediate object of the essay; by tracing the parts of the plant which contains the sap to the organs where the fluid appears to be admitted in the first

[2] P. 21.

instance, we arrive at minute bodies attached to the fibres of the roots, to which Decandolle gave the name of "spongioles," and it appeared that in the healthy state of the plant, these *spongioles* always remained turgid, indicating that the turgid state was, in some way, connected with their power of absorption. The explanation of this process was derived from a series of microscopical observations, made upon an extremely minute vegetable, which attaches itself to animal matter that is suspended in water. A singular phenomenon presented itself when the fibres of this plant were viewed in the microscope; they were seen to emit a number of globules, while the space which these globules occupied was filled up by an equal bulk of water. This singular effect the author ascribes to what he conceives to be a general principle, that is intimately concerned in the operations of both animal and vegetable life, according to which, whenever a cavity containing a fluid is immersed in another fluid, less dense than that which is in the cavity, there is a tendency in the cavity to expel the denser and absorb the rarer fluid. This is to be regarded as an ultimate fact, one which cannot be referred to any of the other known operations of nature: upon this new power M. Dutrochet bestows the name of *endosmose*, "dedans impulsion."[3] The existence of this "physico-organic or vital" action having been thus detected in the spontaneous operations of nature, a series of experiments were undertaken for the purpose of illus-

[3] P. 115.

trating its operation. The experiments consisted in filling membranous bodies, as the intestine of a chicken, with milk or some other dense fluid, and immersing it in water, when it was observed, that the milk left the intestine while the water entered it. And it was afterwards found that a reverse operation took place; if the internal fluid was rarer than the external, the transmission took place in the opposite direction, depending upon what is termed *exosmose*, "dehors impulsion." It appeared that in both cases the energy of the action was in proportion to the difference between the specific gravities of the two fluids; and also that, independent of their gravity, their chemical nature affected their power of transmission. The author here calls in aid of his hypothesis a very curious experiment, which was performed by Mr. Porrett,[4] according to which, by a certain arrangement of the galvanic apparatus, water was caused to pass through a membranous substance; and, connecting this fact with the known operation of the electric currents in the

[4] The experiment of Mr. Porrett's here referred to, consisted in fixing a piece of bladder perpendicularly in a glass jar, so as to divide the jar into two cells; one of these is nearly filled with water, while the other contains a few drops only. A galvanic apparatus is then employed, so as to induce the positive electricity upon the larger quantity of water, when, after some time, it is found that a considerable portion of the water has transuded through the bladder, until the level of the fluid in the negative cell is higher than that in the positive cell; Ann. Phil. v. viii. p. 75, 6. The experiment is certainly a very curious one, but I apprehend that many additions will be necessary, before we can draw the inference from it which M. Dutrochet is inclined to do.

decomposition and transfer of various substances, M. Dutrochet does not hesitate to draw the conclusion, that "l'endosmose et l'exosmose dependent entierement de l'electricité."[5] The cavities in which these changes take place he conceives to be analogous to Leyden phials, having their two surfaces charged with the two electricities; the ultimate effect, or the direction of the current, being determined by the excess of the one over the other. The principle being once established, the application is comparatively easy. The turgidity of the *spongioles* causes the discharge of their contents, and the necessary entrance of the water from the action of endosmose. The turgidity, upon which the whole operation seems to hinge, is, however, effected by what is termed "adfluxion," which would appear to be a previous step to *endosmose,* and would seem to be dependent upon a vital action, different from any of those that are generally recognized. The result is the entrance and subsequent progression of the fluid; and the organ in which this double effect is produced is said to be "l'origine d'impulsion et but d'adfluxion."[6] The action of endosmose is at least as important in the animal as in the vegetable œconomy. It is a principal agent in the circulation, the action of the capillaries being supposed to consist, not in their contractility, but in endosmose. Endosmose is also the main agent in the action of the absorbents, and it is inferred that nutrition, secretion, and indeed all the functions by which a change is induced in the composition of the body,

[5] P. 139. [6] P. 168.

are, in a great measure, to be referred to this source. The above may be considered as affording a very brief, although I think not an incorrect, view of the leading points of M. Dutrochet's hypothesis. The foundation on which it rests is of so novel an aspect, while the application that is made of the new principle is so extensive and so important, that the author himself must admit the absolute necessity of removing every degree of doubt that will unavoidably attach to it, until the observations have been multiplied and confirmed by other physiologists.

Since the publication of my first edition, M. Dutrochet has considerably extended his experiments on this very curious subject, and has given his additional observations to the public in two papers in the Ann. de Chim. for Aug. 1827, and for Feb. 1828; and, more lately, in a separate essay.[7] The author controverts an opinion, which had been entertained by M. Poisson, that the phenomena might be referred to capillary attraction, and he also concludes that they are not electrical. He describes an ingenious instrument, which he styles an endosmometer, by means of which he is enabled to measure the degree of effect produced under various circumstances, and by various chemical agents. These, he finds, differ considerably from each other, so as to render it highly probable, that the peculiar action is connected with their chemical constitution. We have a good detail of M. Dutrochet's experiments and hypotheses in the Ed. Med. Journ. v. xxxi. p. 369.

7 Nouvelles Recherches sur l'Endosmose et l'Exosmose.

## APPENDIX TO CHAPTER IV.

Since the publication of my first volume, several valuable treatises have appeared on the anatomy and physiology of the nervous system. The authors whose works I may select, as more particularly deserving attention, are Prof. Tiedemann, MM. Flourens, Serres, and Desmoulins, and Mr. Mayo.

### *Prof. Tiedemann's Anatomy of the Brain.*

When I wrote the chapter on the nervous system, I was acquainted with Prof. Tiedemann's work on the brain only by means of a brief notice of it in some of the periodical journals. Since that time I have had the gratification of perusing a French translation of it, which seems indeed to have been published before my last volume, although I believe it had not been received in this country.[1] The name of the author stands so high among the physiologists of the present day, that is unnecessary to offer any eulogy upon whatever proceeds from his pen. His experimental investigations appear to have been pursued with persevering industry, so that there seems to be nothing wanting to complete our know-

[1] Anatomie du Cerveau, traduite par Jourdan. We have a very elaborate and judicious analysis of this work in the 82d number of the Edin. Med. Journ.

ledge of those topics to which he has directed his attention, while his conclusions are formed with that cautious spirit, which seldom leaves any room for doubt or hesitation.

The work consists of two parts: the first is entitled Researches on the Structure of the Brain of the Embryo, in the different periods of its development; the second consists of considerations on the different parts of the brain, with a comparative description of their state in man and in other animals. It would be impossible for me to give an analysis of the numerous observations which are contained in this work, without extending my observations to a length altogether inconsistent with my general plan: I shall therefore only select a few of the most important conclusions which he deduces from them.

We learn that the brain and the spinal cord do not exist in the early period of the life of the fœtus, their place being occupied by a limpid fluid; about the fifth or sixth week after conception, a cavity may be detected, containing a whitish fluid, which may be regarded as the first visible rudiments of the nervous system. In an embryo of nearly an inch in length, and of about the period of nine weeks, the separate parts of the brain begin to be much more distinguishable; the spinal cord and the cerebellum are visible, and the part which is afterwards converted into the corpora quadrigemina, acquires a considerable bulk; the cerebrum at this time appears only under the form of a membrane. From this date the development of the brain proceeds with considerable rapidity, so that all the different parts, which enter into the composition of the organ in its perfect state, may be successively traced, although, in most cases, bearing to each other a very different relation as to their bulk and their respective situation, from what they do at a future period. The most interesting circumstance in this part of the investigation respects the order in which the great divisions of the nervous system come into existence. It would appear, that at the end of the second month, the spinal cord, and the two anterior prolongations or peduncles of the brain form, as it were, the basis to which the other parts are subsequently attached. The cerebrum can scarcely be said to exist, while the cerebellum and the tubercles are little more than layers of membrane, which are connected with the cord and peduncles; no nerves can be observed passing off from any part of the brain or spine, nor can any fibres be detected in them by the aid of the microscope; they seem to consist entirely of small globules.[2]

The second part of the work contains an account of each part of the brain and its appendages considered separately, tracing them from their first formation to their perfect state, and instituting a comparison between the human brain and that of the inferior animals. In describing the spinal cord and the medulla oblongata, the Professor takes occasion to animadvert upon the opinion of Dr. Gall, with respect to the relation which the cortical and the

[2] P. 24, 5.

medullary substances bear to each other; in opposition to the doctrine of this anatomist, our author maintains that the medullary matter of the spine is distinctly visible for some time before any of the cortical substance can be detected in it.[3] Professor Tiedemann also opposes another opinion of Dr. Gall's, that the spinal cord is composed of a series of ganglia, corresponding to the nerves which proceed from it; had this been its structure, it is probable, as he remarks, that it would have been apparent in the fœtal state; yet nothing of this kind can be detected.[4]

The question is discussed at some length, whether, according to the common opinion, the spinal cord be a production of the brain; or whether, according to the doctrine which has been supported by Dr. Gall, the brain should not rather be considered as an appendage to the cord, and as a production of it. Our author conceives that he has clearly proved this last to be the correct view of the subject, resting his opinion principally upon the progressive development of the parts, which is related in the first division of the work. With regard to the fact, as to the respective periods in which the parts come into existence, the observations of the Professor are decisive; but, I confess, that the controversy appears to me, in a great measure, a verbal one; the only important inference that we can derive from the fact is, that the functions of the brain, whatever they may be, cannot be exercised until after those of the cord.

[3] P. 128, 9.

[4] P. 134.

Some of the most interesting observations in the second part of the work are those which respect the progressive formation of the cerebrum. By tracing its development in the fœtus, and comparing this with its structure in the different classes of animals, we learn, "que les hémisphéres se développent d'avant en arriere et sur les côtés, et qu'ils s'étendent successivement, suivant la première de ces deux directions, au-dessus des corps cannalés, des couches optiques, des tubercules quadrijumeaux, et enfin du cervelet." The author goes on to remark, that the cerebral lobes become gradually more elevated and arched, and that their convolutions and sinuosities are progressively developed, so as to give the brain its elliptical and almost globular form. Thus the adult human brain is distinguished from that of all animals by the size and the elevation of its hemispheres, in the same way as it is by the greater number of its convolutions.[5]

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## *M. Flourens' Researches on the Nervous System.*

In the first volume of this work, I gave a brief account of some experiments that had been performed on the brain and nerves by M. Flourens, taken from an abstract of them drawn up by M.

[5] P. 147, 8.

Cuvier; they were shortly after published by the author himself in a separate volume.[6] It principally consists of various memoirs, which were read before the Royal Academy of Sciences, during the course of the years 1822 and 1823; the object of these, as expressed in the preface, is to ascertain the properties of the nervous system, and the functions which its different parts respectively exercise in voluntary motion. The nervous system consists of the nerves, the spinal cord, and the brain; and the brain may be considered as made up of the cerebrum, the cerebellum, the corpora quadrigemina, and the medulla oblongata; parts which differ so much in their structure and organization, as to render it highly probable that they exercise different functions. Before he enters upon the detail of his experiments, the author gives an account of the nomenclature which he proposes to employ; a point always of considerable importance for the correct conception of the subject, and particularly so in this case, where he adopts a language which is, in some degree, peculiar to himself.

He conceives that the nervous system possesses three distinct properties: that of volition and perception, which he regards as the same function, and terms sensibility; that of directly producing muscular contraction, which is termed excitability; and a third, which is said "coordonner les movements,"

6 Recherches Experimentales sur les Propriétés et les Fonctions du Systeme Nerveux dans les Animaux vertebrés.

and is consequently named "coordination." The leading doctrine of the author is, that these three functions are exercised respectively by the lobes of the cerebrum, by the nerves and the spinal cord, and by the cerebellum; and the main object of his experiments is to substantiate and illustrate this position. The method which he adopted to prove his hypothesis consisted in removing the different parts of the nervous system, or mechanically irritating them, and carefully noticing the effect produced upon the animal. A great part of the value of the experiments consists in the gradual manner in which the author proceeded from one part to another, and the corresponding observations which he made upon the state of the animal, at each step of the process, by which, at least in some cases, we are enabled to fix the limit of the seat of the different functions with very considerable accuracy. One of the most important conclusions that we may deduce from the experiments is, that mechanical injury of the cerebral lobes does not cause pain or excite muscular contraction, but that these effects always ensue from injury of the nerves, the spinal cord, the medulla oblongata, and the corpora quadrigemina. The cerebellum agrees with the cerebrum in the absence of pain or muscular contraction, when it is subjected to mechanical injury. By pursuing the same method of inquiry, with respect to the especial use of the cerebral lobes, we arrive at the conclusion, that "la mémoire, la vision, l'audition, la volition, en un mot, toutes les sensations disparaissent avec les lobes cérébraux. Les lobes cérébraux sont donc l'organe unique des sensations."[7]

This conclusion is principally founded upon the fact, that the senses of sight and hearing seem to be destroyed by the mutilation or removal of the cerebrum, and that a general state of sopor is induced, which renders the animal incapable of the exercise of voluntary motion. The especial functions of the cerebellum are then examined, when, upon mutilating or irritating this part, a variety of very singular and irregular motions were produced, which did not appear to be properly convulsive, and which seemed to consist in a loss of the power of connecting and regulating the contractions of the muscles, so as to produce the natural and appropriate actions of the animal. The especial result of the removal of the corpora quadrigemina appears to be the loss of sight; the contractile power of the iris is also destroyed, which it is said still remains after the removal of both the cerebrum and the cerebellum. As far as can be judged from the experiments, where a certain degree of injury must unavoidably be inflicted on the other parts of the nervous system, it would not appear that voluntary motion, or the external senses, except that of sight, are necessarily destroyed by the removal of these tubercles.[8]

Another subject to which the author particularly directed his researches, was to determine what parts

[7] P. 35. [8] Par. 2. § 6. p. 42, et seq.

of the nervous system act by what is termed "effet direct," and what parts by "effet croisé." It is well known, that an injury of the cerebrum or the cerebellum is manifested by a loss of the functions of the opposite side of the body; but with respect to the spinal cord, the medulla oblongata, and the tubercles, the point was still undecided. The experiments of M. Flourens seem to prove that the tubercles act like the cerebrum and cerebellum, while the spine and the medulla oblongata produce their effect on the same side of the body with that on which they have been injured.[9]

We have a long train of experiments, the object of which is to show what degree of mutilation or injury the different parts of the nervous system can sustain, without a complete destruction of their functions, and also what proportion the injury of the part and the loss of its functions bear to each other, and how far they possess the power of spontaneously repairing these injuries. Many of the results are very curious and unexpected, but they are of a kind which it is difficult to particularize in this short abstract; I may, however, remark, that they will tend very materially to illustrate many pathological facts, which have hitherto been altogether inexplicable. We have afterwards a long investigation respecting the involuntary motions; and the author inquires, whether there be any common centre, to which the origin of these actions may be referred, as is the case

9 Second Mem. § 9; . 15. p. 100. 122.

with the voluntary motions, and he especially examines what part of the nervous system is immediately concerned in the actions of the organs of respiration and circulation.

M. Flourens' work may be regarded as a very valuable repository of experiments, which appear to have been planned with ingenuity, and have every appearance of having been carefully executed and faithfully detailed. Many of his conclusions are fully warranted by the facts; but in some cases, I should be inclined to draw an inference precisely the reverse of that which has been formed by the author. One of the most important points which he attempts to establish is, that the lobes of the cerebrum are the exclusive seat of sensation and volition; yet it seems quite evident from the result of the experiments, that, after the removal of these lobes, sensation, although rendered feeble or obtuse, was by no means extinguished, while the functions which depend upon volition, such as the various kinds of loco-motion, were still executed by the animal, although it was difficult to excite them into action. The experiments that were performed upon the cerebellum are very interesting, and are perhaps some of the most decisive in their results of any which are contained in the volume. But I do not perceive the propriety of regarding them as produced by the operation of a distinct or specific nervous function. The "coordination" of M. Flourens may be referred to a species of sympathy or association, and the experiments will prove no more, than that the cerebellum is the centre of the sympathetic or associated actions of the nerves that are concerned in voluntary motion. As the author appears, in this case, to have unnecessarily multiplied the number of the nervous functions, so he has united together, under the title of sensibility, two operations of the nervous system, which have a peculiar claim to be considered as distinct from each other, perception and volition. I think we may also very fairly question the propriety of making excitability, or the power of producing muscular contraction, a distinct function of the nerves; upon the same principle, the power of conveying the perceptions of sight or of sound might be regarded as distinct functions. Each of these powers belong to their specific and appropriate nerves, but they are all to be regarded as modifications of the same action, the intimate nature of which is unknown.

The volume concludes with an account of the experiments of M. Rolando,[1] to which I have already referred, in the brief notice which was given of those of M. Flourens. Those of Rolando are very valuable, and it is not a little curious, that experiments so similar to each other should have been executed, nearly at the same time, without the concert or cognizance of the authors. Many of the most

[1] P. 273, et seq. M. Rolando's experiments were published in a work entitled, "Saggio sopra la vera Struttura del Cervello dell' Uomo e degl' Animali, e sopra le Funzioni del Sistema nervoso. Sassari, 1809." The translation in M. Flourens' work is taken from Magendie's Journal.

important results exactly coincide, and mutually corroborate each other; but, upon the whole, those of M. Flourens are considerably more complete and more decisive in their results. I think there can be no doubt that he was ignorant of M. Rolando's experiments when he performed his own.

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## *M. Serres on the Comparative Anatomy of the Brain.*[2]

M. Serres' treatise on the brain, like that of M. Flourens, obtained the prize of the Royal Academy, and was published about the same time with my first volume. Its particular object is to give an account of the brain in the four classes of the vertebrata, and, from the observations made upon them, to ascertain the respective functions of the several parts. In the prosecution of this object, the author has produced a work of considerable size, accompanied with numerous engravings, the whole affording very ample testimony of his skill and industry.

To the body of the work is prefixed a preliminary discourse, which extends to more than 100 pages, in which M. Serres gives an account of what had been done on the subject by his predecessors, and in which

2 Anatomie comparée du Cerveau dans les quatre Classes des Animaux vertebrés.

he points out the parts of the subject which seem more particularly to require farther investigation. In this dissertation he takes occasion to lay down certain general principles, or laws of "Zoognie," as he terms them, which he conceives regulate the formation of the organs, or according to which the different parts of which they are composed seem to be produced. These laws are two in number, and are denominated the law of symmetry and that of "conjugaison;" the first of these is designated as "le principe du double developpement des organes," the second as "le principe de leur re-union." He adds, "De ces deux lois derive toute la morphologie des organs."[3] In considering the progress of ossification, M. Serres dwells much upon a circumstance, which he supposes has considerable influence in the development of the parts, "la marche excentrique de l'ossification de toutes ses pieces." It is stated as a matter of fact, that if we watch the gradual formation of the bones, we shall perceive that the external parts are first visible, and that the interior and central parts are composed of productions from these. It is in consequence of this excentric progress of ossification, that the double development of the single parts, which compose the centre of the skeleton, is effected; and hence arises the law of symmetry, by which, with a few exceptions, the two sides of the skeleton correspond to each other.

The effect of the law of "conjugaison" is next

[3] P. 25.

examined, and its operation is pointed out in the formation of the various cavities, holes, and canals, which are found in the bones, and which are supposed to be produced by a union of what were originally separate parts, or, as the author expresses it, "de l'ingrenure des pieces primitives dont les os sont composés." By the application of these principles, it is supposed, that what we may consider as the mechanical process by which the solid framework of the body is progressively developed, may be explained, and the relation detected which its component parts bear to each other. Nor is it the osseous matter only which is subject to these laws; their operation is said to be equally apparent in all the structures; they are all developed from the circumference to the centre; their growth always proceeds from the exterior to the interior parts, where the union takes place, and thus forms those central or single organs which are found in so many various situations. It is by this operation that all the apertures and canals are formed. The intestinal canal is said to be "un canal de conjugaison, resultant de la double engrenure, anterieure et posterieure, des ses lames qui les constituent primitivement;" and the same applies to the aorta, the trachea, the larynx, the œsophagus, and in short to all parts that possess the same form. The same principles which are supposed to operate in the formation of the individual parts or organs are conceived to be equally applicable to the explanation of the mode in which the different individual organs are formed with respect to each other. In the great cavities of the body, the head, the chest, and the abdomen, the external muscles, in all cases, first come into existence, and those that are situated more internally progressively make their appearance. With respect to the abdomen itself, M. Serres observes, "L'abdomen présente chez les jeunes embryons, une vaste ouverture; les intestins sont hors de sa cavité; à mesure que les muscles se portent de la circonference vers la centre, ils encaissent ces organes; l'hiatus de la ligne blanche diminue progressivement; enfin, à la naissance, il ne reste plus que l'ouverture de l'ombilic, ouverture formée, comme on voit, de la même maniere que celle du système osseux."[4] M. Serres states, that the nervous system exhibits the same laws in its formation, and that in short they regulate the construction of every part of our frame, so that the details in these volumes may be regarded as a continual application of these experimental laws.

The truth of these laws most obviously depends upon the degree in which they accord with the observations upon the gradual development of the organs, many of which are to be found in the works of preceding anatomists, and of which very numerous examples are contained in these volumes. With respect to the universality of the occurrence, which would be necessary for its establishment as a general principle, I do not profess to be competent to form

[4] P. xxxii, xxxiii.

an opinion; but the author informs us that his doctrine has received the sanction of some of the most eminent anatomists and naturalists in France; he especially notices MM. Cuvier, St. Hilaire, Dutrochet, and Latreille as having confirmed it by their observations on the various classes of animals.

The principles of which I have been giving an account are to be regarded as merely introductory to the proper subject of the work, the comparative anatomy of the nervous system. The details into which the author enters on this subject, and which compose the main bulk of his volumes, are very numerous, and have every appearance of having been prosecuted with great industry and accuracy. Into these details I shall not enter; but I shall notice a few of the general principles which he deduces from them, more especially those that are more immediately connected with any of the topics which have fallen under my consideration in the former parts of my work. It is stated, that the spinal cord is formed before the brain in all classes of animals, a position in which, as we have seen, M. Serres is supported by the observations of Prof. Tiedemann. They also agree in rejecting the opinion of Dr. Gall, that the spinal cord is to be regarded as a series of ganglia, from which the nerves originate; indeed, upon the principle of the excentric formation of the parts, we should suppose that the spine rather proceeds from the nerves than the nerves from the spine, and this indeed is said to be actually the case, so far as respects the order of their formation.[5] M. Serres has found, that in vertebrated animals, the bulk of the spinal cord and the brain are, for the most part, in an inverse ratio to each other, and that, in this respect, the human fœtus resembles the inferior classes of animals. The spinal cord and the corpora quadrigemina, on the contrary, always bear the same ratio to each other, both in animals of different classes, and in the human fœtus. These tubercles are the parts of the encephalon which is the first formed, and it appears that, in all cases, their development bears an exact proportion to the bulk of the optic nerves and the eyes. The cerebellum, in every instance, is produced after the tubercles. In the three first classes of the vertebrated animals, the middle lobe of the cerebellum is developed in the direct proportion of the tubercles, while the hemispheres of the cerebellum are in the inverse proportion of these bodies. In conformity with the general principle, man has the middle lobe, and the tubercles the smallest in proportion to the hemispheres of the cerebellum. The spinal cord follows the ratio of tubercles as to its relation with the cerebellum, while the annular protuberance and the optic thalami follow the direct ratio of the lobes of the cerebellum, and the inverse ratio of the tubercles. In this, as in other analogous cases, the state of the organs in the fœtus approximates to those in the inferior animals.

We have many interesting observations on the

[5] P. xxxviii.

presence or absence of the different parts of the brain in the different classes of animals. Fish have no optic thalami; the corpora striata are wanting in fish, reptiles, and birds. These three classes also have no cerebral ventricles, the lobes of their brain constituting a solid mass; the corpus callosum likewise belongs exclusively to man and the mammalia. The pineal gland is found in all the four classes. The cerebral hemispheres are always developed in the direct ratio of the bulk of the cerebellum, and, consequently, in the inverse ratio of that of the spine and the tubercles. The corpus callosum follows the same proportion as the cerebral hemispheres; it progressively increases in size through the different orders of the mammalia, until it arrives at its greatest bulk in man. One of the most remarkable conclusions to which the observations of M. Serres lead him, is the relation which the nerves bear to the brain; in opposition to the commonly received opinion, he supposes that the nerves do not proceed from the brain to the respective organs, but from the organs to the brain and the spinal cord. But, as I have had occasion to remark in a former part of this work, this statement may rather be considered as a new mode of expression, than as an actual difference in the conception of the object, except so far as it respects the order in which the parts become visible.

The author has drawn up an interesting comparison between the four classes of the vertebrata, as to the degree in which the different portions of the brain and its appendages are developed. In fish the optic thalami are the predominating part, the cerebral hemispheres scarcely exist, the olfactory nerves, or, as he terms them, the olfactory lobes, are very considerable, and the cerebellum is only partially developed. In reptiles, the optic thalami bear a less proportion to the other parts, the cerebellum is nearly annihilated, and the olfactory nerve is much diminished, while the cerebrum is more developed. In birds the cerebellum is the predominating part, and the hemispheres of the cerebrum are increased in size, while the optic thalami are diminished, and the olfactory nerves almost annihilated. In the mammalia the cerebral hemispheres become the predominant organ, the cerebellum is also fully developed, while the tubercles are reduced to their smallest size; the olfactory nerves in this class are subject to great varieties.

We have an important principle deduced from numerous observations, that the development of the particular parts of the brain depends immediately upon the disposition of the arterial system of the animal, the circulation thus being the primary action, upon which the existence of the nervous system would appear to depend; this principle is derived both from the observations of comparative anatomy, and from the state of the organs in monstrous productions. We have a curious remark upon the relation which the faculty of instinct bears to the development of the fifth pair of nerves; in man these nerves are said to be peculiarly small, and in the bee as remarkably enlarged, so as to afford us some ground for the opinion, that instinct differs from the rational faculty as well in its seat, as in the mode of its operation.

These observations are to be regarded as only affording a specimen of the interesting and novel information which is contained in these volumes. It is obvious that they must materially influence many of our physiological speculations, and will tend to illustrate some points which have been hitherto involved in obscurity.

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## *M. Desmoulins on the Nervous System.*[6]

This treatise on the nervous system is forced upon our attention, not only by the high character of M. Desmoulins, but by the still more celebrated name of M. Magendie, who, as we are informed in the title-page, composed the physiological part in conjunction with the author. It is upon this part that the nature of my work will lead me more particularly to dwell. The whole treatise consists of five books, the subjects of which are as follows: 1, introduction to the study of the cerebro-spinal system, consisting principally of anatomical details respecting the form and structure of the bones of the spine and the head; 2, of the cerebro-spinal system in general; 3, of the lateral nervous system; 4, physiology of

[6] Anatomie des Systemes nerveux des Animaux à vertèbres.

the cerebro-spinal system; 5, physiology of the lateral nervous systems. Although the two last are those which must principally occupy us on the present occasion, yet I find an observation, in the second part, which appears to me so just and important, which is applicable to the writings of nearly all the continental anatomists, and so fully coincides with the views that I have always entertained upon the subject, that I shall quote the paragraph at length. "Malgré les subtilités et les dénégations de quelques personnes, ces mots, *origine*, *naissance*, *productions*, impliquent donc dans le langage des auteurs qui s'en servent, l'idée qu'une partie que l'on dit née d'une autre, produite par un autre, est réellement sortie de cette partie qui l'aurait formée, poussée par une acte de vegetation. Cela est évident dans tout l'ouvrage de Tiedemann. Il a réellement pris à la lettre, et au sens propre et non figuré, les mots origine, naissance, production. Tel est aussi le sens qu'y attachent manifestement MM. Gall et Serres. Il est donc démontré pour la premiere fois qu'aucune partie du systeme cérébro-spinal n'est produite, n'est végétativement poussée par une autre, mais que chaque partie est formée à sa place par la pie-mère."[7] To the opinion expressed in this last sentence, however, I must venture to withhold my assent, notwithstanding the very high authority with which it is sanctioned. If we limit our speculations to those actions which

[7] P. 241.

are cognizable by the senses, we must, I conceive, suppose that the capillary arteries are the real agents in these processes, and that the membranes are only so far effective, as they serve for a mechanical basis to which these arteries may be attached.

The following observations appear to me important and interesting. M. Desmoulins remarks, that the hemispheres of the cerebrum and the cerebellum bear no relation of volume with the nerves that are connected with them. But this is not the case with the optic lobes or with the middle lobe of the cerebellum. This last always exhibits a constant relation in bulk to that of the fifth pair of nerves, and the same is the case with the optic nerves and the thalami.[8]

I pass on to the fourth book, the physiology of the cerebro-spinal system. The author observes, that there are three modes of becoming acquainted with the functions of the nervous system, and assigning to each part of it its specific office. The first is that of experiment: by removing successively the several parts of the brain and its appendages, and by observing what effect is produced by these successive removals, we attempt to gain the knowledge of the specific uses, both of the parts that are removed, and of those that are left. The two other modes proceed upon the principles of induction. They consist in duly appreciating the facts which are to be obtained by the study of comparative anatomy

[8] P. 273, 4.

and of pathology. There is scarcely any part of the nervous system which is not wanting in some class of animals, so that by sufficiently multiplying our observations, we have the means of discovering the result of every combination of the cerebral organs, with respect to the powers and functions of the system. The symptoms and phenomena of disease afford us the same kind of inductive evidence, for the operation of the several parts of the nervous system, although seldom in that clear and decided manner as in the former case.

M. Desmoulins remarks, that there are three distinct orders of nervous phenomena; those which produce muscular contraction, that which produces sensation; and those which produce thought. The two first are seated both in the cerebro-spinal system and in the nerves; and in each of these systems every nervous function has its appropriate seat and conductor. The third, which is confined to the cerebro-spinal system, gives rise to a variety of faculties, which "consistent très-probablement dans les localisations." The phenomena of consciousness, being very different from those of feeling and thought, ought probably to be regarded as a fourth power, and it is further suggested, that volition may constitute a fifth distinct nervous function.[9]

The first chapter of the fourth book consists of inductions and experiments respecting the spinal cord. By comparing the size and extent of this

[9] P. 536, 7.

part in the various classes of animals we come to the conclusion, that it bears a certain ratio both to the quantity of muscular contractility which they respectively exercise, either as to the velocity or the force of their contractions, and to the sensibility of the body. The author remarks, that the prolongation of the spinal cord is not in proportion to the size, or even the strength of the tail, but depends upon the variety of actions which this part is enabled to execute, as in the prehensile tails of certain monkeys. The general conclusion is, that the length and thickness of the spinal cord in the mammalia are augmented in proportion to the extent and the delicacy of the sense of touch, as residing in the surface of the body, the power of motion remaining the same in the different cases.[1] The spinal cord of a bird is at least one quarter larger than that of the mammalia, in consequence of the greater muscular effort which is required in the act of flying; while, on the contrary, in fish, where the buoyancy of the element in which they are immersed diminishes the necessity of muscular exertion, the spinal cord is proportionally small. The remarks of M. Desmoulins upon the different powers of the different parts of the cord, as to the transmission of contractile or sensitive impressions respectively, coincide substantially with the statement made in the former part of this work; he, however, is led to conclude that it is principally the external part of

5 P. 540, 1.

the cord which is the great agent in the transmission of both kinds of impressions.

The second chapter is on the specific properties of the lobe of the fourth ventricle, an organ which would appear to perform a most important office in the œconomy of the nervous system; for we are informed, that if we successively remove the whole of the cerebrum, then the optic thalami, and lastly the whole of the cerebellum, so as to leave the insertion of the fifth pair of nerves uninjured, the animal retains the consciousness of all the sensations which have their seat in the face, except those of sight; he is said to manifest the perception of sounds, odours, tastes, and mechanical irritation; he cries out when the organs of the external senses are stimulated; the respiration and the circulation proceed; the muscular motions are no more affected than when the cerebellum alone is removed; and even the power of volition would appear to be not altogether destroyed. But by the division of the spine below this lobe, all the functions are suspended, so as to indicate that this is the "lieu de concours et de reunion de toutes les sensations du corps, moins la vue." And it further appears that the different parts of this lobe have their specific functions; one part being more immediately connected with the sensations of the face, another with the respiration, and another with the digestive organs.

With respect to the specific properties of the cerebellum, M. Desmoulins endeavours to show that there is no foundation for the opinion that has been embraced by Dr. Gall and others, that the development of this part of the nervous system bears a relation to the generative faculty. Nor does he agree with M. Rolando and M. Flourens, that the cerebellum is the great agent in producing or regulating muscular motion, an hypothesis which appears to be disproved by the most direct experiments. The opinion which M. Desmoulins entertains respecting the specific use of this part is, that the mutilation and destruction of the cerebellum "neutralisent une force qui faisait equilibre avec une autre force produisant la tendance à reculer. Ce n'est donc pas le cervelet lui-même qui est le siége de cette derniere force, il parait l'être au contraire d'une force impulsion en avant, comme nous le verrons plus tard."[2] Certain experiments are then referred to, which were performed by M. Magendie, and which consisted in dividing one of the pedicles of the cerebellum, the effect of which is stated to have been a rapid rotatory motion of the animal on its axis, which continues incessantly for a considerable length of time, and is only prevented by a mechanical obstacle. The conclusion that the author draws from the experiments is "que deux forces antagonistes circulent par les deux demi-cercles latéraux que forment le cervelet et sa commissure."[3] A no less remarkable effect is stated as being the result of an injury of one of the optic thalami; this, it is said, "entraine irresistiblement l'animal dans une

[2] P. 586, 7. [3] P. 589.

course ou dans un vol circulaire ou de manege, sur le côté dont on a blessé le lobe;" and, what appears perhaps still more singular, we are informed, that frogs and serpents "tournent sur le côté opposé au lobe blessé."[4]

M. Desmoulins introduces his observations on the use of the cerebral lobes by remarking, that the volume of the brain is no measure of the intellect, and that the internal contour of the cranium is frequently not parallel to the external surface, so that we cannot ascertain the relative size of the different parts of these lobes by an examination of the skull. There is, however, a mechanical structure which appears to bear a regular ratio to the perfection of the intellectual faculties; "Ce mechanisme resulte du plissement de la membrane des hémisphères du cerveau."[5] M. Magendie is stated to have been the first to suggest the idea, that there is a connexion between the number of these convolutions and the state of the intellectual faculties. This position is supported by various facts in comparative anatomy, by the comparative state of the fœtal and adult brain in the same kind of animal, as well as by the brains of idiots. Hence is deduced the general principle, that the number and perfection of the intellectual faculties, both in a series of species and in the individuals of the same species, are in proportion to the extent of the cerebral surfaces. This position, it will be remarked, is conformable to the

[4] P. 593. [5] P. 599.

opinion stated above, that the specific function of the spinal cord is seated in its surface.

M. Desmoulins reverts to the hypothesis of Drs. Gall and Spurzheim, and asks whether there be any evidence that particular faculties have their seat in particular parts of the brain; he admits that the doctrine is plausible, but he thinks that the arguments brought forward by these anatomists are inconclusive, because they are derived only from the external form of the cranium; he conceives that it is by the examination of the brain, after the partial or total loss of certain faculties, that we are to gain our information on this point.

We have some singular varieties of muscular motions produced by the mutilation or removal of certain parts of the cerebrum. "Se l'on retranche à un mammifere la voute de l'hemisphere cerebral et le corps strié; aussitôt l'animal s'élance droit en avant et court sans se detourner jusqu'à ce qu'il choque un obstacle."[6] This peculiar motion is said to depend upon the destrnction of the medullary matter, while the destruction of the cineritious part has no immediate effect upon the motions of the animal, but appears to destroy its volition and intelligence.

The fifth book, which treats of the nerves, like the former part of the work, abounds in novel and ingenious opinions; but as this is the portion of it which more particularly consists in physiological details, so we may conceive that we are more im-

[6] P. 625.

mediately indebted for them to M. Magendie, and that they are more especially sanctioned by his authority. We accordingly find many of the doctrines maintained in these chapters which I have already had occasion to notice as occurring in his writings; nor do I perceive any point of considerable importance in which the opinions of M. Desmoulins differ essentially from those of his colleague. For example, in the account of smell, it is stated that the branches of the fifth pair are the only, or at least, the principal nerves of this sense. In the chapter on vision we are informed that no alteration of the eye takes place when it views near objects, and that the supposed adjustment is altogether unnecessary. Upon this opinion I may remark, that one of the principal arguments upon which it is founded, is derived from the comparative anatomy of the cetacea, who see equally well in air and in water, but whose eyes possess a structure that does not admit of a change of figure.[7] But to this argument it may be replied, that the adjustment of the eye may depend upon an alteration in the form or position of the crystalline, independent of any change in the external figure of the organ. The opinion of the author respecting vision at different distances is, that the size alone of the object varies, and that the image is equally distinct at all distances within the natural range of vision. The observation which was

[7] P. 650.

made by M. Magendie respecting the insensibility of the optic nerves to mechanical irritation, is extended by M. Desmoulins to the three pairs of nerves which are connected with the muscles of the eye; at the same time the filaments of the fifth pair that are sent to the eye are exquisitely sensitive. An analogous observation is made in the sixth chapter with respect to the acoustic nerve. The chapter on the properties of the fifth pair of nerves contains nearly the same opinions respecting them which I have noticed above as being supported by M. Magendie; that they are the immediate organ of all the senses except the sight, and that they are accessary even to this sense, because vision is instantly destroyed by their division.

As containing a great variety of minute anatomical details, it is impossible to speak too highly of the value of these works. Nor are they without considerable value as physiological treatises. Yet I think it is not unfair to remark, that the inferences which are drawn from the facts are not always the direct deductions from them, and that when we venture to assume indirect conclusions in investigating the laws of tbe animal œconomy, we are always proceeding on dangerous ground. The general principles, or laws, as they are termed, especially those which are laid down by M. Flourens and M. Serres, I cannot but regard as mere speculations, which we might look upon as innocent recreations of the imagination, were it not to be apprehended that, in some cases, they had biassed the judgment of the observer, an infirmity from which the strongest minds are not exempt. Hence we cannot prize too highly those observations that are made, as far as we can judge, independent of any pre-conceived hypothesis.

Of this description, we may presume, are those of Mr. Mayo, which serve as the basis of his late publication on the structure of the nervous system.[8] The structure was investigated "by hardening the substance in alcohol, and observing in what manner the coagulated substance tears," according to the plan of Reil. The plates are expressive, and we cannot doubt of their accuracy. The second figure of the seventh plate is peculiarly interesting, as exhibiting the actual semi-decussation of the optic nerves, and thus confirming the hypothesis of Dr. Wollaston. Dr. Hooper's "Morbid Anatomy of the Human Brain," although only indirectly connected with its physiology, may be properly noticed in this place, as containing a number of remarkably well-executed representations of this organ, in its various morbid conditions.

In the Phil. Trans. for 1824, we have an account of some new microscopical observations on the brain by Mr. Bauer. They principally concern the comparative anatomy of this organ; the only important fact respecting the brain generally is, that when a

[8] Series of engravings intended to illustrate the structure of the brain, &c.

portion of it, in the recent state, consisting of both the cortical and medullary matter, is viewed by a high magnifier, the rows of globules pass, without any interruption or change of direction, from one part to the other.[9]

Among the additions that have been made to this branch of anatomy, it is necessary to mention the portion of Dr. Cragie's "Elements" which refer to the brain, and especially those pages, from 301 . . 376, where he gives a minute account of this organ, and of the names which have been given to its various parts by the latest anatomists.

9 Phil. Trans. for 1824, p. 3. pl. 1. fig. 3.

## APPENDIX TO CHAPTER V.

### *Dr. Alderson's Remarks on the Beating of the Heart.*

Dr. Alderson[1] has raised an objection against the mode by which William Hunter endeavoured to explain the cause of the beating of the heart. This he supposed to depend, not, as had been generally imagined before his time, upon the distension of the ventricles, but upon their forcibly projecting the blood into the aorta, a curved flexible tube, to the end of which the heart is appended, and somewhat loosely attached to the contiguous parts, the sudden injection tending to straighten the tube, and thus raise up the point of the heart, so as to strike the ribs. To this opinion Dr. Alderson objects on two grounds: first, that in consequence of the direction of the arteries with respect to the ventricle, the effect produced by the sudden injection of the curve of the aorta will be merely to lengthen the ventricle in the direction of its axis; and, secondly, he speaks of it as an obvious fact, "that the impulse of the heart is only felt at the moment of the systole of the ventricles, and hence the heart must have commenced its motion towards the parietes of the chest previously to the blood arriving at the arch of the aorta."

[1] Quarterly Journal, v. xviii. p. 223, et seq.

With great deference to the opinion of Dr. Alderson, I feel disposed to adopt a different conclusion respecting the first point on which he objects to Hunter's doctrine. I conceive, that if a curved elastic tube, that is fixed at one end, and hanging loose at the other, be suddenly injected, the injection will tend to elevate the loose end, whatever may be the direction of the curve with respect to its orifice. As to the second ground of objection, as Dr. Alderson assigns no reason for the opinion which he advances, except an indirect appeal to Laennec, I may, without impropriety, assert, that the beating is not felt at the instant when the ventricle begins to contract, but when the contraction has produced its effect in filling the arch of the aorta. That this is the case, may be distinctly seen by viewing the action of the heart of a frog, where, when the motion is not too rapid, we can watch the whole process, and observe the effect of each part of it. And I may remark, in concluding, that even were we to assent to Dr. Alderson's position, that the motion of the heart originates in the uncounteracted force on the side of the ventricle opposite to the orifice of the artery, still the effect of this force manifests itself in its action on the arch of the aorta, the change of figure of which is the actual cause of the propulsion of the heart against the ribs.

## *Dr. Goodwyn's Remarks on the Derivation of the Blood.*

In vol. i. p. 409, I offered some remarks upon the opinions which different physiologists had entertained respecting the doctrine of the derivation of the blood, when I stated that it is obscurely mentioned by Haller, but "was first brought forwards in a clear and definite manner by Dr. Wilson." On this subject I have received a communication from Dr. Goodwyn, and I am confident that I cannot render my readers a more acceptable service than by laying it before them in the words of the writer.

"With respect to the older physiologists, I think it may be allowed, that Bartholine had a notion of derivation in the diastole, although he did not employ the precise word. 'Diastole,' he says, 'motus accidentarius, est dilatatio cordis; ut *hauriatur* sanguis, per venam cavam, in dextrum ventriculum, et per arteriam venosam in sinistrum;' and although he afterwards says of the diastole, 'passio potius est quam actio,' yet that does not destroy the effect of his first assertion. See Anatomia Bartholiniana, 8vo. edit. p. 371.

"And, with respect to the moderns, I think there is one author anterior to Dr. Wilson, who brought forward in a clear and definite manner an assertion, supported by facts, that the diastole of the heart contributes mainly to carry on the circulation of the blood, by exerting the power of suction, and thus drawing into its cavity the blood from the trunks of the veins. I have said, this doctrine was brought forward by an author, which you may probably think not correct, as it was only in an inaugural dissertation; the writer was J. T. Vanderkemp; the title was 'De Vita et Vivificatione Materiæ Corpus humanum constituentis;' printed at Edinburgh, anno 1782. In page 52, his words are, 'Docet autem per opportune, physiologica ratio, sanguinem per arterias, alterna cordis arteriarumque contractione et *dilatatione* permoveri.' Upon this he has the following note: 'Mirum videri poterit, me hoc loco, cordis et vasorum sanguinem vehentium diastolem, inter causas sanguinem moventes retulisse, quum ex vulgari doctrina, cor suæ dilatationi reluctans, aut ad summum passive se habens,[2] ab impulso sanguine explicari, et distendi credatur. Sed certe ubi cor ranæ vel anguillæ ex corpore exscissum, post singulas contractiones, nullo sanguine distendente, plena se diastole restituens video, ubi mecum repeto, omnem fibram muscularem, remoto post contractionem stimulo, propria virtute in eundem statum se recipere, in quo ante contractionem versabatur, ubi porro in cadavere etiam flaccidum cor auriculasque vitali turgescentia destitutas, pendentesque, vel sic tamen a perfecta diastole propius abesse animadverto; ubi tandem ad explicandum sanguinis venosi motum progressivum vix sufficere sentio, anteriosi sanguinis a tergo urgentis vim insitam; diffiteri

[2] Sic fere Hallerus, El. Phys. t. i. p. 398.

non possum, in hanc me fere adduci sententiam, dilatatum vi suæ fabricæ, cor adtrahere sanguinem æquabili vi illi, qua eundem expellit in systole, et diastoles causam non esse repletionem cordis, sed contra diastolen repletionis, quemadmodum in respiratione, thoracis expansio, irruentis aëris causa est, non effectus."

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## *Dr. Barry's Experiments on the Beating of the Heart.*

The question concerning the cause of the beating of the heart has formed the subject of a series of experiments by Dr. Barry.[1] He opened the thorax of an animal during life, and, by introducing his hand into the cavity, he endeavoured to ascertain the actual condition of the heart and great vessels, as to their state of distension, and their relative position. He performed seven experiments of this kind, and concluded from them that the vena cava is considerably increased in size during inspiration, which he ascribes to the partial vacuum which is then formed in the chest. The force which the venous blood exerts in entering the heart, in consequence of the expansion of the chest and the great vessels behind the heart, is supposed to push this organ forwards, and thus to cause it to strike against the ribs. The expansion of the chest thus attracts the blood, and causes it to fill the great veins, in order to occupy the partial vacuum which would otherwise be produced.

[1] Ann. des Scien. Nat. t. xi. p. 113, et seq.

## APPENDIX TO CHAPTER VI.

### *Dr. Hodgkin on the Red Particles of the Blood.*

I HAVE had occasion, in various parts of this work, to give an account of the microscopical observations that have been made upon different animal substances: and in consequence of their disagreement with each other, I have been induced to remark upon the little confidence which, in most cases, can be placed in them. My animadversions, however, have always proceeded upon general grounds; at the same time that I may be supposed to under-rate the value of the observations, I acknowledge the perseverance and ingenuity of those physiologists, who have devoted their time and attention to this object, and ascribe their failure to the inherent deficiencies of the instrument. My ideas on this subject have, I confess, not been changed by recent occurrences.

In the former part of this Appendix I have given a brief detail of the microscopical observations of Dr. M. Edwards and M. Dutrochet. These, more especially the former, are written in a clear, plain, and unassuming style, while the appearances which he describes are so simple and intelligible, that the reader is scarcely disposed to entertain the possibility of a doubt of their perfect accuracy. They are, however, called in question by Dr. Hodgkin, who has been employing the microscope in the examination of various animal substances, and has given an abstract of his observations.[1] Dr. Hodgkin informs us, that he employed a microscope which, upon comparison, was found equal to the celebrated one lately brought to this country by Prof. Amici. The first object to which he directed his attention was the globules of the blood, which, notwithstanding the older observations of Leeuwenhoek, Haller, and Fontana, and the more recent ones of Mr. Bauer and the French physiologists, are said to be not spherical, and not to consist of a central nucleus, enclosed in a vesicle. The description given of these bodies is that "the particles of the human blood appear to consist of circular flattened transparent cakes, which, when seen singly, appear to be nearly or quite colourless. Their edges are rounded, and being the thickest part, occasion a depression in the middle, which exists on both surfaces."[2] We are told, however, that this convexity is not considerable, and in some of the particles is not to be observed. The estimate of the diameter of the particles is $\frac{1}{3000}$ of an inch; the thickness of the particles is about $\frac{1}{45}$ of their diameter. With respect to the particles of other animals, the observations of Dr. Hodgkin agree with those of MM. Prevost and Dumas, as to their "having a circular form in the mammalia, and an elliptical one in the other

[1] Philos. Magazine and Ann. Phil. v. ii. p. 130, et seq.

[2] P. 131, 2.

three classes." [3] The diameter and the thickness do not bear the same ratio to each other in the different species. The elliptical particles are invariably larger than the circular, but are proportionally thinner; the particles are more numerous in birds, but smaller than in either reptiles or fishes. The central globules, which are described by Mr. Bauer and others as so obvious, and which are supposed by Dr. M. Edwards to perform so important a part in the construction of the body, are not recognized by Dr. Hodgkin, and it would appear that the whole system of central nuclei and vesicular envelops is equally unfounded. Sir Ev. Home's hypothesis, that the particles are not disposed to coalesce in their entire state, appears also to be incorrect; Dr. Hodgkin found them disposed to combine in this state only. This is best seen when the blood is viewed between two slips of glass; and under these circumstances the following appearances may be observed:—"When the blood of man, or of any other animal having circular particles, is examined in this manner, considerable agitation is at first seen to take place among the particles; but as this subsides they apply themselves to each other by their broad surfaces, and form piles or rouleaux, which are sometimes of considerable length. These rouleaux often again combine amongst themselves, the end of one being attached to the side of another, producing at times very curious ramifications." [4]

[3] P. 133. [4] P. 135.

The author's observations upon the intimate structure of the animal textures are equally at variance with those of his predecessors. The globular structure which had been more or less developed by Hooke and Leeuwenhoek, had been confirmed by their successors, and so very clearly and fully made out by Dr. Edwards, we are now informed is all deceptive, and we revert to the doctrine of striæ and fibres, and to these are referred the most minute parts of the muscles, nerves, and cellular membranes, that can be detected by the employment of Dr. Hodgkin's powerful microscope. From this abstract of Dr. Hodgkin's paper, it will appear, that from the evidence which is now before us, it becomes a question altogether of personal authority. It does not depend upon the respective goodness of the instruments employed, because the different observers describe what they saw as being perfectly distinct and obvious, and not presenting that confused or uncertain aspect, which arises from a deficiency in the power of the lens. We are compelled to suppose that all the observers, excepting one, have fallen into some error, either depending upon an optical deception, or resulting from some unconscious and involuntary bias of the mind towards a previous hypothesis. Indeed, the more beautiful is the speculation, and the more completely do the observations chime in with all its parts, the more are they to be suspected. In conclusion, I may remark, that much as the naturalist has been indebted to the microscope, by bringing into view many beings, of which he could not otherwise have ascertained the existence, the physiologist has not yet derived much benefit from the instrument. Except the simple fact of the existence of the globules of the blood and of some of the animal fluids which are derived from it, and of the spermatic animalcules, I am not aware that we owe it any farther obligations, and in spite of all the boasted improvements of modern times, I do not very clearly perceive that we have yet advanced beyond Leeuwenhoek and Hooke, in our power of discriminating minute objects, the former of whom used the simple, and the latter the compound form of the instrument.[5] Professing myself to be deeply interested in the investigation of truth, I rejoice that Dr. Hodgkin has turned his attention to this subject, being well acquainted with his candour and perseverance.

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## *On the Colouring Matter of the Blood.*

In Jameson's Journ. for Oct. 1826, p. 314..7, we have an account of some experiments by Engelhart on the red colouring principle of the blood.

In order to obtain the colouring matter in a state of purity, he diluted the mixture of red particles and serum with fifty parts of water, and exposed it

5 Priestley on Light, p. 218..0; Preface to Micrographia Restaurata.

to a temperature of 150°. In this case the serum does not coagulate while the red globules do so; they separate in the form of greyish black flocculi, and may be removed by filtration. The author found that they contained iron, while the fibre and the serum, treated in the same manner, did not indicate its presence; the quantity of iron nearly agrees with Berzelius's estimate. In the Ann. de Chim. et Phys. t. xxxiv. p. 268, et seq., we have a paper by Rose, confirming the statements of Engelhart, and mentioning some curious circumstances, respecting the influence which certain organic matters possess, of preventing the precipitation of the iron by the usual re-agents.

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## *Dr. Stoker's Observations on the Blood.*

In my account of the morbid changes which the blood is occasionally found to exhibit, I did not allude to the experiments of Dr. Stoker, of Dublin, as I considered those which he had already published as only a prelude to a more complete series which were then in progress. It is to be presumed, however, that his professional avocations have prevented him from putting his intentions in practice, as I believe we have no account of any experiments later than those contained in his "Pathological Observations," which were published in 1823. He had previously endeavoured to show "that the chief use of the liver is to bring the hydrocarbonous principles with which the blood in the vena portæ is charged more nearly to the state of fat, thereby rendering the blood better prepared for the changes it has to undergo in the lungs, as well as to provide for the due supply of fat in different parts of the body, . . . ." To illustrate the operation of the liver, he compared the blood from the vena cava with that from the vena portæ, when it was supposed that the former exhibited some indications of the presence of unctuous matter, which were not observed in the latter; Trans. of the Assoc. Phys. v. i. p. 163, 4. The principal object of Dr. Stoker's experiments, related in his Pathological Observations, is to discover the cause of the formation of the buffy coat of the blood, and to point out in what state of the fluid this peculiar appearance is disposed to manifest itself. He conceives that it has no connexion with slow coagulation, and this appears to be the fair inference from the examination of twenty-seven specimens of the blood in various inflammatory affections, from which we find that the formation of the buffy coat has no relation to the length of time which the blood requires for coagulation. The conclusion which the author draws is, that the formation of the buffy coat does not depend upon any purely mechanical cause, but upon a diseased state of the blood, which is referred more especially to a changed or imperfect chylification. See Pathol. Obs. Introduction; also p. 37 . . 42, et alibi.

## *Mr. Thackrah's Observations on the Blood.*

In giving an account of the blood in my first volume, I unfortunately omitted to mention Mr. Thackrah's "Inquiry," a treatise which contains a full account of the opinions entertained by contemporary physiologists on the nature and properties of the blood, together with a number of original experiments on this substance, both in its healthy and its morbid state. The most important of the original observations are those on the coagulation of the fibrin: from these he draws the conclusion, that "blood coagulates slowly, in regular proportion to the tonic state, or that condition of the system in which the vital powers are strongest;" p. 47. The fifth chapter is "on the cause of the blood's coagulation," and contains an account of a variety of experiments which were instituted for the purpose of examining the effect of those agents which are commonly supposed to influence this process. The sixth chapter is on "the changes produced by disease," and is likewise, in a great measure, experimental. It would not be easy in a short compass to give an analysis of a treatise which is principally composed of detached facts; but I may remark, that the experiments are related with sufficient distinctness, and the inferences that are drawn appear to be, for the most part, fairly deduced from the premises.

## *Dr. Scudamore's Observations on the Blood.*

We are indebted to Dr. Scudamore for "An Essay on the Blood," which is principally occupied with an account of various observations and experiments which he performed, for the purpose of illustrating the circumstances which influence its coagulation. The most important points to which his attention was directed respect the effects of temperature, exposure to the air, the question whether carbonic acid is evolved from blood during its coagulation, the connexion between the rapidity of its coagulation and its specific gravity, the manner in which it flows from the vessels, the form of the cup into which it is received, the effect of vitality, of electricity, and of various chemical agents, he inquires whether heat be evolved during coagulation, and he offers some remarks on the formation of the buffy coat. The most important of the results appear to be the following: his experiments favour the opinion that carbonic acid is disengaged during the coagulation of the blood; blood which has the highest specific gravity coagulates the most rapidly; coagulation is promoted by the blood being drawn slowly from the vessel, and by being received into small shallow cups, probably in consequence of its heat being in this case abstracted more rapidly. When blood exhibited the buffy coat it coagulated more slowly; when it is extravasated, or remains in the vessels after they have lost their vitality, it coagulates very slowly; electricity appears to promote coagulation; heat is disengaged during coagulation, although in small quantity only; it was found that the quantity of fibrin was considerably increased in blood that exhibited the buffy coat, and that the proportion of fibrin was much greater near the surface of the clot than at its lower part.

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## *Observations on certain Morbid States of the Blood.*

Dr. Bright, having been engaged, during the course of the last spring, in investigating the pathology of that variety of dropsy in which the urine coagulates by heat, requested me to ascertain the chemical change which the urine and the blood experience in this disease. I accordingly examined a number of specimens of this kind of dropsical urine: the result was, that it contains a considerable portion of a substance which resembles albumen in all its essential properties, while there is, at the same time, a great diminution in the ordinary quantity of the urea. In the serum of these patients the proportion of albumen was found to be less than in health, while the quantity of animal matter in the serosity was much increased. When this animal matter was separated from the salts, with which it is naturally combined, it appeared, in many respects, to resemble urea, and, as I conceive, was similar to the substance which was procured by MM. Prevost and Dumas from the blood, after the extirpation of the kidney, and probably to that, which, according to the experiments of the Tubingen chemists, enters into the composition of many parts of the body.[6] I was not able to ascertain, to my entire satisfaction, whether the change in the serosity consisted only in an increased quantity of its animal ingredient, or whether the properties of the substance were altered. I feel, however, pretty confident that the latter is the case, that in its natural state it is very similar to what has been termed osmazome, and that, in the disease referred to above, it approximates to the nature of urea.

It does not fall within my province to give an account of Dr. Bright's work;[7] but I may offer my testimony to the correctness of the engravings with which it is accompanied, as I had an opportunity of comparing them with the objects which they are intended to represent: with respect to their execution, they are of unrivalled excellence.

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## *M. Chevreul's Observations on the Blood.*

M. Chevreul informs us,[8] that if the fibrin of the blood be treated with alcohol or ether, a peculiar fatty

[6] See the second volume of this work, p. 376, 379.

[7] Reports of Medical Cases.

[8] Ann. du Mus. t. x. p. 443.

substance is procured, which he conceives to be similar to the matter of the brain. Arguing from this fact, he supposes it probable, that all the constituents of the body may be found in the blood, and that secretion consists merely in their separation from the fluid. The mode in which the experiment is related is perhaps scarcely minute enough to enable us to judge accurately respecting the fact, and, still less, the inference which is deduced from it.

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## *Dr. Davy's Experiments on the Blood.*

Dr. Davy has lately performed a series of experiments on various points connected with the chemical and physical properties of the blood. He conceives that pure fibrin is heavier than serum, but that the mixture of fibrin and serum, which constitutes the buffy coat, is lighter than the mixture of fibrin, serum, and red particles, and therefore floats on the surface. He conceives that in inflamed blood, the serum and coagulable lymph are less viscid than ordinary. The specific gravity of the red particles he found to be 1.087. The formation of the buffy coat does not appear to bear any exact relation to the specific gravity of the blood; in acute diseases, the blood, whether buffed or not, is generally of greater specific gravity than ordinary; in diseases of debility, the reverse. The formation of the buffy coat is supposed to depend on the viscidity of the blood, as connected with the proportion of water, or the complete mixture of its ingredients. He maintains, as the deduction from experiment, that heat is not extricated during the coagulation either of fibrin or of serum.[9] In a subsequent paper, on the coagulation of the blood,[1] Dr. Davy points out an error in my first volume, p. 438, where I assert, that strong agitation prevents the coagulation of the fibrin; he conceives that it actually assumes the solid form, but that, being in small quantity only, it is diffused through the fluid, and thus escapes observation. He controverts some of the positions of Dr. Scudamore detailed above, respecting the circumstances under which coagulation takes place, and the effect of various re-agents on the operation.

9 Ed. Med. Journ. v. xxix. p. 244, et seq.

1 V. xxx. p. 248, et seq.

## APPENDIX TO CHAPTER VII.

### *Dr. Williams's Observations on Respiration.*

In the 2d vol. of the Edin. Med. Chir. Trans. p. 92, et seq., is an interesting paper by Dr. C. J. B. Williams, containing experiments and remarks on the functions of respiration, and of animal temperature. In the first part of the paper, the author considers the merits of the two hypotheses that have been proposed, to account for the changes produced upon the blood by respiration, whether it consists in the absorption of oxygen and the discharge of carbonic acid, or simply in the discharge of carbon, which is converted into carbonic acid after it leaves the blood. Dr. Williams supports the former hypothesis by two experiments, which appear well calculated for this purpose, and by various considerations of a physical and chemical nature.

In the second part of his paper he discusses the theory of animal temperature, and he particularly inquires, how far the chemical change produced in the blood and the air by respiration can account for the evolution of heat. He conceives that it will explain a part of the effect, but not the whole of it; and this additional heat he supposes is disengaged, in conformity with a general law of chemical action, that caloric is liberated in all cases where a compound body is resolved into one that is more simple. This he applies to the case under consideration, by assuming that the secretions and excretions are more simple, i. e. "consist of fewer atoms than the albumen and fibrin of the blood from which they are formed;" p. 105. I think there may be some reason to doubt, how far the doctrine will apply in this instance; but we have a series of very valuable experiments in the latter part of the paper, for the purpose of investigating the correctness of Mr. Brodie's statement, that after the cessation of life, artificial respiration has the effect of lowering the temperature of the body. Dr. Williams performed his experiments upon rabbits and upon birds, and seems to have paid the most minute attention to the resulting phenomena, and fairly deduces from them the general fact, that artificial respiration, when properly conducted, prevents the abstraction of heat, which would otherwise take place, while, in some cases, the temperature was observed to be sensibly increased by this process. His general conclusion is as follows (p. 116): "From a consideration of all the facts which I have now stated, I am led to believe that animal heat is the result of chemical changes proceeding in the body, and which I have endeavoured to particularize as those resulting from the functions of respiration and secretion, and that a due performance of these functions is requisite for the healthy and uniform preservation of animal temperature."

## *Dr. Philips's Observations on the Lungs.*

Dr. Philip has lately presented to the Royal Society "some observations on the effects of dividing the nerves of the lungs, and subjecting the latter to the influence of voltaic electricity." He remarks, that in his former experiments, on the effects produced by dividing the nerves of the stomach, although its secreting surface was so far deranged as to destroy or impair its functions, yet that the visible structure of the organ was little changed. The case is, however, very different when the nerves of the lungs are divided. Both the appearance of the lungs and their organization seem to be entirely altered; in a considerable portion of them, "every trace of both tubes and cells is obliterated," while both in colour and consistence they assume "much of the appearance of liver." The attempts that were made to inject them, when reduced to this state, either by mercury or by other substances, were altogether ineffectual, as it appeared, in consequence of the air tubes and vesicles being, in many parts, completely impervious to the finest injections.

Dr. Philip informs us, that if, after the division of the nerves, the lungs be subjected to the action of galvanism, by transmitting this agent through the remaining portions of the nerves, no affection of the breathing supervenes, and the lungs, after death, are found quite healthy, unless the electricity has been applied of such power, or continued for such a length of time, as to excite inflammation.

I apprehend, there can be no doubt of the fact, as stated by Dr. Philip, and it seems highly probable, that his pathological deductions will be found to be of considerable importance in the practice of medicine. But I am not prepared to assent to his physiological conclusion, "that the effect of dividing the nerves of a vital organ, and separating the divided ends, is not merely that of deranging its secreting power, but all those powers on which its healthy structure depends; and that the effect of Voltaic electricity is that of preserving all these powers." What may be hereafter discovered with respect to other structures and functions, I will not presume to conjecture; but I conceive, that all the effects which are described in the present instance may be referred to a derangement of the secreting surface of the pulmonary vesicles. The actual effect which we may suppose takes place under these circumstances, is an increased, and probably an altered, secretion from the mucous membrane; connected with this, in some way, there would appear to be a diminution or cessation of the absorbing process; hence the tubes and vesicles become mechanically obstructed; the air is incapable of reaching its appropriate receptacles; while, partly from this cause, and partly from the mechanical obstruction, the blood is not able to pass along the minute capillaries, in which it becomes arterialized. I may remark upon these experiments, that the effect of dividing the nerves is not to destroy, or even to diminish, the secretion of the part, but to alter its nature, its absolute quantity being probably augmented.

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## *MM. Breschet and Milne Edwards on Pulmonary Exhalation.*

I have been favoured by Dr. M. Edwards with an account of some experiments, which he performed in conjunction with M. Breschet, on pulmonary exhalation. They opened the thorax of a dog, practised artificial respiration, and injected tinct. of camphor into the cavity of the peritoneum, but no odour of camphor could be perceived in the expired air. When, on the contrary, the tinct. of camphor was injected into the cavity, and the artificial respiration not employed, the odour was very perceptible. The conclusion which they draw is, that the suction, which accompanies the act of inspiration, causes any volatile substance which may be in the circulation, to be exhaled from the lungs.

## APPENDIX TO CHAPTER X.

### *Prof. Tiedemann and Gmelin's Experiments on Digestion.*

During the course of the last and present year, a very valuable experimental treatise on digestion has been published by the Professors Tiedemann and Gmelin.[1] We learn from the preface, that in the year 1823 the Royal Academy of Paris proposed this function as the subject of a prize dissertation. The attention of the writers was to be particularly directed to the chemical changes which the food experiences in the different parts of the alimentary canal, and the examination was to be extended to the four classes of the vertebrated animals. Two of the essays which claimed the prize,—the one now under consideration being the joint production of the Heidelberg professors, the other likewise the joint production of MM. Leuret and Lassaigne,—were selected by the Academy as entitled to honourable mention, but neither of them were considered worthy of the proposed distinction. The commissioners appointed to decide upon the respective merits of the two works, were among the most distinguished physiologists of France, who were not only perfectly competent to make the award, but placed above any

[1] Recherches Experimentales sur la Digestion, traduites par Jourdan.

suspicion of undue bias. It is not very obvious on what principle the decision of the commissioners was determined, whether by a consideration of the positive or the comparative merits of the rival productions. Whether it was that they considered that the works possessed claims so nearly equal, that it would have been unfair to have selected either of them, or whether they were influenced by the circumstance, that in some cases, where the same observations and experiments were made by both the parties, they were not attended with the same results. As the essays are now both of them before the public, every one is at liberty to judge of their claims, and to form his opinion respecting the justice of the decision. It is certain that the work of MM. Tiedemann and Gmelin does not completely answer all the inquiries that are connected with the subject. There are perhaps some points of considerable importance which they have left unexamined, and there may be even certain cases, in which they have failed to draw the correct inference from their experiments; but I feel no hesitation in saying, that their researches are of superior value, that the volumes contain much original matter, that the authors must have bestowed upon them much time and labour, and that many works of inferior merit have received the rewards held out by the Academy. The question, however, respecting the conduct of the Academy is one of comparatively little importance; we are much more concerned with the merits of the two works; and I shall endeavour to select a few of those points which are the most interesting from their novelty, or the most important from the degree in which they illustrate the operations of the functions of the digestive organs.

A considerable part of the work of the professors is necessarily occupied with chemical details; these are, in all cases, detailed with extreme, perhaps with unnecessary, minuteness; there are many experiments given at length, where no conclusions are deduced from them, and where we have nothing but mere matters of fact, without any obvious or assignable application. Indeed, to such an extent is the minuteness carried, that the very perusal of the experiments is itself a task of no small labour, and requires an intimacy with animal chemistry which few individuals can be supposed to possess. There is, however, nothing in the style of the work which indicates a spirit of display or ostentation; there seems to be simply a desire to communicate to the public all the information which the authors themselves possessed.

The work is divided into four memoirs, in which the function of digestion is examined in each of the four classes of the vertebrata: the first, which gives an account of the mammalia, is of course by much the longest, occupying the whole of the first volume and a third part of the second. The first memoir is divided into five sections, in which the following topics are successively treated: 1. Chemical analysis of the saliva, the pancreatic juice, and the bile; 2. Experiments on the state of the organs of digestion in animals while fasting; 3. Experiments on the changes which the aliment undergoes during digestion; 4. Experiments respecting the effect which the ligature of the ductus choledochus produces on digestion; 5. Results respecting lymph and chyle.

The experiments on saliva appear to have been made with great accuracy; they were performed partly on the fluid procured from the salivary ducts of certain animals, and partly on the more mixed substances from the mouth in the human subject. The conclusions which the authors deduce from their experiments are curious and important, and some of the results very unexpected. The solid contents of saliva are stated to vary from one to twenty-five per cent.; there are three proximate animal principles which appear to be essential, proper salivary matter, mucus, and osmazome; to which, in some cases, is added a little albumen, and a little fatty matter containing phosphorus. The salts are numerous, being no less than nine; six soluble in water and three insoluble. The soluble salts are an alkaline acetate, carbonate, phosphate, sulphate, muriate, and sulpho-cyanate; it is to the second of these that the saliva owes its alkaline properties, and which had been generally ascribed to the presence of an uncombined alkali. The author announces the curious circumstance, that the alkali which exists in these various salts is, in man, almost solely potash, while in the dog and the sheep it is soda, with very little potash. The presence of the sulpho-cyanic acid is likewise a very curious circumstance, but of the reality of which the professors entertain no doubt; it is the most abundant in the human saliva, and is scarcely perceptible in that of the dog. The insoluble salts are the phosphate of lime, the carbonate of lime, and magnesia, in very minute quantity.[2]

The composition of the pancreatic juice is more particularly interesting, as taken in connexion with that of the saliva, to which it has been commonly supposed to be nearly identical. We learn, however, from the experiments of MM. Tiedemann and Gmelin that this opinion is incorrect; they inform us that these secretions differ in the following respects: 1. The amount of the solid contents in the saliva is only half as much as that in the pancreatic juice; 2. The saliva contains mucus and proper salivary matter, with perhaps very minute quantities of albumen and caseous matter, whereas in the pancreatic juice the proportions of these principles is reversed, the two latter existing in great abundance, and the two former in very minute quantity; 3. The pancreatic juice is either neutral, or contains a little alkaline carbonate; 4. There was no sulpho-cyanate in the pancreatic juice of the sheep, although it is found in the saliva of this animal; it does not appear that the human pancreatic juice was examined.[3]

The experiments on the bile of the ox and of the dog are numerous and minute, and there are a few

[2] T. i. p. 23, 4.

[3] T. i. p. 41, 2.

on human bile. The most elaborate analyses of bile are those of Thenard and Berzelius; but these unfortunately differ essentially from each other. The experiments of Dr. Prout are said to coincide with those of Berzelius in the more essential points, while, on the contrary, Chevreul, Chevalier, and Lassaigne, approach more nearly to the results of Thenard. The complicated nature of this fluid necessarily gives rise to a proportionably operose and complicated mode of analysis, and our authors appear to have paid the most minute attention to this part of their subject. The result of the whole is, that 91·51 per cent. is water, and that the solid contents consist of twelve proximate animal principles and ten neutral or earthy salts. The proximate animal principles are some of them in small quantity, and not very well defined; of those that are more so, and exist in greater proportion, we have cholesterine, biliary resin, which is termed biliary asparagin, picromel, osmazome, and mucus. The salts are principally combinations of soda, but we have no uncombined soda, according to the common opinion.[4] The bile of the dog did not differ very essentially from that of the ox, although the number of ingredients which entered into its composition was smaller; it contained cholesterine, resin, picromel, and mucus, with various salts;[5] and as far as the confessedly incomplete analysis which was made of the human bile allowed them to form an opinion, it appeared to con-

[4] T. i. p. 83. [5] T. i. p. 88, 9.

tain the same proximate principles.[6] Hence we learn that the analysis of Tiedemann and Gmelin, although not agreeing, in every respect, either with that of Thenard or that of Berzelius, approaches more nearly to the former.

One of the most interesting points of investigation in the second section is the analysis of the gastric juice, a fluid of so much importance in the animal œconomy, and respecting which our information has been hitherto so imperfect. The animals employed in these experiments were the dog and the horse. We learn that not only the quantity, but the nature of the fluid secreted by the stomach is considerably affected by the vital actions of the part, either as excited by food or even by mechanical irritation. In this latter state it always exhibits acid properties, and contains the muriatic and acetic acids in the uncombined state. The existence of the first of the acids in the stomach, it is well known, was announced by Dr. Prout, and the authors assure us that they made the same discovery in February, 1824, a month before they had read Dr. Prout's paper. They conceive that the acetic acid is always present in gastric juice, and to this is referred the lactic acid, which has been said to have been detected in the stomach; this acid, as we are informed, having been found, by some recent experiments of Berzelius, to be identical with the acetic acid. An acid, which is termed the butyric,

[6] P. 90.

is said to have been occasionally found in the stomach of the horse.[7]

It is not easy to select those points which possess the most immediate and direct interest, from the numerous details that compose the third section. There are long trains of experiments upon the change produced on different species of aliment by different species of animals, the dog, the cat, the horse, the ox, and the sheep. In the next place the progress of the aliment is traced through the various parts of the digestive apparatus, the new substances that are formed and the old ones that disappear in the successive stages of the process, first in membranous stomachs, afterwards in the stomachs of ruminant animals, and lastly in the small intestines, the cæcum, and the large intestines of each of these classes. The effect of the saliva upon the aliment is conceived to be partly mechanical and partly chemical; it softens some of the substances and breaks down their texture, while others are completely dissolved by it. This process is thought to be promoted by the alkaline salts which it contains; but I cannot admit the justice of the suggestion, that the sulpho-cyanate of potash may contribute "à anéantir en eux (les alimens) la faculté vitale de se contracter,"[8] for the obvious reason, that the food must have entirely lost this property before it could be supposed to feel the influence of the salts of the saliva.

[7] T. i. p. 166, 7. [8] T. i. p. 330.

In the paragraph which is entitled theory of digestion, the professors remark, that the obvious result of their experiments is, that the digestion of the aliment in the stomach consists in its solution in the gastric juice, and that, by this agent, all kinds of food, both those that consist of single proximate principles, and those that are composed of various principles, are dissolved. Many physiologists, it is observed, had formed this conclusion, but they had not inquired into the immediate cause of this solvent power, or what was the agent by which it was immediately performed. Water alone, at the temperature of the mammalia, is capable of dissolving many of the articles employed in diet, and a considerable unmber of the substances which are not soluble in water are so in muriatic and acetic acid at an elevated temperature, and to these the principal part of the effect is ascribed.[9]

With respect to the small intestines, it was found that their upper part contained a considerable quantity of uncombined acid, which is principally acetic, mixed with a little butyric, and rarely with the muriatic; as we advance along the small intestines, this acid disappears, while, in their lower part, the fluids are found to be alkaline. The functions of the small intestines are explained as follows:—The acid chyme is mixed with the bile, pancreatic juice, and the secretion from the mucous membrane; the muri-

9 T. i. p. 363, . 7.

atic acid unites with the soda of the bile, disengaging the acetic or carbonic acids, with which the alkali was previously combined; and likewise the mucus and the cholesterine of the bile; and hence are produced the white flakes, which have been erroneously supposed to be chyle. Chyle, it is supposed, cannot exist in the intestines, "dans l'etat normal," because it is only in the liquid state that it can enter the absorbents, and it is implied, that it is never to be detected except in these vessels.[1] The pancreatic juice and the intestinal mucus contribute to the effect, but it is not precisely ascertained in what their actions consist; it may be presumed that the operation of the latter is, in a great measure, mechanical.

The fourth section of the second part contains a set of experiments on the effect produced upon the digestion by tying the biliary duct. The results, although many of them important, are perhaps not very decisive as to the state of the aliment in the intestines, but it leads to some interesting observations on the primary use of the liver in the animal œconomy. This is supposed to consist in its separating from the venous blood certain substances which contain an excess of carbon and hydrogen, thus fulfilling a function very analogous to that of the lungs; the lungs removing these elements from the system under the form of gas and vapour, the liver under that of a fluid or semi-fluid substance.

[1] T. i. p. 396.

It is remarked, however, that there is an essential difference between the excretions of these organs, that the first are burned (or rather are similar to the products of combustion) while the latter are still combustible.[2]

The Professors conclude their work with a list of the most important points in which the results of their experiments differ from those of MM. Leuret and Lassaigne. Most of them respect the chemical properties of the substances which were examined; in some cases we may presume that both parties are correct, owing to the different circumstances in which the subjects of the experiments were placed; but, I apprehend, that on most points, where the discrepancies cannot be thus reconciled, the experiments of MM. Tiedemann and Gmelin must be preferred to those of their rivals.

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## *Leuret and Lassaigne's Researches on Digestion.*

The work of MM. Leuret and Lassaigne[3] is of a very different nature and aspect from that of the Heidelberg Professors. It contains a summary of the opinions that had been entertained by preceding physiologists, which is well drawn up, and professes

[2] T. ii. p. 60.

[3] Recherches physiologiques et chimiques pour servir à l'Histoire de la Digestion.

to give the results of various experiments that were made by the authors, but these are, for the most part, stated in a very general way, with very little detail respecting the mode in which they arrived at their conclusions. And it is not a little remarkable, that in a considerable number of those cases, where they have entered more minutely into the detail of their experiments, their conclusions are at variance with those of MM. Tiedemann and Gmelin. Thus, in direct opposition to the experiments of the latter, MM. Leuret and Lassaigne state, that the chemical properties of the saliva are essentially the same in all animals,[4] whereas, we are led to suppose that a very remarkable difference exists with respect to the saline ingredients; and with respect to the animal matter, while MM. Tiedemann and Gmelin suppose that it consists of three distinct substances, MM. Leuret and Lassaigne unite all these together under the denomination of mucus. The account of the bile is given in a very general way, and the analysis of M. Thenard is adopted, without any notice of the different results which were obtained by Berzelius and others. We find the same discrepancy in the analysis of the pancreatic juice; and this is the more remarkable, because it was one of the substances, to which the attention of the French chemists was more particularly directed, while they appear to have been fortunate in procuring it in considerable quantity for the purpose of examination. The Germans inform us that it is essentially acid,

[4] P. 33.

while the French state that it is alkaline; the former point out various circumstances in which it differs from the saliva, while, according to the latter, these fluids are nearly similar.[5] Equally at variance with each other are the accounts given in the two treatises of the composition of the gastric juice; for while the German professors agree with Dr. Prout, that muriatic acid is one of its components, MM. Leuret and Lassaigne suppose that it owes its acid properties to the lactic acid; they inform us that they have repeated the examination of the gastric juice a great number of times, always with the same result, and they particularly enter into the consideration of the process employed by Dr. Prout, and pronounce his conclusion to be incorrect.[6] Independent of the well-known accuracy of this chemist, the fact of the presence of muriatic acid in the secretions of the stomach is now so fully confirmed by other experimentalists, as to leave no reasonable doubt of its existence, in the ordinary state of the action of the organ.

5 Art. 4. p. 94, et seq.

6 P. 114..7. Dr. Prout has replied to these animadversions, and satisfactorily refuted them in Ann. Phil. v. xii. p. 406.

## *Dr. Prout on the Composition of Alimentary Substances.*

A very important paper was read to the Royal Society near the conclusion of the season by Dr. Prout, "on the ultimate composition of simple alimentary substances; with some preliminary remarks on the analysis of organized bodies in general." The following account is taken from an abstract which was published in the Philosophical Magazine and Annals of Philosophy for August.[7] This paper is the first of a series of communications which Dr. Prout proposes to present to the Royal Society; the object of the whole being to determine the exact chemical composition of the saccharine, oily, and albuminous substances, in which all the simple alimentary bodies employed by the higher classes of animals may be comprehended; he afterwards proposes to examine the changes which are produced upon them by the stomach and the other digestive organs. Dr. Prout commences by some preliminary observations on the analysis of organized bodies in general, and especially points out certain objections which attend the use of the oxide of copper: this substance he finds not only to be hygrometric, but also to condense air. He found that the oxide almost invariably gained in weight by the operation, a circumstance

7 V. ii. p. 144, et seq.

which he ascribed, in part, to its combination with oxygen contained in the tube. To obviate these sources of error, Dr. Prout adopted the process of burning the body under examination in oxygen gas. According as the volume of oxygen gas employed was either unchanged, augmented, or diminished, so it was concluded that the body analyzed contained oxygen and hydrogen in the proportion which forms water, that it contained an excess of oxygen, or an excess of hydrogen respectively. The author then described the apparatus which he used for the purpose of ascertaining the composition of vegetable substances on the above principle; he detailed the precautions necessary to be observed in the process, and pointed out its peculiar advantages, the chief of which is that it is not liable to be affected by moisture.

Dr. Prout commenced his examination by the saccharine principle, under which term he includes all those substances in which hydrogen and oxygen exist in the proportion which form water. These are all capable of becoming alimentary, and are termed, by way of distinction, vegetable aliments. Sugar was the first of these bodies that was analyzed; of this at least two distinct varieties, perhaps more, exist. The most perfect form of this principle is sugar-candy, prepared from cane sugar, and the purest specimens of the loaf sugar of commerce are identical with it. It was considered in the abstract as consisting of 44·44 parts of carbon to 55·55 of water. The other variety of sugar was procured from Narbonne honey, and contained only 36·36 parts of carbon.

The next class of bodies which Dr. Prout examined was the amylaceous; to these he applies the new term mereorganized, by which word he proposes to designate all those substances formed essentially on the principles of crystallized bodies, but not capable of assuming the crystalline form, probably on account of the presence of certain foreign ingredients, that enter into their composition. The ingredients here referred to are the minute quantities of various substances found in all organic products, and which, instead of being mechanically mixed with them, are supposed, by Dr. Prout, to perform the most important functions; that they are, in fact, essential to organization. He conceives, that when a crystallized substance passes into the organized state, its chemical composition remains essentially the same, and that the only difference which can be traced in it, is the presence of more or less water, and invariably of minute portions of some of the foreign bodies above alluded to, which appear not only to destroy its power of crystallizing, but to change entirely its sensible properties. Starch is supposed to be the most perfect form of this class of bodies; it differs very much at different times according to its degree of desiccation, but he considers, that when it is perfectly free from water it is identical with sugar-candy. Starch will be generally found to yield from 37 to 43 parts of carbon.

The next principle which Dr. Prout examined was acetic acid; he informs us that he had long suspected that the hydrogen and oxygen in this acid existed in the proportion that forms water, and he found this to be the case by subjecting the acetate of copper to his apparatus. He found the acid to consist of carbon 47·05 parts, and water 52·95 parts. He observes, that it is not known to exist in the mereorganized state, except the acid which has been found in all animal matters, and has been hitherto called the lactic, be deserving of that appellation. I may remark, however, that Prof. Berzelius, upon whose authority this acid principally rested, has altered his opinion respecting its nature, according to the statement of MM. Tiedemann and Gmelin, regarding it merely as an impure acetic acid.

The last substance belonging to this series, which Dr. Prout examined, was lignin, which, according to the experiments of Prof. Autenrieth, is capable, by certain processes, of acquiring properties similar to starch, and even of being formed into bread. Sugar of milk, manna, and gum, were next examined, and afterwards the composition of the oxalic, citric, tartaric, and saclactic acids was ascertained. It is impossible to estimate too highly the value of this communication; we seldom meet with so large a body of important facts, derived from so unexceptionable a source. Every one must look forward with extreme interest to the completion of Dr. Prout's arduous undertaking.

## APPENDIX TO CHAPTER XI.

### *Messrs. Addison and Morgan on Poisons.*

SINCE the publication of my second volume, in which I had occasion to discuss the question of venous absorption, we have been favoured with a series of experiments, which bear upon this point, by Dr. Addison and Mr. Morgan, "On the Operation of Poisonous Agents upon the Living Body." The conclusion which they draw, and which, if not decidedly proved, is at least rendered highly probable, is, that poisons, when introduced into the blood-vessels, "affect the brain and general system by their direct operation upon the nerves of the inner coat of the blood-vessel, and from that cause only," p. 79. If we agree with the authors in this view of the subject, it is obvious, that many of the experiments which have been made, both in this country, and in France, for the purpose of proving venous absorption, are irrelevant and inapplicable.

## APPENDIX TO CHAPTER XII.

### *Mr Yarrell's Observations on the Change of Plumage of the Pheasant.*

A PAPER was presented to the Royal Society, near the close of the session, by Mr. Yarrell, on the change of plumage which the hen pheasant occasionally experiences. Mr. Hunter and Dr. Butter, who had noticed the fact that the female bird sometimes acquires a plumage considerably resembling the male, supposed that it was always connected with the age of the bird, and that it only occurred when she ceased to lay eggs. But Mr. Yarrell has found that the change does not depend upon age, but that it is connected with some disease or imperfection of the sexual organs. He further observes, that in both sexes, their characteristic peculiarities disappear or are diminished when these organs are imperfect, and that, in this case, the sexes approximate so much, that it is difficult to distinguish between them.

## APPENDIX TO CHAPTER XIV.

### *M. St. Hilaire's Observations on the Ossicles of the Ear.*

In giving an account of the ossicles of the ear, and remarking upon their supposed use in the animal œconomy, I omitted to mention, what appears a singular hypothesis of the learned anatomist M. St. Hilaire. It is detailed in his work entitled "Philosophie Anatomique." The object of this work is to show, that the organization of all vertebrated animals may be reduced to a uniform type, and that every part which is found in each class has an analogous part in the other classes. This hypothesis forms the basis of five separate memoirs; but it is the first of these only that is connected with the structure of the ear.

The immediate object of this first essay is to prove that the bony or scaly appendages to the branchiæ of fish, which are concerned in the mechanism of the respiration of these animals, are the analogues of the four ossicles of the ear in the mammalia, birds, and reptiles. These appendages, which were formerly known by the general name of opercula, have received from Cuvier the distinctive appellations of opercule, pre-opercule, inter-opercule, and sub-opercule, names obviously derived from their respective positions. The author endeavours to trace out the analogy between the opercula and the ossicles, by examining these parts in the mammalia, birds, and reptiles, and pointing out the mode in which the gradual transformation is effected. M. St. Hilaire then asks, what is the evidence for the ordinary opinion, that the ossicles are really essential to the sense of hearing. Fish, he remarks, who are without these parts, seem to possess this sense very perfectly, and in birds, whose sense of hearing is supposed to be peculiarly acute and delicate, they are imperfectly developed; it is also an admitted fact, that their form and dimensions bear no proportion to the perfection of the faculty of the organ. Hence he concludes, that in the mammalia, birds, and reptiles, they are superfluous or rudimentary parts, indicating that there are other animals, in which the parts are more fully developed; that the ossicles are not essential to the organ of hearing, and that their use is of a secondary kind, consisting probably in diminishing the force of sounds when too powerful. Their primary use, he conceives, is to form a part of the respiratory apparatus, as is the case in fish, where they are of the largest size and possess the most perfect organization. The "corollaries," which the author deduces, I shall give my readers in his own words. "De tout ce qui précède, je crois devoir conclure que les quatre osselets de l'ouïe ne sont toujours chez les mammifères, les oiseaux et les reptiles, que les quatre os operculaires des poissons; que, vus de plus haut, ce sont quatre materiaux donnés de l'organization, susceptibles d'un *maximum* et d'un *minimum* de developpement;

qu'ils sont portés au plus haut degré de developpement et de fonctions dans les seuls poissons; que dans les autres animaux vertébrés, ils descendent de ce rang élevé, pour tomber dans ce que je nomme les conditions rudimentaires; que comme tels ils sont susceptibles de se rapetisser de plus en plus, quelquefois jusqu'à disparaître entièrement; enfin, qu'incapables, dans les animaux à respiration äerienne, des hautes fonctions de leur primative destination, ils s'y trouvent comme des ilotes au service et à la disposition des organes qui les entourent."[1]

The character of M. St. Hilaire, as an anatomist and naturalist, is well known and generally recognized, and the volume now under consideration bears ample testimony to his acquirements. Yet I must be permitted to say, that I do not feel satisfied as to the truth of the position which it is the object of this essay to establish. Although I fully acknowledge the difficulty which there is in assigning an appropriate use to the ossicles of the ear, yet the mode in which they are placed in the organ, and their connexion with the membrana tympani and the bony canals, impress upon the mind a strong conviction, that their primary destination is in this organ. The gradations which we observe in the structure and use of the analogous parts in different classes of animals, in many cases, afford a strong confirmation of the general truth of M. St. Hilaire's views; but the propriety of the application of the principle in each

[1] P. 55.

particular instance can be determined solely by its individual merits.

The only method by which we can decide, whether these ossicles should be considered as essentially belonging to the ear, or as an adjunct to the respiratory organs, is to ascertain their relative state of development in the different classes of animals, as connected with the perfection of the sense of hearing in its various modifications. For this purpose, the sixth chapter of M. Blainville's work, "De l'Organization des Animaux," may be referred to with much advantage, where an account of the comparative state of these parts in all the different classes of animals will be found. He observes, that there is no part in the ear of fishes which can be regarded as analogous to these ossicles, and in the following remarks he appears to allude to an opinion similar to that of M. St. Hilaire. "Les os, qui avaient été pour ainsi dire empruntés par l'appareil de l'ouïe à celui de la locomotion dans les appendices de la déglutition lui sont rendus, et se modifient pour cet usage, comme nous le verrons en parlant des organes de la locomotion. L'ouverture gutturale de la partie moyenne de l'oreille, loin de disparaître, acquiert au contraire un très-grand developpement, mais pour un tout autre usage, celui de la respiration, comme nous l'avons indiqué en parlant des amphibiens."[2] It is much to be regretted that so valuable a work as M. Blainville's should not be completed.

[2] P. 553.

In the Ann. des Scien. Nat. t. 15, p. 113, et seq. we have a very curious experimental paper by M. Flourens, on the effect produced by dividing the semicircular canals of the ear; very quick and violent motions of the head were produced, the direction of which depended upon the particular canals which were divided.

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