PRIZE ESSAY.

THE CELL:

ITS

PHYSIOLOGY, PATHOLOGY, AND PHILOSOPHY;

AS DEDUCED FROM

ORIGINAL INVESTIGATIONS.

TO WHICH IS ADDED

ITS HISTORY AND CRITICISM.

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Natura in minimis maxima est.

INTRODUCTORY NOTE.

THIS Essay was written in the latter part of 1851, and the many advances the science of structure has since made, has thrown so much light on some obscure points, that the parts relating to these ought almost to be rewritten, to be up with the times.

I have sought to, at least in part, make good this deficiency, by inserting as much new matter as well could be, while the sheets were going through the press.

BOSTON, Sept. 1853.

PREFACE.

PHYSIOLOGICAL Science has such extended relations, and occupies so wide a field, that any one of its departments alone, furnishes us with material sufficient for the study of our whole lives. Each one of its specialities now covers more ground than did the entire fabric as understood by the earlier writers. This is so true that any comprehensive treatise on Physiology written at the present time would be encyclopædial in size; and, if the labour of a single individual, would necessarily be quite imperfect.

These views being correct, the plan of the present work, as indicated by its title, may seem altogether too comprehensive, if not presuming. But a consideration of the whole matter has led me to think that no other plan could be pursued that would enable me so well to advance those ideas and doctrines which experimental inquiry has shown me to be true.

Our most rational Pathology must now be considered as based upon our clearest and most correct ideas of physiological action. In fact, Pathology cannot, like Physiology, be regarded as a distinct science; for it is but the erring condition of the latter—its actions modified and governed by the same laws, but acting upon different premises.

These two branches, then, are efficient mutual aids. There can be no correct understanding of pathological phenomena without a previous knowledge of physiology; and, on the other hand, no one can carefully study the former without having his ideas of the latter made more clear and comprehensive.

It was for these reasons that I did not take for my subject, simply the physiology of cells, which would alone have been sufficiently extensive.

It may not be amiss for me to state that all the statements, facts, illustrations, &c. contained in the essay are my own, when not otherwise indicated; moreover, most of them were prepared and made specially for this work.

It has been a source of regret that so few of the more recent German works have been accessible to me in the preparation of this subject. During the past few years especially, the number of microscopical observers in Germany has been very great, and there are a corresponding number of works being published, of the highest and most trustworthy character. It would indeed be well if American students would divert somewhat their attention from Paris in this direction.

Most noted among these is the masterly work of Kölliker (*Mikroscop. Anatomie, &c.*), of which I unfortunately was able to possess only a small part until after the completion of this essay.

INTRODUCTORY CHAPTER.

HISTORY AND CRITICISM OF CELL-DOCTRINES.

A SURVEY of the rise and progress of any of the inductive sciences fully shows that signal discoveries are never due to the labours of a single individual. For there existed previously, minds that had rudely caught at the same idea, or imperfectly pursued the same line of inquiry; but in which, either from want of requisite data, or more generally from the absence of a peculiar mental capacity, the subject did not reach its full maturity. Subsequently, however, that individual becomes the discoverer, who is so gifted that he can apprehend that peculiar colligation of facts, which always must and always does precede the discovery of an important scientific law. This view of the relation of the Discoverer to the discovery, detracts no merit from the former, for the work, however nearly finished it might have been, would not have been completed, had not the discoverer set the seal of his capacity upon it.

The history of the inductive sciences is full of examples bearing directly upon this point. In physics, such was true of the law of gravitation, as brought out in a perfect form by Newton. Every one had witnessed the falling of bodies towards each other or the earth; but it needed the mind of Newton to perceive in it the expression of a law as wide as nature herself.

Without alluding to other instances, I may say that this view is fully sustained by the historical relations of the subject under consideration. As we shall soon see, the existence of Cells, as component parts of the organisms, had long been known to anatomists and physiologists, but this fact found, for the first time, in the mind of Thos. Schwann, the requisite capacity to perceive in it the fundamental expression of organization, and that, therefore, it must have an application as wide as the range of organized beings.

This *principle of discovery*, as it may be called, the gift of a fortunate few, appears to sustain relations to physical facts, either singly or collectively, other than those with which we are now conversant. So that I can entertain no doubt but that the truth of the whole law was impressed upon the mind of Schwann just as forcibly at the hour of its suggestion, as it now is, after its widely extended verification.

The task of tracing out historically those steps of progress in physiology, which necessarily preceded the advent of the modern cell-doctrines, cannot be otherwise than both pleasant and profitable. There is a difficulty, however, in reducing it to the limits of a single chapter, without making that chapter include several others which treat of particular cells; and, therefore, to avoid repetition, if the following account is incomplete, the remaining portion must be looked for in the following pages, devoted to particular points.

The recognition of the fact that vesicles constitute a component part of the animal economy, did not, of course, occur until after the application of magnifying powers to anatomical structures. When this was made, is a point too obscure to be settled. But it appears to me that the application of such powers, in an intelligent way, cannot be traced farther back than the time of Malpighi and Leeuwenhoek, who seem to have observed wonderful things through their rudest of glasses. Malpighi* recognized the blood-corpuscles as little globules, and Leeuwenhoek† has given quite a good description of them, even to some peculiarities of their form. The labours of both these anatomists, in other departments, were something to the same effect; they made out tolerably well some structures and caught glimpses of others, but did not advance to the point of conceiving that there was any unity in organization, or that the vesicle was anything but *one* of its almost ever-varied forms.

The observations of Hewson,‡ made nearly one hundred years

* Malpighi, Opera Posthuma, London, 1686, p. 92.

† Leeuwenhoek, Opera omnia seu Arcana Naturæ detecta, Leyden, 1687, tom. ii. p. 421.

‡ Hewson, Experimental Inquiries, London, 1774–77. *Vide* also Sydenham Society edition of his works.

Remarks.—Owing to the much greater ease of study, the structure of the vegetable tissue, *as cellular*, appears to have been made out at an early date. It seems to have been first pointed out by Rob. Hooke (Micrographia, London, 1667). But it received a considerable advance from Malpighi, who sent his great work (Anatome Plantarum) to the Royal Society of London, in the year 1670. His observations appear to have been very accurate, and not only did he maintain the cellular structure of plants, but also declared that it was composed of separate cells, which he designated "Utriculi."

From this time until the year 1831, the *cell* (I should say rather *vesicular*) structure of plants was pretty clearly understood by botanists. See Mirbel, Traite d'Anat. et de Physiol. Veg., Paris, 1802. Also, the earlier works of Hugo Von Mohl.

after, although remarkable for their time, and containing many tolerably well-elucidated points, involved no histological relations, and in this respect did not perhaps contribute much to the advance of definite ideas of fundamental structure. In fact, during the 17th and 18th centuries, the microscope, rude as it was, existed in the hands of but few, and was, for the most part, used to gratify a youthful curiosity, instead of being applied to the elucidation of any intricate subject.

The labours of Haller should not, in this connection, pass unnoticed; for, although he may not have elucidated many points by the microscope, yet the doctrines which he advanced, based upon a most profound erudition and an extensive experience, all tended to assign an individuality to organization; and with such a unity of purpose, there would necessarily supervene a seeking for a unity of structure.

It does not appear that any advance was made as to the points under consideration until the time of Treviranus,* who, in 1816, published his account of the tissues, dividing them into three kinds, viz. *amorphous*, *fibrous*, and *globular*.

This announcement is somewhat remarkable for its correctness; but I think it must be regarded as premature, the experience of the time not justifying so broad a generalization.

The work of Heusinger,† which appeared soon after, may justly be considered as marking an episode in the history of animal tissues. For in it we find views expressed, which, although not founded upon careful and extended investigations, fully show the marked tendency of studies of this kind. This tendency here was in the organized, as it had already been in the unorganized world; viz., to seek for the idea of the *whole* in a unity of elements.

Heusinger even attempted to explain how fibres and tubes are formed from elementary spherical globules, by the linear arrangements of the latter.‡

But, remarkable as this may appear, his ideas of these relations if I may thus express myself, seem to have been anatomical, and not physiological; for he does not appear to have conceived of the true physiological signification of these globules.

Passing over a brief period, we find that, in 1833, Robert Brown§

* Treviranus; Vermischte Schriften, Göttingen, 1816.

† Heusinger; System der Histologie, Eisenach, 1822–24.

‡ Heusinger; *loc. citat.* i. p. 112.

§ Brown; Transact. Linnean Soc., London, 1833.

Although this was published in 1833, there are good reasons for supposing that the discovery should be dated 1831.

discovered the nucleus of the vegetable cell, pointed out its constancy, but does not appear to have recognized its physiological relations with the cell itself. Nevertheless, I regard it as a necessary and important step in the gradual development of those doctrines which ultimately were to assume a more perfect form.

At about this time a new ardour for the study of minute anatomy seems to have arisen with many German and French physiologists. This was due, in part, to the very admirable class of scientific men just then appearing, and in part, also, to the better class of instruments becoming available.*

The names of Schultz, Purkinje, Valentin, Wagner, Müller, and others, cannot be too honourably mentioned for their histological labours at this period.

The investigations of Purkinje, in connection with Deutsch,† upon cartilage corpuscles and the process of ossification, served to point out the relations which simple cells might sustain to the more compound tissues.

In 1835, there appeared the remarkable work of Valentin,‡ upon the histology and development of tissues. I think there can be but little doubt that here we find the first traces of cell-doctrines, or the recognition of cells (I will not say nucleated cells), as the bases of all organized forms; and which, subsequently, were so brought out by Schwann, that he may be truly said to have made them justly his own. Valentin witnessed the formation of pigment-cells around pre-existing nuclei; he also instituted the comparison between cartilage cells and those forming vegetable cellular tissue, and pointed out other important relations.§ In fact, I think it may be said of Valentin, that he perceived the true physiological relations of cells as far as he well could without apprehending the grand fact that the nucleated cell is the fundamental expression of organic forms. It was by the appreciation of this grand fact that Schwann has set his seal upon all previous labours—for this was the *vis vitæ* of the whole matter, and, however strongly Valentin may urge his claims,‖ I cannot regard them in any other light.

In the following year Schultz¶ published a work on the circulation

* I refer here to the instruments of Frauenhofer, Shiek, and Chevalier.

† Deutsch; De penitiori ossium structura observationes, Breslau, 1834.

‡ Valentin; Handbuch der Entwickelungsgeschichte des Menchen, Berlin, 1835.

§ Valentin; *loc. citat.*, pp. 194, 209, 287.

‖ See Wagner's Lehrbuch der Physiologie, Leipzig, 1839.

¶ Schultz; Das System der Circulation in seiner Entwickelung durch die Thierreihe, Stuttgard, 1836.

of blood, including the histological condition of its elements. He appears to have comprehended the cell relations of the blood particles, and describes their formation around a pre-existing nucleus. He noticed also many other important points; but his results were not appreciated at the time, and no general signification of these phenomena was entertained.

Immediately succeeding this, there appeared many new works on microscopic subjects. The microscope had ceased, with scientific men, to be an instrument of curiosity, and was now used as an auxiliary for the elucidation of the higher phenomena of life.

Numerous works were published—among the more prominent of which may be mentioned, those of Henle,* Vogel,† Valentin,‡ Donné,§ Boehm,‖ and many others, whose articles are scattered through the various journals of the day.

This brings us up to the period when Schwann commenced his investigations upon animal tissues, and with which those of Schleiden upon vegetable structures were opportunely contemporary.

The above-mentioned work of Henle fully demonstrated that animal tissues may be nourished without vessels—in fact, having a mode of life like that of vegetables. This, as Schwann himself observes, was a very important advance, as it brought the physiological relations of the two kingdoms to the same point. He showed that the *Rete Malpighi* consisted of *nucleated cells*, which grew from smaller ones; thus establishing the fact of the individual power of cells for their own life and increase.

It was about this time that Valentin,¶ when describing the nucleus of the epidermal cells, remarks, that they remind him of the nucleus that occurs in the cells of vegetable tissues.

Still, all these comparisons must be regarded only in the light of *analogy* instead of *homology*; perhaps in the same way that the wings of a butterfly would be compared with those of a bird;—no fundamental unity was perceived.

* Henle; Symbolæ ad anatomium villorum intestinalium imprimis eorum epithelii et vasorum lacteorum, Berlin, 1837.

† Vogel; Physiologisch-pathologische untersuchungen ueber Eiter, Erlangen, 1838.

‡ Valentin; Repertorium, Bds. i. and ii.

§ Donné; Recherches microscopiques sur la nature des mucus et de la matière des écoulements, Paris, 1837.

‖ Boehm; Die kranke Darmchleimhaut in der asiatischer Cholera mikroskopisch untersuchte, Berlin, 1838.

¶ Valentin; Nov. Act., N. C. xviii. pt. i. p. 96.

In 1838, the excellent researches of Schleiden* upon phytogenesis were published. In it, Schleiden pointed out in a clear and definite way the formation of cells in vegetable structures, according to a single and uniform method; and by him, the law of unity of development of vegetable cells was believed and advocated.

At this time, while Schwann was at work upon the animal tissues, Schleiden communicated to him the results of his investigations, and Schwann, aware of what had been done as to animal-cell formation by Valentin and Schultz, seized upon the grand idea of *unity of plan* in the appearance of the elementary forms of organized structures. He gave an individuality, a vitality, to the anatomical details then collected. He caught the first glimpses of a truth, which, although it then needed a verification, was soon to be recognized as the grandest in physiological science.

The correct apprehension, then, of this law of fundamental development, as applicable to *all* organic structures, belongs to Schwann.* The steps of progress preceding it we have just reviewed. It is evident that the same course of inquiry had been imperfectly pursued before, or even that, previously, the same idea had been rudely grasped. In fact, according to the very constitution of science, it could not have been otherwise; for its history has taught us that induction is its broad highway, and it would be equivalent to conferring divinity upon a man to state that he made a grand discovery in science which had no relations whatever with the labours of others who had preceded him.

Such appears to me to be, in brief, the correct view of the origin and complete development of the cell-doctrine. I have based it upon all the historical evidence within my reach. It now remains to take a survey of the subject since that time; or, rather, since it attained its individuality of expression.

If it is true that all in physiology appertaining to animal cells, up to the time of Schwann, was truly made his own, by the interpretation he passed upon them; the same is nearly equally true of the labours in this department, made during the three or four years succeeding the publication of his work. For, those who took up this

* Schleiden; Beiträge zur Phytogenesis. Müller's Archiv, 1838, pt. ii. It appears, however, that these investigations were made some time before this date, for Schwann mentions that Schleiden communicated them to him in October 1837.—*Vide* Mikroskop. Untersuch., (preface.)

† Schwann; Mikroskopische Untersuchungen ueber die Uebereinstimmung in der Structur und dem Wachsthum der Thiere and Pflanzen, Berlin, 1839.

line of investigation, and who, in one sense, deserved the name of original observers (when they recorded their own observations, preferred to quote the original language of Schwann). This is a fact in the history of science well worthy of remembrance, and especially by those who seek the honours due to the correct study of natural phenomena.

In the verification of this grand idea, the wide range over which Schwann extended his labours, has truly made subsequent observers feel that that field had been almost entirely preoccupied. It has also impressed upon them the belief that so much labour could only thus be so successfully accomplished, except under that scientific conviction of fundamental unities in nature; a *teleological* instead of a *physical* view of the relations of natural phenomena; which has always been the guiding spirit to great success in the comprehension of nature.*

But, in thus expressing myself, I would not claim too much for Schwann. Signal advances in any science require the successive and accumulated labours of many worthy men. In the present case, if, on the one hand, subsequent and more extended researches showed that the generalizations, naturally adopted by Schwann, were too broad, yet, on the other hand, this field has since been extended in a hitherto unknown direction.

The doctrine of Schwann is, that all animal tissues originate from cells, and that these cells are always formed around a pre-existing nucleus. As to each of these prominent points, differences of opinion have arisen with subsequent observers.

That all tissues have *not* the necessary pre-existence of cells, was advanced first by Henle,† as to some of the so-called fibrous tissues, and has since been verified as to structures known as *fibrillated.* In other words, a tissue may be formed by the aggregation of granules in a certain way, and without the intervention of true nucleated cells. As to this point there can now be entertained no doubt, for, since the time of Henle, it has and may be constantly observed.

But in regard to the other point, the formation of cells otherwise than *around* a pre-existing nucleus, the opinions have been many and

* I should mention, however, that, judging from a portion of Schwann's writings, he took the *physical* instead of the *teleological* view of organization. See *loc. cit.* Syd. Soc. Ed. p. 187.

† Henle; Traite d'Anatomie générale. Trad. d'Allemand, par A. J. Jourdain, 2 vol. Paris, 1843, tom. i. p. 374.

various. Among the first who expressed their dissent, was Reichert,* who, in his embryological studies, sought in vain for the constant presence of the nucleus in the vitelline cells.

The discovery of Ascherson,† as to the phenomena sequent upon the conference of oil and albumen, was of some importance in relation to this point. For, according to it, cell-membranes might be formed around a drop or minute particle of oil, and without any nucleus. But this doctrine did not have much weight at the time, and we shall see, hereafter, that a different interpretation should be put upon these phenomena than had been supposed.

The phenomena of the multiplication of cells by segmentation, presented new and irreconcilable difficulties. The subject did not seem at all clear upon any explanation; moreover, at this time, Henle‡ had observed appearances connected with cells, which, although he was inclined to adopt generally the view of Schwann, led him to express his dissent of its universality of application.

At this time, the writings of Hugo Von Mohl§ and Nägeli,‖ as to the formation of vegetable cells, supported the doctrines of Schleiden and Schwann; and, considering their authority, tended, no doubt, to confirm the old view. The immediately subsequent writings, however, of physiologists of both the vegetable and the animal kingdom, were of a different character. In 1843, Karsten¶ published his dissertation on the cell, in which he dissented entirely from the "standard view," believing that cells originate without a pre-existing nucleus, and by the expansion of amorphous granules of organic matter.

In 1844, Kölliker,** a most trustworthy observer, after a consideration of the whole matter, expresses his belief in the want of unity of mode of cell formation; and declares that, if there is a *single method* which is invariable, it yet remains to be discovered. This was also the opinion of most of the excellent observers of the time. In fact, the method of Schwann seemed to be admitted as that of the exception rather than of the rule. Although such discrepancies in science are unpleasant at the time, yet, by them, science makes its most signal advances. In the present case, it was now evident that there

* Reichert; Das Entwickelungsleben im Wirbelthierreich, Berlin, 1840, pp. 6, 93.
† Ascherson; Müller's Archiv, 1840, p. 49.
‡ Henle; Lehre von den Gewebe, Leipzig, 1841, p. 154.
§ Mohl; Vermischte Schriften, p. 84.
‖ Nägeli; Ueber Entwickelung des Pollens, Zurich, 1842.
¶ Karsten; De Cella vitale Dissertatio, Berlin, 1843.
** Kölliker; Entwickelungsgeschichte der Cephalopoden, Zurich, 1844.

was much to learn about a point which had been long regarded as quite definitely settled.

I shall not here dwell upon the details of these different views, for they are not easily understood without the aid of figures. But it appears pretty evident, and has since been so verified, that they were based upon correct observations. These differences seemed to increase in exact ratio with those of the observers.

In 1846, Mr. Paget,* in his excellent *Report on the Progress of Anatomy and Physiology*, expressed his doubts of the sufficiency of the then existing theories to explain many phenomena observed in the study of morbid growths. He had seen large growing tumours, in which there was *not* a nucleated cell, being only a collection of fibres; on the other hand, he had met with those, being only a mere collection of cells, and never apparently passing beyond that stage. This led him to the opinion that, in morbid growths at least, the cell is sometimes to be regarded as the *terminal* and not the *transitional* form. How far these views may be regarded as correct will be discussed on a future page.

Corresponding views, that soon followed from a great number of observers and writers, need not detain our analysis here.† Moreover, their writings are scattered through a mass of literature; and, even were any criticism desirable, it could be made with difficulty.

Since 1846, up to the present day (1851), the clearest and probably the most correct views of the origin and nature of cells, whatever may have been *the* mode, have been derived from the careful study of the genesis and structure of morbid growths; which, because they are morbid, are infra-formations, yet are connected with healthy tissue, and therefore may be justly considered as most fit for these inquiries.

* Paget. Report on the Progress of Anatomy and Physiology. July No. British and Foreign Medical Review, 1846.

† Among the writings that have appeared within the past few years, the following may be mentioned as worthy of reference:—

Reichert; Müller's Archiv, 1844–53.—The relations of cells, and the progress of the science, are here thoroughly discussed.

Kölliker; Schleiden's and Nägeli's Zeitschrift, fur Wiss. Botanik, 1845, heft ii.—This is a lucid exposition of the origin and nature of the nucleated cell.

Reichert; Müller's Archiv, heft ii. and iii. 1846.—Meckel; Müller's Archiv, heft i. 1846.—Courtz; Gaz. Med. de Paris, 18 Avril, 1846.—Harting; Botanische Zeitung, 1846, p. 46.—Hugo Von Mohl; Botanische Zeitung, 1846, p. 387.—Nägeli; Schleiden's and Nägeli's Zeitschrift, fur Wiss. Botanik, heft iii. and iv. 1846; see also the translation by the Royal Society.

Gairdner; Rep. Brit. Assoc. Ad. Sc. 1850.—For other references, see the various English, French, and German Physiological text-books of the last four or five years.

PART I.

PHYSIOLOGY OF CELLS.

CHAPTER I.

GENERAL RELATIONS OF CELLS TO ORGANIZATION.

When a fact in nature has once been fully recognized, we are apt to seek for it too broad an application and generalization. This may be due to a fundamental law of human reason—an instinctive tendency to unity in our knowledge of all natural phenomena; it is a kind of intuitive prevision, which is really founded on induction in part, but of the use of which in science we cannot be too careful. Schwann was led to make this application in regard to the histology of all animal tissues; and, although subsequent experience has shown that it is not true as to the exact manner in which it was enforced—that is, the nucleated cell as the necessary basis of all tissues—yet it holds true when considered as to the real philosophy of cell life. As we have mentioned in the history and criticism, there is a class of structures the formation of which does *not* involve nucleated cells. Such are fibrillated and granular tissues, both as healthy and diseased products. They are formed by the aggregation and conference in a regular way, of the primordial granules of organizable liquids. All the other tissues, whatever their function or aspect, are formed through the agency of nucleated cells. In fact, these cells appear to be a physical state, through which, in order to acquire functional organization, these tissues must pass. In many of the compound tissues of the higher animals, the phases of these transformations are not easily traced, for they are transient, and generally not well defined. But, in some of the lower forms of animal life, and especially those which are aquatic, this is not the case, for here the adult forms are in one sense the persistent representatives of the embryonic forms of the higher types. On these accounts, our clearest views of the nature and function of tissues, have, in almost every case, been due to studies upon inferior animals. In the following pages, the advantages thence accruing will be easily perceived. Upon a comprehensive and correct appreciation of the relations of these various phases of cell-transformation, is based all our real fundamental knowledge of physiology.

There is one point concerning all these cell-conditions and transformations, which is now pretty well determined. It is that, in those cases in which the individuality of the cell is swallowed up in the substance of a tissue, the tissues thus formed are subservient to the functions of *organic* life. On the other hand, in those cases in which the individuality of the cell is preserved, so that a single cell is a true histological representative of the whole tissue, the tissues thus composed perform the higher functions of the economy, such as secretion and the elimination of certain forces. The idea here intended to be conveyed will be perceived, if I mention the schlerous and muscular tissues as examples of the former, and the nervous and epithelial tissues as examples of the latter case. This statement is, I believe, quite true with all except the lowest forms of animal life.

The relations of cell-studies to organization as modifying our ideas of the nature, *de ipso*, of function when applicable to animal tissues, require here a brief mention. Our ideas of the peculiarity of the function of any tissue are based upon our experience with it as such. Thus, we have been so accustomed to associate animal motion with the muscular tissue, that the former is not easily conceived possible without the presence of the latter. And, again, we have been so wont to link sensation with the nervous system, that the presence of the former has been deemed impossible without that of the latter. But cell-studies, in the lowest forms of animal life, show that these views are incorrect; for with them we do have *motion* without *muscle*, and *sensation* without *nervous* tissue; and in each of these cases, as we shall more fully show hereafter, the results are due to the agency of *simple* cells. In these facts, which have been carefully made out, is embodied a great deal of physiological truth; for we are thereby taught that function cannot be regarded as the result of this or that material form, but that this form exists as such in virtue of the function being performed perfectly. In other words, it is the acceptance of the *teleological* instead of the *physical* view of the relations of physiology to anatomy; a result, which I cannot conceive could have been obtained, except by a study of these ulterior forms of organized matter. And, while it is the legitimate offspring of such studies, it is at the same time the almost prophetic guide to many of our safest conclusions.

The general relations of cells to organization, as illustrated in the reproductive processes, cannot here be considered without a more extended view of these last than would be compatible with this part of my work. The elimination of a new and distinct being, is, properly, the only true individualizing process of the economy; for, if I may thus express myself, it is the potentiality of life made manifest in a chosen material form. This form is a *nucleated cell*, which, as it is the form in which and by which life dawned upon the inorganic world, so also does it seem the most appropriate form by which all the mutual physiological relations with that world can be best sustained.

CHAPTER II.

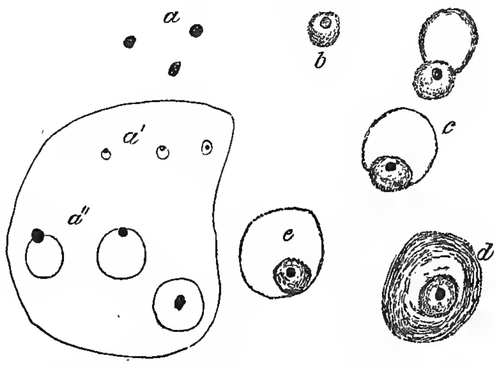
CELL-GENESIS IN GENERAL.

A KNOWLEDGE of the anatomical relations of an organized form, has always preceded the correct appreciation of its physiological conditions. This is so, perhaps, because the former are more easily made out, and when well understood scarcely admit of an hypothesis.

In cell-studies, as we have already seen, this has been quite true. Many botanists and anatomists, before the time of Schleiden and Schwann, understood very well the anatomical relations of cells. The same is true, also, of cell-genesis; for, as has been stated, both Valentin and Schultz had accurately observed how it occurred. The first description of cell-formation, as clearly understood from special study, appeared, as is well known, in the paper of Schleiden, published in 1838. The view here expressed was founded upon studies made upon vegetable structure; it coincides exactly, however, with the view of Schwann, based upon investigations of animal tissues. The brief but comprehensive description of cell-formation given in their works has been stereotyped in the minds of all subsequent physiologists. The well-formed cell consists of a close membrane or sac, more or less transparent, and filled with liquid giving it a globular aspect. In this contained liquid is situated a small solid (?) body, either floating free or attached to the walls of the cell: this is the *nucleus*. This nucleus *may be* also hollow like the original cell-membrane; and, like it, contains a liquid in which is a smaller solid (?) body called the *nucleolus*. The cell consists, then, of three parts, and is called *nucleolated*. But we will take the simple *nucleated* cell, consisting of only the first two; here the nucleus is the *germ* of the whole, and therefore must exist before the cell-membrane itself. Moreover, the nucleus is supposed to differ also as to its chemical composition from the remaining parts of the cell. According to Schleiden and Schwann, therefore, the nucleated cell is regarded as consisting of three distinct parts, viz.: cell-membrane, cell-liquid, and nucleus. The nucleus is the starting-point. These nuclei, or cytoblasts, as they have been called, already existing in a formative plasma, the following is the mode by which the complete cell is formed. I take it from Schleiden, as originally published in 1838:—

"So soon as the cytoblasts have attained their full size, a delicate transparent vesicle rises upon their surface. This is the young cell, which, at first, represents a very flat segment of a sphere, the plane side of which is formed by the cytoblast, and the convex side by the young cell, which is placed upon it somewhat like a watch-glass upon a watch. . . . . The vesicle gradually expands and becomes more consistent, and, with the exception of the cytoblast, which always forms a portion of it, the wall now consists of gelatine. The

Fig. 1.



The mode of cell-formation, according to Schleiden and Schwann.* *a*. Cytoblasts or nuclei. *b*. Same, more highly magnified, showing nucleolus. *a'*. Formation of nuclei. *a''*. More highly magnified. c. Formation of cell-membrane from nucleus (*b*). *d*. Cell entirely formed, nucleolated, and nucleus in centre, free.

* *Vide* Sydenh. Soc. edit. of their Researches.

entire cell then increases beyond the margin of the cytoblast, and quickly becomes so large that the latter, at last, merely appears as a small body inclosed in one of its side-walls."

I have thus quoted this description at length, that the peculiarity of this mode of formation might be clearly seen.

It is at once evident, that, if we admit the view here advanced to be correct, the existence of cells always presupposes the existence of nuclei; the former never being formed without the latter existing previously.

In the history of this subject, I have alluded to some of the views dissimilar to the preceding one, which have been urged by observers since the time of Schwann.

It appears, however, that they have been less inclined to put forward particular views of their own than to assert that they have been unable to witness the phenomena described by Schwann. And some observers,* whose experience in these matters is not inconsiderable, have declared that they have never witnessed the formation of cells in the way above mentioned.

My own experience, although not quite so exclusive, has not been much different. I have, in watching the formation of blood-cells, and that of some pathological corpuscles, seen all the phenomena pointed out by Schwann. But I have failed to recognize their universality, or even commonness. On the other hand, the whole tendency of my experience generally, and the results I have obtained from quite an extended series of investigations made especially with this object in view, have pretty clearly shown me, that, in animal tissues at least, the usual mode of cell-formation is based upon quite a different plan—involving views of the relations of the different parts of the cell to each other which I think pretty well borne out by the results of recent investigations, made by myself and others.

I propose to describe this mode somewhat in detail, not only because I think it new, and humbly claim for it an originality, but because of its commonness.†

We will take the example of the production of new cells in the

* Paget; Report on the Progress of Anatomy and Physiology.—Brit. and For. Med. Review, July, 1846.

Also, Gairdner; Report of the British Association for the Advancement of Science, 1850.

† This new mode of cell-formation I presented in a paper to the American Association for the Advancement of Science, Cambridge meeting, 1849.—See the Proceedings, p. 261.

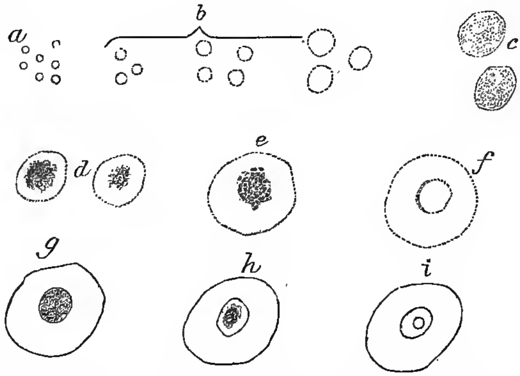
adult tissue. An organizable hyaline plasma having been effused by the nutrient vessels; in this plasma, minute dark points or utricles begin to appear. These constitute the first material manifestations of organization, and, strictly speaking, are the *germs* of future cells. They are minute sacs and not solid particles; a point I have very satisfactorily determined by the aid of the highest and best microscopic power. They appear to be formed by the conference of a minute particle of oil with albumen, uniting under the influence of vitality, according to the so-called Ascherson mode of cell-membrane formation. These minute utricles give the blastematous liquid a dotted aspect; moreover, being hollow sacs with albuminous walls, they are capable of endosmotic and exosmotic action. By endosmosis they increase, and become clearly visible closed sacs, which increase and grow apparently by simple expansion. Having obtained a certain size, the process of the formation of a nucleus begins to take place. This is accomplished in the following manner: The closed sac above described is filled with liquid; this is clear at first, but soon a cloudiness throughout appears. This nebulous appearance commences to recede from the periphery, where there appears a light space, the central portion of the liquid, however, being all the more dark and opaque. This condensation towards the centre going on, it terminates by the appearance in the centre of a minute vesicle or body, which is the nucleus. It is then a *nucleated cell*, capable of all the functions of such particles. After such processes have occurred, it may happen that this vesicular nucleus expands, as did the original cell-wall, and then, the same changes taking place, a body is formed within it; and in this way there is formed a nucleated cell in a closed cell membrane, constituting the true *nucleolated cell.*

But in normally formed tissues, these last-mentioned changes are far from being common; the original cell-membrane rupturing and passing away before the nucleolus is formed.

In brief, the mode of cell-genesis here described consists in the following points: 1st. The appearance of a primordial utricle in a formative blastema. 2d. Its expansion into a clearly seen vesicle. 3d. The partial condensation of its liquid contents towards the centre, giving rise to a new utricle, constituting the nucleus—the whole forming the complete nucleated cell.

It will immediately be perceived that, according to this mode, the cell proper necessarily exists before the nucleus, which is a kind of daughter-vesicle formed within it, and is a secondary product—but not at all dissimilar, either genetically or histologically, from the cell itself. According to this mode, also, there may be cell-sacs without nuclei, the formation of the latter having never, from some imperfection, taken place; then again there may be many nuclei in a single cell-sac.

Fig. 2.



Cell-formation, as I have observed it.—*a*. The so-called granules, which are really utricles, being formed by the union of oil and albumen. *b*. The same, increased in size by simple expansion. *c*. Still larger, and liquid contents cloudy. *d*. Condensation of cloudiness towards centre, leaving periphery clear. *e*. The same, farther progressed. *f*. Condensation finished, ending in a clear vesicle—the nucleus—the whole being the *nucleated cell*. *g*. Cell with liquid contents cloudy, as in c. *h*. The same, still farther progressed, as in *d*. *i*. Condensation finished, ending in a clear vesicle—the nucleolus—the whole being the *nucleolated cell*.

This mode of cell-genesis, which may be regarded as a peculiar one, has been based upon the results of a series of investigations made upon the origin and development of epithelial tissues; and I may also add that all or nearly all of my studies of the intimate character of tissues, both normal and pathological, have tended not only to support, but confirm this view. My investigations upon the vegetable tissues have not been extended, and if from what little I have seen I should be entitled to an opinion, I should say that this mode holds equally valid here, and the phases of development of some of the Cryptogamia appear to have a very good solution with this method of genesis.

The difference between the view of Schwann and Schleiden, and this one just enunciated, is this: The former regarded the nucleus as a primary formation, and as histologically and chemically different from the cell itself which is formed *around* it; while, according to my own view, on the other hand, the nucleus is a secondary product, formed *in* the cell-vesicle, and with which it is in every way homologous.

I have found this mode of cell-formation to be quite general, and serving to explain phenomena hitherto obscure. In pathological products of either a heteromorphous or homeomorphous nature, it may be clearly observed. In fact, in many of these instances, I have been unable to perceive how any other view could be applicable at all.

In the normal textures, and especially those of the lower animals, in which the tissues remain persistent on their cell-stages, the same line of procedure is followed. The phases of genesis which are due to a difference of the cells themselves, cannot be well understood unless we take up their formation, each specially, as it occurs in the animal kingdom.

CHAPTER III.

CELL-GENESIS IN PARTICULAR.

I. The Ovum.

As is now well known, the ovum is, histologically speaking, only an epithelial cell, although we should not forget that, physiologically, it is something more. Now, as the mode of cell-genesis just put forth was based upon studies of epithelial cells, the inference should be valid, that ova are formed in exactly the same way. The results obtained by Prof. Agassiz, as to the formation of ova in the *Ascidian Mollusca*, are in complete accordance with this view.* To these I may add my own results, incidentally obtained over quite an extended range of the animal kingdom, from the genus Tubullaria of the Radiata, to some of the higher forms of the Vertebrata.

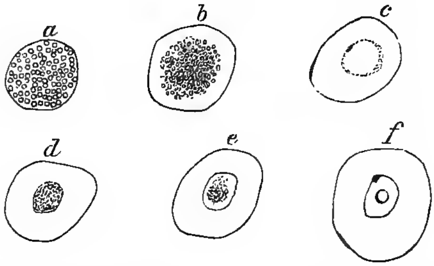
A detail of all these observations would be quite lengthy, and in this place unnecessary, however interesting they might be when written out as a special subject. I will here state the results I have obtained with the class of Birds, and they may be taken as a very good exponent of the others, made in the other classes of the animal kingdom.

At the approach of the period of procreation, there appear in the

* Proceed. Amer. Assoc. for the Advancement of Sc. second meeting. Cambridge, 1849, p. 157.

ovarian stroma, numbers of minute vesicles, or utricles, of a globular shape, and too small to admit of a very definite description. These increase apparently by simple expansion, and when just visible to the unaided eye, are filled with fine granules or utricles. At the next stage of development, a partial replacement of these utricles by a vesicle is perceived; and although I have been unable to watch the phases of change, yet I cannot but believe that it occurs by the same processes as those belonging to the formation of the nucleus, as we have just pointed out. This vesicle or nucleus, at first very small, increases quite rapidly, and in it is formed another vesicle or nucleus, constituting the third of the series. You have then the ovum, which is a great compound cell, the liquid of which is filled with granules constituting the *vitellus**—the nucleus constituting the *germinal vesicle*—and in this last the nucleolus, or *germinative dot.*

Fig. 3.



Formation of ovum as observable in birds. *a.* First appearance of ovum; as a sac filled with granules. *b.* The same, with a condensation towards centre taking place. *c.* Condensation finished, a clear vesicle appearing. *d, e, f.* The same changes repeated, ending in ovum (*f*), consisting of *vitellus*, *germinative vesicle*, and *germinative dot.*

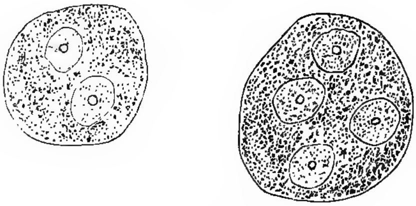
This whole series, however, is not formed until the ovum, as a whole, has reached considerable size, or nearly that of the adult, at least with birds. This *germinative dot* may, as a vesicle, expand, and have within it several nuclei, as is not unfrequently seen.

Such are the phenomena as occurring in the class of birds. But with some of the other classes of animals, there occur peculiarities which will serve to illustrate the real nature of the genesis. On

* By a kind of condensation of peripheral layer of these vitelline granules, a membrane may be formed, constituting the so-called *vitelline sac*, which is pretty constant in the higher animals, but which is equally absent in many of the lower ones. This subject of a vitelline membrane, however, is one on which all are far from being agreed. I will only add that, among the Mammalia, it is admitted by Valentin, Wagner, Wharton Jones, and some others, while the opposite is the opinion of Baer and Bischoff.

this account, I will here allude to some of them. In many of the *articulata*, and especially in the classes *insecta* and *arachnida*, the ova not unfrequently contain several nuclei or germinative vesicles. This was first pointed out by Herold.* But I have frequently noticed it in a series of investigations upon the embryology of spiders, made a year or two since, and in which I have carefully figured this peculiarity under its many forms. These facts go to show that the relations of the germinative vesicle to the ovum, are the same as those existing between the nucleus and the cell—that is, it is a secondary formation; and, as occurs often in cells, there may be a plural production. Moreover, it would appear from this fact that, however important the germinative vesicle may be in the evolution of the new being, it does not hold a single or unital relation to the ovum as a whole, as would necessarily be true were the Schwann mode of formation here admitted.† The vitelline substance is, as is well

Fig. 4.



Ova of common Spider (*Agelena nævia*), showing plurality of germinative vesicles (nuclei). (Magnified 50 diameters.)

known, composed of cells; and these appear to be formed, according to both the direct observations of Prof. Agassiz and myself, in exactly the same way—by the expansion of primordial utricles, in which cell-contents are developed. In stating these views of the genesis of the ovum, I am well aware that I am taking grounds quite dissimilar to those advocated by many of the physiologists of the day. The researches of Wagner, Valentin, and others, all advocate exactly the inverse method of production, that is, that the germinative dot exists as the primary part, upon which the germinative vesicle and vitellus are built up. Such also is the view which Schwann

* Herold, De Aranearum generatione, Marburg, 1824.

† Recent embryological studies, and especially some upon insects, have led me to think that altogether too much significance has been placed upon the agency of the germinative vesicle in the primitive changes of the ovum. In the Aphides, I have observed the egg-buds develop exactly as though they had been true eggs, and yet they have no trace whatever of a germinative vesicle—being nothing but an oval mass of nucleated cells.

very naturally has taken; for, as is true of myself in relation to the cell theory I have advanced, he sought the same class of phenomena in the genesis of the ovum, which is really only a cell, as in the cell itself.

Farther researches in this direction must remove these differences; and although dissimilar opinions must always be expected, yet there may be equally looked for a greater uniformity of results, from the modern improved means of observation.*

* Owing to the importance of the subject, to look at it in an historical point of view (and which also might serve to make clear some of the obscure points), will not be a useless task. The anatomical relations of the ovum were known long before cell-studies, as such, were commenced. But the recognition of its particular parts as essential for certain functions does not date back many years. The germinal vesicle was recognized by Purkinje, in 1825, and called after that by his name. In 1827, Baer discovered the ovum of the Mammalia, which, however, he identified with the germinal vesicle of birds as pointed out by Purkinje. This point, however, was pretty clearly settled a few years after, by Coste, Valentin, and Wharton Jones. In 1835, Wagner pointed out the germinative dot and its relations. And it may justly be questioned if embryology could have been understandingly studied, that is, as to all the functions of the ovum, until these points were made out.

For investigations as to the *formation* of the ovum, considered as consisting of the three above-named parts, we must refer to those of Krause, Valentin, Wagner, Martin Barry, Wharton Jones, and Bischoff and Baer. They seem by no means to have arrived at uniform results. According to Wagner, Krause, Martin Barry, and Baer, the germinative dot exists first, and the remaining portion of the ovum is formed according to the Schwann mode of cell-formation. This, however, is not the opinion of Valentin and Bischoff. The latter believes that the germinal vesicle exists primarily, *in* which is formed the germinative dot, and *around* which, by an aggregation of granules on its periphery (and *not* according to Schwann's method) is afterwards formed the vitellus. In other words, the formation of the ovum is *sui generis*, and having no analogue in the production of any other tissues.

Of late, and since the publication of these writings of those who may be justly called the fathers of the science, the whole subject has been studied in a very thorough manner. I cannot pretend here to even allude to the various opinions put forth, and a history of the whole matter would be a volume of itself. It is evident that at least three distinct opinions beside my own exist as to the mode of ovum genesis: 1st. That of Wagner and others, which is according to the Schwann mode of cell-formation. 2d. That of Bischoff, which is, that the germinal vesicle exists first, the other parts being formed within and around it, as before mentioned. 3d. That of Bergmann, who thinks that the ovum as a sac is first formed, and subsequently the germinal membrane and its contents are formed in it. This is at least enough to fully show the unsettled state of the subject. As the views of Bergmann resemble my own, as above stated, more closely than any other, I will, for a moment, refer to them. His observations were upon the ova of the frog and the salamander. According to him, the vitellus at the commencement of its development consists of granules. These first arrange themselves into large groups, then divide into smaller ones, around which vesicles are formed by means of a viscid substance; you have these numerous cells filled with granules; and the whole contents are formed in a like manner.

I cannot, with some, and especially Bischoff, regard the ovum as having a special formation, because it is an ovum. On the other hand, I naturally look to its genesis as a true exponent of those elementary processes of organization occurring in the tissues, of all of which it is the real parent.

The genesis of the *sperm cell* is exactly like that of the ovum; this is a point concerning which I have devoted considerable time, much more than to that of the ovum. Moreover, the ease of study of its phases of development is much greater. I have seen no evidence of its formation according to the Schwann mode of cell-formation, but it follows exactly in the line of that of common epithelia. As is now very satisfactorily determined, the ovum and sperm-cell are the true analogues of each other; their similarity of genesis would justly have been inferred from this fact alone, and hereafter, when treating of spermatic particles as special formations *in* cells, the other relations of the subject will be discussed. But, without forestalling those remarks, I will only add that, the evidence of this analogy being conclusive, the histological signification of the ovum as *a cell* is placed beyond all doubt. And as *a cell* we are entitled to seek for it, *à priori*, the same kind of genesis that belongs to cells in general, a point which I have just sought to establish.

II. Chyle and Blood-cells.

After those of the ovum and sperm-cell, the genetic relations of these cells next demand our consideration, for they are the first free and independent cells formed in the embryo, and are those by whose nutritive agency the others, which are united into continuous tissues, subsequently acquire their development. I am well aware that, in entering this field, I am taking hold of one of the most intricate and interesting of physiological subjects, and on which account, therefore, there are not only various but widely dissimilar opinions. My own observations, made for the purpose, entitle me, perhaps, to an opinion which is different from any I have seen, but it cannot be satisfactorily illustrated without that brief historical retrospect of the whole matter which will enable us to learn its phases of progress; a labour which, as to many other points also, cannot fail to be instructive.

The existence of these peculiar bodies (blood-cells) in the blood, was first noticed by Leeuwenhoek,* in 1674. It was sufficient for

* Leeuwenhoek; Opera omnia, &c. Leyden, 1687, ii. p. 421.

him to recognize them as corpuscles, and as having a constant presence in that liquid. For nearly the succeeding hundred years, although their presence was well known to many anatomists, but little advance was made in a knowledge of their nature. In 1776, Della Torre* described them as little rings, having a dark centre, and he has figured their piling or clustering together which occurs during coagulation. But the first researches which are entitled to the name of *complete* were made by Hewson,† whose works were published at about the same time. As he appears to have had better means of investigation than the others who preceded him, his ideas of many of the peculiarities of these bodies are quite clear. We cannot wonder that he should have described the human blood-corpuscles as having a dark body or nucleus in their interior, when many observers of the present day persist in the same view. Owing to the publication of the results of Hewson, many new and curious investigations were made upon this subject, and that, too, over quite a range of the animal kingdom. I pass over here a review of those many writers who immediately followed this physiologist, and who have carefully noted the anatomical peculiarities of these cells as they occur in different animals (for these are secondary points to the subject of their genesis), to the writers to whom we will now direct the attention. Any investigations of this kind cannot be said to have been made until about the time of the appearance of the cell-doctrines of Schleiden and Schwann. As we have already seen in the history of cell-doctrines, Schultz‡ was the first who prosecuted these studies with success. He speaks of the blood-corpuscles of the chick as being formed *around* the yelk-globules, the latter existing as nuclei, from which arise vesicles, according to the Schwann method of cell-formation, although at that time Schultz probably had no idea of the signification of these processes.

* Della Torre; Nuove Osservazione Microsoopische, Naples, 1776, p. 82.

† Hewson; Experimental Inquiries, &c. London, 1774–77, part iii. Or the republication of his works by the Sydenham Society. Edited by Gulliver, p. 220, *et seq.* Hewson was the first, I think, who pointed out the fact that the blood-cells of embryos and adults are different. Thus, he says, that those of the embryos of the viper and the chick are not flat and elliptical, but globular, and larger than those of the adult. This was an important fact to be learned.

‡ Schultz; Das System der Circulation, &c. Stuttgard, 1836. In a chronological point of view, the observations of Baumgaertner (Beobaohtungen ueber die Nerven und das Blut, &c., Freyburg, 1839) should be mentioned first. He describes the formation of the blood-cells in the tadpole, by the aggregation of numerous granules around a central nucleus; these liquefy and form a membrane, and thus the corpuscle is formed (p. 40). We shall refer to this point on a subsequent page.

In the embryology of Valentin,* published about the same time, a different opinion is maintained, from observations made since upon the same animal. Valentin regarded them as particular formations from the contents of the vitelline globules, which last, he affirms, are twice the size of the blood-cells. This formation takes place in the first liquor of the blood, which, he says, is completely transparent.

At this time, the constant presence of white or uncoloured corpuscles in the blood of vertebrates did not escape attention, and after Vogel† had pointed out their peculiarities, there naturally followed many speculations as to their nature and use. That they were the younger forms of the red cells, was first put forth by Wagner and Valentin,‡ a doctrine which, as we shall soon perceive, is advocated at the present time. This view appeared to derive weight from a second series of investigations made by Schultz,§ who affirmed that the coloured capsule was principally formed during the process of respiration.

In the next year Schwann's|| observations appeared, and although he expressed no opinion based upon his own researches, yet, from the evident cell-character of the particle, he naturally looked for its genesis according to the usual cell mode, and in support of which he falls back upon the results of Schultz.

Reichert,¶ in the year following, entertains the view that the blood-cells are special formations in the embryo, but out of the material furnished by the vitelline cells, and therefore resemble the primary cells of the organs generally. This opinion is also that of Bischoff,** at least with the oviparous vertebrates; but with the Mammalia, Bischoff regards the blood-cells as metamorphosed vitelline globules.

As to the formation of the blood-cells in the adult animal—the point now under discussion, Reichert†† entertains the singular opi-

* Valentin; Handbuch der Entwickelungsgeschichte des Menschen, Berlin, 1835, p. 296. At this time, Valentin's views do not appear to have been clear and explicit, although subsequently he may have made them so.

† Vogel; Physiologische pathol. Untersuch, &c. Erlangen, 1838.

‡ Valentin; Repertorium, 1837, p. 71.

§ Schultz; Ueber die gehemmte Auflosung und Auschirdung der Verbrauchten Blutbläschen. Hufeland's Journal, Apr. 1838.

|| Schwann; Mikroskop. Untersuch. &c. Berlin, 1839, p. 75.

¶ Reichert; Das Entwickelungsleben, &c. Berlin, 1840, p. 243.

** Bischoff; Traite du développement de l'homme, &c. Trad. d'Allemand, par Jourdain, Paris, 1843, p. 282.

†† Reichert. This is what Simon says is Reichert's opinion, as he learned by a private letter. *Vide* Simon's Chemistry of Man, Syd. ed. vol. i. p. 122.

nion that they are the immediate production of the liver, and that this is the function of this organ.

Henle,* who has paid some attention to the subject, entertains the view that the coloured cells are formed from the lymph-corpuscles; and he regards the lymphatic glands as the chief, though not exclusive seat of the changes thus to ensue.† The views of Remak‡ deserve here an especial mention, not only from their plausibility, but because their real nature is fully illustrated by phenomena recently observed. His opinion is that they were formed in large parent vesicles, which entirely disappeared after the formation had taken place. These large vesicles he found in the lowest stratum of blood drawn from some of the higher animals, which had been allowed to stand for a time. His experiments were quite numerous, but they wanted completeness; moreover, his observations have not been verified, and there are good reasons for believing that the phenomena here observed were of an abnormal and not a healthy character.§

* Henle; Anat. Générale, &c. Trad. de l'Allemand, par Jourdain, Paris, 1843, t. i. p. 491. Also, as given by Simon from private letter; Chemistry of Man, vol. i. p. 121.

† Henle; as given by Simon: Chemistry of Man. Syd. ed. vol. i. p. 121. *Vide* also Traité d'Anat. Générale, &c. Paris, 1843, t. ii. p. 103.

‡ Remak; Medicinische Zeitung, July 7, 1841.

§ The phenomena here noticed by Remak, have been met with only as connected with pathological products, or under abnormal conditions. They may be seen often in extravasated blood, and appear to consist only of a group of blood-cells, around which has been thrown a plasmatic membrane, inclosing the whole as in a sac. This may be well observed in the blood of the elephant. Thus formed, contents of a very heterogeneous character may be brought together. See Kölliker; Zeitschrift für Rat. Med. bd. iv. p. 89. Also, an article in Edinburgh Monthly Journal of Medical Science, Sept. 1851.

This point has considerable interest from other connections. It is well known that in the spleen of the higher and lower vertebrates, and especially the latter, there are often found peculiar bodies which sometimes appear as sacs filled with blood-corpuscles in different stages of dissolution. With these and the like data, Kölliker has advanced an unique view of the function of the spleen, viz., that it is the organ for the dissolution and destruction of the affected blood-corpuscles. The support of this view he has undertaken at considerable length; see art. Spleen, in the Cyclop. Anat. and Phys. and his Mikrosk. Anat. &c. Bd. ii. Milz.

This view seems to have met with anything but the approval of other observers. Some have even denied that these saccular bodies in the spleen existed at all, while others regard them, when present, as quite accidental.

For myself, after having given several weeks' examination to the subject, and especially in conjunction with that of the function of the spleen, I must also dissent from Kölliker's views. My observations extended over great numbers of each class of the vertebrata, and, although I have very frequently found the bodies in question, yet

A short time after this, the subject was taken up and treated in the most thorough manner by Mr. Wharton Jones,* and, considering the wide range over which his observations were carried, and the apparent care with which they were performed, they are certainly the most complete of any we as yet possess; moreover, they are now regarded as of authority on this subject. I shall therefore take them up rather in the manner of a discussion. Jones has undoubtedly laboured to establish a unity of mode of formation of these bodies, not only among the Vertebrata, but even among the Invertebrata. I trust, in making a brief *résumé* of his results, I shall not obscure this already sufficiently obscure subject.

Mr. Jones regards the chyle-corpuscle as the *basis* of the true blood throughout the Vertebrata. It is the starting-point from which all coloured cells must take their origin. In all the vertebrates *below* Mammalia (birds, reptiles, and fishes, in which the coloured corpuscle is distinctly nucleated), this formation takes place in the following manner: The chyle-corpuscle is at first a *coarsely granular*, *nucleated* cell; it begins to increase, and its surface becomes *finely granular*. From this stage, it passes on to having a smooth, clear membrane, and a well-defined nucleus, but is still colourless and globular. The next stage is that in which it assumes its colour and shape, and is then the perfectly formed coloured nucleated cell of these three classes. In Mammalia, however, a farther development takes place, for this cell-membrane bursts, and the *nucleus* of this cell escapes, constituting the true blood-cell; therefore, the red blood-cell of the Mammalia is not the analogue of the red blood-cell of the three other classes of the vertebrates, but of the *nucleus* of the blood-cell of these last. In other words, the blood-cell of Mammalia corresponds to the *products* of the blood-cells of birds, reptiles, and fishes; and, on this account, is regarded a higher formation.

In support of the view of the nature of the *nucleated blood-cell*, met with in the classes below Mammalia, he describes the phases of formation as he saw them in the blood of frogs. For these details, I must refer the reader to his own account. As to his view of the

from what I have seen of them, from the elephant to the eel, I can regard them only as accidental formations, or at least as bodies which have no special function.

For some farther reference to the subject, see Handfield Jones, Lond. Med. Gaz. Jan. 1847, pp. 140, 142; Ecker, Zeitsch. f. ration. Med. vi. p. 264; Virchow, Arch. f. path. Anat. u. klinische Med. i. pp. 452, 483; Funke, Ueber das Milzvenenblut, in Henle und Pfeufer's Zeitsch. i. 1851; Remak, Müller's Arch. 1852, Feb.; and Wharton Jones, Brit. and For. Med.-Chir. Rev. January, 1853.

* Wharton Jones; Philos. Transact. 1846, p. 63, *et seq.*

nature of the blood-cell of the Mammalia corresponding to the *escaped nucleus* of the developed lymph-corpuscle, he remarks for its confirmation that, throughout Mammalia, there is an almost exact correspondence as to size between the nucleus of the *coloured nucleated cell* and the true blood-cell. This *coloured nucleated cell* (corresponding to the *true blood-cell* of birds, reptiles, and fishes) he has never seen in man, but thinks he has observed it in the horse and elephant.

In the embryo of Mammalia, he finds appearances agreeing exactly with the above view of the whole matter. Thus, in an embryo-ox of one and a quarter inches long, he found three kinds of cells in the blood, viz.: coloured and uncoloured nucleated cells, and the true blood-cells; thus having in the same liquid the elements of mammalian blood and that of vertebrates below it.

These last-mentioned phenomena, as observed in the embryo, coincide pretty closely with those observed by Kölliker,* who thinks that the embryonic blood-corpuscles are metamorphosed vitelline cells; but, in regard to the formation of the blood of adult vertebrates, he advocates the view that the coloured cells are the chyle-corpuscles directly transformed. On this point, therefore, he differs widely from Mr. Jones. I have mentioned this fact to show that the observing of the appearances in the embryo, which Mr. Jones has described, does not oblige one to adopt his view of the genesis of blood-cells in the adult animal.

These opinions of Jones I have not been able to reconcile with the appearances I have met, in studying this subject. And, in describing my own observations, I shall point out the grounds of disbelief in the view that, in the three lower classes of the Vertebrata, the coloured cell is the chyle-corpuscle developed and enlarged. But, in the Mammalia, a still wider discrepancy exists, and there are two points for which I have not been able to find the least ground for their reconciliation. The first of these is, the absence of the so-called *colourless nucleated cell* in the blood of adults, which should be found in great numbers; because, according to Mr. Jones, they are the parents of the true blood-cell.† The second of these is, the uniformity as to size of the chyle-corpuscle throughout the Mammalia; while, as is well known, there exists a very wide variation of size with the true blood-cell. In some, the chyle-corpuscle

* Kölliker, Henle und Pfeufer's Zeitschrift, iv. 1845–46.

† Mr. Jones speaks of finding the *colourless nucleated* cell only in the elephant and horse, among the Mammalia. I have often, and quite carefully, examined the blood of the elephant, but have failed to observe the nucleated cell here alluded to by Mr. Jones.

and the blood-cell being about equal, as in man and the quadrumana; while in others, the former is three or four times the size of the latter, as in the musk-deer and goat. It is true that, in nearly all, the blood-cell is smaller than the chyle-corpuscle. But, according to the statements of Mr. Gulliver,* this is not true with some of the larger pachyderms, for instance, the elephant, in which the blood-cell is much the larger. Moreover, the size and general aspect of the true blood-cell hold certain invariable relations with the relative position of the animal as determined by a classification based upon other data. This fact, taken in connection with the great uniformity of size of the chyle-corpuscles, would lead us justly to infer, that, while these last possess only the common relations of cells, floating in a plasma, the true blood-cells have connection with the whole animal as a distinct and specific being. If this inference is correct, it at once shows how we are to regard the mutual relations of these two kinds of cells as existing in mammalian blood.

There is one other point to which allusion should be made. It is the difficulty of reconciling the oval shape of the blood-cells in the camel tribe, with the view that they are the *nuclei* of larger and colourless cells which are globular; for in the blood of all the oviparous vertebrates the nucleus has the shape of the blood-cell itself, which is oval; moreover, in all our cell-studies, it is the law that the nucleus and cell agree, at least in this particular. Mr. Jones refers to this point, and thinks the matter made clear, when he affirms that, in the *Paca*, the blood-cells become globular after the addition of water. He perhaps forgot that this same phenomenon occurs when water is added to the blood of any bird, reptile, or fish, and therefore this explanation is insufficient.†

Since the memoir of Mr. Jones, and up to the present time, there have appeared numerous detached papers on the subject, but most of them have wanted completeness.‡

* Gulliver, Proceed. Zoolog. Soc. London, No. clii.

I should mention, however, that my own observation does not agree with that of Gulliver; for in elephant's blood, which I have examined, I have found the red corpuscle to be 1-4500 of an inch in diameter, and the white only 1-2000.

† I have recently had an opportunity to examine the blood of the common lama (*Auchenia paca*, Illiger). The corpuscles were quite oval, being 1-3000 of an inch in length and 1-6000 in diameter. It is true that, like those of a frog, they became of a more spherical form by the addition of water, but they did not, by any means, lose their oval shape.

‡ I have not thought it proper to burden the general text with even an allusion to these many and varied writings. But, in an historical point of view, some of them

I do not think that the views of Jones have been acceded to by most physiologists. Among these, may be notably mentioned Dr. Carpenter, who has advocated the distinct nature of the chyle-corpuscles, both as to function and distinction, believing that the blood-cells are special formations.* Mr. Hassall† entertains a similar opinion. But after all, the subject is in quite an unsettled state; and this is so because the opinions, justly deserving that name, of most observers have been formed more from negative than from positive data.

Whatever view we may take of the origin of the blood-cells and their relations to the chyle-corpuscles, this much is certain, that the materials of their formation in the adult are derived from the chyle. Therefore, in considering the histological relations of the latter, we shall be most likely to get at correct views of the genesis of the former. This I propose to do first: and whatever views we may thus obtain of what is properly the *reproduction* of these cells in the

are worthy of a brief notice in a note like this. I will go back to a few years preceding Mr. Jones's paper. In 1842, Mr. McLeod published his observations upon the development of the blood-cell in the chick, and which appear to be different from those either of Schultz or Valentin, before mentioned. According to him, in blood taken from the heart of a chick of three days, are found "a number of small granules floating in the field; these enlarge and become clearer in the centre, and when they have gained twice their original size, the central clear part becomes dull, and in a short time it is seen to be distinctly granular. The enlargement of these bodies, with the granular appearance of their centre, seems *not* to depend on the aggregation of granules round a central one, but on a property which they have *in* themselves of enlarging and presenting that figure. During all this time they are quite spherical. In the *second stage*, the central portion gradually becomes less opaque, and ceases to appear granular. It then has the appearance of a slightly flattened round cell, with a nucleus in the centre. The cell is disk-like, rather concave, but the nucleus convex. In the third stage, this concavity disappears, and the whole becomes slightly convex." (*Vide* London and Edinb. Month. Journal, Sept. 1842.) It will be perceived that the mode of genesis, here described, corresponds quite closely with that which I think belongs to cells generally.

Donné advanced the view that the spleen is the great elaborator of the coloured blood-cells. (*De l'origine des globules du sang.* Séance de l'Acad. 7 Mars, 1842.) Kramer has watched the development of the embryonic blood-cells in the frog. He says they are first *embryonic cells*, and then pass through a series of changes, ending in the perfectly formed corpuscles. In the adult, he is of the opinion that the chyle-corpuscle is transformed into the blood-cell by a clearing up and colouring process. (*Vide* Müller's Archiv, 1848, p. 63.)

Mr. Paget found the blood of a fœtus of one month to consist of *nucleated* cells, some of which had two nuclei, and were ovoidal. (*Vide* London Med. Gaz. Feb. 2, 1849.)

* Carpenter, Principles of Human Physiology, &c. Amer. ed. 1853, p. 169, *et seq.*

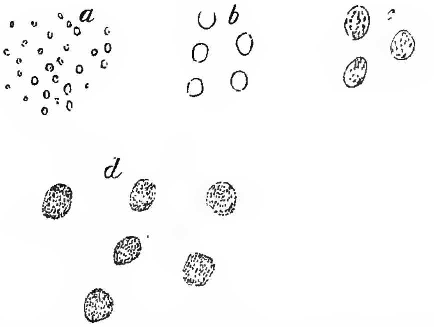
† Hassall, the Microscopic Anatomy of the Human Body, published in Fascio. London, 1847-51.

adult, they can well be subsequently considered in connection with what we shall learn of what is properly their *primitive genesis* in the embryo.

If we take the chyle, as it is just forming, from the smaller chyle-vessels, we find it composed of a blastema, having a universally dotted or minutely granular aspect; or, in other words, composed of a hyaline liquid, in which float innumerable minute particles. These last constitute its only solid particles, and give the chyle its milky aspect. If we examine that taken from larger vessels, and those nearer the thoracic duct, we find, among these granules, small cells or vesicles. These are quite small, and appear to be simply vesicles, having only a hyaline liquid for their contents. There appears to be little doubt that they have not arisen except by the expansion of these primordial granules or utricles—according to the first processes of the method of cell-genesis I have already pointed out. Their simple, vesicular, hyaline aspect shows that they could not be formed *around* a primary nucleus, according to the method of Schwann. Once thus started, they go on increasing by dilatation; their liquid contents become granular, and some of the granules adhering to the inner surface of the sac, give the whole that well-known finely granular or nebulous aspect. Afterwards, the free granules in the interior partially centralize themselves, giving rise to the granulated nucleus. Such is the nebulous, granular-nucleated cell of the chyle, as met with in the thoracic duct. These characteristics become more and more pronounced, as the chyle approaches its influx into the general circulation. Thus far the subject is pretty clear and well understood. The chyle-corpuscles pass into the general current of the circulation, and, in so doing, it is evident that they become subservient, either directly or indirectly, to the formation of the coloured cells. This opinion, aside from any observations, could be justly inferred from their decreased number—being apparently replaced by the true blood-cell. Were this not so, we might justly ask, "What becomes of all the chyle-corpuscles?" This subserviency, according to my own observations, is *direct* in the oviparous vertebrates, but *indirect* in the Mammalia.

In birds, reptiles, and fishes, the chyle-corpuscle is the nucleus *around* which is formed the true blood-cell. I will here describe the phases of this development as seen in reptiles, the Anourous Batrachians. In the blood of these animals, after they have been well fed, two kinds of corpuscles are seen, and in nearly equal number. The first are small spherical or slightly oval corpuscles, having a distinctly marked, granular, punctiform aspect. These are the chyle-corpuscles. The second are the well-known oval, nucleated, lenticular

Fig. 5.



Formation of chyle-corpuscles, taken from lymphatics of a goat. *a.* The first solid particles occurring in the lymph-liquid, and giving it a pellucid aspect—these are utricles. *b.* The same, increased in size by expansion. c. Still larger, with a few granules in the contained liquid. *d.* Contained liquid crowded with granules, the whole forming the chyle-corpuscle. (Magnified 250 diameters.)

bodies, the coloured cells being about three times the size of the first. The blood, thus composed, is to be examined without water, and even then the appearances of a formative process will not be seen at first. Many of the granular corpuscles appear only as such, but with a good light and a clear defining power, a delicate and almost transparent membrane will be perceived encompassing some of these corpuscles; at first, it is seen lying quite close upon it, but by the addition of a little water it is made quite distinct. With a few, this newly-formed membrane may be seen standing off, quite distant on all sides, from the granular nucleus. But it is so delicate that, after expansion to a considerable extent, it is nearly always ruptured by the pressure between two plates of glass. Until it has reached to about half the size of the adult cell, it is spherical and colourless. But after this, several notable changes begin to take place. The nucleus, hitherto *coarsely* granular, assumes a *finely* punctiform aspect, and finally the granules almost entirely disappear; it then has quite an even and smooth appearance. After this, both it and the cell-membrane, by which it is surrounded, begin to assume that oval and lenticular shape seen in the adult form. The contained liquid has a faint reddish hue, and so it may be said that they are partly coloured.

These changes, thus begun, continue, and end in the perfectly developed form. In some instances, however, the routine is a little different; the "clearing up" of the granular chyle-corpuscle takes place simultaneously with the formation of the new membrane around it, so that you have one *clear cell* contained within another. Then again, these chyle-corpuscles may assume a slightly oval shape before the enveloping membrane is formed; thus seeming to show that the idea of their future development is persistent in them, before any steps for its ulterior manifestation have been taken.

Aside from the evidence of this view of the matter, by direct observation of these changes, it is also supported by the fact that there is a close agreement as to size between the chyle-corpuscles and the nuclei of true blood-cells. Not only does this hold true of the blood of reptiles, but also of that of birds and fishes. Moreover, as far as my

Fig. 6.

Formation of blood in oviparous vertebrates.

(*Salamandra glutinosa.*) *a.* The Chyle-corpuscles. *b.* The same surrounded by a delicate membrane. *c.* The chyle-corpuscle become clear by a dissolution of the granules, the whole appearing as one vesicle within another. *d.* The membrane observed at *b.* increased, and having an oval shape. *e.* The well-formed corpuscle, having an oval nucleus partially clear—the whole coloured. These figures illustrate what I have observed with birds, and very many other reptiles. (Magnified 250 diameters.)

own observation goes, the chyle-corpuscles are smaller than the true blood-cells, throughout these three classes*—thus making it explicable that the latter should, in every instance, be formed *around* the former. In the cartilaginous fishes, in which the blood-cells are

* I have found this true of the smallest birds I have examined—and in these, above all other oviparous vertebrates, a deviation from the common rule would be most looked for. I do not know how it may be in the humming-bird; but Dr. Davy says, that here their long diameter is 1-2666, and their shortest 1-4000 of an inch, but he does not mention the size of the chyle-corpuscles. (*Vide* Proceed. Zool. Soc. in Ann. Nat. Hist. July, 1846.)

quite large, much more so than in the osseous ones; and in all the reptiles, these relations are easily seen and satisfactorily made out. In birds, however, they are less clear; but in no instance have I met with appearances not reconcilable with—in fact, which did not demand—the above view.

The nucleated blood-cell belongs to the oviparous vertebrates alone; and I know of no instance in which it has been met with in the blood of adult Mammalia, at least, as a distinct and uniform body.

In the blood of Mammalia, a different condition of phenomena is met with. The perfectly formed blood-cells have *no* nucleus, but consist, each, of a lenticular, generally round vesicle, in which may occasionally be seen a few minute granules.

Generally, the white or chyle-corpuscles are the larger; and, as in the lower classes, they possess no regularly formed nucleus as a single body, but have in their centre, to correspond to it, two, three, four, or even more granules, which are of a quite uniform size and appearance.

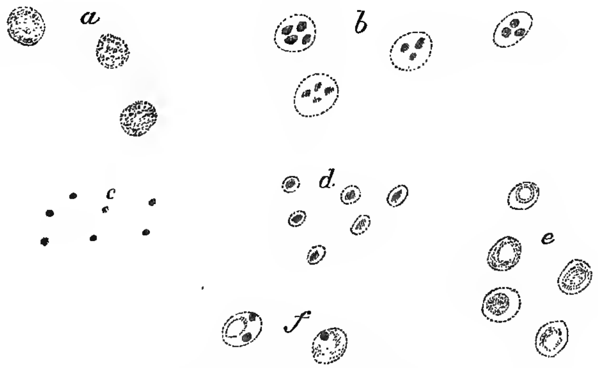
The want of all correlation of size between these chyle-corpuscles and the true blood-cells or vesicles, appears to me to forbid the opinion that the former, as such, are directly concerned in the genesis of the latter; either being produced *in* them, according to the view of Mr. Jones, or *around* them, as is the opinion of Schultz.

My own opinion is, that the blood-vesicles of Mammalia are formed *around* the granules which constitute the compound nucleus of the chyle-cell, and this takes place in the same way as that of the blood-cell in oviparous vertebrates around the chyle-corpuscle itself; the nucleus afterwards becoming dissolved, and, in nearly every instance, entirely passing away.

In procuring the blood from living mammals for examination, we generally take it from some of the terminal capillaries, and such, therefore, as has traversed the whole line of the circulation. On this account, the phases of the genesis are not well marked, and seldom ever seen; the whole field appears, at first, occupied with only two kinds of bodies, the vesicles and the white corpuscles; and, in many instances, these changes have been so completed, that nothing else can be detected after the most careful search; but, generally, a little attention will show that a third form exists. This is a small *spherical* cell, slightly coloured, and in which may be often seen a few minute granules.

These cells are few in number, and vary in size from that of one-third to that of two-thirds or more of the size of the adult vesicle. These I have regarded as the newly forming blood-vesicles, the nucleus of which appears to disappear at a very early period; and, unless attention was directed to the point, they would not, on that account, be distinguished from the adult form, even although only two-thirds its size.

Fig. 7.



Formation of mammalian vertebrate blood.

(Man.) *a.* Chyle-corpuscles. *b.* The same treated with acid, showing nuclei. *c.* The escaped nuclei free. *d.* The nuclei with a delicate colourless membrane. *e.* Well-formed blood-corpuscles—having a dark or light centre according to focus. *f.* Blood-corpuscles with granules—the remains of old nuclei. I take here the instance of man, because his blood is always accessible; but the same phenomena can be witnessed with other higher animals. (Magnified 350 diameters.)

The fact that these phases of formation do not appear boldly distinct in our observations, is due, I think, firstly, to the rapidity of the genesis, and, secondly, to the fact just mentioned, that the specimens observed are generally from the terminal capillaries, by the time they have reached which, owing to the rapidity of formation, these changes are entirely or nearly completed.

In a liquid like the blood—the nutritive one of all the economy, and which is full of a formative plasma constantly in motion, and which has its vitality repeatedly renewed by respiration, the escaped nuclei of the chyle-corpuscles rapidly gather about them, each a plasmatic membrane, which, from the constant motion, is as rapidly expanded and developed.

Aside from the support which I think this view has from observed phenomena, it is the only one, in my opinion, that does not meet with overruling objections. I will here allude for a moment to some of these last, which are constantly occurring. The view of Wharton Jones is, that the blood-cells are the escaped nuclei, perfectly developed, of the chyle-corpuscle; while that of Schultz is, that they are formed *around* the latter. Now, we will take the blood of man and of a goat; for, in them, the chyle-corpuscles are of about the same size, and larger than the blood-cell. It is, therefore, at once evident that the view of Schultz cannot be here maintained.

On the other hand, the view of Jones meets with equally valid objections. For the chyle-corpuscles being of the same size in these two animals, their nuclei (if it is admitted that they each have a single one, distinct) are also equal, and, being metamorphosed, should produce the same sized vesicle; or, at least, these vesicles should not be *smaller* than the nuclei. But this is not true—for, in the goat, the blood-vesicles are three and a half times smaller than the chyle-corpuscle; but, in man, they are only a third smaller.

But these difficulties do not present themselves, if we take the ground that the genesis takes place around the granules, which the chyle-cells contain; for, in every instance, they are smaller than the blood-vesicles, and may therefore well serve as a *punctum saliens* of the development of the latter.

The whole subject, however, will receive additional confirmation, when, in a succeeding chapter, we take up the functions of these two kinds of cells.

In the genesis of organic particles, which, throughout a wide range of animal life, have exactly the same function, we very naturally seek a unity of mode of expression. In the present case, this feeling is gratified by the manner in which we have just viewed the whole subject. It is this: the blood-vesicle in the Vertebrata is the product of the chyle-corpuscle, either as a whole or in part. *As a whole*, with the oviparous classes, in which it gives the formation a persistent nucleated character; *as a part*, in Mammalia, and in which, since it was simply a point of departure, and not an individual form as in the former case, it soon passes away, leaving the part as a simple vesicle.

It therefore may be stated that whereas, in the lower classes, the chyle-corpuscle is *directly* appropriated—in the higher, a step in advance has been taken, and the *elaborated products* of this corpuscle are used in the same way.

The formation of the blood-cells in the embryo necessarily involves changes considerably different from those of what is properly termed their *reproduction* in the adult. In the embryo, their first formation undoubtedly occurs *out of* the vitelline cells, and as such they obey the general laws of cell-genesis, and are nucleated; but it may be properly asked if these are *true* blood-cells? In the mammalian embryo, their reproduction takes place through the medium of the blood of the mother; but in the oviparous classes, it is out of the material laid up in the vitellus.

Now, their reproduction in the adult is out of a material elaborated from the food, which does not exist in the embryo.

These formations, thus necessarily different, cannot, I think, mutually elucidate each other's nature. Moreover, we do not yet fully understand many of the relations of embryonic life; for there are organs which then, and then only, have a functional activity. Such are, for instance, the vascular sanguineous glands; and recent investigations into their character tend to show that it is their part to elaborate a *cell-product* rich in albumen and protein, which is turned into the general circulation.* These cells are nucleated epithelial cells, which, by their association in the blood with coloured particles, might become partly coloured. Therefore, it appears to me that the fact of finding corpuscles of different appearances in the blood-liquid of mammalian embryos, is of no importance to the subject of blood-reproduction in the adult animal, in any point of view. In fact, I think it would be somewhat surprising that, in parts so newly formed, and that too out of nucleated cells, there should not then be found, in the vessels, cells of different aspects.

I have given some attention to these phenomena as occurring in very young embryos, and it may be well that I record here what was observed, and especially as it is confirmatory of the general tenor of the views advocated on the preceding pages.

My investigations have been with both the oviparous and the mammalian vertebrates.

We will look to them in their order.

1. *Oviparous Vertebrates.*—In the chick, the blood or nutritive corpuscles first appear in that portion of the germinal membrane soon to become the *area vasculosa;* this is at about the twenty-seventh hour of incubation. It is not, however, until nearly a day after this that the blood-cells can be well and satisfactorily distinguished. They then appear as cells or vesicles, having a variety of sizes and shapes, both nucleated and non-nucleated, and coloured in proportion to their size. They originate in the vitalized material (oil and albumen) of the vitellus, appearing first as small vesicles, without a nucleus, perfectly

* See a very elaborate Memoir by Professor Ecker, of Halle. Annal. des Sc. Nat. tom. viii. p. 103.

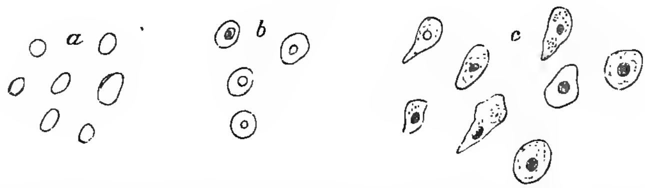
globular, and faintly coloured. These vesicles increase by simple expansion, and their red colour becomes more and more pronounced as their size becomes larger. After having attained about half their full size, a nucleus appears in their centre, apparently from a kind of condensation of the contents, exactly as I have described the nuclear formation of cells in general, on a preceding page; that is, the central portion becomes opaque, which opacity disappears in proportion to the evolution of a distinct nuclear body. Most of the adult cells, therefore, have a nucleus, but generally it is small, and rarely central; in fact, seeming of no service.

When these changes have occurred, you have nucleated cells; but they have not their primitive spherical form, and are quite irregular. I have thought that a part of this irregularity was due to their compression in the newly formed vessels, while their imperfect lenticular character may be due to the fact of their being blood-cells; and this last shape has been gradually assumed as they have been developing.

Thus, it would appear that, when first formed, they originate in the nutritive blastema of the vitalized vitellus; commencing as vesicles, at first colourless, but which afterward become coloured as soon as a circulation is established, and then nucleated.

Now if, after the circulation is well established, say at the end of the eighth or ninth day, the blood be examined, it will be found composed of the same corpuscles, but which have a more regular shape and definite appearance; still, the absence of a *typical uniformity*, as is in the adult, will strike the observer. The whole condition seems to be conventional or provisional. The cells are generally distinctly nucleated; but the nucleus is small and round, and apparently sustains an indifferent relation to the whole as a simple cell. Their aspect is quite epithelial, and I am sure that, were it not for their colour, they would easily be mistaken for epithelial cells.

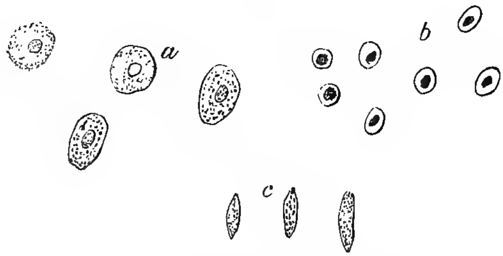
Fig. 8.



First formation of blood in oviparous vertebrates, from *area vasculosa* of chick of thirty hours' incubation. *a*. Vesicles appearing in vitellus; almost colourless. *b*. The same with a nucleus, and coloured. *c*. The corpuscles fully formed and coloured, having irregular shapes. Average size 1-3000th inch. (Magnif. 350 diam.)

This, in fact, would show that they are, as we otherwise well know, simple vitelline products; for, as the ovum, vitellus and all, is, as we have seen, but a great compound epithelial cell, we should expect that the cells elaborated from its contents would take on an epithelial instead of any other type.

Fig. 9.



Blood of embryo chick of seven days' incubation. *a.* Blood-corpuscles coloured, nucleated, and oval-lenticular in shape. *b.* The same, forming, almost colourless and perfectly spherical. *c.* The same as *a*, seen laterally.

In regard to their constant reproduction in the embryo chick, or in fact in any of the oviparous vertebrates, I think it is merely by a repetition of the same processes as those of their formation. Kölliker,* Rees,† and others, are of the opinion that their reproduction is by division or segmentation, each corpuscle producing two by dividing in the middle. I have seen the class of phenomena to which these observers allude, and especially the hour-glass appearance of the globules. But, after considerable observation, I have been unable to satisfy myself that they are reproduced or multiplied in this way. In the young chick, I have frequently seen these peculiar corpuscles in the field, in the different stages of this change. In some, the shape was simply pyriform; in others, still more constricted, so that a small globule appeared connected with a large one; and then, again, the real hour-glass form. But what inclined me to the opinion that this change is not a normal or common one, is the fact, that, in those of a pyriform shape, the nucleus remained unchanged; and also that, in those of the hour-glass form, the nucleus was in one of the parts alone; whereas, in true fissuration, the nucleus is divided as well as the cell. I am therefore disposed to regard these phenomena as I do the crenulation of blood-corpuscles in general, that is, due to endosmose and exosmose.

* Kölliker; Henle und Pfeufer's Zeitsch. iv. 1845–6.

† Rees, quoted in Ranking's Abstract, vol. i. p. 251. Amer. edit.

Fig. 10.

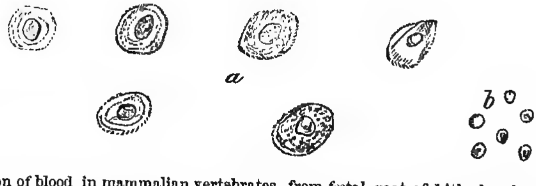
The hour-glass, fissurating appearance of blood-globules in embryo-chick of seven days. *a.* The globules apparently partially divided. *b.* The same unequally so, the nucleus on one side. *c.* The globules before these phenomena occur.

2. *Mammalian Vertebrates.*—In order to see the formation of the blood in these, it is necessary, of course, to take the embryo at a period so early that no vascular connection has taken place with the mother; and as good a time as any is when the bloodvessels are ramifying over the amnion, and just before they have formed any connection with the placental tufts. The blood then found belongs to the embryo alone, and has been elaborated by its own forces. Such was the case with the fœtal goats I examined. In a fœtus three-fourths of an inch in length, and fourteen days old, that is, after conception, the following phenomena were observed.

The blood had only *one* kind of cells. These were nucleated, spherical, having a size of $\frac{1}{1500}$ to $\frac{1}{2000}$ of an inch in diameter. A most careful examination showed the presence of no other forms. Now, what were these cells? To me they appeared only as coloured epithelial ones, and certainly, with the exception of colour, they could not otherwise be described. Their average size was about *four times* that of those of the adult goat.

I am, therefore, of the opinion that the blood-cells of the mammalian embryo are only epithelial cells, exactly as are most of their other individual free cells, and that they have taken the function of oxygenation provisionally, discharging it until the direct connection of the embryo with its mother, exactly as is the case with the embryos of oviparous vertebrates up to the time they escape from the egg.

Fig. 11.



First formation of blood in mammalian vertebrates, from fœtal goat of 14th day, having no vascular connection with the mother, but bloodvessels ramifying on allantois. *a.* Coloured nucleated blood-cells, uniform in size and appearance. Size 1-1500 inch. *b.* Blood-corpuscles of adult goat to show the relative size. Size 1-7000 inch. (Magnified 350 diameters.)

This view of the whole matter makes this entire class of phenomena pretty clear. *The blood of embryos, as such, is of an epithelial nature, and simply provisional; and therefore cannot be compared with the blood of adults, which is elaborated from the food, and which, holding certain typical relations with the animal as a specific individual, the corpuscles have a uniform type-character of their own.*

Both Kölliker* and Wharton Jones,† as above cited, speak of having examined the blood of mammalian embryos. But it appears to me that they have overlooked the fact of the necessity of such examinations being made before any direct connection with the mother had taken place. Thus, Kölliker examined the human embryo of three months, and Jones, an embryo ox of an inch and a quarter long. This can well account for their finding several kinds of corpuscles; as, 1st, the corpuscles of the adult; 2d, coloured nucleated cells; and 3d, uncoloured nucleated (epithelial) cells;—in fact, a mixture of adult with embryonic blood. Moreover, the "granule cells," those both coarsely and finely so, which Jones alleges he found, appear to me to have been only modified epithelial cells in an uncoloured state; or perhaps the cell-products of some of the vascular sanguineous glands, already spoken of.

Thus far, our attention has been directed to the blood of one division of the animal kingdom—the *Vertebrata.* In the other division, however, quite another class of phenomena is met with. As has before been stated, the coloured blood-cell belongs to the vertebrates only. It is with them a special formation, and has a broad physiological signification when viewed in connection with their respiratory apparatus. But in the *Invertebrata*, the anatomical peculiarities do not demand it, as we shall hereafter learn when discussing this point. The blood or rather nutritive liquid of invertebrates has but one kind of cells. These are granular, and closely resemble the chyle-corpuscles of the vertebrates, and of which they can properly be regarded as the analogues. This is so with the classes *Articulata* and *Mollusca;* but in the *Radiata*, this liquid has even a less organized character, for with them it is only the food of the animal in a triturated and partially digested condition, and contains no special corpuscles.

Wagner‡ first suggested the idea that with the Articulates and

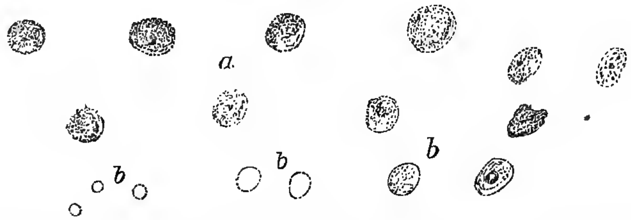
* Kölliker; *loc. citat.*
† Jones; Phil. Transact. 1846, pp. 63–101.
‡ Wagner; Physiology, translated by R. Willis, p. 278.

Mollusks, the blood was *chyle in circulation*, and nothing more. Recently, Prof. Agassiz has taken up this subject, and extended it throughout the animal kingdom. Without discussing the point here, I will only say, that he divides the circulating nutrient liquid into three kinds, viz: *chyme*, *chyle*, and *true blood*. The first belongs to the *Radiata;* the second to the *Mollusca* and *Articulata;* and the third to the *Vertebrata*. This view has much the semblance of truth, and is borne out by many well-ascertained facts. I shall here adopt it provisionally, omitting the consideration of some intricate questions which it involves. If we examine the blood of any of the Articulates or Mollusks which have a distinct circulatory system, we find in it many irregular, granulated, colourless corpuscles, which have some variation of size. These are the only corpuscles it contains, and have been called the "white blood-cells." Anatomically speaking, they consist of a pale plasmatic membrane, or, more properly, involucrum, surrounding a number of granules, in the centre of which last you can, in some, perceive a nucleus. Their shape is generally spherical, but many other roundish shapes are seen. Great numbers of the same kind of granules in a free condition are floating in the liquid. I think it questionable if many of these bodies have a true membrane, or, in fact any covering except one of a simple plasmatic character, such as any group of granules floating in a formative plasma would gather about them. There are others, however, quite small, which have a true membrane. As to their genesis, it appears to me, from the repeated observations I have made, that it takes place exactly like that of the chyle-corpuscle among vertebrates—by the expansion of a primordial utricle into a vesicle, inside of which are collected granules. That this is so, would appear from the fact that in the field may be seen numerous pale vesicles, having a few small granules, and which are apparently these corpuscles in a developing state. After these vesicles have increased by expansion, the granules become more numerous; they may remain in a scattered condition in the cell; or they may collect in the centre, and, by a kind of condensation, form a nucleus, as is sometimes seen. The corpuscles thus formed are colourless, throughout these two classes; in the Annelids, which have red blood, this colour is not due to the corpuscles, but to the liquid in which they float.

In a genetic point of view, the blood of this division, thus simply constituted, has no other peculiarities worthy of mention. The anatomical questions as to the relations of their digestive with their circulatory systems, cannot be properly discussed here.

With this description, I close that which relates to the genesis of normal cells, which exist in the economy, either in an entirely free and independent condition, or are never blended into tissues.

Fig. 12.



The blood of Invertebrates, taken from *Melolontha subspinosa*, consisting of only one kind of corpuscle, colourless, and this resembling the chyle-corpuscle of Vertebrates. *a.* Corpuscles filled with granules, some of them nucleated indistinctly. *b.* Their formation from vesicles, as in the case of the chyle-corpuscles. Although the above is a representation of the blood of insects, yet the same is seen in the other classes, the *Arachnida, Mollusca,* and *Crustacea.*

We now come upon the consideration of the genesis of another class of cells, viz: those which exist in connection with, or united into, continuous tissues—of which they form the elements. This of course includes all of the other special tissues of the animal economy. Such are the *Muscular*, the *Nervous*, the *Fibrous*, the *Cartilaginous*, the *Osseous*, and the *Adipose* tissues.*

III. Nerve-Cells and Nerve-Tissue.

The nervous tissue is the noblest of all in the economy, and, more than any other, has a uniformity of structure throughout the entire animal kingdom. It is composed of two parts, an *active* one, consisting of free cells, and a *passive* one, the agent of the former, composed of tubes, and which may be regarded as the metamorphosed products of the cells. A consideration of the phases of genesis of this tissue, in both its simple and compound states, necessarily involves many points of discussion, which, otherwise situated, would scarcely belong to this work.

As in former cases, we shall, by learning some of the historical relations of the subject, be enabled all the better to comprehend its de-

* The epithelial cell has already been considered: therefore, those tissues which are formed by its metamorphosis, in connection with fibrous and other tissues, cannot again come up for consideration. Such are *Mucous* and *Serous Membranes*, the *Skin and its appendages*, and the *Teeth.*

tails; I propose, therefore, a brief review of the labours and opinions of those who have heretofore studied this subject.

I know of no tissue which has received so much attention as that of the nervous system. This may have been due to the fact that anatomists, recognizing its general relations to the higher conditions of life, have sought a thorough understanding of these last, by the most careful study of its elements.

Then, again, I think it is true that most good working physiologists have a preference in the study of tissues ; and, if I may thus express myself, it has been the good fortune of the nervous tissue, that so many excellent investigators should have had for it such a predilection. As will soon be seen, it engaged the attention of many of the most laborious and trustworthy observers, and this too, at that period when new improvements in means of observation were getting into use, and when, also, quite as much care was exercised in observing, as at the present day.

As is true of nearly all the other tissues, we must, in this case, refer to Leeuwenhoek,* as the one who first described *the elements* of which it is composed. This ancient microscopist has described not only the *fibrous*, but also the *cellular* structure of this tissue. Our astonishment is that, in his day, one should be able to recognize so exactly these parts, and make their proper distinctions. But the instruments of his day did not allow him to perceive any of the *details* of these elements. This statement obtains also for the succeeding hundred years; and although Ledermuller† studied it, he did not make much progress beyond what was then already known. It is true that Prochaska,‡ Fontana,§ and others, have left many descriptions of what they observed in studies of this kind; but, although they may have prepared the way for subsequent discoveries, they certainly laboured under some optical illusions, besides mistaking abnormal for normal appearances. It was the opinion of Fontana, that the nerve-fibres were tubes, in which there existed an actual circulation.

But it was not until after the commencement of the present century that observations were made possessing much importance. In 1816, Treviranus|| made some advances; and, although he was in-

* Leeuwenhoek; Opera, tom ii. p. 309.

† Ledermuller; Microscopische Gemuths-und Augenergoetzungen, Nuremberg, 1763, p. 63.

‡ Proschaska; Structura Nervorum, Vienna, 1779, p. 68.

§ Fontana; Traite sur le Venin de la Vipère, Florence, 1782, tom. ii. p. 202.

|| Treviranus; Vermischte-Schriften, Göttingen, 1816, p. 128.

clined to adopt the views of Fontana, as to the anatomical relations, he rejected the idea of a circulation. Moreover, I think he was the first to declare that the cellular and fibrous parts have the same elementary constitution.

As yet, the difficult points remained to be settled, and I cannot conceive that, with the instruments then in use, this could be done satisfactorily.

About the year 1830, a new impulse was given for researches of this character, by the improvements in microscopes; and, as Henle* justly remarks, it led to nearly as many errors as truths on all these subjects; but they were instructive errors, and, as we shall soon perceive, were the precursors to finally correct opinions.

In 1833, Ehrenberg† took up the study of the cortical substance of the brain, and recognized, beside the nerve-cells and tubes, what appeared to be a third element, apparently consisting of rows of transparent globules, united by a delicate film. These are the now well-recognized varicose tubes, their beaded aspect being undoubtedly due to pressure, the very slightest of which is sufficient to produce it upon their filmy walls. This gave rise to a lengthy discussion, as to the nature and character of these beaded tubes—and there can be no wonder that the whole should still have remained so unsettled, when we consider that the appearance being artificial, each observer was likely to meet with dissimilar phenomena.‡

Treviranus§ was the first to declare positively that these appearances were due to an artificial condition. In this, he was well supported by Valentin|| and others; and subsequently, Müller, Remak, and others renounced their old and opposite view for this.

The discovery which Valentin¶ soon after made, of the delicate membranous envelop of the nerve-tubes, tended to give clear ideas of the real nature of the above-mentioned phenomena. For, in the presence of this membrane, could be traced the various beaded appearances. The work of Remak,** which appeared soon after, and

* Henle; Traité de l'Anat. générale. Trad. par Jourdain. Paris, 1843, tom. ii. p. 340.

† Ehrenberg; Poggendorf's Annalen, xxviii. p. 451.

‡ Among those who participated in this discussion may be mentioned the names of Müller, Volkmann, and Remak.

§ Treviranus; Beitraege zur Aufklaerung, &c. &c. Bremen, 1835–7, ii. p. 25.

|| Valentin; Ueber der Verlauf und Enden der Nerven, Bonn, 1836.

¶ Valentin; *loc. citat.*

** Remak; Observationes anatomicæ et microscopicæ de Systematis nervosi Structura, Berlin, 1838.

which is justly entitled to the name of comprehensive, opened a new field of discussion from the peculiarity of some views there entertained. Remak thought that the nerve-tubes or fibres consisted of three portions: 1. Of an exterior envelop of cellular tissue, which, by its local increase here and there gave the knotted aspect to the whole; 2. A delicate membranous tube, lining the first; and 3. A broad flat band lying within the last. This view was immediately attacked by Valentin;* and soon after, Schwann,† in his great work, pointed out its true meaning, and fully showed wherein lay the error. But he did this by falling back upon his cell-doctrines, and seeking its true explanation in the genesis of those parts from pre-existing nucleated cells. In fact, I think it would have been difficult to have reached the point in any other manner. But, notwithstanding what Schwann accomplished—clearly recognizing many of the relations of the contents of the tube, yet the whole was not yet fully explained, and some phenomena were often presented apparently not reconcilable with the above view. It was perhaps on this account that, about the same time (that is, of Remak's work), Purkinje‡ expressed a still different view. He thought that the tubes had three primitive elements: 1. An exterior sheath; 2. A soft medullary mass, and 3. A central portion, transparent, and which he termed "*axis cylinder.*"

If I correctly understand the matter, this *axis cylinder* of Purkinje corresponds in some respects with the *flat central band* of Remak. But I must confess, with Henle,§ that this point is far from being clear.

But the subsequent researches of Henle‖ have made this matter pretty distinct, for he has pointed out the cause of the error. The conclusion of this matter is this: The nerve-tube, when examined in a very fresh state, is found to consist of a sheath inclosing *liquid* contents of a pellucid aspect. In this liquid, however, changes soon occur. A kind of coagulation of it is perceived; and that portion of it next to the sheath becomes of a white homogeneous aspect, constituting the "white substance of Schwann." But the central portion remains clear, and has the aspect of a nearly transparent axis. This is the *axis cylinder* of Purkinje. When the nerve-tube is subjected

* Valentin; Repertorium, 1838, p. 73.

† Schwann; *loc. citat.* p. 174.

‡ Purkinje; Bericht ueber die Versammlung in Prag, 1838, p. 177.

§ Henle; Traité d'Anat. génér. &c. tom. ii. p. 344.

‖ Henle; *loc. citat.* tom. ii. chap. xii.

to even very light pressure, this last is forced out, and assumes a variety of globular and varicose appearances.

As to the real nature of these phenomena, I shall express an opinion when giving an account of my own observations.

Those discussions, therefore, which seemed to be required in order to get at the truth in these subjects, may be said, as far at least as relates to the nerve-fibres, to have ended some years since; and this was perhaps with the labours of Henle and Valentin.

It now remains, before closing this historical sketch, to notice the progressive condition of the other element of this tissue in an enlarged state—the *ganglionic vesicles* or *cells*.

The existence of these bodies did not, on account of their size, escape the attention of the older anatomists who used the microscope upon the nervous tissue; but their nature or use was not at all understood. And although Ehrenberg* had noticed and figured them, yet the first good description of them was given by Valentin.† He not only described them, but noticed their caudated prolongations, and alluded to their similarity of form to that of the egg.‡ Subsequently, Schwann pointed out their cell-relations; and since then they have not been a matter of dispute, excepting as to their anatomical relations or connections with the nerve-tubes. That some direct connection of such kind did exist, was first inferred from their caudated prolongations, which seemed to be very convenient to attach nerve-tubes. This was first *suggested* by Remak,§ and since then, other observers‖ have advocated the same view. Still later, Kölliker¶ has advanced the same view, stating that in the lower animals each of these vesicles may give rise to *more* than one tube, but that in the higher, a single efferent fibre is the rule.

This is a point to which I have given some attention, and in closing the historical review of this tissue, it may be well to allude to my own views of the matter.

Some time since, I made a series of examinations of the human brain, and that of other animals, to ascertain for myself the real nature of this point. I found *no* appearances justifying the opinion that a *direct connection* ever exists between the ganglion vesicles and

* Ehrenberg; Poggendorf's Annalen, 1833, xxviii. p. 458.

† Valentin; *loc. citat.* 1836, p. 77.

‡ Valentin; Nov. Act. Acad. Leopold. xviii. p. 196.

§ Remak; Observ. Anat. Microsc. de Syst. Nerv. Struct., Berlin, 1838.

‖ Will, Todd and Bowman, Kölliker, &c. &c.

¶ Kölliker; Siebold and Kölliker's Zeitschrift, 1849.

the nerve-tubes, either by the latter joining on to the nucleus or to the caudations of the former.*

I will add, that such is also the conclusion at which Professor Wyman,† of Cambridge, has arrived, from his researches into the nervous system of the Batrachians, and in which the above point received a special consideration.‡

Such are some of the historical relations of the nervous tissue, by the keeping in mind of which we shall the better understand its present condition, both as regards structure and function.

At present, we recognize this tissue as consisting of two elements —nerve-cells (including, of course, ganglionic vesicles), and nerve-tubes. The former is the true histological element, and out of which the latter is only a metamorphosed form. The genesis of each, according to my own observations, now engages our attention.

The formation of the nerve-cells occurs quite early in fœtal life. At first, the whole tissue, whatever is to be its subsequent condition, consists of cells. These are nucleated, and have an exceedingly delicate membrane, inside of which there are generally fine granules. In Mammalia, they are of about 1-2000 of an inch in diameter. Whether they are the vitelline cells metamorphosed, or whether they are new formations from the beginning, out of these last, my own observations do not allow me a decided opinion, for I have never been able to examine sufficiently young embryos in a satisfactory manner.

I am inclined, however, with Valentin,§ to the latter opinion.||

* It is true, that I have not repeated the observations of Wagner upon cartilaginous fishes; and from the full statements of so excellent an observer as Kölliker in the affirmative, the question is indeed an open one; see his Mikroscop. Anat. Bd. ii. p. 390, *et seq.*

† Anatomy of the Nervous System of Rana pipiens.—Smithsonian Contributions to Knowledge, Washington, 1853.

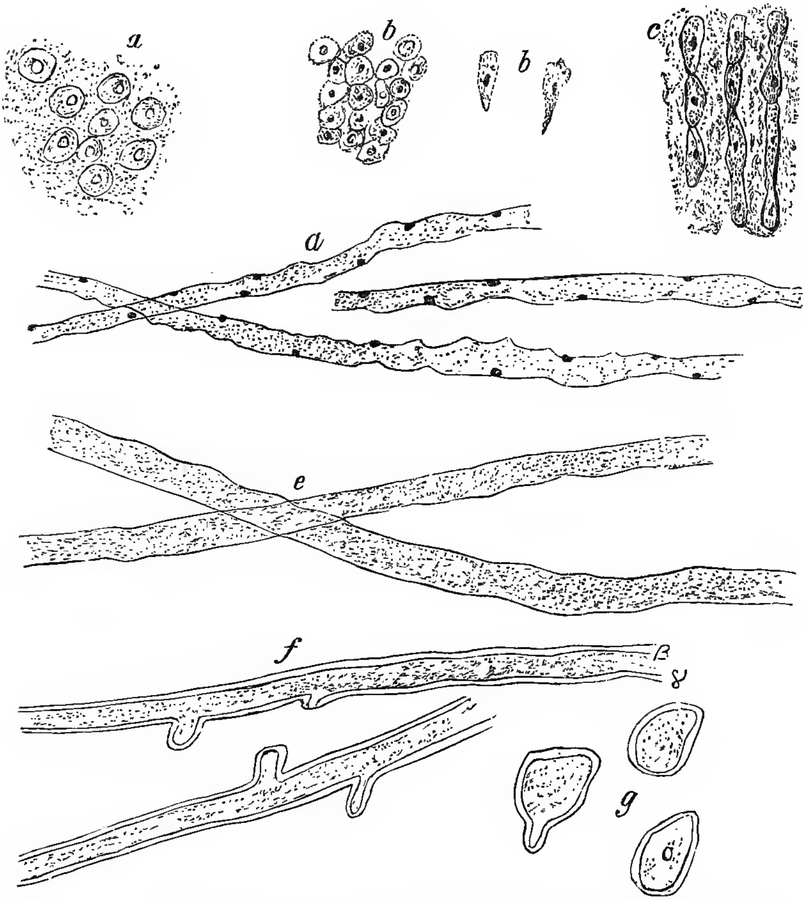
‡ For additional writings on the relation of the ganglionic globules to the nerve-tubes, see especially Hannover (Recherch. microsc. sur le Syst. Nerv. 1844, p. 69, pl. viii. where he has described and figured them from Helix and Limax); Helmholtz (De fabric. Syst. nerv. evertebrat. p. 10), and Will (Müller's Arch. 1844, p. 76). Hannover is quite in favour of the view of their direct material connection. See also Müller's Physiology, Jourdan's French Translat. Littré's Edit. Paris, 1851, vol. i. p. 560, where the direct connection of the ganglionic globules and the fibres is urged with considerable weight. See also a review by me, of Prof. Wyman's work above quoted, in Silliman's Journal, Sept. 1853, where this subject is touched upon, and its historical relations given.

§ Valentin; *loc. citat.* Entwickelungsgeschichte, &c. p. 183.

|| As to the genesis of the ganglionic vesicles or cells, it is the same as that of the common cells. As I have before remarked, they are these last in a hypertrophied condition. This opinion is founded upon the fact that I have traced them, in point of

These primitive nerve-cells have a remarkably uniform aspect, and are generally situated in a light gray, punctiform blastema. In the

Fig. 13.



Nerve-cells; their transition into tubes, and the appearance of the nerve-tubes of the cerebro-spinal system. The first three figures from brain and spinal cord of fœtal goat of 14 days; the remaining ones from nervous system of tadpole. *a.* Nerve-cells from brain of fœtal goat, their formation occurring according to "my own mode." *b.* The same, from spinal cord, beginning to arrange themselves in a linear manner; *b's* caudate cells. *c.* Still farther arranged, and tubes appearing. *d.* Tubes formed; the nuclei of the old cells remaining in the walls. *e.* A nerve-tube from a spinal nerve, without double contour. *f.* Nerve-tubes from same, with double contour. *β.* Axis-cylinder. *δ.* White substance of Schwann. *g.* Globular and cell-like forms assumed by expressed portions of axis cylinder.

embryo of a goat, half an inch in length and fifteen days old, these cells had already formed in that part which was to be the

size, all the way from the former to the largest of the latter. But as to the point whether *any* cell may attain this development, or whether it is the peculiar destiny of a *few*, I cannot say.

cerebrum. In the spinal cord, they had just passed on to the condition of fibres; still, they had left their marks behind, and it was easy to perceive the phases of the change. It would appear that the first step of this fibre-change is a partial arrangement of these primitive cells in a line or series. Then their delicate walls coalesce, and a continuous tube is formed, the nuclei of the component cells still persisting. At first, this tube has a most delicate appearance, but afterwards it may assume a more opaque aspect by the appearance of granules on the wall.

I have said that this tube is formed by the coalescing of the walls, but, as far as my own observation goes, this is a point which has not been demonstrated. On many accounts, it is a matter of just inference; but I have often thought that it might well be formed by a plasmatic membrane, enveloping a row of cells lying contiguous, or nearly so, to each other. At any rate, the persistence of the nuclei in their walls, for a brief period after their formation, shows their cell-origin. It would appear that, subsequently, these nuclei disappear for the most part, and there is then left a delicate tube with transparent contents. The double contour of this tube is due without doubt to the portion of the contained liquid lying next to the sheath, becoming more opaque than the rest, producing the "white substance of Schwann." When the liquid contents of these tubes escape, they very naturally assume a globular form, and their exterior portion in contact with the surrounding liquid in which they float becomes coagulated, and the whole retains its original form. Afterwards, a coagulation of that portion of the liquid of these globules lying next to the periphery may occur, and so these globules may have a double contour as well as the nerve-tubes themselves.

The diameter of these primitive tubes, thus formed, is probably about that of the primitive cells; but after manipulation, however careful one may be, there appears to be a deviation from their original aspect. The fluid contents of the tubes escape either partially or wholly—in the former case, giving it a very irregular appearance, and in the latter, allowing the membranous sides to lie against each other, giving it more the aspect of a delicate thread. But I cannot think that these last delicate thread-like fibres are ever formed as such from the beginning, or, as Remak has supposed, that they are the "*earlier* forms" of the larger fibres. The notions entertained by some of the earlier microscopists, that these tubes formed a circulating system, arose from the globular forms which the contents of these tubes often assumed, exactly as would occur if they had made their escape. I scarcely need say, therefore, that all such appearances are artificial.

As I have already remarked, it was the opinion of Valentin, formerly, that these delicate tubes are the sheaths proper of other tubes of a more opaque character. This opinion has since been clearly shown to be erroneous, and I may add, that what I have recently seen, in my own observations, is to the same effect. Thus, in the gradual development of the nervous system in the tadpole, you have first the primitive cells, and then, in the midst of these, and by changes just described, appear these delicate tubes, without the intervention of any other.

Such are the phases of genesis of the primitive nerve-cells, and their transformation into the white nervous fibres, as I have traced them in fœtal life.

As for another set of fibres, found and described by Remak—the gray or *so-called organic* ones—they need not here detain us long. In showing what they are I cannot do better than to quote the words of Remak,* who was among the first to study them. He says: "They are not tubular, that is, surrounded with a sheath, but naked, being transparent, almost gelatinous, and much more minute than most of the primitive tubes. They almost always exhibit longitudinal lines upon their surface, and readily separate into very minute fibres. In their course, they are frequently furnished with oval nodules, and covered with certain small oval or round, more rarely irregular, corpuscles, which exhibit one or more nuclei, and in size almost equal the nuclei of the ganglion globules."

The opinion of Müller,† who followed Remak, is to the same effect. With such a description, they would certainly seem to be entitled to the name of a distinct set of fibres. Moreover, they are found in the nervous system of organic life in the ganglia. But within a few years doubts have been raised as to the nature of these fibres as thus described; and there are good reasons for now believing that they are rather a form of simple fibrous tissue than true fibres of nerve. I do not propose to discuss the matter here, for I have not investigated to that extent, some of its points, that I should wish to do. But, as yet, my opinion coincides with that of Henle, who regards the common gray nerves as only solid cords of nervous fibres like those

* Remak; *loc. citat.* 1838, p. 5.

† Müller; Physiologie du Système Nerveux, &c. trad. par Jourdain, Paris, 1846, tom. i. p. 126.

belonging to animal life, and therefore that the fibres noticed by Remak and others belong to a different tissue. Such, at least, is what I should deduce from my own limited observations; and as such, the genesis of these bundles of primitive fibres would be the same as that of the same fibres in an isolated condition.

But besides the *true nerve-fibres* (if I may thus designate them) and those *reputed ones* of Remak and Müller, there exists another set, found in the ganglia of the sympathetic system; but they are not peculiar to it alone, for they also occur, but more rarely, in the cerebro-spinal tissue. They are distinguished from the first by their smaller size (being about one-third), and their even, smooth appearance, having all the aspect of a small tube filled with homogeneous minutely granular matter; moreover, they want the double contour of the former. These may be called the *organic nerves*, and this in contradistinction to those fibres of Remak, or those bundles of *ordinary nerve*-fibres which apparently constitute the gray nerves, and which are only a conventional aggregation of the common nerve-tubes.*

The genesis of these *organic nerves* takes place, according to my own observations, in very much the same way as that of the others we have just described. There is this difference, however, that in the common nerve-fibres there appears to be a kind of liquefaction or clearing up of the contents of the cells after they have united to form the tube; and this produces that glass-like transparency for which they are noted. But in the organic fibres, the cell-contents remain,

* A few years since, the question whether or not there really existed a set of fibres peculiar to the sympathetic system, was discussed by some of the continental observers. The conclusion at which they arrived is as I have stated above, viz., that the *gelatinous* fibres of Remak and Müller are not nerve-fibres, but that the nerve-fibres peculiar to the sympathetic system, or nearly so, are such as those of which I have just given the description.

See Kölliker, Die Selbstandigkeit und Abhandigkeit des Sympathischen Nervensystems, an abstract of which is in Mr. Paget's Report on Anat. and Physiology, Brit. and For. Med. Rev. July, 1846.

After all, there is some obscurity about the matter, if we take Remak's statement; for he says that the fibres described by him are much smaller than the primitive nerve-tubes. In this respect they would correspond to the *true organic fibres* I have just described. But in all other particulars they do not agree. Is it not probable that Remak is mistaken in his idea of their size?

For some special details on this subject, see Kölliker, Mikroskop. Anat. ii. p. 528, and Müller's Physiology, transl. by Jourdain, 2d ed. by Littre, Paris, 1851, vol. i. p. 563.

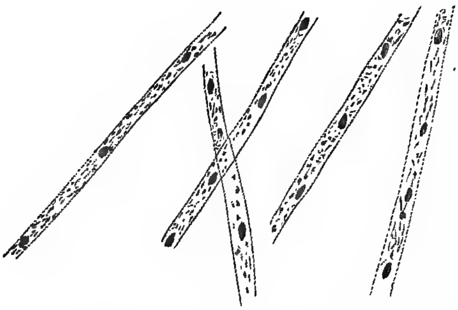
This is a point which studies on the lower animals will tend much to elucidate; and see the writings of Leydig in Siebold and Kölliker's Zeitsch. f. Wissensch. Zool. *passim.*

the nuclei are partially persistent, and therefore the tube presents a grayish-white homogeneous aspect.

The figures which Schwann has given correspond quite closely with what I have observed.

Such are the more general genetic relations of the nervous tissue in both its elementary and compound conditions. As I have before remarked, the greatest uniformity throughout the animal kingdom exists. It is true that among some of the invertebrates the nerve-tubes may not have the same aspect, but homologically they are the same, arising from cells which everywhere have not only an identity of function, but of aspect also.*

Fig. 14.



Gray or gelatinous nerve-tubes, from sympathetic system of a snake (*Heterodon Platyrrhinos*). The presence of nuclei in the tubes shows clearly their cell-origin. Compare the figures I have given with those of Schwann.† (Magnified 350 diameters.)

IV. Muscle-cells and Muscular Tissue.

This tissue has a universality second only to the one we have just been considering, and like it, also, has an homologous structure wherever occurring. But, besides these similarities, there is another, an histological one, and which did not escape the penetrating mind of Schwann. So that it may be said that the muscular and nervous tissues resemble each other in nearly every point of view more closely than any others in the economy. They are properly, therefore, considered in succession.

But we will first refer to its history and criticism, which will enable us the better to appreciate some of its important bearings.

* With some of the Acalephæ, the nervous tissue consists of cells only, and instead of nerve-tubes there are beaded rows of cells—an illustrative histological point. See Agassiz, Mem. Amer. Acad. iv. 1850, p. 232.

† *Loc. cit.* Syd. Soc. ed. pl. iv. fig. 8.

And in so doing, I cannot pretend to take up a retrospective view of it in all its common histological relations, for these would be of but little interest. But its history and criticism, as a *cell-tissue*, is what concerns us. This, of course, does not date back farther than the time of those numerous and valuable histological researches which just preceded the ever-memorable labours of Schwann. Krause* recognized that the primitive fibres were composed of a series of globules. The nature of these, however, he did not fully understand, and the same is true of other investigators of that time; but as clear ideas of its essential structure could only be founded upon a knowledge of its developments, Valentin† appears to have been the first who studied this tissue in an intelligent manner. He recognized the primitive fibre-cells in the ovum, and their successive metamorphoses until the formation of the fibre itself; also the arrangement of the granules, until, he says, they appeared like a string of pearls. In fact, Valentin appears to have proceeded as far in these studies as he well could, considering that he did not then recognize the grand laws of cells which were soon to be brought out by Schwann.

Schwann combined the labours of those who preceded him with his own, and thereby the force of the problem of the genesis of a tissue which remains so short a time in its cell condition, was solved. He pointed out its *necessary* cell-origin, and the phases which, from this last fact, it would pursue. These facts could not be better established, and I am not aware that any advance has since been made.‡ The researches soon to follow could be little else than confirmatory of these first, or such as would relate to its function and mode of operation.

This last (the *modus operandi*) required studies as to its granular

* Krause; Handbuch der Menschlichen Anatomie, Hanover, 1833, p. 57. Another edition of this appeared in 1841.

† Valentin; *loc. citat.* Entwickelungsgeschichte, p. 268.

‡ It may be well to allude to the opinions of Valentin on the genesis of this tissue, and which have been published subsequent to the work quoted above. He thinks that in the interior of the tube produced by the fusion of the cells, there is a central cavity, which remains throughout life. This cavity at first contains the nuclei of the cells inclosed, but after the absorption of these, it is filled with a gelatinous liquid. Between this central cavity and the wall of the tube, are developed the fibrillæ around the nuclei of the cells (what cells?), their increase in number going on at the expense of the cavity. These fibrillæ at first are pellucid, but afterwards become granular, and suddenly assume a striated aspect.

Vide Berlin Encyclop. vol. xxiv. 1840, p. 212; and Müller's Archiv, 1840, p. 204.

I scarcely need say that Bowman's results have shown the erroneousness of this view.

constituency, or the arrangement of the granules in the interior of these primitive cells thus coalesced, giving rise to the striated aspect. In 1840, Mr. Bowman* published his comprehensive memoir, in which the whole subject was most ably treated, and the paper has since been one of reference in this and other languages. Bowman showed that the elementary structure of the muscle is such, that it is not only capable of being divided lengthwise, thereby forming threads, but also cross-wise, forming disks—thus showing that it is composed of particles of equal size, united and arranged in two directions, each one standing equidistant from the surrounding neighbouring ones. This view, sufficiently substantiated by the care of the author's (Mr. Bowman's) own researches, has been since confirmed by many good Microscopists. But a corresponding view which it involved, has not been so satisfactorily made out. This was the view that each of these particles was a cell, and therefore that the whole muscle was only a bundle of small cells, arranged after a regular manner. This view was advanced by Drs. Carpenter† and Sharpey,‡ and is based upon appearances they have witnessed upon prepared specimens. As I have just stated, this point has not been equally clear to others; and, in my opinion, as I shall soon show, it is rather a forced interpretation of appearances.

When a subject had been so thoroughly wrought out, as this had, in its earlier years, it cannot be strange that recent times have not much advanced its condition. And so, since the memoir of Bowman, I do not think that much has been done to change the face of the matter. To be sure, much has been written upon debatable points, and to show that optical illusions have often been mistaken for real appearances.

The *Comp. Rendus*, of 1847, contains several memoirs upon this subject, and especially that of Prevost, in which the genesis of this tissue, with many peculiarities, is traced over a wide range of material.

But, by far the most comprehensive work in this direction, of late years, and in which the finer and more delicate points are wrought out with the improved means we now possess, is the memoir of Le-

* Bowman; Philosoph. Transact. 1840.

† Carpenter; A Manual, or Elements of Physiology, &c. London, 1846. Also, his other more recent physiological works.

‡ Sharpey; Human Anatomy. By R. Quain. Edited by Wm. Sharpey. Second edition, London, 1846.

bert.* And I am happy in saying that subsequent investigations of my own have elicited the same results; and I place the more reliance upon these, since drawings which I made, before the work of Lebert was accessible to me, correspond quite closely with these last.

But however accurate and careful may have been the researches of Lebert, they have a conformatory, rather than an original value, and may be regarded as bringing to the test and showing the truth of the earlier opinions of Valentin and Schwann.†

What these were, may be learned from a description of what I have wrought out for myself, and which will now engage our attention.

If we confined our range of study of this tissue to the higher animals, in which it can scarcely be said to exist at all, except in an ulterior condition, our knowledge of it would be imperfect. We shall do well, therefore, to commence with the lower forms in which it persists in its very earliest stages, that of eels, and as such performs its functions. We find it in this condition in some of the Radiata. Such is the muscular tissue of the pedicle of the attached medusa-form of *Tubullaria*, and, when examined, will be found to consist of cells arranged in a linear series. When the pedicle is in a state of relaxation, these cells are spherical, and are filled with granules. The contraction results from simultaneously becoming flattened, or disk-shaped—their long diameter being at right angles with the way of contraction.

Here you have motion due to the direct agency of cells; in other words, the simplest form of active muscular tissue which is of *cell-origin*. But a muscular or motor tissue may be even more simple than this. In the instance just cited, we have seen that the cells were filled with granules; and I am convinced that to move-

* Lebert; Annal. des Sci. Nat. 1849, et Mai, 1850.

† The following are the names of some papers on the genesis and intimate character of the muscular system, which have appeared within a few years:—

Stannius; Müller's Archiv, 1847, p. 440.

Cramer; Guy's Hospital Reports, vol. vi. part i.

Dobie; Annals of Nat. History, Feb. 1849.

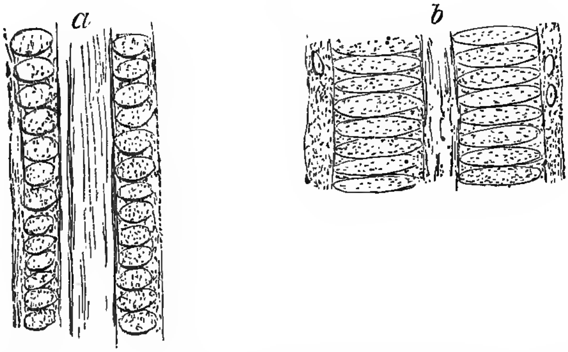
Lebert; Comp. Rendus de la Soc. de Biologie. Paris, 1849, p. 76.

For the Histology, Anatomy, &c. of the Muscular System in all the classes of the animal kingdom, with many interesting details, see Comparative Anatomy by Siebold and Stannius, trans. &c. by Burnett, under the head Muscular System of each class.

See, also, several anatomical and physiological text-books, especially those of Carpenter, Sharpey's edition of Quain, Kirkes and Paget, and Jourdain's translation into French of Müller.

ments of these last is due the change of form in the cell, producing contraction. By a kind of mutual attraction which they take on, they become more closely aggregated; a decrease of space is the result, and the cell-membrane containing them follows their movements, and at the same time brings their lever to produce the secondary results. It is therefore evident that the movements of these granules do not depend upon their being enveloped by a membrane, although the possession of this may increase the definite character of the motion.

Fig. 15.



The *cell*-muscle of the pedicle of the attached medusa-form of *Tubullaria* (nov. sp.), showing the contraction due to cells. *a.* Natural relaxed state. *b.* The state of contraction by flattening of cells.

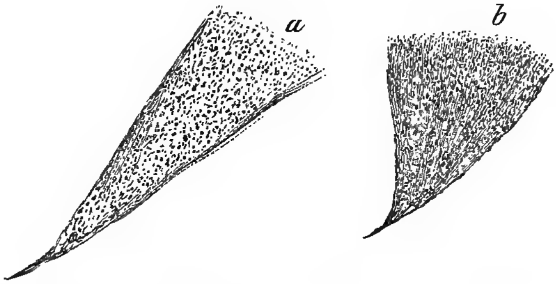
This even more simple form of tissue to which I have alluded, is found in many Bryozoa, and in the integument of many Entozoa, and especially of the class Nematoidea. It consists of a collection of granules, which are not allocated by an investment of cell-membrane.

In the genus *Alcyonella* of the Bryozoa, if the muscle which moves one of the tentacles be examined, it will be found of a fusiform shape, and consisting only of a sac of that form, containing these primitive granules; and, as I have repeatedly watched it, the contraction takes place sequent upon a mutual attraction of these granules, by which they occupy a less space.

We can conceive of no motion simpler than this, and this may be taken as the ultimate expression of muscular movement, divested of those levers and other means for the increase of power which belong to the compound forms of this tissue, giving it, in fact, its peculiar composition. But let it be remembered that *in this mutual attraction of these primitive granules lies all muscular contraction, whether it be in the lowest or highest forms.*

As we advance into higher forms, we perceive that not only has this tissue advanced to the condition of being definitely arranged and allocated by cell-membranes, but these have coalesced so as to form tubes, as is the case with the muscles of *organic life;* but in the muscles of *animal life*, in which this tissue has reached its highest stages of metamorphosis, there is a step in advance of this; for, not only have the cells coalesced into tubes, but their contents, the granules, have become definitely arranged, giving rise to the striated aspect.

Fig. 16.



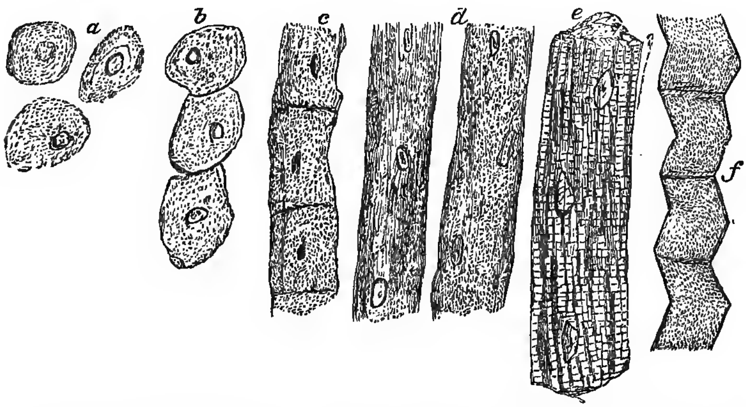
The tentacular muscle of *Alcyonella* (nov. sp.), being composed of a fusiform sac filled with granules. *a.* Natural or relaxed state. *b.* State of contraction.

We will now look to the phases of this muscle formation. In the young embryo, nucleated cells are seen in those localities where the muscular tissue is to appear. I think, with Valentin, that these are formed from the vitelline substance, and are not metamorphosed vitelline cells; at any rate, they are not easily distinguished from those about them which are to form other tissues, if we except their little larger size. Their first change towards a condition of organization above that of cells, consists in their tendency to be arranged in a linear series, or rows. There then appear rows of nucleated cells, having granular contents; next, these cells coalesce and form tubes, at the same time their contents becoming more thoroughly granular. There are, then, at this stage, tubes with homogeneous granular contents, with the nuclei of the component cells still persistent. It is on this stage of development that the fibres of the *organic* muscles rest, and the above, therefore, constitutes their description.

The development still continuing, these fibres, thus formed, increase in size by interstitial deposition of granular matter; at the same time the nuclei, and other remains of their cell origin, for the most part disappear. Then the granules, thus accumulated, begin to be grouped in a regular manner, and when this has been accomplished, the whole presents a very uniform, regular aspect, constituting the true *striated* muscular fibre; but, when fully formed, it is only in those belonging to the lower animals that any traces of the original cell-structure can be perceived. Certainly, in Mammalia, there is no trace left that can be perceived in the normal state. Still, however highly metamorphosed, this tissue does not lose its original cell-traces. For, in a condition of it produced by defective nutrition, and which may be properly termed *retrograde metamorphosis*, consisting in the unravelling of its structure, the marks of the old cells are seen; exactly, if I may use the comparison, as when a house goes to decay, its finish is stripped off, and you see, what was before hidden, that the rafters and beams have composed its real structure.

There is another appearance which is sometimes seen, and which, as it shows beautifully how the traces of the ancient cells perish, may well be mentioned here. I refer to the plication of the fibre often observed in specimens of boiled beef. It is plicated or folded upon itself. But this plication takes place at the weakest point, which is at the union of the old cells with each other. Therefore, the length of each plication should be that of the primitive muscle-cell; and in a

Fig. 17.



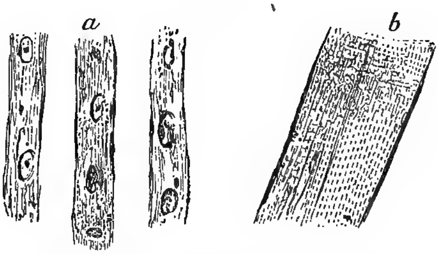
Muscular fibre and its formation; from very young tadpole. *a*. Muscle-cells as they exist in the embryo. *b*. The same, linearly arranged. c. Coalescing and forming a cylinder. *d*. Cells completely fused, but nuclei still persistent, the granular matter unarranged constituting the muscular fibre of organic life. e. Still farther progressed, the granules definitely arranged forming *striæ*, but nuclei visible here and there, constituting the muscular fibre of animal life. *f*. A plicated muscular fibre of boiled beef, the plication taking place at the junction of the ancient cells. The appearances of muscular fibre of the higher animals, and in which no nuclei are visible, I do not figure, for any one can easily see it. I have given only its histology.

specimen which I recently saw, and made the measurements, I found this to be correct.*

It will be seen by the above description that the muscular *fibre*, and *not* the *fibrilla*, is the true embryological cell-element, the production of the *fibrillæ* being an artificial division, exactly as is the transverse cleavage. Moreover, it will be seen that the beaded character of this *fibrilla* is due to a regular arrangement of granules, rather than to a longitudinal series of cells.

In the formation of the fibre and its development, as its increase takes place by interstitial growth or the internal deposition of granules, it would follow that those of the embryo would be much smaller than those of the adult. That this was really so, was made out by Harting,† some time since, from comparisons he instituted between the fibres of embryos and their adults. Recently, however, I have had occasion to arrive at the same result. The muscular fibres of the *gluteus maximus* of a goat of two months (the natural term is five months) I found to be only one-fourth the size of those of the adult. From the phases of development, as I have just described them, of the two kinds of muscular fibre, viz., *animal* and *organic*, it would follow that the size of the latter should be much less than that of the former; in fact, it ought to be exactly what the former is before the changes for striation take place, or as it is in the embryo. Such, indeed, it is in man and Mammalia, as far as my observation goes—being about three or four times smaller.

Fig. 18.



Muscular fibres of a fœtal goat of two months, from gluteus maximus, showing relative size with that of the adult. *a*. Muscular fibres from gluteus maximus of fœtus. Size, 1-1800. *b*. Muscular fibres from the same of adult. Size, 1-500. (Magnified 350 diameters.)

The fact of the fibres in the adult being so much larger than those in the embryo, may explain how the increase of the muscular tissue,

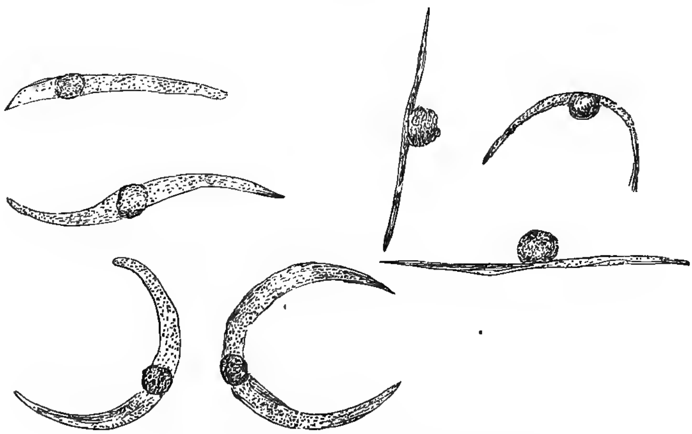
* Henle gives a figure of the same appearance. *Vide* Traité d'Anat. générale, &c. pl. iv. fig. 4, F.

† Harting; quoted in M. Paget's Report on Anatomy and Physiology. Brit. and For. Med. Rev. April, 1846.

as a whole, takes place, either from the natural growth of the body, or from continued exercise. For, as there is no evidence that muscular tissue is developed *de novo* except in the embryo, the increase in size, for instance of the *biceps* and *triceps* of the arm, after exercise, is due to the *interstitial* growth of fibres, which from disuse have hitherto remained in their embryonic condition.

As the subject of muscular tissue will not again come up in this work, its function being too evident to be separately discussed in a following chapter devoted to *function*, I may properly here notice another point of much interest and importance. I refer to the *dependent* relations existing between the nervous and muscular tissues. As is well known, it was the opinion brought forward by Haller, that the muscular tissue had an inherent contractility, aside from any influence of the nervous system. Subsequently, however, this view was disputed, it being argued that, in the cases in which *detached* portions of this tissue exhibited contractile movements, these were due to the presence of a small bit of nerve in them, and which was the agent of the action. Within a few years, however, and since investigations of this kind have been more minute, and extended to the lower animals, phenomena have been seen justifying the view of Haller, although based upon different and much more scientific data. The nerves, in their distribution in the muscular tissue, do not enter the fibres themselves, but form anastomoses on their surface. If, therefore, isolated fibres can be seen, in the field of the microscope, to contract and relax, it is evident that such

Fig. 19.

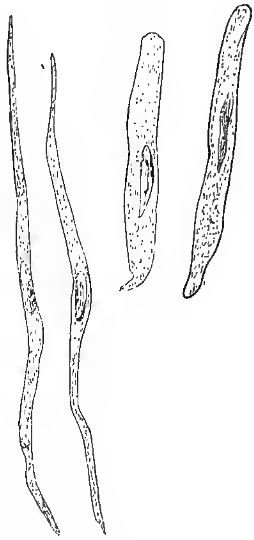


Muscular fibre-cells, ad. nat. del., from human spleen.

motion cannot be due to nervous influence (for there are no nerves), but rather to the inherent power of the tissue itself.

This was seen some time since by Mr. Bowman,* and more recently by Professor E. Weber;† and still later, I have had the good fortune to observe it several times while making some researches upon the viscera of insects; and, as I had in my memory the observations of the two above-named observers, I watched it carefully, and was quite satisfied that it thus occurred.

Fig. 20.



Muscular fibre-cells from the iris. Copied from Lister's paper.

Moreover, in the muscular tissue of *Tubullaria* above mentioned, the contraction can be distinctly observed to take place through the agency of the cells and their contents; there positively being no nervous filaments whatever entering the organs.

As physiologists, we may with candour ask, why should not simple cells or utricles have this power instead of its being entirely referable to a production of cells in the brain or nervous system? We can as well conceive of a muscle-cell having this power as a brain-cell. It is true, however, that in the more important adaptive motions of life, there is necessarily presupposed a unity of power under the guidance of volition or its equivalent.‡

* Bowman; *loc. cit.* Phil. Trans. 1840.

† Weber; Archiv d'Anat. générale, Janv. 1846.

‡ In the preceding account, I have made no allusion to a peculiar celloid tissue,

V. Fibre-cells and Fibrous Tissue.

Under this head, I include all those forms of tissue commonly known by the names of *cellular* and *areolar;* for microscopy has wonderfully simplified our knowledge of its characters. It is true that the relations it sustains in the economy are mostly those of a mechanical nature, and on this account it might seem proper that its consideration should be omitted. But then, again, owing to its very simple character, its presence is almost universal, and of all the tissues, it is the one most easily and rapidly reproduced. On a future page, we shall learn that its pathological relations are of the most important character, and for this reason alone, if not for others, it should here be considered in its normal histological conditions.

Functionally, full as much as structurally, it is allied to the muscular tissue last considered, and in some instances it assumes the active conditions of the latter in a very imperfect manner.

I have, therefore, taken it up in this connection. We will first refer to its history and criticism, brief though it may be.

Although Leeuwenhoek, Malpighi, and other of the older anatomists were well acquainted with the common anatomical relations of this tissue, which, from its mechanical importance, they studied well, yet its histological characteristics cannot be said to have been learned until a much later period, and when better means of investigation came into use. This was not before 1833. At about, or soon after,

found in various parts of the body, and now pretty clearly shown to be of a muscular nature. The tissue here alluded to is found in parts possessing contractile properties, but which contain nothing like ordinary muscle. It is made up of long, large, nucleated, queer-looking fibres, the uncouth remains evidently of the characteristic muscle-cells. Their histological and local peculiarities have been pretty thoroughly worked out by Kölliker (Zeitsch. f. wissenschafl. Zool. i. Abth. 1, 1848). He has shown that the muscles hitherto regarded as composed of "smooth fibres," are really made up of elongated cells with an oblong nucleus, which he calls "muscular-fibre cells." He has found them in the nipple and its areola, in the dermis, Crampton's muscle, the digestive canal, the bladder, in a part of the prostate and vagina, the arteries, the veins and lymphatics, the ureter, the urethra, the Fallopian tube, the uterus, the dartos, the vasa deferentia, the trachea and bronchia, the spleen, and many other organs. See Comp. Rend. de la Soc. de Biologie, 1849, p. 156; and art. Spleen, in Todd's Cyclopædia of Anatomy and Physiology. The composition of the iris is of the same fibres. See a paper by Lister, Quarterly Journ. of Micr. Sc. No. I. Oct. 1852. For the comparative histology of the muscular tissue, with many interesting details in every class of the animal kingdom, see Comparative Anatomy by Siebold and Stannius, trans. from the German, &c. &c. by Burnett, under head Muscular System, *passim.*

this time, a number of microscopists published accounts of its ultimate structure. Among these may be mentioned the names of Krause,* Wagner,† and Valentin.‡ Each and all recognized that it was composed of fibrillæ which were formed by the alteration of cells.

The subsequent labours of Eulenberg§ and Gurlt|| made these points the more clear. I need not here point out the distinctions and differences they made out. Soon afterward, Schwann gave an individuality to the whole, by referring its genesis necessarily to cells. Its formation, in all its phases, he has pretty clearly indicated; and, although on some minor points he may not have been fully correct (at least, according to more recent researches), yet the general outline of the genesis was fully made out.

He showed the existence of the two kinds of fibres, the yellow and the white; the former existing alone in ligaments, tendons, aponeuroses, &c., the latter in the middle coat of arteries, ligamentum nuchæ, &c.; but both kinds are present in the areolar tissue generally. Since Schwann, the labours of Gerber,¶ Henle,** and Todd and Bowman†† among others, have increased our knowledge of its more minute details, its functional and other characteristics; but I am not aware that they have changed materially its aspect in an histological or genetic point of view.

More recently, however, than these writers, this tissue has been the subject of interesting inquiries, as to its *purely* mechanical relations in the economy, as occurring in some localities; as, for instance, in the *skin* and *dartos*, where it has an irritability and power of contraction. This is undoubtedly due to nervous influence of a reflex character. And it may well be questioned if the fibres thus capable belong to the fibrous tissue, and are not rather a low form of unstriated muscular fibre, being intermediate between the muscular and fibrous tissues.

In my opinion, such is the true state of the case, and it may serve to explain the fact that, in some instances, true muscular fibres

* Krause; *loc. cit.* 1833, vol. i. p. 13.

† Wagner; Lehrbuch der vergleichende Anatomie, 1834, p. 61.

‡ Valentin; Ueber den Verlauf und die Enden der Nerven, Bonn, 1836.

§ Eulenberg; De Tela elastica, Berlin, 1836.

|| Gurlt; Lehrbuch der Vergleichenden Physiologie des Haussaeugetheire, Berlin, 1837, p. 19.

¶ Gerber; Handbuch der allgemeinen Anatomie, &c. Berne, 1840, p. 134.

** Henle; Traité d' Anat. générale, &c. tom. i. p. 374.

†† Todd and Bowman; Physiological Anatomy.

have been thus found.* The true fibrous tissue, therefore, has no immediate connection with the nerves, and sustains merely mechanical relations.

We will now refer to its genesis as based upon my own observations. On some account there may be a propriety of dividing this tissue into several varieties as Schwann and Henle have done, but certain it is that it is composed of two elements only, wherever found. These are what have been well designated as the *white* and *yellow* fibres.

The former consist of inelastic bands of a very variable size, and present markings with their long diameter. This lining, however, does not indicate a finer subdivision, for the fibre cannot be split up into fibrillæ.

These fibres, apparently thus composed, present often a wavy aspect; thereby allowing of some extension and retraction. Such is the element of which the tendons, ligaments, aponeuroses, fibrous membranes, &c., not to mention the pathological products, are composed. It is emphatically a cell product, and the phases of formation can be easily watched. First, you have round or oval nucleated cells; these become fusiform, or lengthen at both ends, their liquid contents becoming more opaque. In this state, they become united together in a serial manner, not so much by a coalescence of the contiguous walls as by a joining together through the intervention of a plasmatic blastema. Thus formed together, the nuclei remain for some time, and there is an irregular splitting up of the old cell into finer fibres, though not of a uniform character.

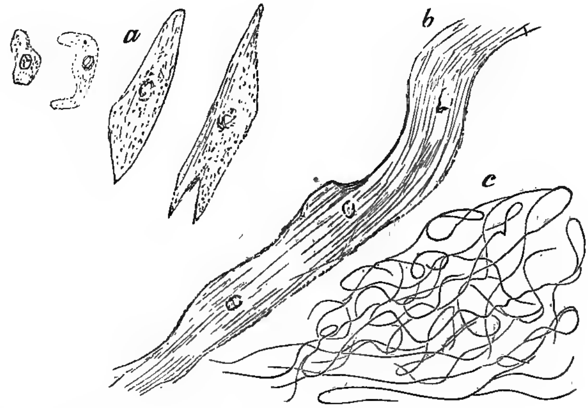
These formative cells not having a proper individuality, at least as to size, the fibres formed from them have a variable dimension; some of the larger ones have a size of 1-500, and the smaller ones of 1-2000 of an inch in diameter. This splitting up of the cell, to which I have just alluded, appears always to take place from its periphery towards its centre; so that, in the process, you first perceive that the elongated cell is bifid or trifid, &c., this extending more or less its whole length. The phases of formation thus described can be traced in the embryo, or perhaps full as well in the pathological products of a fibrous character, in which this tissue persists on its embryonic stage.

The *yellow* fibres may, as Schwann has said, be regarded as distinct in their character and formation. They consist of long smooth fibres, curled upon each other when no extension is made. Their size, like that

* See Comp. Rend. des Séances de la Soc. de Biologie, à Paris, 1850, p. 38.

of the other variety, is variable, being between 1-3000 to 1-7000 of an inch in diameter. Their locality is in the middle coat of the arteries,*

Fig. 21.



White fibrous tissue and its formation, from a tendon. *a*. Fibre-cells in their early stages of growth. *b*. United into a cylinder and split longitudinally, the nuclei still seen. *c*. A representation of the filaments separated from the fibres—their coiling, twisted aspect. (Magnified 250 diameters.)

the chordæ vocales, the ligamentum nuchæ, and the ligamenta subflava, besides being combined with the white fibres in various parts.† The mode of genesis of this tissue has been a point upon which different physiologists have expressed dissimilar opinions. Schwann‡ speaks of having examined the ligamentum nuchæ of a fœtal sheep, which he found composed of longitudinal fibres containing nuclei of cells. He therefore infers their formation by the coalescence of cells, as is true of the other variety. But, according to his own account, his observations were not complete or fully satisfactory. The opinion of Valentin§ is quite different, for he thinks they are formed by the apposition of matter around fibres which are formed by the coalescence of cells.

Henle‖ regards them as formations from the nucleus, or, as he would call them, *nuclear-fibres;* exactly as are formed many fibres found in vegetable tissues. He, however, bases his opinion more upon ana-

* Henle thinks that the elastic tissue of the middle coat of the arteries is different from that of the ligamentum nuchæ, &c. (*Vide* Traité d'Anat. gén. tom. i. p. 440.) But this difference may be a local instead of an histological one.

† For an account of the localities of this tissue, *vide* Eulenberg; *loc. citat.* p. 13.

‡ Schwann; *loc. citat.* p. 151.

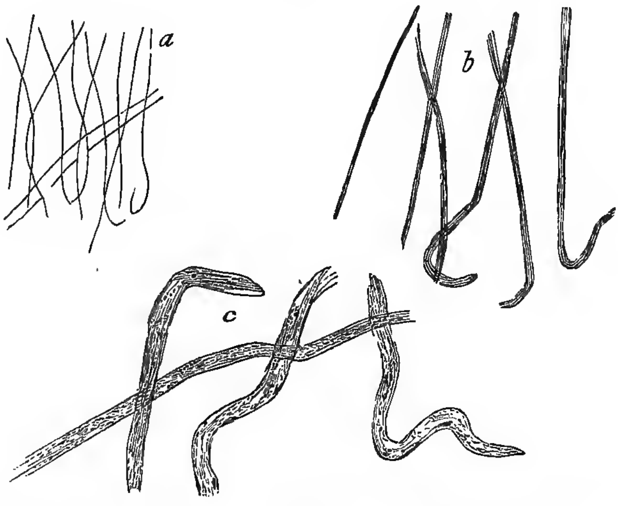
§ Valentin; Müller's Archiv, 1840, p. 216.

‖ Henle; Traité d'Anat. générale, &c. tom. i. p. 437.

logy than upon real observation. It is true, as he observes, that, because they do not appear affected by acetic acid, and, under any treatment, show no unequivocal nuclei, they resemble nuclear formations. But this does not prove them to be such, although it would certainly seem to show that they are not of direct cell-membranic origin. My own investigations have been made upon the ligamentum nuchæ of fœtal and adult goats.

This tissue seems to make its appearance at a very early period. In a fœtal goat of 21 days, its existence was marked by the appearance of long and delicate fibrillæ having a diameter of 1-25,000 of an inch. These were most probably formed by the linear aggregation of granules or utricles, as I have described on a preceding page. It did not appear to me, therefore, that they had a cell origin, as is true of the white fibrous tissue.

Fig. 22.



Yellow Fibrous Tissue and its Formation. *a*. Fibrillæ from ligamentum nuchæ of a fœtal goat of 21 days; size, 1-25,000. *b*. Same, from the same locality, of a fœtal goat of four months. *c*. Fibrillæ of tissue in its adult form; size, 1-3500.

In a fœtus of the same animal, of four months (that is, one month before birth), this tissue was still of a grayish-white aspect, and the fibres were about 1-9000 of an inch in diameter, having the aspect of threads, and revealing no farther structure after the action of acetic acid. In the adult goat, they had a yellowish cast, and were of a diameter of about 1-3500 of an inch. Any treatment by the strongest acid showed no farther structure.

From these facts, it would appear that the yellow fibrous tissue *is not of cell origin, but is a form of fibrillated tissue*, which increases regularly with the animal until it has attained its full size. In the new-born goat, it is nearly three times smaller than in the adult, and therefore resembles the gradual growth of the muscular fibres, of which I have spoken on a former page.

VI. Adipose Cells and Adipose Tissue.

This tissue is always found associated with the one last considered, and Schwann, in his chapter on areolar tissue, considered it as a part of it. In the present case, I have thought proper to take it up in a separate division, but in the close connection of succession. The adipose tissue is strictly a cellular one, although these cells are interwoven generally with the fibrous element which serves as its mechanical support. These cells are spherical when not subjected to pressure, have a smooth contour, although they soon lose it when removed from the body and placed in a lower temperature, for their oily contents contract by cold. Their size is very variable, sometimes they are observed quite small (1-2000 of an inch), and then again they are so large as to be distinguished as glistening points by the naked eye.

As usually seen, they appear as simple vesicles, filled with oil, and it is rare that any trace of a nucleus is seen. Fat-cells, therefore, cannot be said to be *nucleated cells*, whatever opinion may be entertained of their origin. This fact has given rise to some dispute as to their formation. The question is, are they to be regarded as cells having always a pre-existing nucleus, or are they vesicles, which may or may not contain nuclear matter. Valentin* has noticed their appearance in the human fœtus (between the third and fourth month) in the palm of the hand; he describes them as collections of little cell-like bodies, which also exist isolated from each other. But these facts throw no light upon their primitive formation, a point which, from direct observation, is not yet well made out.

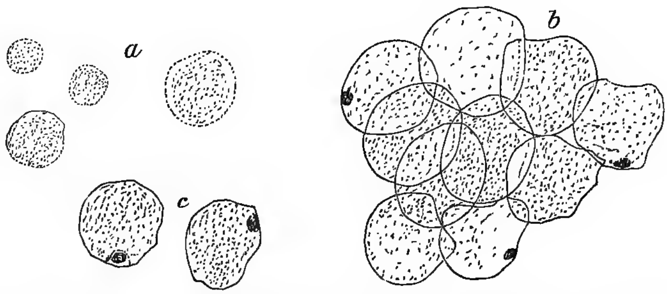
Schwann very naturally inferred their cell-origin according to the usual mode he described, and his opinion appears to have been confirmed by the fact of recognizing in the walls of some a nuclear body. Henle's† opinion is to the same effect. But the view which I am obliged to take, as based upon my own observations, is different; it is,

* Valentin ; *loc. citat.* Entwickelungsgeschichte, p. 271.

† Henlé; Traité d'Anat. gén. tom. i. p. 424.

that they do *not* always form around a pre-existing nucleus, but arise as simple vesicles. These vesicles are formed primarily, according to that method Ascherson* has pointed out, and to which the attention has been called on a former page—simply by the formation of a plastic membrane of albumen, around a minute globule of oil. This body then increases and dilates by the accumulation of oil within, ultimately producing the vesicle of a full-grown size. During this growth, nuclear bodies may form within, in the same way—by the union of oil and albumen. This is the reason why you sometimes meet with cells having, in their interior, one, two, three, or even more vesicles or nuclei; or, there may be a single one of a well-marked character, having the aspect of a single nucleus. Such are the phenomena I have frequently met with, and especially in the formation of these cells in the mesentery of very young tadpoles. It is true that there are frequently seen whole groups of cells, in each of which is perceived a uniform nucleus. I therefore wish to be understood, that I do not deny that these bodies may exist as nucleated cells, but insist that they are often formed as simple vesicles.

Fig. 23.



Fat-cells and their formation, from bottom of orbit of eye of a dog. *a.* Vesicles without a nucleus, filled with fat. *b.* Fat-cells fully formed, irregular in shape from pressure, and for the most part without a nucleus *c.* Two cells with traces of nucleus or granular matter adhering to membrane. (Magnified 250 diameters.)

VII. The Cartilaginous and Osseous Tissues.

I have thought best to consider these two tissues under a single head, because the one (the latter) can be regarded only as the other furnished with a mineral product, so combined with it, that the functions of organic shall be carried on in connection with the more mechanical relations of inorganic life. We will, however, take them up separately, and that in the order in which they are produced.

* Ascherson; Muller's Archiv, 1840, p. 46.

In cartilage, we have a good example of the union of the simple, with the metamorphosed cell structure; the former being individual cells; the latter, the fibrous tissue in its finer and coarser forms. A consideration then of this tissue, in connection with the fibrous one we have just passed, is quite proper; and it will appear the more so, when we shall have shown that in some instances they pass imperceptibly into each other, and could not be distinguished, excepting for the *simple cell* element of the one, and the absence of it in the other. A review of the historical conditions of this subject as a whole, will well repay us, in rendering more clear some of the obscure points of the development and passage of the one into the other.

As we have seen on a foregoing page, the history of the cartilaginous tissue as one composed of cells is intimately connected with the history of the development and nature of cells in general; and in it, it may be said, the cell doctrines of animal tissues took their origin. It may, therefore, seem unnecessary to again pass over this field. But, in this connection, it is important as including the details of the subject.

As Henle has justly remarked, it is quite singular that a structure so apparent as is this in most of its relations, should have remained so long imperfectly understood. For its study, in anything like a satisfactory manner, does not date back but a few years previous to the active labors of Valentin and Schwann.

The older anatomists, who, in the historical accounts of the other tissues, seemed to have seized hold of points in a most wonderful manner, do not appear to have entertained very clear and correct ideas of its intimate anatomical and physiological conditions. The earlier labours of Weber* and Wagner† may be said to have laid the foundations of its more thorough study soon after. Others, it is true, and among whom may be mentioned Krause,‡ were labouring here at the same time. But in 1834, Purkinje,§ who has, as it were, identified himself with this structure and that of bone, published some observations which may truly be said to be the first embracing the histological features of this tissue, for he traced in it the early conditions of those corpuscles of bone now known under his name.

* Weber; Meckel's Archiv, 1827, p. 233.

† Wagner; Lehrbuch der vergleichenden Anatomie, Leipsig, 1834, p. 62.

‡ Krause; Handbuch der menschlichen Anatomie, Hanover, 1833, bd. i. p. 48.

§ Purkinje and Deutch; De penitiori ossium structura observationes, Breslau, 1834.

In the following year, the work of Valentin,* so often alluded to, appeared, and in it were contained many valuable details. Valentin watched the appearance of this tissue or cells, in very young embryos. He recognized the cartilage-corpuscles, and the cavities in which they are formed, but did not agree with Purkinje, that the former were only early conditions of the osseous corpuscles.

However, Valentin's opinions were well grounded in observations, and the interpretation of these last was as correct as well could be, considering the then existing ideas of the elementary forms of structure.

Soon after this, came the observations of Miescher,† published with those of Müller, which were remarkable for the care with which they were conducted, and the correctness of many of the views entertained. To Miescher is due the establishing of one point, viz: the necessary connection between cartilage and bone, the former as the invariable precedent of the latter.

The same remarks, with but little alteration, will apply to the contemporaneous work of Meckauer,‡ and all these may well be said to have paved the way for those labours of Schwann, upon which the whole grand idea of the cell was based. To what Schwann has done, it would be almost superfluous to allude. As far as regards the simple genesis, he did not, perhaps, advance farther than Valentin before him: but he traced out its phases as the basis of cell-doctrines; and on this account his observations on this tissue were much more numerous than upon any other. It is quite remarkable, however, that this very kind of cell should also have been the one, the study of which led to the first doubts as to the universality of Schwann's and Schleiden's cell-doctrines.

Since the work of Schwann, the labours in this direction have belonged for the most part to points other than those of its genesis, such as its nutrition, and its relations to the osseous tissue. In this connection may be mentioned the work of Henle,§ which will soon arrest our attention, when I speak of the history of bone-studies; of Toynbee,‖ devoted especially to the nutrition of cartilage, and then

* Valentin; *loc. cit.* Entwickelungsgeschichte, p. 26.

† Miescher; De inflammatione ossium eorumque anatomie generali. Accedunt J. Müller's observationes de canaliculis corpusculorum ossium atque de modo, quo terrea materia in ossibus continetur. Berlin, 1836.

‡ Meckauer; De penitiori cartilaginum structura symbolæ. Breslau, 1836.

§ Henle; Traité de Anat. générale, &c. Trad. par Jourdain. Paris, 1843, tom. ii. chap. xiii.

‖ Toynbee; Memoir on the non-vascular tissues. Philos. Trans. 1841.

of Todd and Bowman,*—in which Mr. Toynbee's views of the genetic relations of this tissue and its transformations are embodied. In 1844, Valenciennes† published a memoir on the cartilage of the cartilaginous Fishes, and the Mollusca. The subject received extensive attention, and, as I have since laboured some in the same field and met with similar results, I will state here the conclusions at which he arrives, although they have a more prominent zoological than histological importance. "1. That the cartilage-cells are generally arranged in such regular plans that it would be possible to determine by microscopical examination the order or even the genus of the animal, from the character of its cartilage. 2. That none of the cartilage-cells have, in any species, canaliculi communicating with them; and, 3. That gelatine and not chondrin is abundant in the cartilages of cephalopods." Such results are of more importance than at first would be supposed. They relate to the condition of this tissue, as found in that class of animals resting on the lowest point of the vertebrated kingdom, and where it so persists on its embryonic type that we have a chance to study its peculiarities; and, as will hereafter be shown, our clearest views of the nature of ossification are derivable from allied though not from the same sources.

Quite recently, there has appeared a very valuable paper on the formation of cartilage and its nutrition, by Dr. Leidy.‡ Of this paper it is hardly necessary for me to speak; for, being wrought out with the characteristic care and elegance of the author, it has become well known to all familiar with this subject.

Such are the prominent points in an historical retrospect of the cartilaginous tissue. It is now proper that our attention should be turned in a similar manner to one of its ulterior conditions, viz. that of bone, which need occupy us but for a brief period.

Singular as it may seem, it was the opinion of some of the older anatomists, that bone was not formed *out of* cartilage, but was a product *de novo*, replacing the latter. So thought Albinus,§ and others of his time.

It is true, however, that, however obscure may have been the *derivative* nature of this tissue, both Malpighi|| and Leeuwenhoek¶ had

* Todd and Bowman; Cyclop. Anat. and Phys., Art. Osseous Tissue.

† Valenciennes; Comp. Rendus, Nov. 25, 1844.

‡ Leidy; Amer. Journ. Med. Sc. January, 1849.

§ Albinus; Academicarum adnotaliorum. Libri octo. Leyden, 1754. Lib. vii. cap. vi. p. 77.

|| Malpighi; Opera posthuma, London, 1797, p. 47.

¶ Leeuwenhoek; Anatomia Ossium, Leyden, 1689, p. 11.

perceived to some extent its *immediate* composition, recognizing what they called "fibres." Most willingly do I omit a consideration of these elder authors.

Well-grounded labours, and those the influence of which extended forward into the modern times, do not date farther back than the time of Purkinje,* whose results were published by Deutch, in 1834. He recognized the corpuscles known under his name, with the canaliculi, and the concentric arrangement of the lamellæ. But this signification does not appear to have been clearly made out. He, or rather Deutch, supposed that these radiating lines (canaliculi) were tubes filled with calcareous matter, and therefore served some ulterior purpose in ossification. Soon after this, the work of Miescher,† already quoted, appeared. He followed closely in the paths already trodden before him, but expresses a different view from that of Purkinje as to the nature of the canaliculi. He did not find them filled with ossific matter; neither could he trace them through the concentric lamellæ. I notice this point here, because it well shows upon what minute points of structure men were at that time engaged.

As I have previously remarked, a high estimate is to be placed upon the labours of Miescher, not only from the great care with which they were made, but because he recognized the necessary pre-existence of cartilage for the formation of bone, and supported the view in a manner which would be well worthy of researches of the present time. This foundation being laid, the labours immediately subsequent were upon still more important points—the immediate phenomena of the osseous formation, and the part which each portion played in producing this result.

In the works of Gurlt,‡ Schwann,§ and Gerber,|| these subjects have received the most careful attention, and as the views therein expressed, although dissimilar, will frequently be alluded to hereafter, it may be well to notice them here in a particular manner.

Schwann, from the thoroughness of his studies in this direction, should be entitled to a very correct opinion as to the nature of cartilage, in both its primary and secondary conditions. He was in-

* Purkinje; *loc. citat.*

† Miescher; *loc. citat.*

‡ Gurlt; Lehrbuch du vergleichenden Physiologie des Haussaengetheire, Berlin, 1837.

§ Schwann; *loc. citat.* 1839.

|| Gerber; Handbuch der allgemeinen Anatomie der Menschen und des Haussaengetheire, Berne, 1840. Or Gulliver's edit. London, 1842.

clined to adopt, as correct, the opinions of Miescher, who preceded him, and especially as to the important point of the identity of the Purkinjean corpuscle with the pre-existing cartilage-cell, and *not* with its *nucleus*. He therefore regarded the canaliculi "as hollow prolongations of the cells into the cellular substance." This may appear a small matter; but, as will soon be shown, on it rests the clearest doctrines of osteogenesis, viz. those of *immediate substitution.*

Gerber, on the other hand, and with him, Gulliver, I think, are of the opinion that the Purkinjean corpuscle is the *nucleus* of the cartilage-cell, and that the canaliculi are nuclear stellations. This view has, I well know, some *analogous* support in the stellate nuclei of many vegetable cells; as, for instance, those of the common geranium (*Pelargonium*). The same view was entertained by Todd and Bowman.* Now, if the opinion that the nucleus and the cell are histologically the same, as I have advocated on a previous page, be assented to, it would appear of no importance to cavil about this point; for, whatever capacity would be given to the cell, would be given to the nucleus also, and therefore we could look for stellations of the one as well as of the other. But this point has other bearings, as we shall soon perceive.

The investigations of Mr. Erasmus Wilson,† made in the summer of 1841, seem to have been attended with particular results. He had watched the development of bone, and expressed the opinion that the canaliculi deserved the name of "converging tubuli," from their manifest tendency towards the Haversian canal; and as this tendency appeared constant, it gave him the impression that they served as a necessary means of connection of the periphery with the centre of the concentric lamellæ. I am not aware, however, that Wilson entertained any positive opinion of his own on the mooted point of Purkinjean corpuscle and cartilage-cell.

On this topic, the view of Henle‡ is different from both those of Schwann and Gerber above mentioned. He "regarded the Purkinjean corpuscles as the cavities of cells, of which the thickened sides form the fundamental substance, and the osseous canaliculi are the tubes which penetrate the cavity of the cell in the thickened sides of this last, as are the porous canals of vegetable cells."

But I must confess that this description, given in Henle's own

* Todd and Bowman; Physiological Anatomy, p. 108.

† Wilson; The Anatomist's Vade Mecum, London, 1842.

‡ Henle; Traité d'Anat. générale, &c. Trad. par Jourdain. Paris, 1843, vol. ii. p. 409.

words, does not appear clear to me, neither do I think the reasons for it, subsequently stated, sufficient for its adoption.

Flourens,* in the same year, published his results upon the formation of bone. But they relate chiefly to another department—its production in connection with the periosteum and the so-called medullary membrane, the existence of which he supports, regarding it as a mere extension of the former. These observations were continued, and published in 1845;† and in the same year there appeared a long paper by MM. Brullé and Rugenny.‡ To this last, however, I need not here allude. The researches of Mr. Tomes§ were quite extensive, and were published soon after. He described, among other things, the ultimate structure of bone lying between the canaliculi and lacunæ, as consisting of minute spherical granules, and not being a homogeneous substance, as usually described. In regard to the other microscopic points, his view is that, after the formation of the ossific tubes (canaliculi) in the process of ossification, the latter are filled up by a deposit of osseous granules, and while this deposition is going on, small cells are left, which are the rudimentary Purkinjean corpuscles.

More recently, and within a few years, important researches have been made; and it is singular that, after so many varied opinions should have arisen, some of the earliest views are advocated. Thus, Hassall|| agrees with Schwann, and says: "The Purkinjean corpuscles are to be regarded as complete corpuscles, the canaliculi of which are formed by the extension of the cell-wall, which is proved by watching the formation and development of bone."

This same view has since received a very able confirmation from the studies of a most excellent observer, Dr. Leidy;¶ and these were made upon the ossifying frontal bone of the human embryo of two inches. Here he traced the immediate transition of the cartilage-cell into the Purkinjean corpuscle.

In this connection, the name of Dr. Sharpey** should not be overpassed without mention. His studies upon osteogenesis have been quite extensive, and he has revived the old opinion, that true bone is

* Flourens; Comp. Rendus, 1841.

† Flourens; Comp. Rendus, Dec. 8, 1845.

‡ Brullé and Rugenny; Annal. des Sc. Nat. Nov. 1845.

§ Tomes; Cylop. Anat. and Phys. Art. Oss. Tissue, March, 1846.

|| Hassall; Microscopical Anatomy of the Human Body, London, 1847–50.

¶ Leidy; Proceed. Phila. Acad. Nat. Sciences, 1848.

** Sharpey; Human Anatomy, by Jones Quain. Ed. by R. Quain and Wm. Sharpey, London, 1847; Introduction.

sometimes developed *in membranes*, and without the pre-existence of cartilage. This point he thinks he has demonstrated by watching the development of the cranial bones; and Dr. Carpenter (as he affirms in his *Human Physiology*) mentions that his own researches have confirmed them. For a description of these changes I must refer to his work, as they cannot be clearly and laconically stated here. A criticism upon this view will be passed when I state my own results; and, by way of conclusion of this historical view, I will add that, quite recently, two excellent observers, Kolliker* and Robin,† after a somewhat comprehensive *practical* review of the whole subject of osteogenesis, express their concurrence in the old opinion of an *intramembranous* form of bone-genesis. This last would certainly be enough to make one hesitate as to an opposite opinion; but, as I shall now proceed to show, the whole matter can be quite definitely comprehended by proceeding step by step.‡

The true cartilage tissue, when existing as such, is always invariably the same. Two varieties, however, dependent upon a degree of organization, are formed. The first, called *cellular-cartilage*, or comprising those of a transient nature, and ultimately passing on to the formation of bone, consists of nucleated, well-defined cells, lying in a semi-solid punctiform stroma. The second, called *fibro-cartilage*, consists of the same kind of cells, lying in a network of fibrous tissue, which last is only a farther developed condition of the punctiform stroma. In this last, this fibrous tissue may so increase that the true cellular element entirely or almost entirely disappears; and hence the transition of *fibro-cartilage* into *fibrous tissue*. Another division of cartilage is into the *temporary* and *permanent;* but this is conventional only, for they differ in no essential particular from each other.

It is evident from the above view that all cartilage is originally the same, viz. *cellular*, appearing as such in the embryo. Its primitive formation there takes place, according to my own observation, in the following manner: At those points at which cartilage,

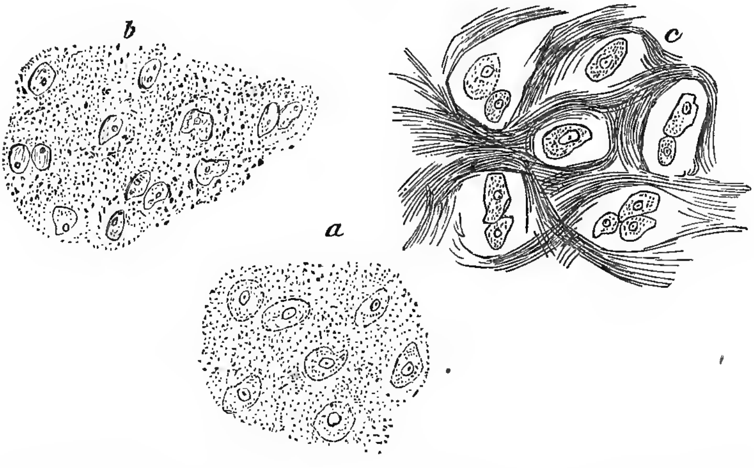
* Kölliker; Anatomie Mikroskopische, &c. bd. ii. p. 376.

† Robin; Mem. de la Soc. de Biologie à Paris, 2ieme ann. 1850, p. 119.

‡ I regret being unable to allude in some detail to a comprehensive Memoir on this subject by Tomes and De Morgan, recently presented to the Royal Society. It is not yet published, but there is an abstract of it in the Lond. Med. Times and Gaz. Oct. 2, 1852. From this abstract, I should judge they do not believe in the formation of bone without a pre-existent cartilage. It would also appear that they think that the *nucleus* of the cartilage-cell becomes the Purkinjean corpuscle.

and afterwards bone, is to appear, there exist cells, which, as to physical characteristics, cannot be distinguished from those which are to form other tissues. A part of these cells are condensed into a punctiform stroma, leaving open spaces here and there, in which persist the original cells, from one to three or four in each cavity; there is, then, a finely granular stroma, in the midst of which lie free nucleated cells. At first, this separation is marked out by faint lines only; but, subsequently, it becomes more marked, and, finally, after consolidation, presents the true *cellular cartilage.* As this stroma is formed *closely* about the cells, it is not correct to say that *cavities* are at first formed in the stroma, and in which the cells repose. However, subsequently, the cell-membrane, lying in direct contact with the stroma, blends with it and is lost; and then there appears a cavity in which the *nuclei* of the old cell persist. But as these changes take place at an early period, one does not often see anything but the nuclei; and as these are nucleated, and resemble cells, it is proper that they should be designated as such.

Fig. 24.

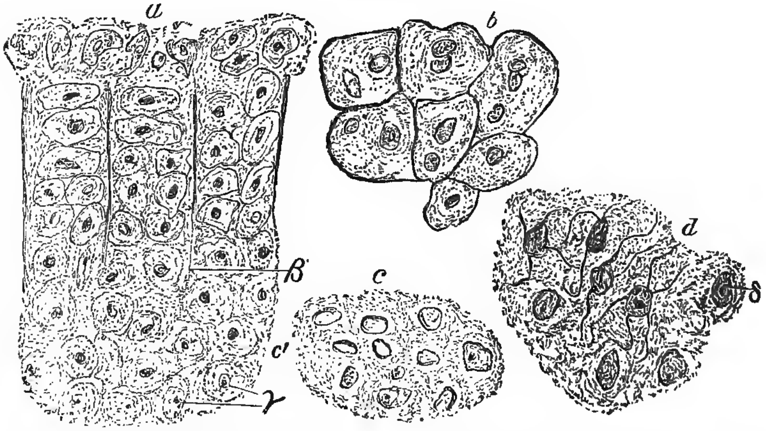


Cartilage and its formation. *a*. Cartilage just forming, appearing as cells lying in a finely punctated stroma, from vertebral column of fœtal goat of one month. *a*. Cellular or temporary cartilage, from femur of a fœtal goat. *c*. Fibro-cartilage (permanent), the punctated stroma having become fibrillated, giving the whole a nidiform aspect. From articular cartilage. (Magnified 822 diameters.)

In *fibro-cartilage*, the same early changes occur, but the stroma is farther developed, and a fibrous or fibrillated tissue is the result; when this has occurred, the cells (or rather cell-nuclei) lie in *nidiform* cavities, each of which quite resembles a bird's nest with eggs. It is only the *cellular* form of cartilage which is developed into bone, and this osteogenesis is, as we shall now perceive, only a kind of displacement of one material for another. We will suppose the cellular cartilage is formed; then, as the ossific matter is about to be deposited, the vascularity of the cartilage is much increased, so that the substance presents a pinkish hue; then a kind of liquefaction of the intervening stroma appears to take place, so that the cartilage-cells seem no longer confined irregularly, but are, for the most part, free to assume almost any relative position.

A tendency with them to an arrangement in linear series is then manifest. These rows run parallel with the long diameter of the bone, and are separated from each other by the intercellular matrix, which consists of the partially liquefied stroma. It is thus, when these rows are formed, that the future bone may be said to consist of a fasciculus of tubes in which the cartilage-cells are situated; and I have often thought that, if a tranverse section could be then made, it would present a very uniform appearance of this kind. This intercellular matrix constitutes the primitive ossific rete, in which the calcareous salts are first deposited. This first deposition having taken place, the cartilage corpuscles are situated in cup-like or rather cylinder-like cavities.

Fig. 25.



Formation of bone in cartilage, from tibia of fœtal goat of two months. *a*. Cartilage-cells arranging themselves in a linear manner, and septa appearing between them. *c.′* The stroma and cells so far ossified that all are blended together. *b*. Transverse section of *a*, showing the cylindrical tubular arrangement. *c*. Ossification still farther progressed, the aqueous portions being absorbed, the cells are brought nearer together, and appear shrunken and irregular. *b*. Still farther progress, the canaliculi shooting out from the cells, in some of which there is a trace of the old nucleus; thus showing that the Purkinjean corpuscle with canaliculi are the transformed cartilage-cells. *β*. Appearance of septa fading away. *γ*. Nuclei disappearing. *δ*. Nucleus persistent. (Magnified 200 diameters.)

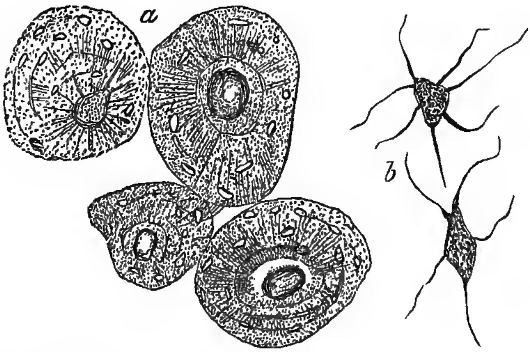
Before this has occurred, however, the cartilage-cells, and the substance immediately surrounding them, are likewise charged with calcareous matter. The cells become smaller, and, in contracting, assume irregular forms; the septa, separating the tubes in which they formerly lay, become more and more indistinct, from the universality of the calcareous deposition. Finally, there is perceived a grayish mass, with but little regularity, and variegated with these irregularly shaped bodies—the future Purkinjean corpuscles. But we will look at the process a little more minutely. By this calcareous deposition, the aqueous parts of the tissue disappear or are absorbed, the size of the whole is reduced, and the cartilage-cells brought nearer together; the tissue, therefore, is much more compact, but still does not lose all of its original characteristics. The tubes, of which we have spoken, form the *concentric lamellæ*, the corpuscles having arranged themselves in a regular manner around in it; and, therefore, a transverse section shows the bone to be made up of *solid cylinders*, instead of *hollow tubes*, as before the deposition of the calcareous matter.

The cartilage-cells are transformed into the Purkinjean or osseous corpuscles. This I have clearly observed, and have traced all the phases of the change. When they exist in the cup-like cavities, their nucleus is prominent. But as ossification goes on, the nucleus gradually crumbles away, and, by the time ossific matter is deposited in the cell-walls, little of it is seen. The cell-walls, however, remain, but have become shrunken, and, from the absorption of the aqueous portion of the intervening substance, they are brought nearer together; however, they hold regular concentric relations to the central portion of that which was the tube, and this central portion is to be the Haversian canal. When the cartilage-corpuscles begin to shrink, radiating lines are seen running from each; these appear to commence with the corpuscle and radiate from it, and in shooting out in every direction, they meet with those of neighbouring corpuscles, and thus are joined together on every side. Thus far all seems clear. The cartilage-cells lie in the midst of an amorphous stroma; this last partially liquefies; the cells then arrange themselves in rows; these rows are separated from each other by this intercellular stroma, and form more or less regular tubes; in the walls of these tubes the ossific matter is first deposited, and afterwards in their contents, both cellular and granular. These tubes form, each, a system of concentric lamellæ—from the arrangement of their contents in a concentric manner—and this occurring around a central cavity, forms the Haversian canal. The Purkinjean or osseous corpuscles are the pre-existing cartilage corpuscles changed by the ossific processes.

The next question is, what are the canaliculi? Are they prolongations of the cartilage cell-membrane, as Schwann has supposed? As far as my own observation yet goes, I must say that I have seen no evidence that such is true, excepting their general appearance. On the other hand, there are facts which oppose such a view. In the first place, the canaliculi do not commence to form until the calcareous matter begins to be deposited, and, as the cell-membrane is then either filled with calcareous matter or absorbed, it could not well send out prolongations. Moreover, these prolongations are often of such a length, and branch and rebranch so often upon themselves, that it can scarcely for a moment be entertained that they are thus formed. I ask, is it not more probable that they are channels for the escape of aeriform matters from the interior of the cell? For the cell, existing in the midst of an ossifying mass, would retain its animal matter longer than the rest of this tissue; and this animal matter would give rise to gases seeking their escape in every direction by percolating the surrounding semi-solid mass; and in behalf of this view, perhaps Wilson has rightly named them "converging tubuli," formed as they are by currents naturally seeking or converging towards the nearest outlet.

I do not, however, express this as a well-grounded opinion, but simply as a probable hypothesis. We shall soon see what other reasons there may be for its adoption.

Fig. 26.



Microscopical view of Transverse Section of Bone, from Femur (human). *a.* The concentric lamellæ, with Haversian canal and Purkinjean corpuscles. (Magnified 175 diameters.) *b.* Purkinjean corpuscles isolated, to show irregular form and the canaliculi. (Magnified 450 diameters.)

In these phases of formation, some of the nuclei of the cartilage cells, or even other adventitious cells of a small size, may not be dissolved, but become ossified as such, as are the cells in some of the lower fishes. You would then find them as cell-like granules, scattered irregularly through the osseous tissue. These are not the granules spoken of by Mr. Tomes, but distinct cells of a larger size, varying from 1-3000 to 1-2000 of an inch in diameter. It is in this way that I account for the occasional presence in the spongy tissue of the long bones of small spherical bodies, first discovered by Dr. Holmes, of Boston, who met with them early in 1849. Subsequently, however, Lebert* has fully noticed them. As their presence is not constant, and does not apparently involve any pathological condition, the above view of their nature appears to be correct.†

Such appears to be the mode of proceeding of the formation of the *compact tissue* of the bones of the higher vertebrata, as I have studied it in fœtal goats. The process is simply one of *substitution*, with that contraction and modification of form which must necessarily follow when a soft is replaced by a sclerotic tissue. This law of substitution is everywhere the same; but, to be sure, there is a difference in some of the steps of its progress in the different kinds of bones.

Fig. 27.

The peculiar long corpuscles sometimes met with, from medullary tissue of head of femur. The corpuscles, for the most part, have nuclei, some of them several. (Magnified 350 diameters.)

The spongy character of the internal or middle portion of bones appears to be produced by the absorption, by the numerous vessels there situated, of the lighter portion of the primitive cartilaginous

* Lebert; Compt. Rend. des Séances de la Soc. de Biologie à Paris, Oct. 1849, p. 149.

† I think the "ossified cells," as they are called by Tomes and De Morgan, must be the same bodies. See their "Memoir on the Structure and Development of Bone," read before the Royal Society, but of which I have seen an abstract only in the Med. Times and Gaz. London, Oct. 2, 1852.

base, and a consolidation towards the periphery. This, however, is a point having a teleological bearing, for by such an operation, the bones possess the greatest strength and lightness combined, attainable with the same amount of material.

These phenomena of genesis just described belong especially to the higher vertebrates.

In the lower classes, they are far from being of this complicated character. Such is the case with many fishes. The concentric lamellæ do not exist, and therefore there does not occur any linear arrangement of the cartilage-cells; but they become ossified *in situ*, their canaliculi radiating from every side, giving the whole a most regular and beautiful appearance. Everything is left exactly as when a soft tissue, cells, nuclei, and all, giving you the impression that it had *suddenly* been charged with calcareous matter. Such appearances may be seen in some of the bones of the sword-fish.

The cartilage of cartilaginous fishes may be said to differ from the common cartilage of the higher vertebrates. In fact, it cannot be properly called true cartilage, but is, if I may so express myself, osseous tissue in a cartilaginous dress. This will be clear if I make one or two explanatory remarks. As has already been stated, Valenciennes* has shown that the cartilages of cartilaginous fishes and the Cephalopods contain gelatine, and *not* chondrin. Now, Müller† has shown that all ossifying bone contains chondrin and not gelatine, and after the process of ossification has taken place no chondrin is found, but all is gelatine. Therefore, bones are, so to speak, *gelatinous*, and not *chondrinous*. The same we have just seen is true of the so-called cartilages of the cartilaginous fishes. This will make clear the statement just made. I much regret that no examinations (at least of which I am aware) have been made of the chemical character of the embryonic condition of cartilage in these lowest fishes, for I should venture to predict, upon the above premises, that they would be found *chondrinous* and not *gelatinous*.

The tissue forming the cartilaginous skeleton of these fishes, as I have had an opportunity to examine it, is composed of oval or spherical cells, resembling the common cartilage-cells at an early period of development. These have become *hardened in situ*, but not *calcarified*, and never have I met with any having the canaliculi

* Valenciennes; *loc. citat.* Comp. Rend. Nov. 25, 1844.

† Müller; Poggendorf Annalen, Bd. xxxviii. p. 316; or Ueber den feineren Bau der Geschwülste, Berlin, 1838, Erster Band, pt. ii.

radiating from them. From all these data, I think we may justly conclude that, in these lower fishes, we meet with bone-cartilage and not with true bone.

The cartilaginous and osseous tissues, as we have thus just described them, may well be said to belong to the *vertebrata* only. Among the *invertebrata*, the only class having any claim to our consideration in this respect is that of *cephalapoda*. With these I have no experience; but Siebold* says that their skeleton has a structure like that of the true cartilages of the *vertebrata*. But this similarity is probably an *analogous* instead of an *homologous* one; exactly as may be said of some of their other tissues; for our most comprehensive knowledge of these two kingdoms leads us to infer that all true homology ceases with the boundary of each.†

In regard to the mooted point, whether true bone is ever formed *without* the pre-existence of cartilage, it follows from the above that, as true bone contains osseous corpuscles—and these are modified cartilage-cells—the answer would be in the negative.

VIII. Crystalline-Lens Tissue.

This is the last tissue to come under our consideration, and I should have omitted it here altogether, had I not watched the phenomena of its genesis. This genesis takes place from cells, originating, almost without a doubt, according to my own mode of cell genesis already described.

As would be inferred from its very function, there is perhaps no tissue in the economy having so delicate a structure. It is no wonder that the attention of the early anatomists should have been excited to the consideration of the fact that an *organized* structure should be able to act exactly like an unorganized vitreous substance. Its very function precluded any correct knowledge of its structure with the ordinary aids of vision, for it is nearly transparent. However, from studies upon the inferior animals, its tissue had long been recognized to be "fibrous." But how these fibres were arranged so as not to interfere with the transmission of light was the mystery. This point Sir David Brewster‡ first elucidated by means of polarized

* Siebold; Comparative Anatomy by Siebold and Stannius, trans. from German, &c. &c., by Burnett, vol. i. § 231.

† Since the above was written, I have had an opportunity to study the cartilage of the Cephalopods as illustrated in the common Squid (*Loligo illicebrosa*), and my observations agree with those of Siebold above referred to.

‡ Brewster; Philosoph. Transact. 1833.

light, and showed that they were united by dentated edges which lock into each other.

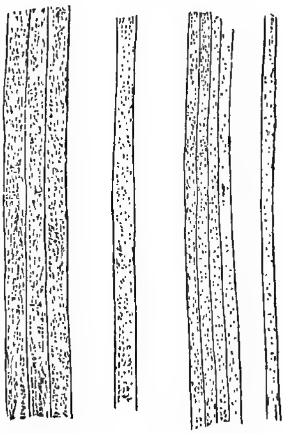
Since the days of cell studies, this tissue has been shown to take its origin in cells like that of others. This was first pointed out by Valentin* from studies upon embryo sheep. Since then the same has been observed by Werneck† and Schwann;‡ the last of whom has applied his cell-theory to the phenomena observed.

These last three observers agree as to the nature of the metamorphosis producing the fibres, viz. that the fibres are simply elongated cells, which cells, Schwann has concluded, arise always from a pre-existing nucleus, because many of them were found nucleated. However, Schwann freely admits that his observations on this subject have not been complete.

More lately still, the subject has been examined by Martin Barry§ and Mr. Toynbee,|| both of whom take an opposite, or rather different view from that of Schwann, viz. that the fibres are formed by

Fig. 28.

a. *b.*



a. Crystalline-lens fibres from a cat. *b.* Crystalline-lens fibres from an owl (*Strix virginianus*). All represented slightly granular for effect.

the coalescing of several cells into one, forming at first a tube after the manner of that of muscular fibre.¶

* Valentin; *loc. cit.* Entwickelungsgeschichte, p. 203.

† Werneck; Anemon. Zeitschrift, bd. v. p. 414.

‡ Schwann; *loc. cit.* p. 100.

§ Barry; Embryological Researches, third series.

|| Toynbee; Memoir on the Non-Vascular Tissues. Philos. Trans. 1841.

¶ In anatomical and histological works, the fibres of the crystalline lens are described as having dentated edges, which fit into the corresponding structure of the

The following is what I have observed both as to the genesis of the cells and their transformation :—

In the fœtal goat of two months old, and six inches in length, the crystalline lens is biconvex, and made up of three parabolæ, the apices of which meet in the centre, thereby leaving at that point a spherical triangular space. The central portion of the lens, extending to near the periphery, is opaque, and this opacity is greatest in the centre, becoming less and less so as you approach the outer edge. The part most opaque is composed of minute granules or utricles, which, as you approach the outer edge, are larger and larger, until on the very border of the opacity they are seen as quite sizable vesicles, some of which are nucleated, in fact, cells. Thus you have, in this opaque central portion, an appearance of all the transitionary stages of minute utricles to large vesicles and cells ; and this development has occurred by mere expansion of the former, and *not* by the *Schwann mode* of cell development.

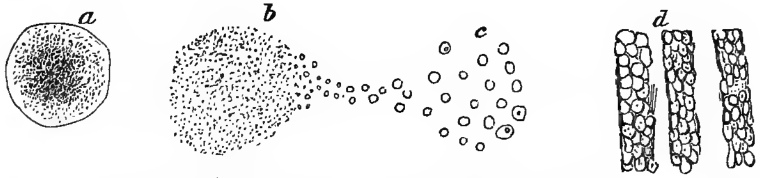
At the confluence of the opaque portion with the clear peripheric border, there is perceived a tendency of the cells or vesicles to arrange themselves into rows or serial longitudinal groups, which rows are parallel with the long diameter of the parabola. Then, by the confluence of these rows of cells, tubes are formed, and from these the fibres. Therefore these tubes have their origin, *not* by the elongation of single cells, as Schwann has supposed, but by the coalescence of piles of cells into one continuous tube. These observations, therefore, correspond with those of Barry and Toynbee, as above mentioned. In this instance it was truly beautiful to observe, how the clearing up of the structure took place just in proportion as the constituents assumed the cell-form; thus showing that this tissue only attains its wonderful function by the remarkable process of cell-genesis—a process by which form and order appear to start out of chaos, and that which is dark and obscure is made almost as clear and transparent as light itself. And when one has seen these

contiguous fibres. This serration was, as is well known, first pointed out by Brewster with the lenses of fishes. But from a rather detailed examination of the subject of late, I am satisfied, that, whatever may be said of it with fishes, it does not belong to the higher animals. With *perfectly fresh* specimens from man and many of the higher mammalia and birds, I have been unable to perceive the least dentation. The fibres are perfectly smooth and contiguous; this has been the invariable appearance when very fresh specimens were used. I cannot but believe, therefore, that this crenulated condition is due to a change in the fibre after death ; a change, perhaps, like that leading to like appearances in the blood-corpuscle, and due to a partial abstraction of its liquid contents, the sheath of the fibre becoming ruffled.

things, does it not bring home to the mind with suggestive grandeur and beauty, the recollection of that fiat of the Deity, which is recorded—“And God said let there be light, and there was light?”

We will now see how these phenomena are manifested in *oviparous vertebrata.* In the chick which I have especially examined, the phases of formation are best examined at about the eighth day. Here, as in the former instance, a part is cellular and granular, and another part tubular, or soon to become so.

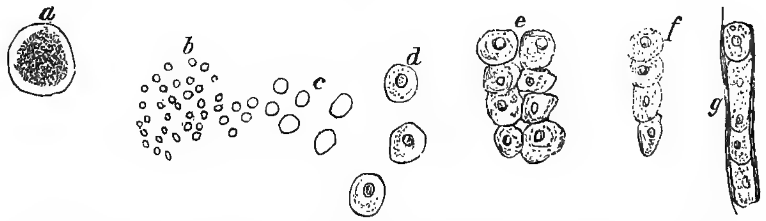
Fig. 29.



Formation of crystalline lens in mammalian vertebrates, from fœtal goat of 2 months. *a.* The lens with a dark centre, clearing towards periphery, where there is a light transparent border. *b.* Minutely granular structure of centre, composed of very minute utricles, which become visibly vesicles as you approach the outer edge. *c.* Vesicular or cell-structure of portion where pellucid border passes into transparent periphery. Some are distinctly nucleated. Size 1-2000. *d.* Structure of transparent periphery composed of tubes formed by linear aggregations of the cells.—*a, b,* and c, magnified 225 diameters. *d,* magnified 450 diameters.

The minute vesicles are here observed, gradually expanding, until they have reached a certain size, when they become nucleated. There are, therefore, both nucleated and non-nucleated cells. When the cells have become nucleated, they begin to arrange themselves into rows, and then by coalescence form tubes. There is, however, this difference between those of the goat and chick, as I have observed: In the goat, several rows of cells form a tube, while in the chick the same result is obtained by a single row. Afterwards, the nuclei gradually disappear, and beautiful transparent fibres appear.

Fig. 30.



Formation of crystalline lens in oviparous vertebrates, from chick of 8 days. *a.* The lens with a dark granular centre, and transparent border. *b.* Granular or minutely utricular structure of the central portion. *c.* Vesicular structure of portion intermediate between the centre and clear border. *d.* Vesicles that have become nucleated. e. The same arranged in rows. *f.* The same coalesced or fused partly. *g.* A single row of cells, forming a tube, not so much by a coalescence of their own walls, as by a plasmatic membrane enveloping them. (Magnified 350 diameters.)

The crystalline-lens, therefore, is truly a tissue of cell-metamorphosis, and from its delicacy and beauty has well thus briefly arrested our attention.

It will be seen that the phenomena of this metamorphosis, as I have observed them, are of the same nature as those involved in the formation of muscular fibre, that is, by a kind of intersusception, and *not*, as Schwann has thought, by immediate fibre-transition.

With this, I close the genesis of cells in particular, and their metamorphoses into special tissues.

CHAPTER IV.

CELL-FUNCTION, OR THE PRODUCTS IN CELLS.

In the preceding chapter we were engaged in tracing out the genesis of cells in particular, and the phases of their metamorphoses into special tissues. However carefully this was or may have been done, it gave us no insight into the conditions of cells as *individual*, active agents. As yet we have become acquainted with them only as physical objects, and we know them as we know a machine, by its size, shape, and general aspect. Such is the broad distinction between anatomy and physiology: the former teaches us nothing of the latter, and the *vice versa* is nearly equally true. Our knowledge of what cells *do* (not become) must be learned of itself, and proved by experience. I propose, therefore, to consider them in this light, taking up each class by itself, as was done in the genesis.

I should mention, however, that cell-function, or the products *in* cells, can belong only to individual cell-structures, or those in which the identity of the organ as such is not lost in a compound tissue.

I. Epithelial-Cell Function.

The epithelial cell is the great secreting agent of the animal economy. By it, the essential products of life and its reproduction are eliminated; and on it, rest all the grand phenomena of nutrition and secretion. Its full discussion, therefore, would involve many points for consideration, which otherwise would not fall within the domain of this work. I have already pointed out its anatomical peculiarities, which, however diversely it may be situated, are invariably the same.

It is the *essential* structure in all glandular organs, constituting the skin, and covering all mucous and serous membranes. These are facts so well recognized in science, that their consideration in detail is not necessary. It would be easy for me to take any gland, and, by reference to my sketch-book, demonstrate its leading features. The *mammary gland*, for instance, could be taken, and by reference to observations throughout the mammalian vertebrates, its intimate structure be shown to consist of ampullæ, lined with epithelial cells, by which alone the milk is formed or elaborated from the effused plasma of the blood.

Or, in the same manner, I might take the liver, which appears to have an existence in the animal economy, even more common than that of the heart itself,* and show that the elimination of bile is performed by cells alone among the lower invertebrata;† they exist as simple cells, having no basement stroma and no ducts; in fact, in the simplest form. As we advance, we find these cells having more complicated relations with nutrition and secretion, and wrapped into an organ of a separate and distinct character. Without entering into any of these discussions as to the minute anatomical relations of these cells, and which has been a very mooted point of late, I think I may say that the fact of the cell-origin of the bile or its analogue is quite clearly established.

The remarks just made belong in common to all the other glands of the body which elaborate liquid products, wherever they may be situated. It is not for me to take up here their anatomical relations. I have finished with them when I make the general statement as a scientific fact, based upon my own, as well as upon the observations of others, that the peculiar products of each gland are eliminated by them *as nucleated cells*. If it is asked *how* such is accomplished, I should reply, by the action of the cell-wall; it being the peculiar property of the walls of liver-cells, for instance, that they should elaborate bile from the *plasma* of the blood, which transudes through them and is changed to bile by coming in contact with the nucleus. The same is true of other secreting cells. Beyond this, physiology and experience have taught us nothing; and what is more, it is now difficult to conceive how they ever can. The *why* that one cell should

* The remark of Haller is not broad enough. He says: "Laté per animale regnum hepar dominatur, et si paulo angustioribus finibus continentur, quam aut intestinum—aut con."

† Siebold; Comparative Anatomy, by Siebold and Stannius, transl. by Burnett, vol. i. § 37. (This I have seen myself also.)

secrete bile, and that another, exactly like it in physical aspect, should secrete milk or saliva, will long remain an enigma in physiological science.

The *solid* products in epithelial cells now deserve our attention, and they possess full the interest of those we have just been considering, because they have an individuality of their own. I refer here to the embryo as the product of the *ovum*, and to the spermatic particle as the product of the *sperm-cell.* What the function of these two cells is, and how it is discharged, and what is the nature of the product, is worthy of our consideration in a more than usual degree. Some detail will, therefore, be necessary.

We will first refer to the homological conditions under which the ovum and sperm-cell are to be regarded. The ovum is the sole product of the ovary, and the sperm-cell that of the testicle. In a histological point of view they are the same, and although it might be difficult to make this plain with the higher animals, the conclusion is *forced* upon us when we study some of the invertebrata. With many of these last, there are no outward distinctions of sex, and those which are internal are purely microscopic. With many of the Radiata, upon which I have of late been considerably at work, this is especially true. The testicles are situated exactly as are the ovaries, and the testicular ampullæ correspond in number, size, and general aspect with the ova, and one can be distinguished from the other only by its contents. This is so, for instance, with the asteroid Echinoderms.

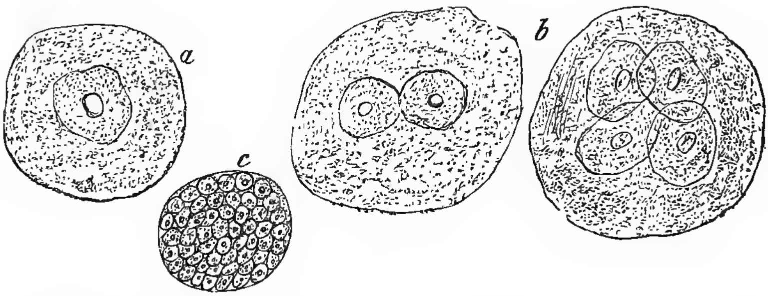
In the Bryozoa, which are androgynous, I have found these distinctions even less marked. Thus in the genus *Alcyonella*, there are a few cells in one part of the animal, which go on to form ova, while other cells, apparently identical in physical points, go on to form spermatic particles.

Among the Annelida, the same is seen in a truly beautiful manner. Take, for instance, the genus *Nais*. Here you find the ova, situated in a row on each side of the alimentary canal, each consisting of a little grain about the size of a pin's head. In the male there are similar bodies, having exactly the same locality; these are sperm-cell capsules; and the only difference I have found between these little grains is, that they are white in the male, and yellow in the female.

Among the higher invertebrates, as, for instance, with the Articulata, these homologies of the two organs are distinct, but less so than in the instances I have just quoted. From all this it may be safely inferred, that the ovary and testicle, as individual organs, consist only of a tissue which supports a cell-structure, which is the essential part. Now as this cell is an epithelial cell, we can truly say that, histologically, they are one organ. But perhaps the best part of the argument is yet to come, for by illustrating the processes they undergo, the morphological identity will be clearly made out, although we should not forget that there is a broad teleological difference.

The ovum exists at first as a simple nucleated cell, and it goes on to the attainment of its full size by the endogenous formation of cells within it. This last takes place out of the fertile blastema, which transudes its walls, and according to the laws of cell-genesis already pointed out. Having attained its full size, this endogenous increase ceases, and then the ovum is ready for fecundation. By this last process, it is endowed with an individual potency, upon which succeed changes ending in the elimination of a distinct being. These phenomena have their first expression in the segmentation of the vitellus, dividing equally into two, then into four, eight parts, and so on, until the result of the last division is quite small, the whole being reduced to a granular mass, out of which the embryo is shapened and formed.

Fig. 31.



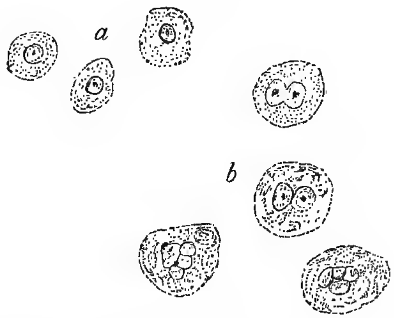
Ova and the vitelline segmentation. *a.* An ovum in natural state. *b.* Segmentation singly and doubly occurred. *c.* Segmentation completed, and ovum ready for the morphological changes of the embryo. (Reduced size.)

But if this segmentation, for this definite object, is always indicative of an impregnated state, the simple phenomenon of segmentation has not this signification, for it occurs in many ova, to a limited extent, before fecundation has taken place. This I have observed with the ova of fishes and many of the Articulata.

You may ask: What, then, is the signification of vitelline segmentation? To this it may be replied, that it is the first and only unmistakable expression of vitality we know a cell or an ovum to possess. In common cells, and in the unfecundated ovum, it is an abortion;

but from the act of fecundation it receives an individual power, so that it goes on, ending in the preparation of the material from which

Fig. 32.



Ova of common Cod (*Gadus morrhua*), showing limited fissuration of vitellus. *a*. Ova resembling nucleated cells. *b*. The same, with fissuration to number four.

the new being is evolved. Here it has acquired a positive character; and perhaps the best evidence that segmentation to a definite end lies at the bottom of the primitive development of all new individual beings when produced by means of eggs, is the fact that it occurs most extensively in those portions of the embryo that afford the highest expressions of the animal as such, viz. the nervous system.

The changes supervening upon this vitalizing process of segmentation, and which are the working out of the future embryo and the allocation of its organs, constitute the science of embryology. They involve details as numerous as the types of the whole animal kingdom, and cannot, therefore, be taken up here. What I have here said of the animal ovum, belongs equally well to that of the vegetable. For this I cannot rely upon my own investigations, but trust to the excellent researches of Robin.*

We will now refer to the sperm-cell, the analogue of the ovum. First, it is seen as a simple, nucleated, epitheloid cell. It increases, and after having attained its adult size, it experiences processes which mark its vitalizing character. This consists, as in the ovum, of a segmentation of the nucleus or vitellus, first into two, then into four, eight, &c. parts, the parent vesicle expanding in the meanwhile. So that, after a time, we find the parent vesicle filled with many

* See an article by Robin, in Compt. Rendus, 1849, titled "Ovum, the existence of an, as well in the male as in the female of Plants and Animals; producing in the one case spermatozoa, and in the other the primitive cells of the embryo."

nucleated cells. These, of course, are of a uniform size, and usually of an even number of the multiples of two.*

The next changes are those concerned in the immediate formation of the spermatic particle, which always occurs according to one of two modes, the character and peculiarities of which will be made clear as we proceed. The parent vesicle is filled with nucleated cells, produced by segmentation of the nucleus or vitellus. This is the point from which we will now start. According to one method, and which I have called the *special-cell mode*, the process of division here ceases, and *in* each of these cells is produced a spermatic particle. But according to the other method, and which I have called the *fasciculus mode*, a kind of liquefaction or farther subdivision of these cells ensues, until the whole substance is reduced to a fine granular mass. Then out of this mass, by longitudinal fissuration, the spermatic particles are produced; as, if I may use this homely comparison, a stick of timber is split, with the grain, into sticks; the whole forms a fasciculus, and hence the name of this mode. After this general statement we will look at the subject a little more carefully.

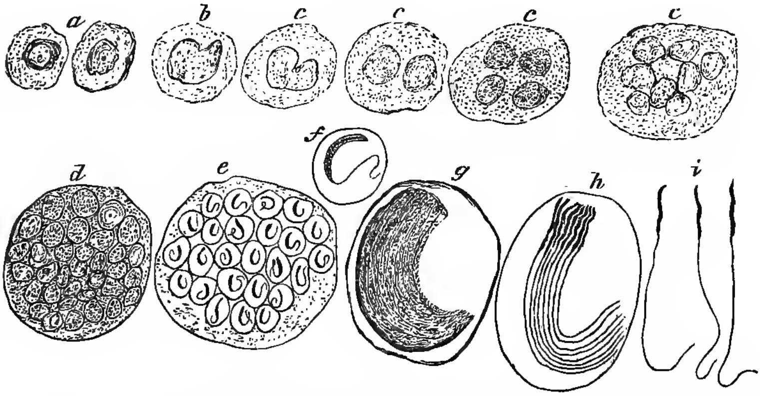
It will be seen that the preliminary changes of the genesis of the spermatic particle are identical with those of the ovum. This great fundamental fact at once establishes a unity throughout nature in the process of reproduction. To settle this fact, my own observations have extended over the entire range of the animal kingdom, embracing most of the grand types of structure. Beside, I am now extending it to *species*, as rapidly as time and opportunities will permit. The fact, however, I now consider as well authenticated as any in histology.

Our next inquiry is, how is the spermatic particle formed? According to the *special-cell mode* it is *a metamorphosed nucleus*. Of this I have satisfied myself beyond a reasonable doubt. It can be well observed among the mammalian vertebrates, or in the Arachnida. According to the *fasciculus mode*, the particles are formed, probably, from an arrangement in a linear series of the granules produced by the extended subdivision. These unite, and, blending, form continuous threads. But this is a point of which I am far from being equally well satisfied. The phases of this change can be best observed in

* Sometimes this segmentation is not regular, and then the number of the daughter cells does not correspond to the multiples of two; thus, there may be seven instead of eight, one of the cells not segmenting. This is sometimes observed likewise with the ovum.

the *insessorial* birds. It may be asked if, in this case, it is not the nucleus of the pre-existing cells which is metamorphosed? To this

Fig. 33.

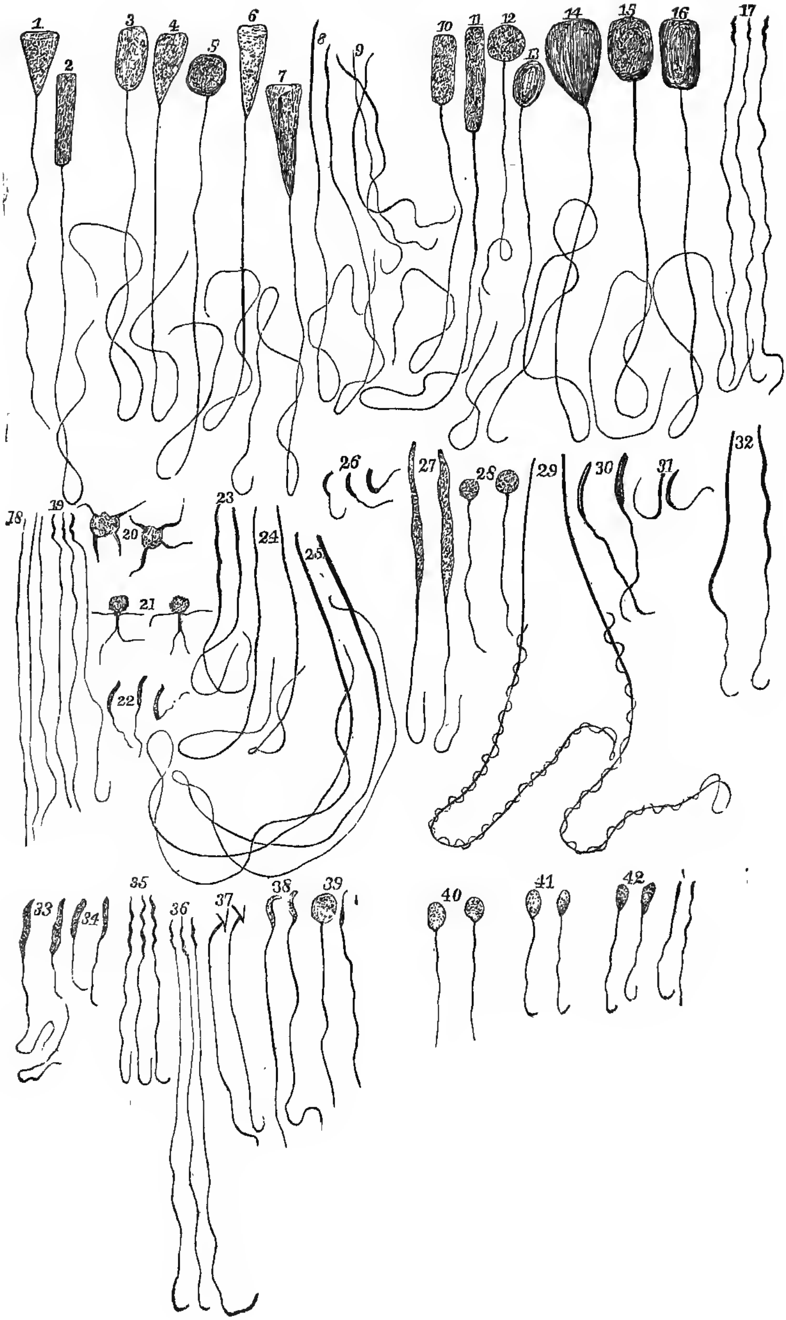


Formula of the two modes of sperm-genesis. *a.* Epithelial cells of the testicle nucleolated, which are to be sperm-cells. *b.* The same with the nucleus undergoing fissuration. c. The fissuration still progressing. *d.* The parent-vesicle filled with cells, the result of subdivision. At this point the fissuration ceases, when the spermatic particles are produced according to the *special-cell mode.* *e.* Formation of spermatic particles according to the *special-cell mode;* each divided cell producing one particle. *f.* One of the cells still higher magnified, showing how a spermatic particle replaces the nucleus. *g.* Formation of spermatic particles according to the *fasciculus mode;* the granular mass being the result of the liquefaction of the cells of *d.* *h.* The formation taken place, the spermatic particles lying in a bundle. *i.* Spermatic particles isolated, showing plicated aspect of head.

I should be inclined to answer negatively; and the evidence against any such supposition is, 1st, you perceive the whole granular mass to be divided in this way; and 2d, the fascicular manner in which the spermatic particles are found in the parent-cell could not, seemingly, in very many instances, have been attained, only by such a mode of formation. It is, however, a remarkable fact which I have observed, that the number of spermatic particles thus produced, corresponds with the number of cells existing before the granular liquefaction takes place. Thus seeming to show that the individuality of the cell is not lost by this last change, but appears again with a certainty in the result produced. Thus, for instance, a parent sperm-cell contains sixteen vitelline cells; these liquefy and become a granular mass, but out of this last are produced sixteen spermatic particles. There is not, therefore, a real unity in their genesis after the preliminary changes have occurred, if we are to judge from appearances alone; but if we consider the identity of the nature of the cell and its nucleus, as formerly established, this unity is established.*

* Since the above was written, some special investigations I have made on the spermatic particles of insects, would lead me to think the point still an open one, if all

Fig. 34.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Figures of spermatic particles of a representative of each family throughout the animal kingdom, showing the variety of forms met with, beginning with the lowest and ending with man. 1. Astrangæ Danæ. 2. Alcyonella (nov. sp.). 3. Tubullaria laryngæ. 4. Actinia marginata. 5. Asterias spinosa. 6. Echinocidaris punctulata. 7. Mellita quinquepora. 8. Tænia (nov. sp.). 9. Lumbricus terrestris. 10. Ascidia amphora. 11. Botryllus stellatus. 12. Polyclinum (nov. sp.). 13. Ostrea edulis. 14. Mytilus edulis. 15. Anodon fluviatilis. 16. Unio complanatus. 17. Eolis Bostoniensis. 18. Buccinium obsoleta. 19. Limax agrestis. 20. Pilumnus Harrisii. 21. Pagurus longicarpus. 22. Epeira vulgaris. 23. Hæmatopinus suis. 24. Melolontha subspinosa. 25. Antiope vanessa. 26. Libellula grandis. 27. Platessa flesus. 28. Leuciscus chrysoleucas. 29. Salamandra symmetrica. 30. Tropidolepis undulatus. 31. Rana pipiens. 32. Heterodon niger. 33. Emys picta. 34. Gallus domesticus. 35. Muscicapa tyrannus. 36. Fringilla melodia. 37. Didelphis virginiana. 38. Neotoma floridana ('lateral view). 39. Sciurus hudsonicus. 40. Capra capricornis. 41. Canis familiaris. 42. Man (' lateral view). From 1 to 7, and from 10 to 16, inclusive, magnified 1500 diameters; the remaining, 350 diameters.

The SPECIAL-CELL MODE of the genesis of spermatic particles occurs in the *Mammalian Vertebrates;* all the *Grallatorial Birds,* also the *Accipetres* and *Scansores;* in the *Chelonian, Anourous Batrachian, Saurian* and *Ophidian Reptiles;* in the true *Osseous Fishes;* in the Arachnida; the families *Libelullidæ, Phryganeidæ, Phasmadæ,* and *Andrenidæ* of the Insecta; in all the *Crustacea;* in the *Gasteropod Mollusca,* and in all the *Radiata.*

The FASCICULUS MODE occurs in the *Insessorial Birds;* the *Caudate Batrachians;* the *Plagiostome Fishes;* the *remaining families* of the Insecta; the *remaining Mollusca;* the *Annelida.*

The spermatic particles eliminated according to the *special-cell mode* generally consist of a staff, to which is attached a delicate tail. On the other hand, those produced according to the other mode have a filiform aspect, there being generally no distinct division between the staff and a tail.

The variety of forms, of which each animal has one, and one only, is quite extended, and presents to us a field of study having not only the deepest interest, but the highest zoological importance, and especially as relating to classification; for as the spermatic particle is the simplest representative we have of the potential whole of the animal, so may we well regard its forms as constituting the basis of a classi-

spermatic particles are not developed by the *special-cell mode,* being therefore metamorphosed cell-nuclei, and subsequently grouped in a regular fasciculate form. Thus, the very long filiform spermatic particles of some of the Lepidoptera, I have seen developed in special cells, while in other cases these particles may be observed in such a perfect fascicular form as to indicate that they were developed in bundles; however, these bundles are never observed within parent cells, as is true of those of the sparrows. In these last (sparrows), both the size and the length of the particle would seemingly preclude the view of their special-cell development; for, in some instances, the number of particles thus grouped in a parent cell was such that had they had their development in cells specially, these cells must have been so large that a corresponding number could not have been contained in that parent cell. But the subject needs a careful revision as to this point.

fication founded upon the simplest and most natural data. But both a description of these forms, and a detail of these classific relations, is a subject of itself alone.*

There can now be no reasonable doubt that the function of the spermatic particle is that solely of fecundation, and that this last is produced by it alone. In the first place, the morphological identity of the two particles, the *ovum* and *sperm-cell*, as I have already pointed out, would of itself lead us to infer that the *special* product of the latter was for individualizing the whole of the former. In fact, such an inference would be in perfect accordance with our highest ideas of the unity pervading natural phenomena. But, aside from this induction, the point has been settled by direct observation.

The experiments of Spallanzani† proved this point as clearly as it well could be at that period. Since then the observations of Kölliker,‡ Newport,§ and Agassiz,|| have conclusively settled the matter. The evidence consisted in having the ovum so situated that nothing of the semen but the spermatic particle itself should come in contact with it. I will add to the above the evidence of my own observations, which serve not only to elucidate the above-mentioned point, but also to show that one, or at least a few particles are sufficient for a single ovum. In some of the low Acaridæ, as, for instance, those of the genera *Sarcoptes* and *Dermaleichus*, the generative organs of both sexes are situated on the inner surface of the abdominal sac. In the female these consist of ova, and in the male they are sperm-vesicles.

Now the number of ova and sperm-cells correspond quite closely; so that while in some species you find ten or twelve ova, there appear to be only about the same number of spermatic particles.

There is no semen proper, for the sperm-cells discharge their contents as spermatic particles, and the only liquid is that which belongs to the cells. The long period of copulation of these animals is secured by the strong anal hooks of the male fastening into the abdomen of the female.

Here, then, just the experiment you desire, is performed by na-

* For a farther discussion of this point, see a paper of mine, Relations of Embryology and Spermatology to Animal Classification. Proceedings of the Amer. Assoc. for the Advancement of Science. Albany meeting, 1851, p. 312.

† Spallanzani; Nouvelles Recherches sur les dicouv. microscop. London, 1769.

‡ Kölliker; Die Bildung der Samenfäden in Blaschen, Nuremberg, 1846.

§ Newport; Philosoph. Transact. 1851, p. 169; and Proceed. Roy. Soc. June 17, 1852.

|| Agassiz; Oral communication.

ture; and a better proof of the sufficiency of a few particles could not be obtained.

From similar experiments, it has been shown that, in the act of fecundation, the *simple contact* is enough; in other words, the spermatic particle loses none of its material form by discharging its function of vitalization.* If it is asked, *how* this function is performed, to this it must be replied, that this has not yet been reached by physiological inquiry. And what is still more worthy of remark, it is difficult, in the present state of our relations to science, to conceive how we shall ever be much better able to seize hold of the intimate nature of these phenomena. We may call it by this name or by that, and such words may be adopted, although they serve to cover up our ignorance; for they are, in some way, and to some extent, expressive of conditions which the scientific mind by analogy alone can comprehend. But, at the same time, it should be remembered that the *ultimata* of all science having anything to do with vitality, must ever rest on words or conditions equally as vague and unsatisfactory.

However, some special considerations on this point will be offered in the philosophical portion of this work.†

* See farther, a discussion of this intricate subject in Silliman's Journ. Nov. 1853.

† The bibliographical relations of the subject of spermatic particles here deserves notice. The older anatomists, Leeuwenhoek,* Ledermuller,† Spallanzani,‡ Gleichen,§ and others, not only recognized their existence, but even studied their form and peculiarities. This is especially true of Spallanzani. At the beginning of the present century, they excited considerable attention, and were especially studied by Prevost and Dumas,‖ and others; and the same is true of the time of the improvement in microscopes, about the year 1830. Dujardin,¶ Czermak,** Treviranus,†† Von Siebold,‡‡ and Wagner,§§ each contributed considerable towards the advancement of these studies. But, on the other hand, they tended to retard its true progress, by asserting that these particles are *animals*, a view which was supported by Valentin,‖‖ Gerber,¶¶ Schwann,*** Pouchet,††† and others. To Wagner‡‡‡ and Kölliker§§§ we are indebted for a complete

* Leeuwenhoek; Anatomia, seu interiora semen, Lugd. 1687.

† Ledermuller; Physikalische Beobachtungen du Samen thierchen. Nuremberg, 1756.

‡ Spallanzani; Nouvelles Recherches sur les décour. Microscop. London, 1769.

§ Gleichen; Abhandlung ueber du Samen, und Infusions thierchen. Nuremberg, 1788.

‖ Prevost et Dumas; Annal. des Sc. Nat. tom. i. and ii.

¶ Dujardin; Annal. des Sc. Nat. tom. vii. pp. 291–297.

** Czermak; Beitrage zur Lehre von den Spermatozoen. Vienna, 1833.

†† Treviranus; Tiedemann's Zeitschrift, vol. ii.

‡‡ Von Siebold; Müller's Archiv. 1836, s. 232, and 1837, s. 381.

§§ Wagner; Fragmente zur Physiologie du Zeugung, 1837.

‖‖ Valentin; Repertorium, 1837, p. 134.

¶¶ Gerber; Allgemeine Anat. &c. p. 210.

*** Schwann; Mikroscop. Untersuch. &c. Berlin, 1839.

††† Pouchet; Théorie positive de l'ovulation spontanée, &c. Paris, 1847, p. 321.

‡‡‡ Wagner; Hist. de la generation, &c. Bruxelles, 1841, p. 26.

§§§ Kölliker; *loc. cit.* Berlin, 1841.

The great fundamental fact at which we have arrived by these studies, is the morphological identity of the ovum and sperm-cell; and, by way of conclusion, I subjoin, in a tabular form, their expression, in a brief manner:—

The Sperm-Cell.	*The Ovum.*
1. Is a nucleolated cell.	1. Is a nucleolated cell.
2. The nucleus, increasing and becoming granular, undergoes segmentation.	2. The nucleus (vitellus), increasing and becoming granular, undergoes segmentation.
3. The result of this segmentation is, that each of the subdivided cells forms a spermatic particle.	3. The result of this segmentation is, that *all* the subdivided cells, by a metamorphosis, form an *embryo*.
4. The function, then, of the sperm-cell, is to eliminate the vitalizing spermatic particles.	4. The function, then, of the ovum, is to eliminate the vitalized product, the *embryo*.
5. In the lowest sense of the term, the spermatic particle is *alive;* that is, it is an organized acting form. But it is so only in the lowest sense, since it holds no relations whatever with the external world. On this account, it can never be an *animal.*	5. In the lowest sense of the term, the embryo is *alive;* that is, it is an organized acting form. But it is so in the lowest sense of the term only, since it sustains no relations to the external world. It is *not an animal* until it does.

The above views were published by me in a memoir, presented to the Amer. Acad. of Arts and Sciences, July 1851: Researches on the Origin, Mode of Development, and Nature of the Spermatic Particles among the Four Classes of Vertebrated Animals.

Note.—In the foregoing account of the morphological relations of the spermatic particles as cell products, nothing was said relative to analogous phenomena in the vegetable kingdom. The subject of the existence of true spermatic particles in plants has been such a mooted one, that I did not think proper to introduce it in the text. But for my own part, I believe that, as to morphological conditions, the phenomena of reproduction in the animal and the vegetable kingdoms, are analogous throughout. In the study of nature we must not let names confuse our ideas of things; and, call them what you will, I think there is no doubt that there are in plants particles for fecundation, exactly corresponding to the spermatic particles in animals. The doctrine of vegetable spermatic particles has been treated with ridicule by Schleiden (Principal of Scientific Botany, &c., translated by Lankester, London, 1849, pp. 99, 359); but aside from the weight of analogy, recent investigations have shown that it is not so easily disposed of. Without discussing the subject historically, I will allude first to the remarkable memoir of Robin, on the existence of an ovum, as well in the male as in the female of plants and animals; producing in the one case spermatozoa, and in the other the primitive cells of the embryo; see Compt. Rend. 1849. The object of this memoir is to show the com-

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explosion of this view, besides otherwise treating the subject in a very profoundly physiological manner. They have figured and described many forms, and given the whole subject an individuality it never can lose.

But by far the most comprehensive article that has appeared on the subject of the spermatic particles, is that of Wagner and Luckardt, in the Cyclopœd. of Anat. and Physiol. Art. Semen.

plete morphological identity of the male and female reproductive primordia, throughout all organized forms. Not only are the cell-phenomena of the male and female products of the same kingdom identical morphologically, but also of the same sexes of the different kingdoms. Leaving out the weight of analogy, *à priori*, the observed processes of cell life clearly indicate that there are in plants male particles of fecundation corresponding to the spermatic particles with animals. These are the pollen-grains with the phanerogamia, and the contents of the antheridia with the cryptogamia.

See, for a farther consideration of this subject, a memoir of Thuret on the anthers of Chara, &c. Ann. d. Sc. Nat. xiv. p. 65, but especially the work of Hofmeister, "Vergleichende Untersuchungen der Keimung, Entfaltung und Fruchtbildung höherer Kryptogamen, und der Saamenbildung der Coniferen." Leipzig, 1851.

Cilia.—We now come to the consideration of another part of our subject, of the products of epithelial cells. I refer to the formation of *cilia;* and although it is not, it is true, an endogenous production of cells, like those we have just considered, yet its discussion cannot be better taken up than in this connection.

These cilia are solely the product of epithelial cells, and give to those on which they are found, a function different from that of the true secerning organ. They are minute teat-like processes of the cell-membrane, capable of moving backward and forward, and therefore have a waving oscillatory motion. These cilia exist on the epithelial cells of those membranes which have an external outlet, and therefore may there be regarded as truly excretory organs; for by this oscillatory movement towards the external opening, they are able to convey an astonishing amount of material. As bearing out the homology of the epithelial cell, and the ovum, they are found existing on the surface of the latter, especially among the Mollusks and Radiates; and here their potency as organs of locomotion cannot be appreciated, unless one has studied these lower forms, for they really constitute the active agents of their life and motion.

When existing on cells, they are upon one of its surfaces only, studding its crown, or the part opposite that by which the cell is attached to the basement-membrane. When existing upon ova, they generally cover its entire surface. Their formation upon epi-

Fig. 35.

Formation of Cilia from Mucous Membrane of Frog (*Rana pipiens*). *a.* A cylindrical epithelial cell, the upper portion of which presents a bulging. *b.* The same, still farther progressed. *c.* This "bulged" portion beginning to split. *d.* Cells on which this splitting has taken place, forming cilia.

thelial cells, I have had the good fortune to observe, and their genesis here may be considered as an expression of their formation everywhere.

A nucleated epithelial cell exists upon a mucous membrane; if cilia are to be formed from it, it lengthens and becomes somewhat cylindrical in shape. A part of the end which is free then becomes a little constricted, or partially separated from the rest, as though a minute thread had been drawn around it. This constricted portion is the part out of which the cilia are to be formed. It then divides into fasciculi, each of which splits up into cilia.*

The formation of cilia, therefore, is only the splitting up of *a portion* of a cell into filaments. But, on the other hand, there is reason to believe that each of these cilia is tubular; in other words, hollow like the finger of a glove. If this is true (and I can assure any one that it is a point exceedingly difficult to settle definitely), there is a deception in the appearances presented; for they then must be regarded as *ex* vaginations of the cell-wall, or the pushing out of teat-like processes from its surface; and this could well take place in that constricted portion which I have described.† But this is a point needing much more study and attention. However, from what I have really observed, I must, for the present at least, regard cilia as *digital portions of cell-membrane.*

They therefore should have the capacity of cell-membranes; and when this is fully considered, it can cause no wonder that all the varieties of motions here observed should be alone due to the intrinsic power of the cells themselves, and without the agency of those tissues by which similar phenomena in higher and more compound life are necessarily connected.

Lasso-cells, or the tactile and prehensile cells on the surfaces of Medusæ.—It may seem somewhat strange that I should include these cells under the head of epithelia. But, from a somewhat careful study I have of late given them, I am inclined to the opinion that, histologically, this is their nature. And as such, a consideration of

* The observations of Kölliker are exactly to the same effect. His investigations were made upon the epithelial cells of the oviduct of Planorbis corneus; *vide* Beitrage zur Kenntniss, &c. Berlin, 1840, p. 33, pl. i. fig. 12. My own observations were made upon epithelia of the mucous membranes of Batrachians.

† For farther details on this interesting subject, I may refer to a paper published some years since. See Histological Researches on the Development, Nature, and Function of Epithelial Structures, Amer. Journ. of Med. Sci. N. S. vol. xx. p. 70.

their products may be properly introduced here. Even were this last not true, I do not think I could well desist giving them here a careful review, on account of the many beautiful phenomena presented.

And without forestalling my subject, I may say that the studies I have made of these lasso-cells have been to me a source of more delight, and has given me a broader view of the signification of elementary forms, than any other of equal magnitude I have ever taken up.

By lasso-cells, I mean a kind of cell which is found upon the body, and especially upon the tentacles of the *Polypi* and *Acalephæ*, as well also as upon some of the *Mollusca*, and which also exist upon the external surface of many of the internal organs. These cells are generally of an oblong oval shape, of a variable size, and contain in their interior a long filament coiled up upon itself. This filament is thrown out, and thereby serves as a prehensile organ by winding around the minute object to be caught.

This cell is attached by one surface, one of its short diameters, and from the other extremity, the filament passes out. This filament is a part and parcel of the cell, a continuation of its membrane by a slender tube coiled up in the interior; its expulsion, therefore, is by its inversion, or a turning inside out. In other words, and perhaps expressing it more clearly, the whole is a cell, which has a long process of itself extending into its interior, and which is thrown out by inversion, exactly as may be a finger of a glove.

But to look at the subject a little more particularly. This filament is a very delicate tube, and has upon its surface, as observed when out of the cell, numerous little barbed points, which are arranged somewhat regularly, and all of which point backward towards the cell. However long and delicate this filament may be, these barbed points exist to its very extremity. This is a point which does not appear to have been before recognized, and which I did not perceive myself until quite recently, when working with condensed light. There appear to be two varieties of these cells; first, those containing nothing but the tube, and which, as such, connect with the cell; and second, those into which there is a kind of stigma, or stock, from which the filament proceeds, and which is the bond of connection between it and the cell. This stigma is about twice the size of the thread, has a somewhat ventricose shape, and, like the tube, has numerous points. It is of course inverted in being thrown out. Another difference between these two kinds of cells, is, that with the stigmated ones the thread is rather short, say from ten to fifteen times the length of the cell, while in those not stigmated the thread is very long, and often sixty to eighty times that of the cell.

That this slender tube is thrown out by a process of inversion, is proved by watching it, as Prof. Agassiz and myself have repeatedly done, and also by the fact that, when in the cell and coiled up, the barbed points on the thread, and especially upon the stigma, where they may be well seen, point forward, or, rather, in the opposite direction to what they do when it is uncoiled. The beautiful design in these barbed points upon this thread cannot fail to strike us with admiration; for, pointing backward, they serve well to retain the object around which the thread is thrown. These cells are truly prehensile organs, and in handling these animals, it is from them that the itching, nettling sensation is produced, and in some of the Actinia they are so large that you can feel the threads on your finger.

These coarser forms, however, are not the ones in which these phenomena of the lasso-thread can be best studied, but with the coral polypi (*Astrangæ Danæ*, Ag.) they can be most thoroughly made out.

But before I proceed any farther, I will refer for a moment to the history of these forms, that we may see what has hitherto been known on this subject. Wagner* appears to have been the first to notice them with the Actinia, and he has described them as spermatozoa. Subsequently, they were studied by Erdl,† who traced their presence in other genera, and described some of their peculiarities. Quartrefages‡ also observed and carefully figured them. In the same way they have been noticed by Siebold.§ With the exception of Wagner, all these authors regarded them as prehensile organs, serving to seize upon and secure their food. However, all their more minute and more beautiful relations were not known, and this part of the subject has been recently taken up by Professor Agassiz,‖ who has quite exhausted it; and, in so doing, has brought into notice phenomena and physical peculiarities which, in point of beauty, have, I think, no equal in the whole department of histology.

* Wagner; Wiegmann's Archiv, 1835, ii. p. 215, pl. iii. fig. 7.

† Erdl; Müller's Archiv, 1841, p. 423, pl. xv. figs. 3, 6, 8, 9.

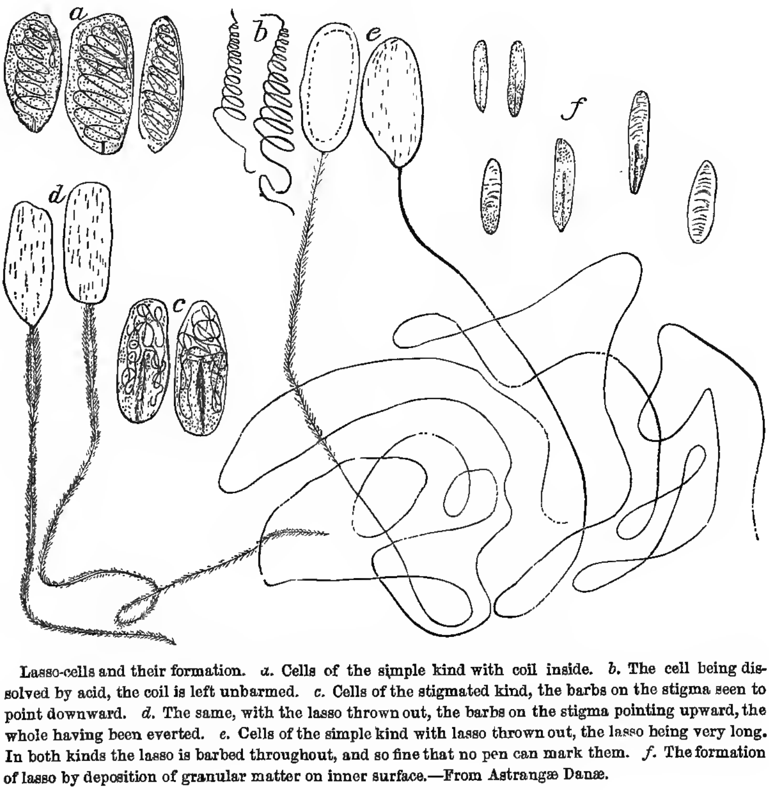
‡ Quartrefages; Annal. des Sc. Nat. xviii. 1842, p. 81, pl. ii, figs. 4, 6.

§ Siebold; Comparative Anatomy, transl. by Burnett, vol. i. § 28.

‖ Agassiz; From a forthcoming memoir on the coral polyps, to be published by the Smithsonian Institution; but see especially some notes to the translation of Siebold and Stannius's Comparative Anatomy, § 28. Agassiz's drawings are unsurpassed.

When such is the condition of the subject, it might well be regarded as out of taste for me to take it up with any pretensions for its farther elucidation. But it has been my good fortune to make out clearly many points hitherto obscure; this success, however, I attribute to my superior instrument and some other advantageous means for investigation. I have already mentioned the anatomical points which I have made out for myself; we will now refer to the genesis of this body, as I have had the good fortune to observe it. It exists, first, as a small sphere, in fact, a cell (and an epithelial one too, I think), having probably the same mode of primary formation as other epithelial cells. Gradually, it assumes an oblong shape, and has a dark-lined appearance in the centre. This, I think, is the nucleus, or at least is dark granular matter occupying the central portion of the cell. At the next stage, you perceive the spiral thread or tube faintly appearing in a spiral condition, and, as the

Fig. 36.



Lasso-cells and their formation. *a.* Cells of the simple kind with coil inside. *b.* The cell being dissolved by acid, the coil is left unbarmed. *c.* Cells of the stigmated kind, the barbs on the stigma seen to point downward. *d.* The same, with the lasso thrown out, the barbs on the stigma pointing upward, the whole having been everted. *e.* Cells of the simple kind with lasso thrown out, the lasso being very long. In both kinds the lasso is barbed throughout, and so fine that no pen can mark them. *f.* The formation of lasso by deposition of granular matter on inner surface.—From Astrangæ Danæ.

cell increases, so this thread increases likewise, and becomes more and more distinct until fully formed, when the transparency of the cell-wall allows it to be seen most perfectly. It then appears coiled in a very perfect manner, the width of each coil being about that of the short diameter of the cell. It would therefore appear that the nucleus is the starting-point at which the formation of this tube commences. In the stigmated variety it persists to a certain extent on its original form, but in the other kind it is entirely metamorphosed into the tube. This may account, in part, for the greater length of tube in the latter than in the former. In my opinion, this tube is formed by a spiral deposition of matter on the inner surface of the cell, exactly like the formation of the spiral vessels or threads in vegetable cells by the deposition of cellulose. Not only is this view supported by analogy, but also from the fact that the formation, when first occurring, is invisible, being probably finely granular albuminous matter, exactly as is true of cellulose in the vegetable cell. Farthermore, the formation of the spiral threads in vegetables is of nuclear origin, and such we have seen to be the case with these cells.

It is true that the filament is *solid* and not tubular in the vegetable cell, neither has it any connection with the cell-wall by continuity. But this fact does not invalidate the view I have taken; for if a long fibre is to be formed inside of a cell, and to be capable of being projected from it, and yet retain its connection with the cell, it is difficult to conceive how the formation could have been otherwise; and as the very first phenomena are invisible, it seems proper that we should explain them on the grounds of analogy. But it may be asked, what is the signification of all this? As it appears to me, it is this: You have here an endogenously formed cilium; in other words, one form of a ciliated epithelial cell, having a single cilium which is formed *inside* of the cell instead of *outside*, as is the case with ordinary ciliated cells; for, the cilia on common cells we have seen to be only external protrusions of the cell-wall, while in the present case, it is, in one sense, an internal protrusion. This well accords also with the idea of its true epithelial character, which we have asserted on other grounds.*

* I am not yet fully determined as to the morphological relations between these lasso-cells with their filament, in animals, and the cells with spiral formations, in plants. There is, however, this difference in the outset, as to the structure of the filament. We have seen it to be tubular with the lasso-cells, but in plants it is not hollow but solid, and has a circular, an elliptic, or a quadrangular section. Moreover, there is

The spiral, in all my examinations, has invariably been from the left to the right. Thus formed, it is ready to be thrown out, and this projection takes place in the following manner: By some irritation of the cell, probably altering the endosmotic condition of its membrane, there is tendency to expel its contents; and, as I have before said, the filament is thrown out by being everted. This is accomplished so quickly as to almost entirely escape the vision, and when all run out, the whole has the aspect of a sac-like body, into which is inserted a long delicate tube.

It may be asked if the barbed points experience this eversion in like manner? My opinion is that they do *not;* and I base this upon the fact that, when the tube is coiled, they occupy an inverse relative position to the one when uncoiled. They would, therefore, be formations *in* the tube rather than *upon* it.*

By way of conclusion, I will add that when one sees a tube, the *whole* thickness of which is not more than $\frac{1}{25000}$ of an inch, and whose walls are studded with fine and regularly arranged points, and also having a delicacy so great that the whole is capable of being almost instantly turned inside out, he naturally views with wonder the almost infinite plasticity of animal membranes, and his mind is so opened to the wondrous capability of organized forms, that there

no evidence that these filaments in vegetable cells are the result of an internal protrusion of the cell-wall, but they appear to be rather the result of a granular deposit on its internal surface. These remarks apply to the common "spiral vessels," as they are called. But the spiral fibres observed in the Antheridia of Charas, Mosses, and Ferns, appear to be of a different character, and if not shown to be vegetable spermatozoa, may prove to be morphologically analogous to the lasso-cell contents.

Upon the peculiarities of structure of the spinal fibres or vessels in plants, see Schleiden's Principles of Scientific Botany, &c. transl. by Lankester, London, 1849, p. 42; Mulder; the Chem. of Verget. and Animal Physiol. transl. by Johnston, Edinb. 1849, p. 420; and upon the last topic (these fibres as seen in the Antheridia of Charas, &c.) see a memoir of Thuret, Ann. d. Sc. Nat. xiv. p. 65, and Meyen's Physiologie, iii. p. 223.

* The following are some of the genera, in all the species of which the lasso-cells may be found: *Actinia, Meandrina, Manicina, Astraæ, Campanularia, Syncoryne, Coryne, Cyanæa, Aurelia, Thaumatias, Tiaropsis, Hippocrene, Sarsia,* the Hydroid and true Medusæ. Without doubt, farther researches will show that very many other genera are similarly endowed.

For the biography of the lasso-cells, see, beside the authors already quoted: Wagner, Icon. Zoot. tab. xxxiii. figs. 8, 10, 11, A, B, C.; also in Wiegmann's Arch. 1841, i. p. 39; Ehrenberg, Wiegmann's Arch. 1841, i. p. 71, tab. iii.; Kölliker, Beitr. zur Kenntniss d. Geschlichts, v. &c. p. 41; Milne Edwards, Ann. d. Sc. Nat. xvi. p. 223, pl. viii. fig. 9; Philippi, Müller's Arch. 1843, p. 62, tab. v. fig. 9; and Will, Horæ tergestinæ, &c. pp. 79, 81, tab. ii. figs. 23–25.

ensues a train of reflection, than which nothing can be more serviceable or conducive for his broadest comprehension of physiological science.

II. Chyle and Blood-Cell Function.

In the somewhat extended remarks on the genesis of these cells, there was necessarily included some statement of their function. This, however, was not definite, and the importance of the subject demands that it should be considered under a separate section. We have seen that there are various and widely dissimilar opinions as to their genesis. A still greater want of uniformity exists as to opinions of their function. At first, it certainly appears singular that the use of so common a product should remain so long not definitely and indisputably settled. But those who have been at all engaged on these subjects know full well the difficulties attending their satisfactory study. However, I think there are now sufficient data for a safe and legitimate conclusion respecting this matter. In order that the full idea of the subject may be embodied in one consideration, I shall run over the ground in a somewhat comprehensive manner.

We have seen on a preceding page that the nutritive circulating fluid has a rank quite corresponding to that of the animal in which it is found. In the Radiata, it appears little else than the triturated food, from which the nutritive portions are extracted more by pressure than by solution, for there is no elaboration except that of simple division. The circulating nutritive material is only *chyme*, and these animals cannot be said to have any blood.

Among the remaining Invertebrates a special elaboration takes place; for, beside the oral trituration there is a digestion, and the nutritive liquid is the result of this last. It has a higher character, which is expressed in the presence of corpuscles.

In a low sense of the term, these animals can be said to have blood, but this blood is only *chyle* in circulation. As we pass from the Invertebrata to the Vertebrata, an additional element is introduced, for not only does the circulating liquid have chyle, but true blood also; and which last they have, not because they are vertebrates, but because they are of a higher type of structure, and demand a more extended nutrition.

On a preceding page, I have given the reasons why we should regard the white corpuscle of the blood of vertebrates, and the common corpuscle of the blood of the invertebrates, as analogues (not homologues) of each other. The coloured corpuscle belongs to the Vertebrata alone.*

It is asked what is its use? The answer to this question includes that of all the corpuscles of the blood; and a very natural way of seeking it, is by consideration of the difference of the nutritive phenomena in these two grand divisions of the animal kingdom.

This difference consists in the amount and extent of the oxygenation of the tissues. The relations of this oxygenation to tissue are mostly those of a vitalizing character. Among the invertebrata, the simple function of nutrition, or the adding of plastic material to be elaborated by the tissues, is performed by the circulatory system, simple as it is: while the conveying of oxygen is accomplished by another apparatus, which of itself has as wide or even a wider dissemination than the former. But with such a plan the oxygenation cannot be very thorough and minute. The general structure of the vertebrates, on the other hand, does not readily admit of a similar arrangement, and even if it did, it could not meet the activity of their functions, which demand a more minutely distributed form of nutrition and oxygenation. This is accomplished by the nutritive and oxygenating systems being contained in one, so that whenever there is borne the means of nutrition, there is also borne those of oxygenation and vitalization. This general statement prepares us for the view which we are about to take; that it is quite natural that such a circulatory liquid should possess two elements, viz.: 1st, those of simple nutrition, consisting of the plasma and the colourless corpuscles; and, 2d, those of oxygenation, consisting of the coloured corpuscles.

Now, if we are led to adopt this view, from a survey of the nutritive functions as occurring in invertebrate animals, we find it confirmed by an analysis of the phenomena met with in all the higher forms. Thus, in the lowest vertebrates, which seem to exist on

* I scarcely need mention that when coloured blood is met with among the Invertebrata, the colouring matter is in the plasma and not in the corpuscles. Such is true of many of the Annelida, where the corpuscles have the same pale, granulated aspect as in the Insecta. Moreover, this colouring matter seems to hold other than necessary relations with this liquid, for Milne Edwards (*Annal. des Sc. Nat.* tom. x. p. 197) has shown that there are Nemertini having colourless blood.

I cannot regard the observation of Wagner (*Zur vergleich Physiol. d. Blutes*, pt. i. p. 8) as correct, when he alleges that he has seen real red corpuscles in *Terebella.*

Both Leeuwenhoek and Hewson seem to have laboured under a similar mistake in regard to the blood of grasshoppers and larvæ. (See *Hewson's Works, Syd. Soc. ed.* p. 234.)

the confines of the two kingdoms, the myxinoid fishes, and with which oxygenation is very feeble, and almost wanting, the coloured corpuscles are few.* As we pass up the scale the same relation exists. Thus, in the class of birds, which, from the relations they sustain with the external world, have a vitality of the tissues generally not exceeded by that of any other animal in the whole animal kingdom, giving them the greatest amount of strength and agility with the least weight; in these, you find the largest number of coloured corpuscles. In man, we notice the vitality and elasticity of his whole system in proportion to the amount of red or coloured corpuscles in his blood.

We cannot, therefore, resist the conclusion that the coloured corpuscles are necessarily connected with the process of respiration or vitalization by oxygen, of the tissues—in fact, are the *oxygen-carriers*, the *travelling respiratory organs* of this division of the animal kingdom. This conclusion, thus arrived at, is supported by the following facts, of the truth of which any one may easily satisfy himself:—

Dark blood both in and out of the system assumes a lively reddish hue upon being exposed to oxygen, and at the same time carbonic acid is disengaged. The mere serum, which contains no corpuscles, has none of these phenomena.

The consumption of oxygen and the disengagement of carbonic acid is in direct ratio with the amount of corpuscles.†

In the embryo, as we have already seen, these corpuscles are not coloured until they come in contact with the air directly or indirectly; we may therefore regard their red colour as consequent in some way upon their condition as oxygenizers; a fact borne out by all the phenomena of respiration and nutrition with which we are acquainted.‡ The immediate cause of this colour we know very well to be the presence of a chemical constituent called *hæmatin;* but why this should be red instead of blue, no chemistry has yet been able to decide. I

* *Vide* Yarrell, History of British Fishes, London, 1836, vol. ii. p. 468.

† Simon's Chemistry of Man, Sydenh. Soc. ed. vol. i. p. 154.

‡ Simon (Chemistry of Man, Sydenh. Soc. ed. vol. i. p. 120) speaks of blood-corpuscles being found in the thoracic duct of rabbits and horses. Thus apparently showing that these corpuscles become red before they enter the lungs. But I cannot regard such facts as at all influencing the truth of the opinion I have above expressed, for it is quite probable that these corpuscles were here extravasated. Thus, Dr. Davy (quoted in Hewson's Works, Syd. Soc. edition, by Gulliver, p. 277) found a small portion of red crassamentum in the thoracic duct of a man who died suddenly of apoplexy.

think it pretty well decided that this colour exists in the cell liquid, and not in the membrane.

When these corpuscles thus formed are brought in contact with the air, their reddish tint becomes more pronounced, which is probably due to some chemical change in the hæmatin. They then absorb oxygen in virtue of their being cells, and thus laden they carry it into all the vascular tissues, and receive in return carbonic acid, which appears to displace the former. This rotation of function may give rise to that change in their form mentioned by Mulder,* viz. that the corpuscles of arterial blood are *bi-concave*, while those of venous blood are *bi-convex*. These phenomena, however, I have not seen, and it appears to me to be a most difficult subject of microscopical research; for the moment venous blood is exposed to the air, as being put under a microscope, it becomes arterial.

After all, it may be remarked that, in the present state of our knowledge, it is as difficult as it is hazardous to form any conclusions upon the causes of all the differences between arterial and venous blood. This much, however, I consider well established, that the red corpuscles are intimately connected with tissue respiration, and I shall here briefly state their function, as being that of *the vitalization of tissues, which are constantly experiencing the metamorphosis of life. This metamorphosis involves two prominent phenomena, the consumption of oxygen, and the elimination of carbonic acid; all of which is performed by the agency of the coloured corpuscle.*†

* Mulder; Chemistry of Animal and Vegetable Physiology, translated by Johnston, p. 342.

† I scarcely need say, that a variety of opinions have been entertained as to the function of the coloured blood-cell. Wagner,* Henle,† Wharton Jones,‡ and Newport,§ regard them as the elaborators of the fibrin; in fact, give them that function which I have ascribed to the white corpuscles. But this view appears to me quite untenable for several reasons. In the first place, fibrin is undoubtedly formed in the chyle, and in which no coloured cells exist. Then, again, in inflammation, the amount of fibrin is increased, without an increase (on the other hand, there is rather a diminution) of the coloured cells.

The opinion of Mulder‖ is even still different. He regards their function the production of an oxy-protein compound; for, he affirms that the blood-corpuscles are really surrounded with an envelop of oxide of protein, in the lungs. But these views, based upon chemical data almost exclusively, have not been generally received with favour. As for myself, I do not feel competent to discuss them.

* Wagner; Physiology, translated by Willis, p. 448.

† Henle; Traité d'Anat. générale, &c. tom. i. p. 457, *et seq.*

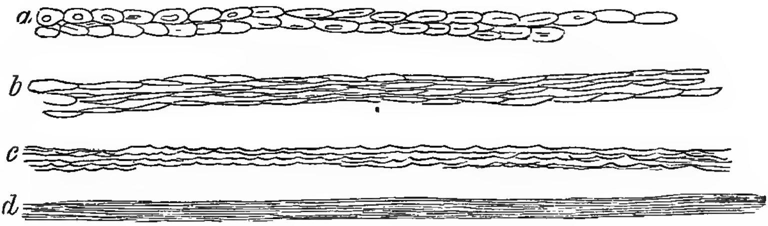
‡ Jones; Brit. and For. Med. Rev. xiv. p. 597.

§ Newport; Proceed. Roy. Soc. Feb. 6, 1845.

‖ Mulder; Chemistry of Animal and Vegetable Physiology, translated by Johnston, p. 332, *et seq.*

We have thus considered the blood-corpuscle as discharging a function as a simple individual cell, but there is one other point of its use which should be briefly referred to in this connection. I refer to the immediate transformation of these corpuscles into compound tissues. That this sometimes did occur, or in fact was the means by which tissues in general were formed, was long ago entertained by Martin Barry.* But the data on which this opinion was founded have not since been verified. Since then, the observations of Dr. Zwicky,† upon the metamorphoses of the thrombus, go to show that blood possesses the power of becoming organized and developing itself into a tissue. I have not studied the subject of the phenomena to which Dr. Zwicky alludes, but some appearances I have observed in the blood of Amphibia show me that it may be transformed into a fibrillated tissue; but whether it is to be regarded a normal or abnormal condition, I cannot say. Thus, the blood of frogs will often show these changes: At first, the corpuscles are floating about, or lying in a loose irregular manner in the field; next, they begin to arrange themselves in a linear series; then, a coalescence of the corpuscles take place, giving the tube a wavy outline on each side; this last finally disappears, and there appears then a more or less smooth fibrilla. The blood thus changed has a fibrillated, tough feel and structure. This, however, may be a function of reserve in nature, to meet the accidents of the life of tissues; but such phenomena are not involved in the ordinary round of nutritive action, as will hereafter be fully shown.

Fig. 37.



Blood of Frog, undergoing a kind of organization. *a.* Blood-globules arranged in a linear series. *b* and *c.* The same, changing into fibrillæ by flattening and lengthening. *d.* Arranged into fibrillæ, forming a fibrillated tissue.

It now remains for us to inquire the function and use of the white or colourless corpuscles found in the blood. We have already seen

* Barry; Philos. Transact. 1840, p. 601.
† Zwicky; Die Metamorphose des Thrombus, Berlin, 1845.

that they are formed in the nutritive liquid which is derived from the food. And as formations of this kind I have been inclined to regard them, in both a physical and teleological point of view. Physically, they are the individual expressions of the formative power of that liquid in which they are found; and as such fibrin necessarily enters a good deal into their constitution, therefore, in this respect, they may be regarded as the organized fibrinous parts of the lymph—constituting the vehicle of fibrin in the blood—the elaborators of fibrin. On the other hand, teleologically, they hold certain future relations with that higher form of blood-cell, the coloured corpuscle. Chemistry has proved that the nucleus of this last is mainly composed of fibrin, and my preceding observations have shown us that it is *around* this white corpuscle, as a whole, or in part, or its analogue, that the coloured cell is formed. Thus, in the *oviparous vertebrates*, we have seen that the whole corpuscle serves as the nucleus of the coloured cell; while in the *mammalian* it is the elaborated product of this corpuscle, or its nuclei, which serves the same purpose.

This opinion of the function of the white corpuscles, which is based entirely upon a genetic and physiological view of the subject, finds its support in pathological phenomena. Thus, in inflammation, as the investigations of Andral and Gavarret* have shown, the quantity of fibrin is increased, and this increase is attended with a corresponding increase in the number of white corpuscles, but without an increase of the coloured cells. This I have repeatedly witnessed myself. But whether this abnormal condition of the blood constituents can be said to be due to an excess of fibrin *per se*, or to a want of its ultimate elaboration through the vitalizing agency of the coloured corpuscle, I do not know. But this much is certain, that when tissues begin to be inflamed their bloodvessels contain an excess of white corpuscles.† And when a large viscus, like a lung, is inflamed, this peculiarity becomes general, nearly all the chyle-corpuscles seeming to remain as such in the blood plasma.

These abnormal blood conditions will be more fully discussed hereafter, in their proper place.

I shall briefly state the function of the lymph-corpuscles to be—*To elaborate fibrin from the chyle plasma, and, at the same time, serving, either in whole or as parts, for the bases of the coloured cells.*

* Andral and Gavarret; Recherches sur les modifications de proportions de quelques principes du sang dans les Maladies, Paris, 1842.

† But this condition of things is due to general relations of the blood instead of local ones, as we shall hereafter perceive.

III. Nerve-Cell Function.

The nervous tissue, taken as a whole, is both the originator and distributer of mental force. In the higher animals, its existence is necessary for what may be called mental manifestations; or, in other words, it is *the* tissue alone in and by which such forces are manifested. But in some of the lowest of the invertebrates this is not true, for in them no nervous system can be detected; yet they have motions as varied and adaptive as others of the same class, which have true nervous tissue.

The distribution of the nervous forces belongs to the tubular portion of this tissue, which has little other than mechanical relations; we shall not need, therefore, to allude to this part of the subject, but our attention will be directed to the cells themselves.

I scarcely need say that the nerve-cells are the producers of nerve-force, and that by them or their analogues, alone, it is eliminated. They are, moreover, to be regarded as the terminal, and not as the transitionary conditions of this tissue as an active one. By their agency, and probably acting as do other cells, viz. by endosmose and exosmose, the nervous power is eliminated; and it is their property to in some way produce this, just as it is the property of another cell to secrete milk. In a mere potential point of view the one instance is just as easily comprehended by the mind as the other. Beyond this I may safely say we know nothing, for here is the end of true physiology. It is certainly the highest philosophy that teaches us that there are boundaries to our knowledge even of the simplest things. We must learn *of* what *cannot* be known. Even were thought, as a cell-product, just as materially expressed as is milk or saliva, should we then know any more about the matter? We see an image on the retina, and the act of seeing is thus visibly demonstrated; but after all, material as it is, does it in the least explain vision? We have already seen that the layer of retina, exposed to the image, is composed of cells; yet as such they receive and conduct an immaterial something to the sensorium. This is a fact which we can appreciate, and which is well settled, but it explains nothing. We can as well conceive that material cells should eliminate immaterial forces as that they should receive immaterial images.

CHAPTER V.

RETROSPECTIVE VIEW OF CELL-LIFE.

THE preceding account may be said to constitute a biographical sketch of cells in all their conditions of life. This has necessarily been a matter of detail, and we have scarcely paused in our course to consider the nature of the processes under description. In bringing the physiological part of our subject to a close, I have thought proper to take a survey of the whole matter, that we might learn the signification of cells in physiological science. This I shall do in as brief manner as possible. The great outstanding fact, which appears before us as the result of these studies, is, that there is a fundamental unity of all organization. This we have seen to consist in elementary particles, which in both animals and plants are formed upon a common plan. It was the opinion of Schwann and Schleiden, who truly originated this view, that this common plan consisted in the pre-existence of a solid, fundamental body (the nucleus), around which is formed a membrane, ultimately expanding and constituting the cell. It has been one of my objects, in the preceding pages, *not* to show that this view was not founded in fact, but to show that there could not be claimed for it an universality of application. This I hoped to accomplish by an attempt to demonstrate another principle of formation; which is, that the fundamental idea of a cell is *a simple vesicle*, and that a nucleated cell is simply one vesicle containing another within its walls. With this view, the nucleus and cell are identical in nature, and this gives to the whole doctrine a unity quite wanting with any other view. For, according to Schwann, the nucleus and cell possess dissimilar natures. With Schwann, the nucleus is *exogenous* and *germinative;* with me, the nucleus is *endogenous* and *reproductive.* The difference between our views cannot perhaps be better and more briefly stated, and by those familiar with this class of phenomena it will be understood. It is not for me here to recapitulate the grounds on which this view is founded, for I could not more briefly state them than I have done.

The mind so naturally seeks a unity in nature, and especially in the formation of an elementary fundamental part, that plurality of methods as to her workings may of itself be almost considered as an argument against the correctness of any of them. I have often been inclined to put that stress upon this point, as almost to lead me to believe that we do not yet understand the *nature* of cell-processes, and that, when we do, there will be perceived an unvarying unity, transcending in beauty and simplicity any and everything we have yet learned in organic life. Still, we must reason from what we *know*, and not from what we would wish; and although this idealism of a unity should always be a conservative element in our studies of natural phenomena, we should bear in mind that it is to be accepted in science only when borne out by unmistakable facts. On this account I still cling to the truth of what has been observed. In both plans of the formation, or rather at the end of it, there is one feature which is alike and common. I refer to the cell being *nucleated.* This is a condition of its most perfect and adult form, which necessarily exists in virtue of its being a complete whole. In fact, I may say that, as a certain form and size is the organic perfection striven for and to be attained by animals, just so is the nucleated condition the organic perfection to be attained by cells. It is true that the view of Schwann involves this idea as well as that of my own, but not, I have often thought, with the same fulness and completeness. For with Schwann, the nucleus being pre-existent and formative, its organic agency is past when the cell has been formed around it, and then the cell is nucleated, in memory (if I may thus express myself) of its past and formative condition. But, according to my own view, the formation of the nucleus is secondary, the sought-for perfection of its adult life, the offspring of it as an organic particle in its fulness of power; and as, in man, his perfection as an individual is expressed in that fulness and potentiality of his qualities constituting true manhood, so, in the cell, it is likewise a secondary production constituting its nucleated character.

But after all these discussions, the cell, as it appears to us—a material elementary particle—has an invariable physiological signification. However produced, it is the culminating point of organization; and if I may, upon so plain a subject, use a metaphor, it is the ground on which life and matter meet, mutually embrace, and then go hand in hand together, but showing in their united countenances that they bear in constant memory the conditions of their first embrace.

But this is only one part of our subject; we have only alluded to it in the light of development; we have simply traced an organic particle to its adult condition. The actions, the "doings" of it, after having reached its prime of life, constitute the remaining portion of its biography. Here, also, the same tendency towards a fundamental unity of action is manifested.

We have seen that, although the statement that all tissues take their origin from cells is not true to the letter, yet it is correct according to the true philosophy of the subject. Thus, we have learned that some tissues are formed from granules or utricles, and not from nucleated cells ; but at the same time it was shown that the signification of these utricles is the same as that of cells. It may be truly said that the fundamental phenomena attending the appearance of organized structures is always the same, that is, *cellular*. The more immediate grounds of this opinion need not here be reviewed. The experience on which it is based is as authentic as any we possess of other phenomena of the external world.

The two conclusions of these studies of cell-life are, then, 1. The existence of an elementary particle, having an invariable unity of expression, *the cell*. 2. The universality of the application of this particle for the formation of organized parts, *tissue*. The philosophic mind cannot conceive of two results connected with the material world, which come home to it with more significant truth and beauty. For experience has here taught and made manifest to us, that which the highest philosophy has always prophesied—simple *unity* amid ever varied *diversity*.

PART II.

PATHOLOGY.

CHAPTER I.

INTRODUCTORY.

The domain which we have just left, Physiology, is by no means so widely separated from the one upon which we now enter, Pathology, as might be supposed from the commonly received doctrines of medical science. I do not make this remark, however, as applicable to anything but their material expressions. For the difference as to essential nature between truly normal and abnormal phenomena, must of course be wide. But in our conversation with the material world, either natural or unnatural, we can learn about it only through the intervention of material forms. It is in this way that we have learned all that we truly know in physiology, and it is in this way that we are now to inquire into pathology. And if from the minute character of our inquiries, the expectation is raised that thereby will be solved the mystery of the intimate nature of disease, that expectation will certainly remain unfulfilled and disappointed. In a general way, it may be said that pathology is but an erring physiology. This expresses a great deal that is true of its nature, and although perhaps not the whole truth, it approximates so closely, that it will serve as the basis of our inquiries.

Such a view is well calculated to remove from the mind many erroneous notions; one of which, for instance, is, that disease is a self-existing entity, which view, if well entertained, cannot but impede our correct interpretation of its phenomena; for we shall be constantly struggling between a fancy and a fact. Then, again, the ideas which we have of health and disease must be relative, since we have no positive data by which the one can be determined in contradistinction to the other. Our conception of normal life must be extremely indefinite, and especially so, since the steps of its transition to that which is abnormal have not been well made out.

Another question which arises at the outset is, does disease always have a material expression, and that too of a corresponding and invariable character?

A negative answer to this would be deemed by many as quite *unphysical*, not to say unscientific; but in the present state of our knowledge I must regard it as by far the most correct; for we are to reason from what we know; and although analogy is of great service in such matters, yet we cannot be too careful of its use. We should very naturally say that, in virtue of the great fact constantly before us, viz. that vitality has its expression only in organization, which is tangible and capable of being analyzed, so should we always have a tangible expression of any perversion of that vitality. This may be seemingly very scientific, but at present it is so only negatively; for there are many transitory morbid changes of the vital phenomena, many morbid conditions known by the name of functional, which leave no traces in the matter or organs in which they occur, at least as far as we can now detect, by the most indefatigable research.

Too high an estimate must not, therefore, be placed upon these intimate microscopical studies of pathological conditions and phenomena. In the first place, with all the material product at our command which we could ask, we must expect to know of disease only conditionally; and in the second place, we must not be surprised to meet with many of the best expressed pathological phenomena holding apparently no corresponding material relations.

If these two points be well borne in mind, much error and confusion will be avoided.

But let us return to our original proposition: Pathology is only an erring physiology.

We can understand from this, why the genesis and general laws of pathological cells should be the same precisely as those of physiology. And I make here this general statement, founded upon a pretty widely extended observation, that, *both as to their genesis and general aspect as cells, those which belong to abnormal cannot be distinguished from those belonging to normal conditions of life.* The genetic and general relations of cells in physiology and pathology are, therefore, the same. But the bifurcating point in the road appears when we begin to inquire about the destiny of each. Physiological cells must always be considered *teleologically*, that is, as having relations with a future and determinate result, in the attaining of which they fulfil their destiny. With pathological cells, however, all these conditions are absent. They exist as cells in virtue of the previous existence of an abnornal formative material, which must have an organic expression of its forces. This expression is necessarily a cell; but there it ends; it sustains no higher and future relation for a definite result.

Abnormal cells, or rather cells produced under abnormal conditions of life, therefore, are not characterized by any type or true individuality. I know very well that some, such as those of pus, tubercle, and cancer, have a uniformity of appearance quite remarkable, but I cannot regard this as having any teleological signification at all, but rather as due to a corresponding uniformity of condition of the abnormal plasma in which they are formed. I do not think that this view at all disparages the scientific accuracy with which they should be described. For experience has shown us that this uniformity of abnormal plasma is so constant that it may always be counted upon in our determination of its products. With these considerations, founded in fact, the distinctions between physiology and pathology as based upon cells, are, therefore, not only broad, but definite, as far as they go. I may say, farther, that they are the only distinctions upon which we can at present insist; remove them, and I can perceive no reason why all our pathology should not at once be resolved into physiology. It may be asked if this resolution of the two thus into one, is not desirable, as simplifying our ideas of organic life both normal and abnormal? I say *no*, even were it possible; for it immediately takes away from the common phenomena of organic life their philosophy as manifested in their teleological bearing.

There is one other result which may well be deduced from the foregoing remarks. It is, that all pathological products are necessarily *infra* formations; they are below the standard of those of health, of which they may or may not take on some of the characteristics.

Pathological formations may be divided into two kinds only, viz: 1. Those which simulate the type of the healthy tissues, called *homœomorphous;* and 2. Those which have characteristics of their own, called *heteromorphous.* All abnormal products are necessarily one or the other of these kinds. It is true that the latter are most important in a practical point of view, on account of their peculiarity of life, widely separated as it is from that of the normal forms. Such, for instance, are cancer, tubercle, and pus. Under the former, on the other hand, are included all those forms of tissue which are abnormal, not so much because they are dissimilar from healthy forms, but because they want very much the definite character of these last as active tissues towards a conservative, economical end in the system. They are, therefore, less severe, and more amenable to treatment. But still, in an histological point of view, they are not the less worthy of our consideration.

It is difficult to grasp this subject unless we take it up in a particular manner. Pathological products as material forms are superventions in and upon the healthy parts. In a causative point of view, they are therefore referable to nutrition and its perversions. And I shall take up the subject in this light, even though there is necessarily a blending of physiological with pathological phenomena.

CHAPTER II.

NUTRITION AND ITS PERVERSIONS THE BASIS OF PHYSIOLOGY AND PATHOLOGY.

Two conditions mark the existence of the healthy living tissue. These are, *decay* and *repair*; the former occurring because the tissue is living—the latter, because in order to live it must preserve its physical identity.

This round of actions, constituting the sum total of those making up the life of the adult tissue, has its foundation in a function which we term *nutrition*—a word to which, in later days, we have been inclined to give a more pregnant signification than in former times; and this, because it thus embraces in function, either directly or indirectly, the whole phenomena of animal life. Many have thought that the relations of reproduction, or the origin and rise of the new being, should be viewed as belonging to another category of actions. But they have allowed themselves to be deceived by the importance of these processes; for we have seen, on a preceding page, that such phenomena are only *cellular*, and are therefore only those of nutrition seeking an individuality of expression.

Nutrition, then, being considered as the basis of physiological science, it will be my object to show that its *perversions* can be viewed as the foundation of our rational pathology.

But that my meaning may be fully comprehended, I shall run over briefly the leading features of this nutrition as a physiological function; a task which I have omitted in the preceding pages, anticipating, as I did, its consideration more properly in this connection. It is not a new, but it is a most important physiological truth, that the blood or its analogue is the source from which all the conditions of nutrition arise. It therefore follows that in it we should clearly recognize the elements of all these different tissues. The bloodvessels form a series of channels permeating the tissues, and terminating, as it were, in a set of vessels functionally different from either arteries or veins, the *capillaries*, which are the dispensers of the nutritive fluid to the tissues. And although these vessels cannot be traced minutely into every tissue or part, yet their function on such tissues is always indirectly perceived. It is asked, How are these capillaries the immediate agents of this nutritive function? It is by transuding through their parietes the hyaline plasma of the blood, into the parts through which they pass, which plasma is immediately appropriated by the contiguous tissue, or transferred by endosmosis through granular or cell-structures, to those more distant. This hyaline blastema is structureless, but it contains within itself the elements of structure. It is entirely amorphous, but it possesses in a latent form all the individualities of the different tissues. After effusion, it may serve its function as a pure plasma, by bathing the tissue, and filling the vacancies made by liquids passed away. But this, I think, is not common, and belongs almost exclusively to the sclerous tissues. It generally gives rise to more solid products. These are utricles and cells, with all their various metamorphoses. The primitive utricle appears as the first material expression, and in tissues of a purely utricular character, such as the muscular, its development does not extend beyond this point, but as such it is appropriated. But in tissues having a *persistent* cell-structure, these utricles pass on to cells, replacing those passing away. I believe that this hyaline plasma, immediately upon its effusion, and before the primitive utricles have appeared in it, is, whatever be its locality, identical in character. The reason why it has afterwards so many ultimate expressions of development, appears to me to be due to another cause. It is because, directly upon its transudation, the plasma receives, in coming in contact with a tissue, the impress or type of that tissue; so that, whatever the tissue may be, the plasma, in serving any purpose, follows directly in train of the *idea* on which the tissue is expressed.

Let me illustrate this doctrine by referring, for example, to the epithelial tissue. This, as we have already seen, is composed of a layer of cells, situated upon, and attached to, a basement-membrane. The cells thus attached, whatever be their function, are constantly passing away, and must be renewed. To effect this last, a plasma is effused by the contiguous vessels. This, as soon as it comes in contact with this tissue, takes on its *epithelial* type, and the primitive utricles developed in it immediately pass on to the ulterior condition of epithelial cells. Other examples might be cited to show, in the same way, this beautiful *type-power* of tissues, without which the continuity of structure could not be maintained. The full appreciation of this idea cannot be too strongly insisted upon; and I will again express it in a laconic way: A liquid containing the elements of structure, upon being brought in contact with a living solid, is immediately impressed with the type-character of the latter, and, therefore, must subserve its repair. It is not properly *a selective* power of the tissue, but a *living act*, occurring because the tissue has an individuality of its own, which it can impart. I regret that I cannot illustrate this by any reference to common examples of animal life; but as it is one of those immaterial acts in physiology, we can appreciate it only by the recognition of the fact. I might perhaps liken it well to the act of fecundation, in which a spermatic particle, by simple contact with the ovum, impresses upon it the full type of the male parent; and, to carry the comparison still farther, if the completeness of an individual can, in this way, be stamped upon an ovum, so, in the same way, in the act of nutrition, may the singleness of a tissue be stamped upon a hyaline plasma.

I regard the recognition and application of this *type-power of tissues* as one of the happiest results of modern physiology, not only as illustrative of the higher tone of our present studies in this direction, but also as enabling us to grasp many of the hitherto hidden forms of function in this science. On account of its value as the hidden spring of the various nutritions, let me still farther notice its character.

If it is asked, what is this *type-power*, I should say that its nature can be best expressed by an imperfect metaphor. It is the *memory* of the immaterial idea on which a tissue is developed, still persistent during its material life. And to carry the metaphor still farther, this memory may be bright and active, or may be fast fading away, according to the age of the tissue.

The younger the tissue, the more full and complete is its individuality and type-power; and if it suffers a lesion in its very early life, this breach of continuity is thereby so thoroughly repaired that its physical identity is preserved. This is the reason why in wounds with very young children, the healing of them leaves no cicatrix. The type-power extends fully and completely into the plasma effused for repair, and this repair therefore has all the character of true interstitial nutrition. As the individual advances in life, and passes into or beyond its adult period, this type-power appears to die out, or at least to lose some of its strength. This is the reason why at that time lesions are not perfectly repaired; the material not taking on the character of the contiguous tissue, and an adventitious product occurring in the place of the lost part. The same reason may be assigned for the fact that tissues having suffered no lesion sometimes atrophy—the plasma effused not being appropriated, but, taking on a new character, may give rise to new and morbid forms.

I might enter more fully into the consideration of this most interesting of subjects, and it would afford me much pleasure to take up its illustration in some of the most delicate tissues both in man and the lower animals. But in this place I have thought proper to sketch only its general character, which is to serve as the foundation of considerations of another character, soon to follow.

It is quite necessary that we should be very familiar with this great *high-road* of physiology, in order that we may well know where the *by-road* of pathology divides from it.

Thus far, we have seen that two conditions are necessary for correct and healthy nutrition; these are, 1st. That the plasma effused shall be healthy, and such as may be fit for appropriation; and 2d. That the type-power of the tissue to be nourished shall be sufficient to make the appropriation. Such being the requisites of the healthy nutritive process, the perversion or suspension of one or both of these conditions gives rise to what has been justly termed *abnormal* nutrition—a state of the animal tissues which we have good reason to believe is at the very foundation of pathology. To illustrate clearly this point, we will take up separately each division.

I. Perversions of the Character of the Plasma.

There appears to be a law of affinity or congruity of action in tissues, which must be regarded as a very powerful conservative of their integrity. By this, I mean that when the circulatory vessels do not contain the proper elements for the tissues, the latter do not call for the effusion of their plasmatic liquids; or, to impersonate the matter, as would perhaps John Hunter, I should say that the tissues, perceiving the incongruity of the nutritive fluids, refuse to have them effused. But still, it often happens that this inappropriate plasma is effused, and then, not being at all reconcilable to the type of the tissue, yet possessing a certain vitality of its own, which perhaps is still farther urged on by the very fact of its being in contact with a *living* tissue, the course which it pursues is wayward, and thus we have heteromorphous pathological products.

This perverted or abnormal plasma varies much as to its capacity, but is always *below* that of health. Its capacity is expressed in the character of its products. When quite low, granular and low cell compounds are the result. Such in fact are *pus* and *tubercle*, which appear to constitute the lowest expressions of a plasmatic formative power. When of a higher character, it gives rise to the highest pathological products, which often seem to have a kind of individuality of form and function. A good example of this is seen in *cancer*.

The heteromorphous products, then, of pus, tubercle, and cancer, I have considered as due to perversions of plasma. But perhaps the subject will come home more clearly to the mind if I say that *inflammatory products* may be regarded as due to this same perversion of plasma. I do not mean to say that such products can always be traced as the results of inflammation, but as far as yet studied there appears to be here a connection, at least, showing us that when we shall know more of the matter, our most comprehensive idea of inflammation will include all the conditions under which the heteromorphous products occur.

In here touching, then, upon the subject of inflammation, I shall not be considered as diverging from the main point of our discourse.

I do not pretend to define inflammation, because I think we have not yet sufficient data to convey to the mind a clear idea of its character. Still, if I say that there appears to be coexistent with it, a want of healthy relation between the bloodvessels and the blood, I think I have stated pretty clearly all that is really known about the matter; and even here it is very far from being certain if this *absence of relation* is not the first known *effect*, instead of the *cause* of the inflammatory process.

It is, therefore, a waste of time to dwell upon that, of the nature of which we have as yet so feeble an appreciation. We must take the results as we find them, waiting for the ultimate cause until we have more data.

I have said that the first visible sign of inflammation is an absence of the healthy relation between the bloodvessels and their contents. This leads to a partial suspension of the function of both, and also causes that function which does occur to be of an abnormal character.

Where the processes otherwise nutritive are very active, scarce any appropriation of this plasma takes place, and the products arising in it, viz. granules and corpuscles, seem so alien to healthy tissues, that they are expelled as foreign substances. Such is *pus* in all its forms, and such, I think there is reason to believe, is *tubercle* also. When, however, the process is less active in its character, the plasma is appropriated to a certain extent; but the tissues thus badly nourished, sink below their normal type, and when these conditions are kept up for any length of time, they seem to take on a character of their own. This is often seen as one of the consequences of a previously acute inflammation of an organ, but especially is it observed in indolent ulcers. It is thus we see that the long-continued use of a perverted plasma, by a tissue, serves to modify the *type-power* of the latter. And in speaking of the minute pathology of some organs on a future page, I shall have occasion to enter more fully into the peculiarities of these changes and their consequences.

It may be asked, how the etiology of cancer can be considered as belonging to the perversions of the plasma? The reply to this is that, although eminently a morbid product, it differs widely from those of which we have just spoken, in possessing in a high degree a life of its own. The plasma in which it takes its origin, has a capability not much below that of health, but still has a character as different as the results produced. Although passing on to the higher cell-structure, its inferior character is betrayed by the objectless nature of its termination; for these cells appear to be the *ultimate* result of a morbid action, rather than the material agents through which a higher function is to be performed.

The cancerous structure, however high it may appear in an anatomical point of view, is aimless and without function.

In speaking thus of pus, tubercle, and cancer, as the results of a perverted plasma—heteromorphous products—I think I can best express their mutual relations and dissimilarities if I use a figure, and say that, normal nutrition being considered the great high-road, those forms of abnormal nutrition producing pus and tubercle, would be considered as small roads diverging from it, at nearly right angles. Whereas, that form producing cancer would be considered as a much larger road, diverging at a smaller angle; in fact, often afterwards running parallel with the former. But all these diverging roads never get back upon the main one, and therefore have a termination unlike anything of true function.

In concluding this section, the relations which this perverted plasma holds to inflammation, may be thus briefly stated. We cannot conceive of this want of harmony between the action of the capillaries and the constituency of the blood, unless we suppose at the same time an exciting cause. Now, as it is true that the more we investigate minutely their conditions, the more do we find the inflammatory process coexistent, and as, in the instances of pus and granular forms, the relations of cause and effect can be directly traced, we have a right to infer that the same is true, even when these relations cannot be fully made out.

And so, on the whole, we seem to be justified in regarding inflammation, whatever is its nature, as a condition always preceding, and in all probability causing, this perversion of the effused plasma, and therefore the immediate cause of pathological heteromorphous products.

II. Perversions of the Type-power.

We now enter upon the consideration of quite another class of phenomena. As the vessels are healthy and the blood normal, these phenomena cannot be viewed as being in the same category as those of inflammation we have just considered. I have therefore thought proper to designate the results of such conditions as the *homœomorphous non-inflammatory* products; in fact, they are forms which, while they are really morbid, partake nevertheless of the type of the healthy tissue, as far as that condition will allow. We have just seen how, when the type-power was good and the plasma bad, the dissimilar results were referable almost entirely to the plasma. We shall now see that, where the inverse is true, the results produced have more an affinity with the tissue than with the plasma. In one sense it can scarcely be called a perversion of type-power, but rather a decrease or increase; but, on the other hand, these words do not express the whole, for, beside these variations of amount, there is involved a pathological principle not easily or readily expressed. Suppose that, from some unknown cause, the type-power of an epithelial tissue in the body had become changed. The normal plasma is thrown out as usual, but it is not delicately and nicely appropriated as in health, and although the tissue has given it its impress, yet this is all, for the relations of size and shape appear to be absent. There therefore appears a new form which, while it bears the outward aspect of the healthy tissue, is an abnormal product; it is, as it were, the representation of disease under the garb of health. This product is *epitheloid* but not *epithelial.* Such, for instance, is the so-called cancer of the lip, the cancer of the antrum, &c. &c. In the same category may be considered many, if not all, the hypertrophies of tissues, where a product appears with the general character of health, but with the profligacy of disease.

It is not difficult for us to understand how, in some of these immense growths, a sufficient amount of plasma is supplied, for it appears to be a law in the nutrition of tissues that the greater the demand the greater the supply; and so, when the demand even by a morbid product has once been made, it is furnished, and the whole may go on increasing, the vessels conforming to these changes exactly as though a healthy tissue was experiencing a rapid normal growth. But if we thus have products from what, in one sense, may be called an *increase* of type-power, constituting *hypertrophy*, there is quite a different class arising from a *decrease*, in fact a suspension of it, and which ought in this connection to be noticed. I cannot say that it is primitively of tissue origin, but, at any rate, it seems to be a dying out of the type-power; in fact, so thoroughly is this the case that the individuality is gone, and nothing is appropriated; and, in accordance with the law just mentioned, there seems ultimately to be scarce any supply, and then the tissue loses its physical identity, and gradually recedes to its primitive utricular condition. This condition of things has generally been considered in the light of an *atrophy*, but I have thought that it merited a distinction, and propose for it the name of *Retrograde Metamorphosis.* Its leading characteristics may be thus briefly stated: Bichat's definition of life, as applied to an individual, was: "The sum total of the functions by which death is resisted." Now, what applies to an individual, may be taken as, at least in part, applicable to a tissue. Its life consists in the conservation of those two conditions by which its integrity is maintained. These I have already regarded as involved in what is called nutrition—the balancing of decay and repair. Now, whatever function a part may discharge, it is necessary that it (the function) should be kept up in order that the nutrition should continue normal; but when from either an unknown cause, or from a suspension of function, the part ceases to have that ever-changing vitality, then it seems to lose its type-power, and the small quantity of effused plasma is feebly appropriated, the vital cohesion of the tissue in a measure disappears; so that although there is strictly no decomposition, yet the individuality of the part is gone, and, with it, all those forces that elevated and sustained its character above that of the primitive elements. We have, then, in place of the normal tissue, what is called a granular mass, and, as such, it cannot be regarded as a special product, for it is the same, whether it occurs in a muscular or in a glandular organ. It consists simply of oil and albumen, uniting in their usual way. But this subject has been treated more fully in another place.* I have said that this condition differs from *true atrophy;* for this last can scarcely be regarded as an abnormal state, it being only a decrease of tissue-function, which we see daily exemplied in the muscular tissue. The same is true in an inverse sense of *true hypertrophy*, which is *not*, as I have before said, the cause of *homœomorphous products;*—they are, both, rather variations of nutrition as to *quantity* than as to *perversion.*

Such is a rapid survey of some of the perversions of nutrition which lie at the foundation of many of our best views of pathological changes. It may be asked if, under this head, may be included the causes of all pathological phenomena having their expression in a material product? With our present knowledge, I do not think that the question can be positively answered. Nevertheless, I think I am safe in saying that the *tendency* of the present inquiry is to show that in abnormal nutrition is to be found the cause of all *organic* pathological changes.

These considerations may, perhaps, serve as the groundwork of our subject; it being now our task to look into the specific character of its details. I shall, therefore, take up first the subject of heteromorphous products—each in its proper character—commencing with the lowest. But before this, the phenomena of inflammation, as elucidated by the microscope, should be considered, and this will serve as a fitting prologue for the ensuing pages.

* For its farther discussion, see an article of mine on "Tissue and its Retrograde Metamorphosis," in the Amer. Journ. of Med. Sc. n. s. vol. xxii. p. 22.

CHAPTER III.

THE MICROSCOPICAL PHENOMENA OF INFLAMMATION.

I THINK that it is as true in physiology as in psychology, that the nearer we get, or, rather, think we get, upon the very elements of any phenomena in question, the more inclined are we at the same time to believe that, after all, we are mere outside spectators of the scene going on before us. This remark has an apt illustration in phenomena we are now about to describe.

It is evident that if we are to see the real phenomena attendant upon the inflammatory process, we must study the affected tissue *in situ*.

The tissues of nearly all the higher and warm-blooded animals do not admit of any direct experimental inquiry of this kind; so that most of that which relates to investigations in this direction, physiologists have been obliged to make out from experiments made upon such of the lower animals as have tissues of sufficient transparency.

Some of the Batrachian reptiles have been selected for this purpose, and although they are objectionable on the ground that we must, even there, suppose that the same phenomena occur, under similar circumstances, in the higher animals, yet they have formed the only basis we possess of all direct inquiries of this kind.* Kaltenbrunner,† in 1826, was the first to demonstrate these phenomena, and most of the physiological microscopists since have verified and carried out these inquiries. I scarcely need here describe the details of these experiments, for most have read of them, and not a few seen them. However, those made within a few years have led to some change of opinion as to the real character of the phenomena witnessed.

I will describe the processes as they are usually seen to occur in the web of a frog's foot. The interdigital membrane being spread out upon a plate of glass and viewed with a power of 300 diameters,

* Bat's wings can be well used, and, within a few years, the phenomena in question have been quite satisfactorily studied in this way, with the advantage that the bat is a warm-blooded, mammalian animal.

† Kaltenbrunner; Experimenta circa statum Sanguinis et vasorum in Inflammatione, Monachii, 1826. (A rare work, which I have not seen in this country.)

you perceive the capillary bloodvessels existing as channels in the tissue, and the blood and lymph corpuscles rushing along in them with a kind of pulsatory movement. This is the healthy circulation. But, if the membrane be irritated, the appearances quickly change. In the first place, the vessels become narrower, and there is a consequent increase in the blood's current. This destroys the natural pulsatory movement, and gives a crowded aspect to the corpuscles in the vessels. But this condition of contraction is soon followed by the opposite (constituting the second stage), relaxation, in which the vessels appear larger; in fact, their diameter is increased one-third to one-fourth. Accompanying this relaxation, is a stagnation of the blood's current, and the dilated vessels, crammed with red corpuscles, present a very red aspect. With this cessation of the current, cease the primary phenomena. This constitutes congestion, and after congestion comes effusion. But we are led to inquire the cause of this stagnation. We have first, contraction, and then, relaxation of the vessels, and this stoppage occurs with the latter condition. Now, in my opinion, it is due mainly to a change in the natural rhythm of the vessels or their walls, and when this has once occurred, the relation of the blood to the vessels is quite altered—at least, mechanically—and a "clogging up" ensues. This alteration extending over a small surface, a much larger area is included from the direct connection, so that the clogging up of a single vessel may lead to the same with four or five others connected with it.

It will be seen that in this account I place no stress upon the behaviour of the white corpuscles. Mr. Addison* and Dr. Williams,† both of whom, and especially the former, appear to have studied the subject considerably, take a different view of the subject. According to them, the first changes consist in the sudden appearance of a great number of the white corpuscles, which begin to adhere to the sides of the vessel; and, by a repetition of these adhesions all along, the caliber of the vessel is obstructed, and hence stagnation. If we may credit their statements, there seems to be an actual increase in the generation of these white corpuscles—which appear to have the power of rapidly multiplying themselves—for they suddenly appear, and that, too, from an unknown quarter. So that the primary phenomenon of the inflammatory process is, according to these observers, a change of relation between the bloodvessels and the white corpuscles.

* Addison; Lond. Med. Gaz. Dec. 1840, and Jan. and March, 1841.

† Williams; Lond. Med. Gaz. July, 1841.

But this view appears to me incorrect, and its fallacy will be apparent to any one who carefully considers its phenomena. In the first place, from what we have already learned of the nature and relations of the white corpuseles, it is, to say the least, quite *unphysiological* to suppose that there is a sudden increase in their generation at the time of the irritation. I have always regarded this as the outstanding objection to this view. Moreover, the experiments were made upon frogs, and the irritation produced by acetic acid; and I cannot but believe that they were deceived by the nuclei of young blood-corpuscles, which were caused to escape, and rendered distinct by the acid, and which quite closely resemble, as we well know, the colourless or white corpuscles. In fact, the action of the acid is so sudden that, in many cases, the replacement of red by apparent white corpuscles, is, as it were, by magic. Then again, there is another source of error. It is well known to all who have experimented much with frogs under the microscope, that the relative quantity of the constituents of their blood varies very much. Thus, in young and full-fed frogs the number of white corpuscles is very great, even exceeding that of the red; whereas, in old and more famished ones, the opposite is true. Now, it is apparent, that if the experiment is made with the first, one would be quite likely to be deceived by their unusually great number. While, if frogs be taken in the spring of the year, when half famished, a quite different aspect is presented.

Mr. Addison thinks he finds support for his opinion in the fact that blood drawn from a pimple, the base of a boil, the skin of scarlatina, &c., in the human subject, presents an unusual number of the white corpuscles. This fact may have been correctly observed, but all my inquiries in this direction lead me to believe, that if, in such cases, there is a disproportion between the white and red corpuscles as to numbers, it is not local, but exists throughout the entire quantity of blood. In proof of this I may mention the fact, which I have often verified, that if the finger of almost any cachectic, tuberculous patient be pricked, the blood therefrom will be found to abound in white corpuscles. Moreover, such persons are those most generally affected with cutaneous eruptions, boils, &c.

In making these remarks, I wish to be understood as not urging any objection against the view that, in inflammation, there is an increase generally of the number of white corpuscles, or rather that there is a deficiency of the formation of the red ones out of them.

For this view I believe correct in the main, but it appears to be rather referable to general and constitutional conditions.

I am therefore inclined to the opinion of Dr. Bennett and Wharton Jones, that an especial abundance of the white corpuscles in an inflamed part is neither a frequent nor constant occurrence. And this, I may add, is borne out by the experiments of Remak, who observed that those portions of blood *first drawn* in inflammation contained but few white corpuscles.

What, then, are the primary phenomena of inflammation as observable by the microscope? As we have just seen, they may be regarded as the following: 1. Contraction of the vessels, with a consequent acceleration of the current. 2. Dilatation of the vessels. 3. Stasis, or cessation of the current. This constitutes that condition of congestion which necessarily precedes the inflammatory process. But the confusion must be avoided of supposing that these phenomena *constitute* inflammation; for this is not really so; and we may have them all, as in cases of ordinary congestion, without any inflammation following. But at the same time, I should remark that we ought to be careful about the use of this word *congestion;* for it would appear that the congestion preceding inflammation is quite different from that which is a simple hyperæmia of the vessels. They may pass into each other, but the general experience is that there is no necessary connection. But in the initiatory phenomena of inflammation, what is the cause of the stasis? I have already expressed the view that it might be due to a disturbance of the natural rhythm of their action. But this, I am free to confess, does not explain the whole matter. We know very well that the outstanding causes of inflammation are of two kinds—traumatic or external, and idiopathic or internal; and, as far as we understand the subject, the sequent phenomena are the same in each and both. If we say that inflammation is due to an altered relation between the bloodvessels and their contents, I do not think we are any nearer the ultimate cause; for this is only expressive of a condition.

In inflammations of a traumatic origin, we regard the disturbance as commencing with the parietes of the vessels; but in those which are idiopathic or constitutional, we trace the first trouble to the blood, which subsequently acts upon the vessels. But there is, I believe, a third form, not originally due to either, and which has been well described by Mr. Paget.* It may, in one sense, be called of nervous

* Paget; Lectures on Inflammation, &c. in Ranking's Abstract, No. 12; or Lectures on Surgical Pathology, &c. 2 vols. London, 1853. Vol. i. p. 292, *et seq.*

origin, and I can see no reason why the nerves should not so control the vitality of the vessels as to lead to these phenomena. Notable instances of this form are, inflammation of the conjunctiva after severe use of the retina; the excited state of the optic nerve being communicated to the filaments of the nerves of the conjunctiva, causing them to alter the nutrition of the part; also, inflammation of the testicle from severe irritation of the urethra, which is only to be explained by a disturbance of the nervous power controlling the nutrition of the testicle.

There are also many phenomena connected with neuralgia coming properly in the same category.

As yet, therefore, we cannot look for the cause of inflammation in any one circumstance alone; or, in other words, we as yet know nothing of the intimate nature of those causes producing the initiatory phenomena observable by the microscope. Undoubtedly the chief of these last is the stagnation, and, in my opinion, when we have discovered its relations, we shall then have learned quite definitely about the whole matter.

In conclusion, I will say that, if we are to remain on the ground of demonstrable facts, it cannot be said that the microscope has been of eminent service in elucidating the real nature of the inflammatory process. In fact, that it should be, would be expecting too much. With means of physical examination, we can only seize hold of physical appearances. The forces of matter and that which concerns vitality, cannot, at present, be studied by these aids. And it is my opinion that the intimate conditions of this pathological state are not only beyond microscopy, but beyond chemistry. The microscope, in revealing to us the outstanding phenomena, such as I have described, has accomplished all that could be justly expected; and although this is not much, it has served at least as a *physical* basis of our knowledge of the subject.

The secondary phenomena of inflammation commence immediately succeeding upon the stasis. They consist of two kinds; first, the effusion of the plasma as a hyaline liquid into the contiguous parts; and second, the formation of utricular and cell products in this plasma.

These formations constitute the heteromorphous products, which, in pursuance of the remarks I have hitherto made, I have seen fit to consider as at least the sequelæ of the inflammatory process. In the following chapter, I shall take them up in special detail.

CHAPTER IV.

HETEROMORPHOUS PATHOLOGICAL PRODUCTS.

I. Utricles or Granules.

We have already traced, in a rapid way, the features of abnormal nutrition, as a condition; we will now take up the description of its material expressions.

Thefluid hyaline blastema, we have already seen, is the first product of the diseased action. Rarely, however, does it remain as such, but advances toward a higher state.

The chemical nature of this fluid, as proved by analysis, is *oleo-albuminous*—that is, composed of oil and albumen. But these elements as they first exist are transparent, and some combination is necessary that they may be made visible, appearing as separate particles.

This is accomplished in physiology exactly as in pathology, and I have already given its description. The utricles thus formed are also in appearance exactly like those of health, and like them, too, they are the basis of all the higher cell-forms. These forms may be the *pus*, the *tubercle*, or the *cancer* cell.

The most simple heteromorphous product is of course a plasma or blastema perfectly hyaline, which has not sufficient vital capacity to give rise to even these low products. Such are, for instance, many serous effusions, transudations of the larger bloodvessels, which analysis shows to be mostly composed of water. Notable instances of this are the effusion of a *healthy* blister, and the effusion into the plasma consequent upon the more *laudable* forms of inflammation of that organ. As such, they may be entirely absorbed exactly as they were thrown out.

A little more active form of this liquid is that in which these utricles appear, but which have not capacity enough to pass on to cells. Such are many effusions, from the so-called scrofulous inflammations, or those occurring in individuals whose natural standard of vitality is below that of true health. These are never wholly absorbed, but leave their marks behind them in the form of adhesions, lymph, &c. &c. These last have the lowest form of organization—the fibrillated structure, and the details of which are exactly like those of health already described.

Utricles and utricular formations constitute a large portion of pathological products, and a large portion of these products are directly referable to the inflammatory process. I have already said that a utricle is only a cell in embryo; but in these low plasmas the ulterior state of a cell is the fate of *comparatively* few utricles. In fact, this remark is true, literally, of all pathological plasmas; and this is the reason of the wide disproportion in numbers and amount of utricles and cells in pathological formations. *Most* utricles do not get beyond their primitive condition. This is a state of things quite fortunate; for, were every pathological utricle capable of cell-development, there is reason to believe that our physiological condition would oftentimes be obliged to recede before engrossing pathological growths.

In utricles, then, we have the commencement of solid abnormal products, and these utricles have always the same aspect; they may have a lower or a higher destiny, according to the vital impulse of their blastema. We will now look to their ulterior forms.

II. Cells and Cell-forms.

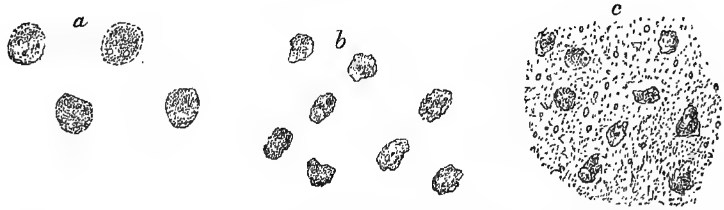
The cell is the highest condition attainable by a heteromorphous pathological product; it is the ultimate and not the transitional form. These cells, born with no preconceived end or aim, and arising in a blastema which has no physiological impress, have lives ending in abortion. They fulfil no function, except in some instances that of a feeble and imperfect reproduction; they soon pass away, or if they grow (not developed), they show their want of any affiliation with the economy, by their being recognized by it only as foreign bodies.

Most of them are as imperfect in their physical conformation as they are in their physiological destiny. On this account, they can, with the exception of some of the higher forms of cancer, be recognized as morbid by their anatomy alone. In our physiological studies, we have been led to place a high importance upon the *nucleus* of the cell, for it not only indicated the adult and potent condition of the cell, but had otherwise a vital value. In pathological cells, it may and often does exist; but if so, it is generally imperfect, and is so little expressive of the power of the cell, that it does not rise to the dignity of unity, but exists in several parts, constituting the so-called *nuclei* of cells.

1. *The Tubercle-Cell.*—This I regard as a very good example of the lowest cells or cell-forms existing in either healthy or diseased tissues. It constitutes the expression of a very slight elevation of an organ-

ized form above that of a utricle. The plasma in which it is formed is filled with utricles, which have no distinguishing features from those of others. Some (but few) of these utricles increase and form a cell-membrane, containing a clear liquid, in which last, granules appear, but which remain as such, never forming a true nucleus. Thus it is, that, as far as my observation goes, tubercle-cells never have a nucleus proper, but only several granules. These cells thus formed vary in size from $\frac{1}{2500}$ to $\frac{1}{3500}$ of an inch in diameter. As I have sometimes observed them, quite soon after formation in the tissue of the lung, they were even larger than this—about $\frac{1}{2000}$ of an inch in diameter. But they are rarely seen under these circumstances; and as usually observed, taken from tuberculous matter of various organs, they are much smaller, and present a much less regular contour and appearance. They then have a somewhat shrivelled aspect, are rather angular, and the granules are more distinct. This is due, I think, to a partial exosmosis of their contained liquid. Upon being treated with acetic acid, the membrane becomes quite transparent, thus rendering the contents more distinct. The distinguishing characteristics of the tubercle-cell belong, therefore, to its low condition. Moreover, it is an adventitious product, and always exists as non-nucleated, having a finely granular aspect. These peculiarities are sufficiently well marked to make it always distinguishable by the microscope, and especially so, if combined with that *first-sight knowledge* which long-continued observation always gives. So that one may be able, the moment he sees the cell, to pronounce whether or not it is tubercular. Then, again, these cells have a uniformity of appearance quite remarkable, so that, once fully recognized, one would know them pretty certainly afterwards, wherever formed.

Fig. 37.



Tubercle-cells in various stages, from lung of patient dying of acute phthisis. *a*. Freshly deposited cells lying in the structure of the lung. *b*. Old tubercle-cells from tuberculous masses, the usual appearance of the cells as found in the yellow masses. *c*. Usual appearance of tuberculous matter under the microscope—cells in the midst of granules. (Magnified 350 diameters.)

I do not, of course, regard this uniformity of appearance as any indication of their possessing a distinct type; because, as I have before remarked, morphological types do not belong to pathological structures. Moreover, the tubercle-cell, wherever formed, or in whatever organ, or in whatever animal—horse, dog, &c., is the same. This uniformity, therefore, must be considered as quite referable to the plasma in which they are formed.

But this tubercle-cell, although always present in tuberculous products, constitutes only a small part of their substance; for, from the very fact that it is one of the lowest products, utricles compose by far the greater part of tubercle as a product. And fortunate it is that these granules are nearly all of them thus feebly endowed; for, were they all capable of cell attainment, man would stand no chance against the ravages of a disease which even now, as it is, decimates his numbers.

These utricles are, of course, composed of oil and albumen, but in many instances, and especially when the blood is rich in oily matters, the oil predominates over the albumen, and you then have oily particles scattered through the mass. It is not uncommon, therefore, to see in the field of the microscope great numbers of oil-globules in a free state, beside the utricles and cells. Fat-globules, utricles, and cells are, then, the visible microscopic parts of the tuberculous product; and, from a quite extended series of examinations I have made upon tubercle, not only as occurring in nearly every organ of the human body, but also as appearing in some of the lower animals, I am satisfied that these are the only parts properly belonging to it. It never attains a farther organization; on the other hand, fortunately, all its tendencies are of a retrograding character. Pathologists, guided by external appearances, have divided tuberculous products into two kinds, the *gray* and the *yellow* forms; the former occurring in a disseminated manner, the latter in large masses. But this distinction has no foundation in a difference of the cell, but is due to there being a greater amount of fatty and utricular matter in the yellow than in the gray variety; which last is composed mostly of cells.

In thus giving the tubercle-cell and its accompanying plasmatic forms a distinctness of existence—a kind of individuality, it may be asked if I regard it as a true inflammatory product. To this I reply in the affirmative, meaning thereby that the effusion of the tuberculous plasma is the result of an abnormal condition of the blood generally, which condition, in the present state of science, must be considered as belonging to inflammation, as we have been led to understand the meaning and intimate signification of this last.

This abnormal condition of the blood has been called the *tuberculous dyscrasia;* which, judging from its products, has an undeviating uniformity. This leads me to express the opinion which I have long entertained, that the appearance in the system of the tubercle-cell is *always* due to a general rather than to a local pathological condition. I know it is true that its expressions are sometimes simply local, but this is rare, to the misfortune of our race.

The peculiarities of its deposition in the different organs and tissues of the body do not properly belong to this work, and however important and worthy of consideration in a practical and therapeutical point of view, they do not relate to their histology, for this last is always the same.

When the tuberculous product has reached the cell-form, it has attained its highest point of organization. Then, like all morbid products which are produced under physical and not with teleological relations, it dies, and undergoes all those peculiar changes of which an organized product is capable while existing in a living tissue. It retrogrades to its primitive elementary condition, then a chemical dissolution takes place, the aqueous portions are absorbed, a thick wall of cicatrical tissue is formed around the residue, and thus the radical cure of tubercle is sometimes effected. Such, my own experience teaches me, not unfrequently occurs in the pulmonary tissue, and in the lymphatic glands. The course of tubercle, then, is such as we should well expect it would pursue.

But in thus regarding the tubercle-cell as strictly an inflammatory product, it is asked, how are to be reconciled many of those phenomena of its appearance in organs, and especially in the pulmonary tissue, indicating at first a dissimilarity to the usual inflammatory conditions?

I can well appreciate the force of this objection, for I once regarded it as sufficiently valid to make the distinction broad between these two processes.

Louis and other pathologists have shown that it is true that incipient pulmonary tuberculosis generally commences at the apices of the lungs, while pneumonia or ordinary inflammation nearly always commences at the base.

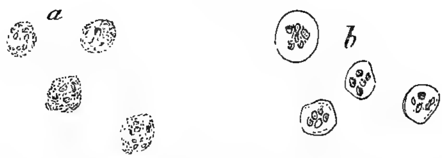
Then, again, both the clinical course and the gross pathological anatomy of these two affections are quite distinct, and seem never to completely fuse into each other. But these distinctions are not more wide than the intimate causes of these two affections, as far as we understand them. As I have before remarked, the causes of tubercle are as deep and general as the very constitution of the individual; they are constitutional, and this is the reason why they may be hereditary. On the other hand, the causes of simple inflammation of the lungs are of a more local and particular nature, and due, generally, to direct influences of external agents. These should be sufficient reasons, I think, why their outward manifestations are somewhat dissimilar.

From the very fact that the inflammation of tuberculosis is constitutional in its origin, we might well infer its very low character. This, the soundest practical experience has shown to be true; for although even its very earliest expressions are attended with a general febrile condition, yet this last is always most successfully managed by a *sthenic* rather than an *asthenic* mode of treatment; and this, I may add, is one of the happiest and most important results of modern pathology; and although its first recognition was due to clinical studies, yet we have learned fully to appreciate it from the results of microscopical analysis.

2. *The Pus-Cell and Pyoid Forms.*—We now come upon those products which are, without dispute, directly referable to the inflammatory process. As far as the most careful experience has yet shown, they are never found without there has been a pre-existing inflammation; neither does there ever occur inflammation without their formation to a greater or less extent. Under the above head, I include not only the true pus-corpuscle, but all those cell-like, granular forms with which it is usually associated to a greater or less extent. An inflammation exists, an abnormal plasma of the blood is thrown out, and in this, the first traces of an organizing force are manifested, as usual, by the appearance of utricles. But these have no distinguishing characteristics, looking exactly like those of tubercle or cancer. Soon, however, cells are formed from them in the usual way, and, in what is called "laudable" or creamy pus, they compose the greater part of the solid portion of the pus-liquid. Comparatively few granules or utricles remain as such, and thus is manifested its formative power over and above that of tubercle just described. The cells thus formed consist of a membrane filled with liquid, in which are three or four granular bodies, which, aggregated together, constitute its nucleus. It is, therefore, a granular nucleated cell, having a diameter varying between $\frac{1}{1500}$ to $\frac{1}{2000}$ of an inch, but the average of which, according to my own experience, is about $\frac{1}{1800}$ of

an inch. I am not aware that its size or aspect varies to any extent, from whatever part it may be taken. The same is true with reference to the other mammalia I have examined—such are, notably, the horse and dog. The true pus-cell, therefore, presents a very great uniformity of appearance, perhaps full as much as that of tubercle. When treated by acetic acid, the cell-wall becomes quite transparent, the nuclei are most distinctly seen, and, if the action of the acid be sufficiently direct, the wall is quite dissolved, and their escape takes place; pure water enlarges them by endosmosis, so that they appear full and plump. There can be no doubt, therefore, as to the existence of the cell-membrane.

Fig. 38.



Pus from an abscess. *a.* Pus-corpuscles, granular and nuclei, indistinctly seen. *b.* The same treated with acetic acid, revealing a clear membrane, inclosing three, four, or more nuclear granules.

However, in these days, a detailed description of the appearance of the pus-corpuscle under the microscope, seems quite unnecessary, for every person can see for himself, and that, too, which no words can as well describe. Although the pus-corpuscle, thus formed, as the product of what is called a true "healthy" inflammation, can always be definitely described, yet all inflammations are not equally active, and their products are very far from being as well formed. The pus-cell being the highest of its cell-products, the others with which we meet are of a lower character—they are cell-forms, but retain a good deal of their utricular character. Such are the *pyoid* corpuscles described by Lebert,* and such also are the *inflammatory corpuscles* of Gluge,† and many other illy-formed and badly-defined bodies which have no proper name, but which are met with in indolent suppurations, and especially in the expectoration.‡

* Lebert; Physiologie Pathologique, Paris, 1845, tom. i, p. 46.

† Gluge; Anatomisch. Mikrosc. Untersuch. zur Pathol. Minden, 1838, p. 12.

‡ Gruby has described very many forms of pus-corpuscles occurring in different parts of the body. He thinks the peculiarities are constant, and therefore puts much stress upon them, a view not at all supported by more recent labours. See Gruby's work in Latin—Observationes microscopicæ ad Morphologiam Pathologicum, &c. Vindobonæ (apud Singer et Gœring); a partial translation of this may be found in the Microscopical Journal, 1840–1842.

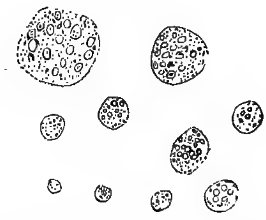
The *pyoid corpuscles*, although often having well-marked characters, scarcely deserve a separate name; for the products of inflammation

Fig. 39.

Pyoid corpuscles from meningitis, showing that they differ from those of pus in being larger and more delicate. (Magnified 350 diameters.)

being all abnormal, we cannot well expect any adherence to a definite type, but a variation or oscillation to a considerable extent. They occur as the products of inflammations of cachetic individuals, mixed with a few true pus-cells. As I have seen them, they are one-third larger than the latter, and contain, within their interior, many small granules instead of nuclei, and, on the whole, seem more delicate; and, if I may thus express myself, are more rudely put together. They are, therefore, easily distinguished from the "*normal*" pus-cell. Both from their aspect, and the individuals in whom they occur, I should say that they constitute the transitionary form between the tubercle and pus-corpuscle, if the present state of our knowledge of these products would allow us to admit that there ever occurs any transition from one to the other. And then, again, it may be asked if such phenomena would not lead us to believe in this very transition? For my own part, I cannot say that they would or would not; but, at any rate, they have constituted one ingredient of my opinion that tubercle was really an inflammatory product.

Fig. 40.



Inflammatory corpuscles, being only enlarged sacs filled with granular utricular matter. (Magnified 350 diameters.)

The *inflammatory* or *granulation corpuscles* of Gluge were pointed out by him long ago. He regarded them as products of inflammation, but was decidedly in error in attempting to explain their appearance or presence as the nuclei of blood-cells. Their distinguishing features from those of the pus-corpuscle are much more marked than from those of the pyoid corpuscle we have just considered. And yet, these features can scarcely be described; for the corpuscle, as a whole, shows a want of regularity in almost every particular—seeming to be the rudest expressed form of organic power—under the semblance of a cell. They are simple sacs, filled with a fluid in which float granules, in numbers from two or three to so numerous as to make the whole a semi-opaque object. As might thus be expected, the variability of their size is very wide—from $\frac{1}{5000}$ of an inch to that discernible by the naked eye. However, the size most frequently met with, is that of about $\frac{1}{1500}$ of an inch in diameter. When in contact with chemical agents, they behave like delicate forms; that is, the envelop begins to be dissolved, and therefore is rendered more clear, and finally ruptures, discharging its contents. In this respect, it does not differ from ordinary corpuscles of any kind, the envelops of which have been rapidly formed or are delicate. Their mode of formation might almost be deduced from their physical appearance, for they are only sacs containing granules and fluid. As far as my observation goes, they appear to be formed by the expansion of utricles into larger sacs, and as the cell-process here ends, the whole result is abortive—not only no nucleus being formed, but the cell-like body showing no individuality as to size. Thus, in some forms of cancer, in which a low, indolent inflammation supervenes, they attain a size so as to be easily visible with the naked eye. The same is true of them as found on the inner surfaces of ovarian cysts. But these monstrosities are comparatively rare, and they may be said to have an *average* size, a term which is not properly applied to cells or cell-like bodies having an individuality of their own.

I think it is well determined that they are the results of the inflammatory process alone, and in an effused blastema of a very low order; and this inflammation is of a simple nature, having no peculiarities, as might be said to be true of that of tubercle. On these accounts, much value is to be attached to their presence; since, wherever found, the question of inflammation on other premises cannot be raised. They are met with in dropsical fluids, of any source; as those of hydrocele, cystic disease of the ovary, ascites, &c.; also in organs having a low chronic inflammatory process, or at least where the inflammation is of a resolvable instead of a suppurative character —such as in the sputa of pneumonia, inflammations of the skin, and in that low form of nephritis known as Bright's disease.

And I will add, that my own observation has shown that their presence in the urine is nearly always certainly diagnostic of this disease.* They are also found in all pus to a greater or less extent, as this last may be the less or the more "laudable."

3. *The Cancer-Cell.*—The appearance of carcinoma in the system takes place under circumstances more obscure than those of any other morbid product with which we are acquainted. This is due to its high morphological character; for, although a heteromorphous formation, it stands far above those we have just been considering.

It will be seen that I have included it under the head of inflammatory products. I admit that, in so doing, I am stepping a little beyond the boundaries of demonstrated facts. But the position appears to me tenable. As a heteromorphous product, all my experience has led me to regard it as the result of an *abnormal nutrition*, and on a preceding page I have expressed my views as to the relations of this abnormal nutrition to the inflammatory process. Then again, its appearance often holds the relation, with inflammation, of cause and effect. Such is true as occurring in or after wounds of a part, or in the female breast, immediately sequent upon its inflammation. But in these instances we must not put too much stress upon the local lesion; for, as cancer, like tubercle, is always dependent upon a constitutional dyscrasia, its local appearance has other than simply local relations. This statement is broad, and expressive of many of the characteristics of the product; but, certainly, the tenor of its whole history, as we now understand it, is to this effect: its earliest appearance, in a distinct and unmistakable form, is as a few cells, as I have sometimes observed. We cannot trace it any farther back; but all analogy is in our favour for supposing that, like tubercle, these cells are formed in an unhealthy plasma effused from the blood. Moreover, upon the inner surface of the testicular tubes, where I have observed these cells just appearing, they seem to replace the normal epithelial cells there situated, and to be formed like them. As the first appearance of the disease is imperceptible in the majority of cases, there cannot be traced any coexistent hyperæmia of the part, or any congestion of the bloodvessels leading to an effusion of the morbid plasma; but in all probability, a condition of this kind does exist; and the fact that a mechanical injury, such as a blow or a compression, will sometimes give rise to the local appearance of this product, supports

* See, for some farther details on this point, an article of mine, The Microscope, and Renal Affections, Amer. Journ. Med. Sc. n. s. vol. xxii. p. 373.

this hypothesis; for, in those cases, there is an effusion of the blood-plasma from the local congestion; and we can justly infer that the reason of its appearance, in that particular locality, is because the mechanical injury was *there* received.

As I have just remarked, the appearance of an adventitious cell or cells, constitutes the first indubitable evidence we have of the cancerous product. This cell, the first, is also the last expression of this product, it being the *terminal* and not the *transitional* form. It is nucleated, and oftentimes has several distinct nuclei; but as to size, form, and other external characteristics, there are none, it is my present opinion, which are constant and peculiar, and by which it may *always* be distinguished from other cell-forms. Formerly, my view was, like that of most other microscopical pathologists then and now, that the cancer-cell *as such* has physical peculiarities, by which it can always be distinguished from other cells. But, from a careful review, of late, of the subject, based not only upon observations of the cells as physical objects, but upon a consideration of them as constituting an expression of pathological action, I am free to confess that I do not consider my former opinion tenable.

All or nearly all the microscopical relations of the cancer-cell which have any claim of being peculiar to it alone, may, I think, be stated in the following brief manner: The cancer-cell, when fully formed, is simply a nucleated cell; it may, however, have several nuclei, or none at all; it often is of an irregular shape, fusiform, caudate, &c., &c.; and its size is frequently equally variable; but the evidence of its being not only pathological, but of what is called a cancerous nature, cannot and does not depend upon any one of these points alone, but rather upon a combination of them all; and that, too, united with the fact of these cells occurring in tissues, and under circumstances where they cannot be regarded as a healthy product. From this general statement, it is plain that the cancer-cell has no claim of having a type either of structure or general appearance. As a cell, or as cells, it forms the highest heteromorphous product with which we are acquainted, for it is structurally and morphologically quite above both tubercle, and pus, and pyoid forms. Its differential diagnosis, which I think can now be made out in nearly every case, depends upon these considerations, which, when fully examined, will be found to be amply sufficient.

Suppose, for an illustration, that a small portion of a cancerous tissue has been sent to a microscopist for microscopic examination. He decides, in the first place, that the tissue in question is not a normal one, from the fact that its cell-constituents possess no *type* peculiarities. As a heteromorphous product, then, the diagnosis lies between its being

Fig. 41.

Examples of cancer-cells, showing some variety of shape and general irregular character. *a*. Very regular nucleolated cells, from cancer of the arm. *b*. Irregular cancer-cells, and such as are more commonly met with. c. Cells very large, and in which daughter-cells have been formed. (Last two from cancer of the breast.) I give here a few figures only, because a correct idea of the cancer-cell, as an aberrant form, can be learned only from direct study by means of the microscope.

tubercle, a pyoid form, or cancer ; and, from what has just been said, one can easily see that the decision that it was the last, or cancer, would not be difficult with a person who has long practised upon these products. But I may here remark that its differential diagnosis from certain homologous morbid products, although in my opinion quite clear, cannot always be as distinctly and positively made out. This point, however, belongs to a future page. All homœomorphous cell-products possess, to a considerable extent, as I shall soon attempt to show, the type-characteristics of the tissue to which they are homologous. All heteromorphous cell-products are characterized, as I have before said, by an absence of these type-forms. From these data, I think the experienced eye can always, or nearly always, decide correctly upon cancer-cells, when they exist in numbers sufficient to form a distinct product. On the whole, the high degree of organization is that which characterizes the carcinomatous product from that of all others of a heteromorphous nature ; and, according to the scheme laid down on a preceding page, it may be justly inferred that the line between cancerous and non-cancerous products is always clearly defined, and also that they never pass insensibly into those of a healthy character.

The question may be fairly asked, In what consists the so-called "malignity of cancer-cells ?" Space does not allow me here to criticize or even examine the different definitions given by various writers to the term *malignant* ;* but if we are to understand by it the power

* It is quite remarkable to observe the obscurity of this point, not to mention the incompatibility of the notions entertained. Some writers define malignity to

of a morbid product to replace a healthy tissue, to reappear, after removal, in the same or in a distant locality, and finally to compromise the life of the individual—if this, I say, is the definition of *malignity*, I think a tolerably satisfactory answer can be given to the question. It lies, in the first place, with a heteromorphous product, and its appearance here or there is but a local expression of a constitutional dyscrasia. So far, the peculiarities of cancer are parallel with those of tubercle. But it differs from tubercle in possessing a much higher organization. The constituents of tubercle are only cell-like forms; they have not risen to the dignity of nucleated cells. Their life, therefore, is finished with their formation, and the product increases by addition rather than by growth. Not so, however, is it with cancer. Its constituents are nucleated cells, whose existence is not completed by their simple formation; for they grow and increase, and new cells are formed within them, from their nuclei. They may be said to constitute potential germs, capable of an indefinite growth and reproduction, requiring only a nutritive material in which they may be formed.

The so-called malignity of the cancerous product may, then, be said to be due to its constitutional origin, united with a power of individual reproduction by the endogenous and exogenous formation of cells. In fact, it may be said to be due to this last alone; for, I may add, did tubercle-cells possess the high character and individual power of those of cancer, they would be equally as malignant.* Its capability of self-propagation and perpetuation, after being once formed, by the successive multiplication of cells, gives it a power of almost limitless expansion. Its liability to a plural appearance is, of course, due to its constitutional origin, for this is equally true of tubercle. But its liability to a reappearance in the same locality after removal, is dependent, probably, upon both the constitutional infection and the circumstance of a small portion being almost necessarily left behind, which serves as a basis for the building up of

be the sum of the properties possessed by cancer. Others define the word as that of incurable. Others base a definition of the term upon anatomical appearance, as, for instance, Dr. Hodgkin, who regards growths with a cystiform structure as malignant. But with a majority of writers, the term appears to have a very vague signification. The notion of a malignant disease exists in their minds as *an entity*, possessing irresistible destructive agencies and tendencies, manifested by a reappearance in a worse form as often as apparently entirely removed.

* The independent vitality of the cancerous tissue has been well shown by some curious experiments by Dr. Leidy, who introduced portions of it beneath the integuments of frogs. See Proceed. of the Acad. Nat. Sc. Philadelphia, vol. v. p. 212.

a new product. For, both theory and experience teach us, that a cancerous product being *entirely* removed from a locality, that locality or tissue is not any more liable to its reappearance than it was previous to the first invasion. We may, therefore, conclude that the malignant character of the cancerous epigenesis, in contradistinction to that of other morbid products, is, after all, due to its high power of individual cell-*reproduction.* This is reducing the whole to morphological peculiarities, which are based upon our best appreciation of the nature of morbid cell-products.

Hitherto, we have considered only the true histological product of cancer—*the cell.* But the cancerous tissue, from the very fact of its being a pathological one, includes other elements, which may well be mentioned here. In a very few instances, I have met with the cancerous tissue as consisting *entirely* of cells; but this is quite rare, and occurs only when this epigenesis exists in its highest state. Elements of an inferior character generally enter largely into its composition, and these are such as are found in most other morbid growths.

One of the most constant of these is an amorphous granular substance, produced by the condensation of the granular matter. This is often so tough as to resemble a fibrous tissue; and, in cancers of a slow growth, forms a large ingredient of their substance. It is the framework on which the more delicate parts rest, and, in the higher forms of cancer, the grades of its transition into a fibrillated tissue are imperceptible.

The second element, almost equally as constant, is granules of various sizes, scattered everywhere. These are often expanded into cell-sacs, which are crowded with fat-globules. These fat-globules exist in a free state also, and often in such abundance as to give to the whole mass a decidedly fatty character. Crystallized fat, or cholesterin, is often found.

And thus it is that fat and albumen, in these imperfect conditions, form a large portion of the non-solid constituents of cancer. In many instances of this disease as occurring in the breast, it was evident to me that its rapid increase was due to a simple effusion of these elements, rather than to the increase of cells. In fact, my own experience is that the secondary forms of cancer, or the primary ones, after the cachexia is fully established, are for the most part made up of these low amorphous constituents.

In those of larger size and more active character, *bloodvessels* are found. These have, as Berard* has shown, the most simple structure, being tubes whose walls are composed of a single membrane. But the other peculiarities of the bloodvessels insisted upon by Berard, I have never been able clearly to make out.

It does not belong to me in this place, to dwell at all upon those peculiarities of physical appearance which many authors have thought sufficiently well marked and constant to serve as the basis of a very minute classification;† for these are points of much less scientific importance than the histological relations we have just considered. In fact, it may be fairly questioned if such divisions are in any sense scientific; and in support of this, I need only refer one to the incongruity of opinion existing upon this subject.

There is, however, this much certain, that the growth and condition of a cancerous epigenesis is *dictated* very much by the activity of the nutrition of the part in which it appears; and its first appearance is undoubtedly the same wherever it may be, that is, consisting of a blastema in which appear cells. Thus, when occurring in highly vascular organs, such as the brain and the eye, its growth is rapid, and consists almost entirely of cells. These forms have been called *encephaloid.* But when occurring in tissues less vascular, and in which the nutrition is less active, its growth is much more slow, and the cancerous blastema secondarily effused, less organizable. So that cells are not so numerous, but the growth is, for the most part, made up of a coagulated, fibrinous, granular fluid, constituting a kind of fibrillated or fibrous tissue.

These forms have been called *scirrhous*, and when a pigment forms a constituent of either the *encephaloid* or *scirrhous* forms it is then called *melanoid.* Such are the distinctions which have been made, and which are convenient for use. But even these, few as they are, pass into each other on every side. Some call certain forms scirrhous which others think scirrho-encephaloid, &c. &c.

It is enough for me that I should look at the subject of cancer as an histological product—as the expression of a constitutional dyscrasia, under the form of the highest pathological cell with which we are acquainted.

A discussion of the exciting causes of cancer, its development, distribution in this or that organ, its decay and consequence, belongs to

* P. H. Berard; Dict. de Médecine, in vol. xxx. art. Cancer.

† For a good illustration of this subdivision of forms, see Walshe's work, The Nature and Treatment of Cancer, London, 1846, p. 10.

its clinical history. It would of itself be sufficient for a volume, as it already has been in several instances (Walshe and Lebert); and, following the plan pursued in the consideration of the other heterogeneous products, I purposely omit here anything of this kind.*

CHAPTER V.

HOMŒOMORPHOUS PATHOLOGICAL PRODUCTS.

In the pathological portion of this work, our attention has hitherto been occupied with a consideration of those morbid products which have no histological affinity with healthy tissues. They were, therefore, properly termed *heteromorphous;* and in the scheme of the origin of all morbid products which I advanced on a preceding page, I regarded them as the results of perversion of the plasma, which not being at all acceptable to the tissue into which it is effused, its low organizing power gives rise to products of an inferior character, which are at the same time quite dissimilar from any found in the healthy economy. But in discussing pathological conditions, one always perceives, on account of the anomalous nature of the subject, that the landmarks and exact definitions of natural science cannot be used. Thus, although I think that heteromorphous products are due to a perversion of the plasma or that condition known by the general term *inflammation*, yet, in some instances, this does not seem to express enough. For in the case of tubercle and cancer, another condition is coexistent (I do not say causative). This condition is what is called a constitutional dyscrasia, or an assemblage of depraved conditions leading to local expression of disease.

* Owing to the attention which the subject of cancer has excited with all pathologists from the earliest times, its bibliography is very full. I cannot pretend to give it here; but it will be found carefully and excellently drawn up in Dr. Walshe's work on Cancer (The Nature and Treatment of Cancer, p. 246, London, 1846). Since the work of Dr. Walshe, however, several excellent monographs have appeared, among which the following may well be mentioned here: Lebert; Physiologie Pathologique, Paris, 1845, tom. ii. p. 241. Vogel; Zeitsch. für Natur. Med. Bd. iii. 1 heft. Bruch; Die Diagnose der boesartigen Geschwulse, Mainz, 1847. Virchow; Archiv fur Pathol. Anat. und Physiol. Bd. iii. Bennett; On Cancer and Cancroid Growths, Edinburgh, 1849. Lebert; Traité pratique des Maladies Cancereuses et des affections curables confondues avec le cancer, Paris, 1851.

We can get only approximatively at the intimate relations of disease; this may as well be stated first as last. Both microscopy and organic chemistry have quite failed to teach us anything respecting the ultimate data of pathological phenomena.

I have thought these remarks somewhat called for, before I proceed any farther, for, in regarding all pathological products as due either to a perversion of the plasma, or the type-power, I would not wish to be understood as thereby expressing the view that, by such scheme, the nature of all pathological phenomena can be comprehended.

The remaining morbid products which we have to consider belong to another class. In every instance they simulate the physical appearance of the healthy tissues. They are therefore properly called *homœomorphous.*

As I have stated on a preceding page, I do not regard these products as simple hypertrophies of the healthy tissues. For hypertrophy is simply the increase of the normal form, without the addition of a morbid element.

Then, again, homœomorphous products, although they simulate, yet they do not exactly resemble, the healthy structure. They always want their beautiful regularity and uniformity of physical contour, not to mention the entire want of function. I admit that, judging from mere physical aspect, it is often quite difficult, if not impossible, to draw the line between a true morbid epigenesis and a simple hypertrophy of the normal tissue. But, in my opinion, the line of demarcation is always distinct histologically, and in no instance is there a transition of the one into the other. These remarks, although applicable to all homœomorphous morbid tissues, are especially true, as we shall soon see, of the so-called hypertrophies of glands.

I regard all homœomorphous morbid products as simple *epigeneses*, or as always analogous new formations *upon* the healthy tissues. And when I use the word *always*, I make a statement of more physiological importance than would at first be supposed, for I thereby deny that new analogous tissues are ever produced, except in conjunction with the normal ones, through the influence of which alone they are developed. In fact, it appears to me that to admit the opposite view, that tissues, like the normal ones, may be formed anywhere adventitiously, is tantamount to the admission of the doctrine of *equivocal generation;* because, morphologically speaking, an animal is only a collection of individual tissues, and each of these tissues may be said to have in one sense an individuality as distinct as that of the whole animal itself.

The pathological relations of this doctrine are of very great importance, and I am inclined to look with doubt upon those descriptions of the appearance of new tissues in localities quite distant from those in which the same occur normally.

Aside from considerations of this kind, doubts might be properly raised from the fact that, in most instances, the evidence that the tissue in question is like the normal one, is based upon gross appearances alone. Every-day experience is showing us that, in important cases, we should not, and generally cannot, judge of the nature of a tissue by its mere physical aspects to the naked eye; and what is more, the liabilities for deception are such that no familiarity can remove them. I shall have occasion on a future page to refer to instances of this kind, and also to show that the microscopical structure is, and should be, the only reliable evidence. I may add that, upon a point of this importance, I do not think the *ipse dixit* of every observer should be taken; for aside from the comparatively few who well understand microscopy, the number is much fewer of those who know well the characteristic microscopic appearances of tissues.

I shall therefore take up homœomorphous morbid products in the light only of *epigeneses*.

I. Epithelial Epigeneses.

From the fact of the wide, indeed almost universal distribution of the epithelial tissue, its pathological epigeneses are more frequent and more important than any other belonging to this class of products.

There is one other circumstance which may be considered as favouring their frequent occurrence. This tissue is composed of simple cells, often existing to be sure under various combined forms; but its nutrition is necessarily delicate, and quite liable to be perverted, or rendered abnormal, thus giving rise to these products.

As a normal tissue, it necessarily possesses an individual type of structure and appearance; but this has a variety according to the locality. The pathological formations follow somewhat in the routine of this variety, so that, in the same way, there may be said to be different kinds of epithelial epigeneses. I am not, however, disposed to insist upon these refinements.

Both for the sake of convenience, and as a somewhat natural division, epithelial epigeneses may be separated into three groups :—

1. *Those occurring upon mucous and serous membranes.*
2. *Those occurring in or upon the skin.*
3. *Those occurring in the ovarian tissue.*

1. THOSE OCCURRING UPON MUCOUS AND SEROUS MEMBRANES.

This group contains by far the largest number of these products; for they are liable to occur anywhere, where these membranes are distributed. But, according to recorded experience, they are rarely met with except in a few localities.

These are, in the uterus,* in the bladder,† in the mouth, antrum, and lip, on the pia mater,‡ pleura, and peritoneum. But the instances which do not belong to the mucous membrane of the mouth are few and isolated. The mucous membrane of the lower lip, where it passes into the epidermic tissue, is a very favourite locality, and the growth formed here is the so-called *cancer of the lip.*

These epithelial tumours have the same structural peculiarities wherever found. They are composed of epithelial cells, or the remains of such, combined with low cell-like or vesicular products, all lying in a coagulated, greasy stroma. At the earliest period of their growth, these cells cannot be well distinguished from those of the normal tissue with which they lie in contact; but they soon show their pathological character by their want of uniformity of size and shape, and their short lives. Thus, they seem to experience the variety of form and shape which is just compatible with their preserving their epithelial-cell type, and, being without function, their membranes burst, after which they are inclosed as flattened scale-like forms, in the cell-liquid and unorganizable plasma which has coagulated. This unorganizable plasma contains fat both combined and free; and crystallized fat or cholesterin is often found. Very rarely do they contain much or even any fibrous tissue. The whole consists of an abortive cell-product, and therefore without object and without limitation. The physical peculiarities of these tumours due to their particular locality need not be mentioned here; they belong rather to the practical than to the histological study of the subject. But

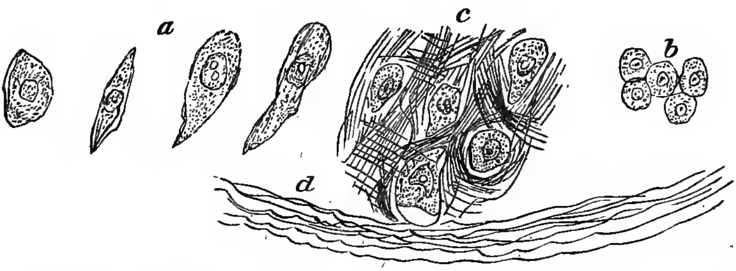
* Lebert; Physiologie Pathol. &c. tom. ii. p. 16.

† For those of this kind I refer to the cases of *trichiasis*, recently so thoroughly studied by M. Rayer. *Vide* Memoirs de la Soc. de Biologie, à Paris, 1850, p. 167.

‡ Lebert; *loc. citat.* tom. ii. p. 67.

that occurring on the lip, or the so-called *labial cancer*, needs, however, from its frequency and notoriety, to be separately spoken of. Until within a few years past, it has been regarded as a true cancerous product; and even now it is regarded by some pathological microscopists as the transitionary form of the non-malignant into the malignant tissue.

Fig. 43.



Example of the cell-constituents of epithelial epigeneses. From "cancer" of lip. *a*. Abnormal and irregular epithelial cells. *b*. Normal epithelial cells from point where epigenesis arose. *c*. Cells and fibrillated tissue, the latter forming the framework of the epigenesis. *d*. Fibrillated tissue isolated.

But the most careful and thorough histological studies of this tumour do not indicate that it is malignant under any circumstances. In fact, such is my own view, based upon an experience not very limited, and that, too, with this question in view. Moreover, its whole history and economy is of the same import, and, as far as my own observation goes, there is no proof that it is even connected with a constitutional dyscrasia; and I will add that its liability to return always depends upon local instead of general causes.

A question of considerable both histological and practical importance has arisen of late. It is whether, by the aid of the microscope, these simple epithelial tumours can always be discriminated from cancerous products occurring in the same locality.

In giving an affirmative answer to this question, I know that I am expressing more than many pathological microscopists would be willing to allow. But falling back upon my own experience, which I may be allowed to say has not been inconsiderable, this is the opinion which I maintain, and, were it not out of place, I might cite here several prominent instances, illustrating quite well the basis of this view. If it were asked in what consisted the distinction, I should reply, in the absence of all individual cell-type in the one (cancer); and the presence of it in the other (epithelial epigenesis); and the appreciation of these differences in every, or almost every, instance, is something not to be described or figured, but to be learned only by long-continued study of morbid products.

Meckel* laid down the law that morbid tissues take upon themselves the morphological characteristics of the normal tissues with which they lie in contact. It might be argued that, this law being true, cancer occurring in an epithelial tissue would be so epitheloid as not to be distinguished from a simple epithelial product.

But it is my opinion that this law of Meckel holds true only of homœomorphous products. In fact, like the "law of analogous formation," of Vogel,† it seems expressive only of the same condition of nutrition that, on a preceding page, I have intended to convey in the terms *perversion* of the *type-power*. But it does not extend to that other condition, *perversion of the plasma*, which is the source of heteromorphous products. But as we are not yet thoroughly acquainted with all the phenomenal conditions of either physiological or pathological products, this is a point upon which, from due respect to science, a *positive* opinion might well be deferred. At all events, the course of study I have pursued has led me involuntarily to believe that the line of demarcation of these various products is much more clear and distinct than would be admitted by other observers.‡

2. THOSE OCCURRING UPON THE SKIN.

These constitute the so-called epidemic tumours, which include not only formations upon the skin, such as nails, horn, hair, and external tumours; but also formations *in* the skin, or beneath its surface, such as encysted growths, the former being only an *eversion*, while the latter is an *inversion* of the skin. The contents of these external tumours

* Meckel; Manuel d'Anat. gen. descrip. et Path. &c. trad. de l'allemand, Paris, 1825, tom. ii. pt. ii. p. 213.

† Vogel; Traité d'Anat. Path. gen., French ed. Paris, 1847; or, transl. by Dr. Day, Amer. ed. p. 114.

‡ The bibliography of epithelial epigeneses is confined almost entirely to that of the so-called *labial cancer*. The following is a reference to some of the cases, and writers who have devoted particular attention to the subject:—

Warren; Surgical Observations on Tumours, &c. Boston, 1839, p. 342.

Valentin; Repertorium, 2 ter Abth. p. 311, 1838.

Gluge; Anat. Mikros. Untersuch. II. Heft, p. 136, 1838.

Velpeau; Lec. Oral. de Clin. Chirurg. Paris, 1841, tom. iii. p. 138.

Walsh; *loc. cit.* p. 255.

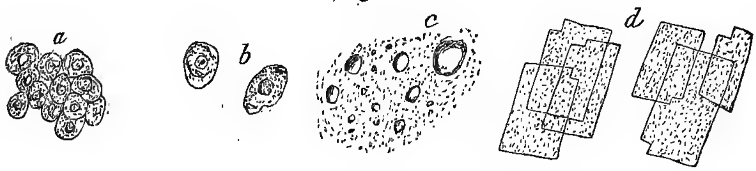
Lebert; Physiol. Path. &c. Paris, 1845, tom. ii. p. 376; alo, Traité prat. des Mal. Cancer, &c. Paris, 1851, p. 611.

Bennett; On Cancer and Cancroid Growths, Edinburgh, 1849.

I have observed, microscopically, upwards of 25 of these epigeneses.

(such as *condylomata*, &c.) need not here be detailed, for, both generally and microscopically, they are the same as of the epithelial ones just de-

Fig. 44.



Example of the cell-structure of epidermic epigeneses. When *upon* the skin, or eversions, their structure is exactly like that of epithelial tumours. The above are appearances of the inversions, or tumours, beneath the surface. *a.* Layer of pavement epithelial cells lining the sac. *b.* Isolated cells. *c.* Free fat, in globules. *d.* Cholesterin. All from an encysted tumour of the breast.

scribed. In the instances of horn, hair, &c., these last follow so closely in the line of the normal tissue, that, were it not for their irregular occurrence, they could not well be discriminated therefrom; but their want of the normal tissue individuality is very apparent. In regard to cystic or follicular growths in the skin, they are, as I have just remarked, simple *inversions*, and are cavities (the external opening or origin of which may be closed up partially or completely) of greater or less size, lined with an epithelial membrane, and filled with epithelial products, mixed up with those forming in a very low plasma, or secreted by the contiguous glands. Such products consist of coagulated albumen, free and combined fat, cholesterin, and cerumenous matter. They often attain a large size by simple displacement, and good examples of them in their smallest form are not unfrequently seen on the end, or about the base, of the nose.*

* The literature of the epidemic tumours is very scattered, and owing to the singular classifications of morbid growths of some authors, they are not easily referred to. They are generally included under the head of *encysted tumours*, when they are situated *beneath* the skin. But when they are *eversions*, or above the skin, they have been known and described by a variety of names. But for more especial descriptions, based upon microscopical evidence, see—

Gluge; *loc. cit.* part iv.

Vogel; *loc. cit.* Amer. ed. p. 220.

Lebert; Phys. Pathol. tom. ii. chap. i.

Walshe; *loc. cit.* p. 544, *et seq.*

Bennett; On Cancer and Cancroid Growths, Edinburgh, 1849, *passim.*

In the follicles of the nose there lives a parasitic animal (*acarus folliculorum*), first described by G. Simon (Müller's Archiv, 1842, p. 212). It has since been thoroughly studied by E. Wilson (Lond. Philos. Mag. June 1844, and Phil. Trans. 1845). I have seldom found it, and think it quite rare. It belongs to this locality, just as the *Pediculus capitis* does to the head.

3. THOSE OCCURRING IN THE OVARIAN TISSUE.

Under this head I include all cases of the so-called Cystic Disease of the Ovary. In other words, I regard this form of ovarian disease as simply an epithelial epigenesis developed in the ovarian stroma, and, more specially, this upon the Graafian vesicles.

This view may appear singular, but I base it upon a pretty extended investigation of the subject, recently made. I cannot here enter into the full details of this consideration. But the leading features of this doctrine may be briefly and concisely stated, as follows: On a preceding page, I remarked that, morphologically speaking, the ovum was only an epithelial cell; and an ovary may be considered as being only a local (and the only one in the body) collection of epithelial cells, destined for a definite object. But, until fecundated, these epithelial cells are subject to those morphological laws which appertain to all cells. The human ovary, then, is simply an epithelial tissue, and, therefore, we ought not to be surprised to find it subject to those morbid conditions which are constantly occurring in this same tissue elsewhere. Its whole structure consisting solely of epithelial cells lying in a fibrous stroma, the number of these morbid conditions must be quite few. In fact, they are almost necessarily limited to an epigenesis of the cells of the stroma. Experience shows that in almost every case this belongs to the cell-structure, and therefore an epithelial epigenesis may be truly said to embrace almost the whole list of ovarian diseases.

If we carefully refer to the subject, it will be seen that the human ovary is a tissue in which we might well expect disease would occur. Until the age of puberty it is passive, and exercises no function. But after this epoch it is the seat of a periodical disturbance. Each month there occurs in it a temporary congestion, leading to the evolution of a peculiar product, the ovum.*

Its nutrition as a tissue, therefore, is constantly disturbed, and with individuals whose general condition is not what is termed "healthy," this disturbance may lead to a permanent change, which, once established, is constantly fostered by each ensuing monthly period. This disturbance, known by the name of *ovaritis*, constitutes, in one sense, nearly the sum total of ovarian disease. This, modern

* I adopt unhesitatingly the view of Bischoff, that an ovum is discharged each month, in the human female.

experience of some value and extent has quite clearly demonstrated, and upon data of a gross character alone.*

From this abnormal nutrition, an epigenesis arises, and the Graafian vesicles, instead of pursuing that line of normal development which would lead to the evolution of ova, deviate therefrom, and monstrous enlargements and growths ensue. As closed sacs, they are filled with liquid, and, this increasing, they rapidly dilate, and, having attained a certain size, are lined by an epithelial membrane, which arises according to the law of analogous formations. This last is truly indicative of the epithelial origin of the cysts, and we could have no better evidence of their histological nature. The gross appearance of an ovary filled with cystic disease is well known—a collection of sacs of all sizes, which may or may not communicate with each other; in fact, it is only and especially in those cases where the abnormal development has been general and pretty uniform, an enlarged condition of the normal structure of the ovary; for a thin slice of a healthy ovary has, under the microscope, very much the aspect of ordinary cystic disease; and I have often thought that, could the same be magnified with a solar microscope, we should have an appearance quite like that of the advanced cases of this disease. As it is of an epithelial epigenesis, every structure appertaining to it has an epithelial character; especially is this true of the pavement epithelial layer lining the sacs. In the liquid of these sacs may be found floating epithelial cells, single or in clusters. But in an epigenesis of this size and character, the abnormal nutrition is manifested otherwise than by its simple growth. Active inflammatory conditions are always met with, and these give rise to the various products here observed. Such are collections of pus and pyoid forms, effused blood, albuminous matter, fat, free and confined, and cholesterin. These, together with the serum, constitute the products and contents of the cysts. The uniformity of these results in an histological point of view, can scarcely be appreciated except by those who have studied these morbid products microscopically. And, notwithstanding the variety of forms under which ovarian disease appears and has been described as based upon gross appearances, yet, microscopically, they are (judg-

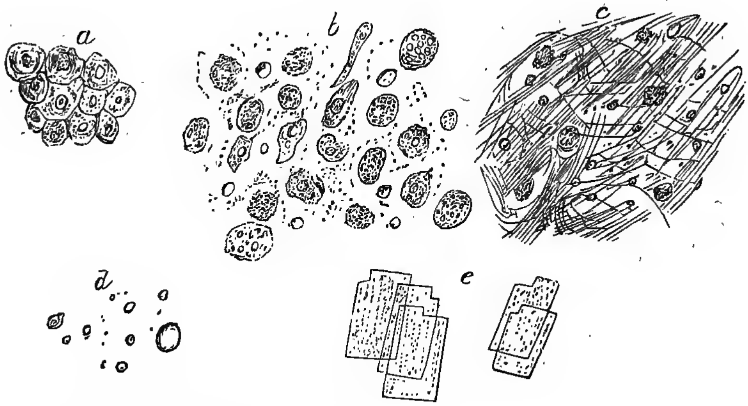
* On the Pathology of Chronic Forms of Ovarian Disease: by E. J. Tilt, London, Lancet, 1849, vol. ii.

Also, by the same; On Diseases of Menstruation and Ovarian Inflammation, London, 1850.

Dr. Tilt has collected 248 cases of this disease, traceable directly or indirectly to menstrual irregularities, or uterine disturbances.

ing from my own experience), all resolvable to these epithelial elements, and others due to inflammation. Take an ordinary case: We find that the larger cysts have thickened walls composed of a fibrillated granular tissue, the result of inflammation. New vessels have formed on or in its substance, and its inner surface is lined with an epithelial layer, except perhaps in the most severe cases, where various inflammatory products of albuminous, fibrous, and granular forms take its place. The liquid contents of the sac are of a corresponding nature; for when the sac has a true epithelial lining, the liquid is mostly serous and clear, although there are, floating in it, flakes of epithelium peeled off from the inner surface. But when the cyst is large and its inner surface is covered with inflammatory products, in place of the epithelial membrane, the contained liquid is more dark, and its contents more heteromorphous in character; such are pus and inflammatory corpuscles, the latter of which are crowded with dark granules, and often attain a size so as to be distinctly seen by the unaided eye.

Fig. 45.



Examples of microscopical forms met with in ovarian cysts. *a.* Pavement epithelial cells, forming the lining membrane of all the cysts. *b.* The pus, pyoid, inflammatory, and irregular epithelial cells formed in the liquids of the sacs, the dark or brown colour of the liquid being in a ratio corresponding with the amount of inflammatory products. *c.* Structure of the *encephaloid-looking matter*, often found on inner surface of cysts, being composed of fibrillated tissue inclosing granular corpuscles, fat, and albuminous matter. *d.* Free fat in globules. *e.* Cholosterin.

In the excess of the inflammation, the capillaries may have burst, and then, blood forms one of the ingredients. These various products, in their different combinations, give rise to the peculiarities of the contained liquids of the sacs, which vary as to colour, as is well known, from that of straw to that of dark chocolate. All these various appearances can be generally noticed in an ordinary case of this disease, and, in quite an extended series of cases which I have had the good fortune to study, I have never failed to trace clearly the epithelial epigenetic relations of the affection.

It is not for me to say that cysts in this organ may not, and do not, sometimes, have another origin, being, as Rokitansky* observes, "new formations from the beginning." But cystic disease of this nature I have never had the good fortune to observe, and I cannot easily conceive of its occurring in an organ such as the ovary, composed of only vesicles and stroma. Real carcinomatous disease of the ovary, attended with an enlargement of the Graafian vesicles, sometimes occurs, and we have an additional evidence of its real nature, in the fact that the same disease existed in other portions of the body. Such cases have been recorded by Baillie, Cruveilhier,† and Rokitansky. But, without doubt, such cases are quite rare; and on those recorded cases, in which the presence of malignant disease was not met with in other parts, I cannot place much reliance; for, in the more advanced forms of common cystic disease, appearances are observed having all the gross aspects of real carcinoma; and some of the best pathologists of the present day have acknowledged that, trusting to gross appearances, they have been deceived thereby. This is a point which well might be enlarged upon with interest, for, aside from its importance, there could be well illustrated in it the advantages attending, in fact, the necessity, of the microscopic analysis of all morbid products.

At this late hour of our labours it would not be proper to pause to vindicate, or even to urge, a course of inquiry, the truth of which we have necessarily taken for granted all the while. From its not uncommon occurrence, and its almost necessary fatality, epithelial epigenesis occurring in this tissue has an importance quite exceeding that of its occurrence elsewhere. These relations, however, are more of a practical nature, and I have desired to look at the subject only in an histological point of view. Even in this light, I should not have discussed it thus fully, had I not felt the justness of asserting my claim for the priority of the view of *the single nature of the cystic disease of the ovary, it always being an epithelial epigenesis of the Graafian vesicle.*‡

* Rokitansky; Pathol. Anat. Syd. ed. vol. ii. p. 332.

† Cruveilhier; Anat. Path. du Corps Humain, &c. Paris, lib. v.

‡ For a farther discussion of this interesting subject I may refer to an article of mine, illustrated by cases, Cystic Disease of the Ovary, as elucidated by Microscopical Inquiry, Charleston Med. Journ. vol. vii. Jan. 1852, p. 27.

It now remains for me to notice, by way of conclusion, another form of cystic disease of the ovary, which is so allied with the preceding that it may well be alluded to in this connection. I refer to those ovarian cysts in which are found hair, teeth, and sometimes bones. I have had a few opportunities to examine these piliferous cysts, and in every instance they had all the characteristics of enlarged Graafian vesicles. They were lined with an epithelial layer. When we consider that hair is, histologically, only metamorphosed epithelium, it is not difficult to comprehend that it should sometimes arise as well in these localities as elsewhere. The facts as stated by Cruveilhier, that it is found in males, and also in other than sexual organs of the body, would lead us to conclude that it is to be regarded only as one epithelial epigenesis occurring within another, and having no essential dependence upon the ovary. Cysts of this kind have undoubtedly a close relation with those constituting encysted dropsy of this organ; and, from all I have seen, may be referable to the same causes.

Of that other class of cases, coming, however, in the same category, in which teeth and pieces of jaw-bone are found in ovarian cysts, many curious examples have been recorded. It has not been my good fortune to see any of these cases, and concerning them, therefore, I am entitled to no scientific opinion. But this much I will say: that the time has now arrived when our knowledge of the relation of the sexes, based upon careful experience, enables us to justly take a decided view of the matter. With regard to the human female, the hypothesis that a new individual being, or parts of such (thus showing that a new individuality had at least commenced), can be produced without the conjunction with the ovum of the fertilizing fluid of the male, cannot, in the present state of science, be for a moment entertained. I well know that, in many of the remarkable cases recorded, the virgin aspect of the sexual organs is strongly insisted upon. But in all these matters, we must consider that it is unphilosophical to place an hypothesis founded upon statements of which we are not absolutely positive, against a view founded upon common experience and supported by all analogy.*

* For cases of this kind deserving notice, see the following: Anderson, Ed. Med. and Surg. Journ. vol. ii. p. 180; Cruveilhier, Anat. Path. livr. xviii. and xxxvi.; Abernethy, Med. Chirurg. Trans. vol. i. p. 35; Clapp, Lond. Med. Gaz. vol. xliv. p. 282. Also, Nouv. Dict. de Méd. et de Chirurg. Prat. art. Ovaire.

II. Vascular Epigeneses.

Under this head, I do not mean the new vessels formed in tumours of either a heteromorphous or a homœomorphous nature, for such are direct prolongations of the normal vessels, and can only be regarded as morbid from the relations under which they are found. They exist in virtue of the law of congruity of tissues; that is, there must be a nutritive supply, where the nutritive function is to be performed. Strictly considered, therefore, they are physiological and not pathological formations. Not so is it, however, with true vascular epigeneses, which are found holding no relations of function. Such are the erectile tumours, collections, or little knots of bloodvessels, either venous or arterial, existing usually directly beneath the skin. They generally include in their midst a small quantity of areolar tissue, and are capable of a temporary erection like the ordinary erectile tissue. In almost every instance they are located somewhere on the upper portion of the body. In by far the majority of cases they are obviously congenital, and I think there is reason to believe that, in those few instances where they seem to arise anew after birth, this is really not so, but is due rather to a sudden increase of that which has been formed congenitally, but so small as not to be perceived. We are, however, quite imperfectly acquainted with the morbid conditions on which these epigeneses depend. Like all morbid products, they appear to be, as far as I have examined them, infra-formations; and, although there are vessels, yet these are much less perfect than the healthy ones on which they rest.

In an histological point of view they possess but little interest. When examined microscopically, after the tumour has been removed from the body, they will be found composed of vessels with quite tenuous walls, all bound together by a delicate areolar tissue, which shoots up between the meshes. This areolar tissue is an embryonic formation; that is, the common areolar tissue persistent on a low stage of development. The cells, therefore, are fusiform, and have not passed into true fibres.*

* These tumours have been known by several names—thus: Telangiectasis, Hæmatoma, Hæmatoneus, Nævus vasculosus, Aneurysma per anastomosin, &c. &c. Its literature is very scattered, and I cannot here notice it.

III. Muscular Epigeneses.

The muscular tissue, as we have seen on a preceding page, is a true embryonic formation, and on this account we should not expect to find it subject to the morbid phenomena of epigenesis.

This I believe is really the case. For, although possessing a well-marked nutritive energy, there is no evidence, as far as I am aware, that it is ever repaired after it has been removed by accidents or wounds. In all these instances, and especially when analyzed by the microscope, the tissue of repair has been found to be of a fibroid character, the true muscular element being absent.

In regard to its real epigenesis in a distinct form, there is yet no evidence based upon direct observation that it ever occurs.* That it is thus produced, has been inferred from the consequences only. But for my own part, I am quite disinclined to admit it on such premises alone.

In the instances where the voluntary muscles of the extremities or the muscular tissue of the heart become greatly increased in size, there is no evidence, based upon microscopical analysis, that this increased size is due to the formation of new fibres. On the other hand, as far as I have examined the matter, it seems referable to the fact that the fibres, already existing, gain in size, exactly as we have seen to be the case with the increase of this tissue from the young animal to the adult.

The enlarged volume of muscular tissue due to increased function, as in the instances of that of the heart and those of the extremities, is therefore properly not a pathological, but a physiological phenomenon. It is an *hypertrophy*, rather than an *epigenesis*.

Those cases in which it has been asserted that this tissue is formed in exudations, and where the evidence that it is muscular tissue is not based upon a microscopic examination, I cannot for a moment admit as being worthy of consideration. They are probably founded on error, and, on an histological point of this importance, I should be quite unwilling to admit any but the very best evidence.†

In regard to the epigenesis of the non-striated muscular fibre,

* Since the above was written, I have met with two notices of the alleged appearance of true muscular fibres; one by Rokitansky (Weiner Zeitschrift, 1849), in a tumour of the testicle, the other by Virchow (quoted in Gluge's Atlas of Path. Histol. transl. by Leidy, p. 34), in a tumour of the ovary.

† See Müller's Archiv, 1834, p. 451.

Vogel* thinks it sometimes occurs, and has given figures representing it. These appearances I do not remember to have met with in any tumours, and on that account I would not wish to appear to deny the fact of its occurrence. Still, if *ever* really an epigenesis, it is at least rarely so; and much care is necessary in discriminating between it and certain fibroid tissues which are not uncommon, and which simulate very closely the appearance of organic muscle.

IV. Nervous Epigeneses.

What has just been said of the muscular, is quite applicable to the nervous tissue. It is an embryonic one, and one, too, having the same or even a higher physiological dignity. Its occurrence, therefore, under abnormal conditions, would not be looked for. I am quite inclined to doubt if it ever is renewed after injuries.† It is true that the continuity is made up by a tissue seemingly capable of performing, to a certain extent, the nervous function, but there is no evidence that this tissue exactly resembles the true nervous tissue which has passed away. I know that this view is not acceded to by Steinrück,‡ Nasse,§ and others. Still, more careful and extended inquiries are needed; and I feel pretty confident that, unless the cases be in very young individuals, the view of the non-reparation of true nervous substance will be found to be the correct one. In regard to real epigeneses of nervous tissue, we have as yet no good evidence that they ever exist. In the cases of hypertrophy of nerves, and the so-called nervous tumours (*neuromata*), the new tissue is not that of nerve, but one of a delicate fibroid character. Such, at least, has been true of some few cases I have had the good fortune to analyze microscopically.

There is an additional reason why we should not expect the occurrence of the true nerve-tube under these abnormal relations. It is this: We have seen that it is formed from nerve-cells, and these are originally formed from the vitelline globules; and we should little look for the formation of these cells in the track of a nerve, and especially when the nutrition was abnormal.

I do not wish to limit my views by my own experience; still, I am not at all inclined to the opinion that new formations of true nerv-

* Vogel, *loc. cit.* Amer. ed. p. 176, pl. iv. fig. 4, and pl. vii. fig. 2.

† I refer here only to the human subject; for, as is well known, and as I have myself had the good fortune to observe, this tissue in common with the others is renewed in the restored parts of tritons, salamanders, &c.

‡ Steinrück; De nervorum regeneratione, Berlin, 1838.

§ Nasse; Müller's Archiv, 1839, p. 405. See also the same, 1840, p. 270.

ous tissue ever take place, excepting, perhaps, in very young individuals, where the *type-power* of the tissue still persists to a sufficient extent.*

V. Cartilaginous and Osseous Epigeneses.

Here we touch upon the abnormal relations of tissues of less rank and physiological dignity, and we shall find them much more clearly marked and unequivocal in their nature. And although from their intimate connection I have included them under a single head, yet we shall best look at them separately.

1. *The Cartilage Epigenesis.*—For the first description of this product in an intelligible manner we are indebted to Müller.† From both its chemical and microscopical peculiarities he gave it the very proper name of *enchondroma.* It is a true epigenesis, and consists of a product exactly resembling normal cartilage of the transient form. By both Müller and subsequent writers, it has been described as taking its origin both upon bone and in soft parts quite distinctly separated from it.

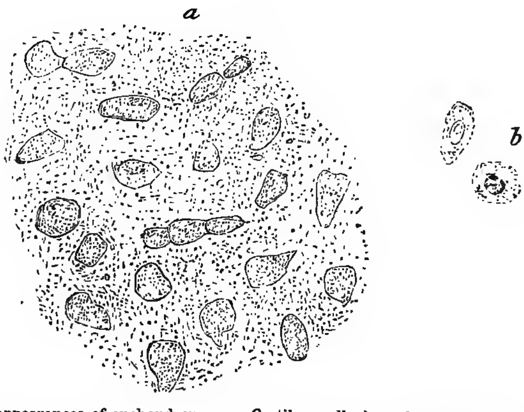
Regarding it, as I do, as a true epigenesis, I cannot consider that it ever originates except in contiguity with its analogous tissues. For this reason, I am inclined to distrust the statements of those authors who allege to have met with it in the interior of tumours and other soft tissues. The fact that, in almost every case of this kind, the evidence that the tissue in question was cartilaginous, was based upon a gross, and not upon a microscopic examination; this, I say, would be sufficient to lead me to distrust their conclusions, even was there no histological point at stake. Much less, therefore, should I be willing to accept any such assertions to be true, when, in so doing, I should be introducing a new feature into our views of the relations and powers of tissues; for, as I have remarked on a preceding page, the declaration that a new individual tissue can arise *per se* in the midst of tissues of a dissimilar nature, is pretty much the same as asserting the doctrine of *spontaneous generation.*

* At the end of these remarks upon the epigenetic relations of the muscular and nervous tissues, we might properly take up the consideration of the subject of the regeneration of lost parts in the lower animals. The relations of this process and that leading to epigeneses in general are somewhat similar; but then again they are different, the one (epigeneses) being due to an *asthenic,* while the other (regeneration) to a *sthenic* condition of the type-power of tissues.

† Müller; Ueber den feineren Bau der Geschwulste, Berlin, 1838, pp. 31–49; or West's translation.

The number of even all the alleged cases of this kind is quite small, and a very small proportion of these were examined by the microscope.* In these few cases, I am inclined to the opinion that, as is often true of encysted tumours, they had, originally, a connection with the analogous tissue; but which growth, and change of form and size, had finally quite obscured. As usually occurring, its favourite locality is about the joints of the hands, and it may be said without exaggeration that nine out of ten of all the cases recorded have been upon these parts. Vogel† makes a distinction between *cartilaginous exostosis* and *true enchondroma*, and thinks that true enchondromata do not ossify, they being the fibrous form of cartilage. My own experience does not warrant this distinction, and as yet we seem quite unacquainted with the conditions leading to ossification in these products. Removed from the body, the incised surface presents a smooth, semitranslucent, turnip-like aspect. Under the microscope, appearances usually quite like those of normal cartilage are seen; a granular or semi-fibrous stroma, in which lie cells which are nucleated and irregular in aspect.

Fig. 46.



Microscopical appearances of enchondroma. *a*. Cartilage-cells in a finely granular stroma. *b*. Cells treated with acid, showing nucleated character. From specimen taken from the hand.

Ossification here takes place, as in normal cartilage, by the deposition of calcareous matter in the stroma or cells. Very rarely,

* As far as I am aware, this remark is true of those of Müller and Bennett only. See references at the end of this section. The pamphlet of Mr. Gamjee, ("On a Case of Ossifying Enchondroma in the Testicle of a Horse,") quoted by Mr. Paget (Lect. in London Med. Gaz. Aug. 1851), I have not seen.

† Vogel; *loc. citat.* Amer. edit. p. 211.

however, does it proceed either very regularly or extensively, and in some few of these growths, simple osseous spiculæ are only found.

In nearly every instance, the cause of this form of epigenesis is traumatic, being due to an external injury. But in a case I saw some time ago, and in another, since reported (see that of Cobbald), the disease seemed referable to constitutional causes.

The passage of these tumours into those of a fibroid nature sometimes occurs, by the disappearance of the true cartilage element—the cell. The tumour is then simply a collection of fibrous tissue; but its practical relations are not thereby changed in the least. The opposite change, that of fibrous tumours into enchondromata, I cannot believe ever occurs; because that presupposes that the cartilage element is added spontaneously.

However, all these points have not been well studied; for the microscope has not been made to bear upon them, and we must look to future inquiries to make them more clear.*

2. *The Osseous Epigenesis.*—Pursuant to the doctrines I have inculcated on a preceding page—the invariable origin of bone in cartilage—it is evident that true osseous tumours are only a farther development of those we have just considered. And if cartilaginous tumours are rare, those in which ossification has taken place are rarer still.

It is quite important to correctly distinguish between false and true osseous growths; for the former are generally confounded with the latter; they are not uncommon, and are only calcareous deposits occurring almost anywhere, being due to the interstitial deposition of earthy salts in a fibrous tumour, or some inflammatory product. Such are many forms met with in fibrous membranes

* For farther details, together with some cases, see an article of mine, The Microscopical and Histological Relations of Enchondroma, Amer. Journ. Med. Sci. n. s. vol. xxiii. p. 397. The following is the bibliography of cartilaginous epigenesis, or the so-called *enchondroma.* I have here included *none* of those cases which might well be regarded doubtful: Müller; Ueber den feineren Bau der Geschwulste, Berlin, 1838, pp. 31–49. Glüge; Anat. Physiol. Untersuch. zur Pathol. Minden et Jena, 1838–41, ii. 153–87. Cruveilhier; Anat. Path. du Corps Humain, Paris, 1842, livr. 34, pl. 4–5. Frogley; Med.-Chir. Trans. 1843, vol. xxvi. p. 133. Goodsir; Lond. and Edinb. Month. Journ. Feb. 1843. Hertz; De Enchondromate, Erlangen, 1843. Lebert; Physiol. pathol. &c. Paris, 1845, tom. ii. p. 213. Ancell; Med.-Chir. Trans. vol. xxv. art. xv. Adams; London Lancet, 1847, vol. ii. p. 463. Bennett; On Cancer and Cancroid Growths, Edinb. 1849, Obser. 48–49. Fergusson; London Lancet, 1850, vol. i. p. 455. Lloyd; London Lancet, Dec. 1850. Kingdon; Lond. Med. Gaz. Sept. 5, 1851; also, Cobbald (the case apparently hereditary), Ed. Month. Journ. Aug. 1851.

everywhere, in tendons in the coats of bloodvessels, and in the interior of various tumours.

But the true osseous substance is of course found only in connection with normal bone, although it possesses all the elements of normal bone; yet, generally, their structure wants that regularity and definiteness of arrangement found in normal bone. More than this, it has no histological peculiarities, and its clinical and practical relations are the same as those of enchondromata, from which they always arise.*

VI. Fibroid Epigeneses.

We have come upon the lowest class of epigeneses occurring in the economy. Under the term *fibroid*, I include not only *fibrous*, but *fibrinous*, *fibro-plastic*, and all other allied forms which have received many and various names.

For convenience sake, a division into several varieties, as authors have done, may be necessary; but histologically, the case is different, for they are composed of a single and the same element, although existing under some variety of form.

The frequency of their occurrence, and the wide range of their localities, is in perfect keeping with the commonness and universal distribution of this tissue.

If our subject was the fibrous growths only, we might pass it by with a very few remarks, and yet do it justice; for, both in their histological composition and in their clinical and practical relations, they have the most indifferent peculiarities. It is true that they have favourite localities, such as the uterus, the nasal fossæ, &c. and also that they sometimes seem to possess a kind of individuality; but generally they have none other than the most simple vegetative relations. To this point, however, I shall revert hereafter.

By *fibrinous* products, I mean those due to the extravasation of

* The literature of osseous tumours is much less extensive and full than that of those of cartilage. Many remarkable cases may be found scattered through journals and periodicals. Something, however, may be learned of them in the following works: Gluge; Atlas der Path. Anat. pt. ii. Müller; Ueber ossificirende Schwämme oder osteoid Geschwulste, &c. Müller's Woch. heft v. pp. 396–442. Ducrest; Mém de la Soc. Méd. d'Obser. de Paris, t. ii. pp. 318–452. Lebert; Phys. Pathologique, &c. Paris, 1845, tom. ii. pp. 219–241. Vogel; *loc. citat.* Amer. edit. pp. 185 and 212. I do not here refer to the literature of the older and well-known authors, both because they are well known, and because their cases, although seemingly true ones, were not proved to be positively such by microscopic analysis.

fibrine from the bloodvessels, which afterwards becomes firm and undergoes changes, so that it is nourished and perhaps increased in the system as an analogous product. Being therefore due to traumatic causes, an extravasation rather than an exudation, it is scarcely an epigenesis in the true sense of that term. Velpeau,* in his brilliant exposition of the phenomena of contusion, called the attention of surgeons to this subject, upon which he placed the highest importance, regarding, as he did, this sanguineous effusion as the matrix of all, or nearly all, the fibroid growths in the body. I cannot here enter into an analysis of the points of this somewhat celebrated theory. It holds true in surgery much more than medicine; that is, with tumours due to direct external causes, rather than with those of a latent and constitutional origin.

The plasma of the blood, together with its colouring matter, is extravasated, and instead of being absorbed, remains as such. The colouring matter disappears, and a kind of semi-organization takes place in the product. A fibrillated tissue is the result, inclosing imperfect globules together with granules. Histologically speaking, therefore, they are of the lowest character, and I have thus briefly alluded to them, to avoid the appearance of omission in the line of my discourse.

The remaining form of fibroid epigenesis, the fibro-plastic or sarcomatous tumours, is of more importance, both histologically and practically.

Until the work of Lebert,† these tumours received a variety of names, according to the different gross aspects they presented.‡ But Lebert gave them this definite and appropriate name from a microscopical study of their elements.

Their elements are uniformly the same, and in a general classification may well serve as the basis of a distinct name. Nevertheless, in a true histological point of view, the elements are the same as those of fibrous tumours, the peculiarity of the tissue being due to the stage of development they have attained. On this account, I have included these epigeneses under the term of *fibroid* products. The histological peculiarities of these tumours may be briefly stated to be those of the fibrous form, resting on their embryonic stage of development. Therefore, instead of long slender fibres, matted to-

* Velpeau; De la contusion dans tous les organs, Thèse de concours, 1833.

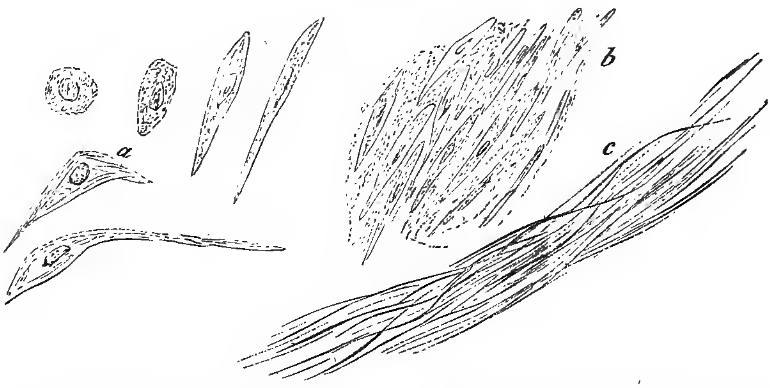
† Lebert; Phys. pathol. &c. Paris, 1845, tom. ii. pp. 120–160.

‡ Among these names may be mentioned Sarcoma, Steatoma, Osteo-sarcoma, Osteo-steatoma, besides, Fibrous Tumour, &c.

gether, forming the fibrous tumour, there is found a tissue made up of fibres and cells in all stages of transition, into fibres which are elongated, fusiform, &c. In the younger forms, the cells greatly predominate, but in the older ones, the fibrous tissue constitutes a large ingredient.

These cells are well formed, nucleated, and closely resemble those of epithelium. And, in many cases, where this epigenesis occurs in glandular organs, it is so combined with epithelial products, as to leave a doubt in the mind which was the original epigenesis.

Fig. 47.



Example of the microscopical structure of fibro-plastic epigeneses, from a "sarcomatous" tumour of the submaxillary gland. *a*. Cells changing in part to fibres, but not losing their cell character. *b*. The same bound together in a regular manner, as sometimes found. *c*. Fibrillated tissue accompanying the cell-growths.

The microscopic elements, then, of these fibro-plastic tumours may be stated to be cells, fibres, and granules; the fibres being the product of the cells, and the cells being of an epitheloid type. My own experience with these growths has been upon those springing from some glandular organ; and in all these cases, there was involved, to a variable extent, a morbid epithelial product. And although there is reason to believe that the primitive epigenesis in these cases is in the fibrous tissue of the gland, yet it afterward necessarily includes the epithelial structure. Judging, therefore, from the *result* we have at last, I should say that the definition of fibro-plastic tumours, as we usually meet with them, is *a fibrous and epithelial epigenesis combined*. This also will convey a good idea of the microscopic elements usually met with. However, in some that do not involve any epithelial element, the whole tissue is composed of fusiform and fibre-like cells. As to the localities of appearance, these tumours occur throughout the economy; but, as I have before said, they are generally connected with glandular tissues—such are the lymphatic, parotid, maxillary, and mammary glands. In almost every instance I have examined, the locality was one of these parts. According to the experience of Lebert, however, they are not unfrequently connected with the skin, and with mucous and serous membranes. The practical relations of these tumours are the same as those we have just spoken of. They are, of course, perfectly innocent in their nature and tendency, and the aid of the surgeon is required to check their rapid growth and consequent mechanical inconvenience; when once *entirely* removed, there is no liability to a return. In fact, I may say that their rapid increase is nearly the only reason for their being objects of especial regard, for they rise and increase generally without pain, and do not involve constitutional disturbances.

In closing these remarks upon *fibroid epigenesis*, it may be proper for me to allude briefly to a collateral point. This is, whether these fibroid tumours ever degenerate into cancer. With many of the elder surgeons and surgical writers it has been a favourite opinion that a tumour may be innocent at first, but afterwards degenerate and become malignant or cancerous, and they think that the fibroid tumours are those in which such changes usually take place. If we are to judge of tumours and their peculiar nature by their clinical history alone, such an opinion might be considered correct. But I need scarcely add that the opinion now is, that the soundest and most complete experience with these growths does not justify any view except that of the singleness of their nature throughout the whole period of their existence; that is, a cancer is a cancer from the beginning, and the same is true of other growths. I will add that, since the microscope has been made to bear upon these points, I am not aware that there is any evidence that such metamorphoses take place. If a tumour, originally considered innocent, afterwards proves itself cancerous, it is far more probable that it was cancerous from the beginning than that it afterwards became so. From the scheme of the origin of pathological products I have laid down on a preceding page, it will be remembered that cancer was regarded as a heteromorphous, and fibrous tumours as homœomorphous products, due in fact to quite different abnormal phases of the nutrition. On this account, it would, *à priori*, be difficult to conceive of their occurring together; from this reason alone, aside from the sufficiently auxiliary ones we have in experience, I should say that the hypothesis of the degeneration of fibrous growths into cancer is contradicted by the best and clearest views we possess of these abnormal tissues.*

VII. Adipose Epigeneses.

Under this head, I do not refer to the abnormal (?) increase and development of the adipose tissue generally, constituting obesity or corpulency; for, in most respects, this is to be regarded as merely a hypertrophy. On the other hand, I refer to the local appearance of the fatty tissue under circumstances and forms justly intimating that it is due to a perverted nutrition. Such are the so-called fatty tumours, and other adventitious fatty growths in various parts. A careful distinction must also be made between these true histological products and those due to a simple deposit of fat in the shape of globules in various tissues. The adipose tissue is as truly an individual one as any we have considered, and although of a common nature, and low physiological dignity, we are to look for the same rigid histological relations as in the others. True fatty tumours, therefore, are always epigenetic, and, when containing the true fat-cell, they invariably take their origin from the normal tissue. Their microscopical structure* so closely resembles that from which they spring, that they appear to be only a hypertrophy of the healthy tissue. In these cases I invariably found fat-cells imbedded in a fibrous stroma. The relative quantity of each of these elements varies, and in this fact may be found the reason of the variety of forms given by writers. Thus Müller‡ speaks of these products under two species, *Lipoma* and *Cholesteatoma*, and under each of these gives several varieties. But, I repeat, you cannot apply the scientific term *species* to pathological formations.

Common fatty tumours, taking their origin beneath the integu-

* Owing to the commonness of their occurrence, the literature of fibroid epigeneses is quite full. I cannot here even think of referring to the elder surgical writers who have written upon this subject. Below is a considerable portion of the literature of the subject, since it has been studied histologically and microscopically.

Gluge, *loc. citat.* part iv. Fasergeschwulste; Valentin, Repertorium, 1837, p. 270; Müller, Uber den feineren Bau und die Formen du Krankhaften Geschwulste, or West's transl. pl. iii. figs. 14, 15; Amussat, Mém. sur. l'Anat. Path. des Tumeurs fibreuses de Uterus, Paris, 1842; Berard, Diagnostie différential des Tumeurs du sein, Paris, 1842; Lebert, Physiol. path. &c. Paris, 1845, tom. ii. pp. 83, 120; Vogel, *loc. citat.* Amer. ed. p. 199; Bennett, On Cancer and Cancroid Growths, Edinb. 149.

† I give no example of the microscopical structure of the adipose epigenesis, for, in nearly all respects, it is the same as the normal tissue.

‡ Müller; *loc. cit.* West's transl. p. 152.

ment, not unfrequently lobulated, and known generally by the name of *lipomata*, present a pretty uniform aspect. But then, again, they may vary according to their locality, and to the relative amount of the different elements their formation may involve. In some instances they are so much fibrous that Abernethy* was led to conclude that oftentimes they were only fibrous tumours that had absorbed fatty elements. This opinion is of course incorrect, although from clinical studies and gross examinations it might seem to be borne out.

The direct causes of these growths are equally as obscure as those of any other morbid products we have considered. They are sometimes congenital, and may occur at any age, or in any part of the body. A very common locality is the subcutaneous tissue of the back and shoulders, and, according to statistics, they are more common in females than in males. The fact that they not unfrequently arise simultaneously in different parts of the body, and this, too, without apparent cause, seems to indicate, not only that their ulterior causes may be constitutional, but also that we are far from being acquainted with their general etiology. They are, of course, non-malignant in themselves, but life may be endangered from the enormous size they sometimes attain. Like all homœomorphous products, they are liable to return if care is not taken that their matrix is entirely removed. Fatty degeneration of, and fatty deposition in organs, both internal and external, have nothing to do with the subject in question. They are not only histologically different, but are due to other causes.†

With these remarks, I close the subject of special organized pathological products. It is true that, in so doing, I omit a notice of diseased forms, which have been described under distinct names in various works. Such, for instance, are the melanotic and colloid products. I have not spoken of the first of these, because they seem to be only other growths of both an heteromorphous and homœomorphous character, of which pigment-matter forms an ingredient. Judging from my own studies they have no other peculiarity, and do not deserve a distinct name except for convenience sake. As for the

* Abernethy; Surgical Observations, London, 1814, p. 9.

† The literature of this form of epigenesis is equally as loose as that of the others we have considered. As histological examinations of tumours are of recent origin, it is difficult to ascertain the true nature of many growths spoken of by the older writers. The following are some of those who have studied the subject microscopically: Müller, *loc. cit.* p. 152, *et seq.;* Gluge; Abhandlungen zur Physiol. und Path. 1839, p. 130, 1841, p. 185; Heyfelder, De lipomate et steatomate, Leyden, 1842; Lebert, *loc. cit.* tom. ii. p. 100, *et seq.*; Bennett, On Cancer and Cancroid Growths, Edinburgh, 1849.

colloid products, they may be regarded in a somewhat analogous light; for the most part they seem to be cancerous formations, containing an unusual quantity of albuminous and gelatinous material. My own experience of several cases is to this effect alone, and I know of no reason for the opinion that the *colloid* should constitute a distinct characteristic rather than an accidental one.

The domain of pathology is so extended, that it may well be prudent for me that I do not enter upon the discussion of points not directly relative to the subject in question. The part which cells play in pathological products is certainly very extensive; they may be said to constitute the chief, but not the sole actors; and, therefore, in treating of the subject of pathological products, as far only as appertains to their peculiarities and parts, I necessarily treat it quite imperfectly; but, at the same time, not necessarily indistinctly or obscurely, for these studies lie at the very foundation of the whole structure.

The distinct subject of the pathological relations of the blood, I have not touched upon, although they are of the most important and fundamental character. Its imperfect condition at the present time would necessarily preclude any special detail of scientific value. There are very many difficulties in the way, which must be removed before microscopic examinations of the blood will furnish, of themselves alone, positive data in the determination of disease. It is evident that by the microscope we can appreciate only two abnormal modifications of this liquid; the first, is imperfections in the structure of the red and white corpuscles, and the second, the absence of their normal mutual numerical relations; in other words, variations in the structure and healthy proportions of its solid component parts. But restricted as the field thus is, I am confident that it will yield much upon careful study and inquiry. My own observations have shown me that the microscope may indicate blood changes long before they would be suspected from the general aspect of the individual. It might, indeed, seem a fair question, how far blood extracted from the end of the finger would be a fair sample of that of the system generally; but it is much more so than would at first be supposed, as I have had reason to believe from continued experiments. However, in such examinations, the blood should be taken at different times in the day, and always observed without the use of water. It is to be hoped that this subject will receive the careful attention it deserves.

The department of *unorganized epigeneses* must likewise be omitted. It contains many interesting chapters, and one of them, not the least so, is that relating to the *urine*; and this is so, not only because this excretion is a good exponent of the chemico-vital condition of the system generally, but because its peculiarities can be most beautifully made out by the aid of the microscope. But I will not dwell farther upon these inviting topics.

Pathology, as manifested in cell-studies, we have seen to be not an entirely distinct, but only an erring, fallen condition of physiology. A thorough appreciation of this conclusion is of more value than might be supposed. It is true that these pathological cells often have the exact physical semblance of those of health, but if their imperfection is not traceable in their physical organization, it is manifested in their meaningless destiny. More than this we cannot well insist upon if we remain in the domain of strict science.

PART III.

PHILOSOPHY.

THE PHILOSOPHICAL RELATIONS OF CELL-STUDIES.

Most of the philosophical relations of cell-studies may, I think, be comprised under two heads, viz: 1. The relations of cells to the *physical* or the *teleological* view of organization; 2. The direct agency of cells in the production and manifestation of nervous power, the intellectual processes, &c. I propose to consider each of these separately, and at some length.

I. The Relations of Cells to the Physical and to the Teleological View of Organization.

The cell is the fundamental unity of organized forms. It is the chosen material form through which organization gains its first expression. To use a metaphor, it is the only altar on which life and matter are married, to pursue together a common and definite end.

These premises being true, as all modern inquiry clearly proves, we can justly seek, from a study of this cell, a manifestation of all the conditions of organization, as far as they are capable of being made evident through material forms. We can also expect, by a consideration of these conditions, to be able to comprehend the higher and ulterior relations of organization as occurring in permanent individual forms.

Two dissimilar views of the real nature of organization have been urged in modern times.

That the bearings of the subject in question may be fully understood, a brief description of both of these views is here required. The first is, that every individual organism exists in virtue of a predominant idea; this idea, therefore, preceded the organism, of which this last is the true material manifestation. Organisms are, therefore, if I may so express myself, the incarnations of pre-existent ideas in nature. An organized form is simply a colligation of molecules of plastic matter occurring under the direction of a determinate idea or force. This force must, it is evident, be peculiar, and differing from any belonging to matter generally. It is an immaterial one, and can be likened to no other in nature with which we are acquainted. In its conception we should not confound it with our idea of life; for between them a broad distinction can be drawn.

Thus our idea of life is necessarily connected with, and dependent upon, that of organization; because, from the limitations of experience, we have not and can not conceive of it as a distinct and separate entity. On the other hand, our conception of this determinate idea, or that of the *individuality* of organization, although involving some of the same data as that of life, occurs from relations sufficiently dissimilar to show well its distinction. Thus, while our idea of life is only *coexistent* with that of organization, our idea of this individuality is not only *coexistent*, but necessarily pre-existent to it. We do not comprehend life except in conjunction with matter under peculiar forms; on the other hand, we conceive of this determinate idea or individuality as existing before a trace of organization appears.

This view of organization, which widely separates the organic from the inorganic world, as we can comprehend each through the expression of its forces, which gives an inherent dignity to individual existence, because there is always an end in view—this view of organization, I repeat, is called the *teleological* one.

The other and second view is fundamentally different. Adopting it, one does not recognize that the forces of organized are more in number or different in character from those of unorganized matter, the fact of organization being due to a certain combination of powers possessed by all material forms. And when this combination has once taken place, there necessarily results in virtue of it, and the forces impressed on matter in its beginning, a certain end, which is called individual existence. The common phenomena of organization, therefore, are due to the blind working of the laws of necessity, and which are irrespective of any purpose; they also involve no conditions excepting those properly belonging to chemistry and physics.

According to this view also, the so-called "individual adaptation" does not result from a determinate idea seeking an expression, but rather ensues from a combination of blind forces which the Deity impressed upon matter in the beginning. Design in nature, therefore, would be only another set of terms for necessity of physical condition.

Again, when we see the habits and conformation of an animal *suited* to the circumstances under which it is found, we are not to attribute this to an idea pre-existing before all, and seeking its material expression, but are to regard the whole in the light of a necessity; for the very fact of the existing state of conditions implies that, were they different, the animal would not exist, and therefore the very data we reason from determine the idea of necessity, as far as justly applicable to works of an Almighty power.

This view of nature, which ranges all its phenomena under the domain of physical forces, thus giving, in one sense, the same dignity to inorganic as to organic forms—this view of organization, I repeat, is called the *physical* one.

In this brief description of these two widely dissimilar views of organization, I may have failed to express clearly the grounds of their distinction; for nothing is more difficult than the successful expression of the exact definitions of mere modalities of matter. In a laconic and perhaps comprehensive form, I would say that the *teleological* view is the full definition of *Development*, while the *physical* view is the equally full definition of *Combination*.

We will now turn to the arguments for and against each of these views, at least as far as dependent upon cell-studies.

The first question is, does the adaptation perceived in the organic differ in essential character from that perceived in the inorganic world? An affirmative answer must, I think, be given to this question. The adaptation seen in the inorganic world has reference only to existing circumstances, and the only surety of its continuation is in the persistence of the forces on which it depends. Such, for instance, is that of the planetary system. It is properly called an adaptation only in virtue of the existing harmony found. But this harmony, however perfect, indicates no definite end, which is the final cause of its existence.

On the other hand, with organic nature, other and different relations are seen. For, besides the adaptation just spoken of, and which conduces to its general harmony with matter, there is superadded an *individual* adaptation of the whole to an end seemingly in view from the beginning. Indeed, we can truly say, with Kant, that this end constitutes the very definition of an organized form.

In all our investigations into the structure of animals and plants, we involuntarily (in one sense) proceed upon the principle that *nothing was made in vain.* If, at any time, we have even a misgiving of the whole truth of this principle, our scientific ardor, as hopeful of real results, is immediately relaxed, not to say harmed. This involuntary admission of a doctrine, shows how secretly yet how firmly is our mind, in its study of natural phenomena, linked with the *teleological* view of organization.* We cannot free ourselves from it, any more than we can free ourselves from the silent yet constant admission of the truth of the great physical doctrine that *nothing comes by chance.* This question, therefore, which I have put, might well seem not only superfluous, but highly unphilosophical. But I was led to commence with it from the fact that a very sagacious mind† thinks it correctly answered in the negative. We will, however, pursue this subject a little farther. I have said that the adaptation of the inorganic world has reference to existing circumstances only, while that of the organic world, beside being individual, has always reference to events or contingencies of the future. This point is well illustrated in the phenomena of reproduction.

When animals were first created, the footing on which they commenced life was the same in each. But why, accepting the *physical* view of organization, were their powers of reproduction formed exactly in accordance with their probabilities and liabilities of existence, as all zoological experience constantly shows is true? It is the characteristic of physical forces that they act upon the conditions of the time, and no physical force can act on the probability of future contingencies. The amount of animal and vegetable life on the earth's surface, in localities not disturbed by human or other agencies of an accidental nature, has not changed for an indefinite period of time. In those places the mutual relations of animals and vegetables exist now as we have reason to believe they did in the beginning.

There is no evidence that there has hitherto been a constant oscillation, and that the present rest and harmony is one ultimately and necessarily gained. Now in all these animals and plants, we find their reproductive powers, the capacity of their organs of generation, exactly in accordance with their liabilities to destruction. Were this

* I think it may be truly said that the strength and ever-constant presence of this principle in the minds of great naturalists, has been the grand secret of their success. To prove that this is so, the instances of many notable men might be adduced. In the case of Cuvier this was pre-eminently true, and it served as an ever-faithful intuition to open to him domains of knowledge hitherto untrodden. There is another naturalist now living, whose splendid labours, as I know personally, have been based upon this same principle.

† Schwann; Microskop. Untersuch. &c., or Sydenham Soc. ed. p. 187.

the place, the truth of this remark might be illustrated by many special instances. But it is evident that, by this almost prescient adaptation, the numerical relations of species are preserved unchanged; and those who are familiar with the leading results of zoological research at the present day, can scarcely entertain this subject in the light of a question.

The next point of discussion is, whether, admitting organization to exist always in virtue of *determinate ideas*, it is not unphysical and unphilosophical to assert, as we must, that these ideas or peculiar forces exist previous to the material forms in which they are ultimately expressed? This question is so specious that at first an affirmative answer appears to be the only correct one. But a little consideration will, I think, show its error. Our knowledge of physical phenomena is derivable from observation only; and when points are urged upon us which we have not observed, and which also do not seem compatible with hitherto existing observation, they are liable to be pronounced unphysical. But the history of any of the inductive sciences shows that the singularity of any hypothesis or opinion concerning physical phenomena is no *à priori* argument against its truth. This remark is aptly true of the present case. The hypothesis that matter should have determinate ideas, not expressed by physical phenomena at the time, but which are latent, and to be developed under special forms in future, this hypothesis, I repeat, should not be rejected on the ground alone of its singularity; and much more will be admissible if its singularity can be disproved. This disproval may be found, I think, in the numerous analogous phenomena of generation. Here we are driven to the admission of the existence of forces, long before the formation of the material organs in which they ultimately find their expression.

Take, for instance, the *cicatricula* of a robin, and that also of a hawk; physically, they resemble each other exactly, and no microscopical analysis can detect between them any material difference. Yet no one will pretend to deny that in the one lies concealed all that belonging to a robin, and in the other all that which belongs to a hawk. This will bear no other interpretation. Here you have very dissimilar powers and forces contained in apparently identical material forms; and unless we declare that these different powers and forces existed before and aside from the material form, instead of gradually accruing to it as a particular character was assumed, we shall be at a loss to account for the certainty and uniformity of result in every case. Again, the primordial material condition of every animal is a nucleated cell, which in every case has exactly the same physical aspect. Now in its growth to the perfect form, the new material constantly added is extraneous, and there would appear no reason why it should always suit the conditions of the case, were there not an underlying pre-existing idea. Indeed, we might expect an occasional blending of dissimilar forms. But this never occurs, and few points connected with organization have been more positively determined than the distinct, unchanging individuality of animal and vegetable types.

If this point needs farther illustration, or even that which is better, we can find such in the male phenomena of generation. It may be well to discuss briefly a few of these points.

Within a few years, three prominent facts have been satisfactorily established concerning the function of the spermatic particles. These are, 1st. That they are the only fecundating particles of the semen. 2d. That a few of these particles are sufficient to fecundate a single ovum. 3d. That the physical phenomena of fecundation are those of simple contact alone, of the spermatic particles with the ovum; there being no material loss on the part of the former.

We know very well how important and active a part the male plays in the production of the new being, and I scarcely need add that the offspring partakes equally of the physical peculiarities of both parents. But in no way can it receive those of the father except through the medium of the spermatic particles. And so, however strange it may appear at first, yet the conclusion is irresistible, that a single spermatic particle must contain, concealed within it, not only the general physical peculiarities of the father, but mental dispositions also, and, as is too often true of our own species, morbid taints superadded to all.

The third and last point in the discussion of this subject, is the objection to the teleological view of organization urged by Schwann,* and that, too, concerning the general phenomena of the subject we have just left—generation. I will quote the entire passage, that there may be no misapprehension of the idea intended to be conveyed. He says: "If we assume each organism to be formed by a power which acts according to a certain predominant idea, a portion of this power may certainly reside in the ovum during generation; but then we must ascribe to this subdivision of the original power, at the separation of the ovum from the body of the mother, the capability of producing an organism similar to that which the power, of which

* *Loc. citat.* Syd. Soc. 2d, p. 189.

it is but a portion, produced; that is, we must assume that this power is infinitely divisible, and yet that each part may perform the same actions as the whole power. If, on the other hand, the power of organized bodies resides, like the physical powers, in matter as such, and be set free only by a certain combination of molecules, as, for instance, electricity is set free by the combination of a zinc and copper plate, then also by the conjunction of molecules to form an ovum, the power may be set free by which the ovum is capable of appropriating to itself fresh molecules, and these newly-conjoined molecules again by this mode of combination acquire the same power to assimilate fresh molecules." Schwann is here evidently urging the doctrine of catalysis in organic forms. This same doctrine has lately been pushed to its limit, for similar reasons, by Mulder.*

And thus it would appear that some, in the study of organic phenomena, are willing, rather than appear what they call "unphysical," to summon to their aid the blindest physical forces, instead of admitting the existence of those which are special and distinct, and constantly urged upon them. The prominent objection Schwann urges, is the physical absurdity that the division of a power or force does not decrease it.

But this is falling back upon the properties of physical forms only, in which the mathematical axiom, that *a part is less than the whole*, is true. But it is the peculiarity of immaterial agents that they cannot be subject to the same definitions as those of a material nature. We have yet to escape from a strange confusion of ideas on these points, and, when considering these higher forces, we must not tacitly deny their higher and immaterial character by subjecting them to material definitions. For on these grounds, the very existence of that of which we feel most certain, namely, *thought*, might well be denied. In fact, I think many of us have yet to learn that the mathematical axiom above quoted holds true of the immaterial agents.

Perhaps this remark is true even of electricity, for in the phenomena of *catalysis*, we witness the transference of force or quality without an apparent loss from the body, from which the transference takes place.† To urge that only that which is substantial can be

* The Chemistry of Vegetable and Animal Physiology, Eng. trans., Edinb. 1849, pp. 28, *et seq.*

† In regard to this point, Mulder (*Loc. cit.*) takes a different view. He thinks that in the case of a magnet magnetizing a piece of iron, there is no *transmission* of force, but simply an awakening of slumbering forces in the iron. It might well be asked if

transferred, is to reason on the ground that a phenomenon in nature is impossible because it eludes our observation and analysis.

In the phenomena of generation, we certainly have proved to us that there may be a division of power or quality without a real loss; in other words, that there may be an indefinite number of divisions, and yet each of these possess the capability of the whole from which the division was taken or made.

Thus, as I have before said, the spermatic particle is the potential representative of the whole male being. It contains within it the totality of the peculiarities of the individual in which it has been formed. These spermatic particles are constantly formed, and, in the case of any of the higher animals, literally millions are yearly evolved; yet each possesses the powers of the other, and of the whole animal; and the animal certainly loses none of its individuality by such a constant division.

The objections of Schwann to the teleological view of organization, are, therefore, more specious than valid, because the reality of the very conditions objected to has an expression in quite common phenomena.

These same teleological views might be shown to be almost imperatively forced upon us by a general review of developments imperfectly attained—the subject of monstrosities. Here you not unfrequently have conditions of organization, in which imperfections of physical form reveal distinctly the perfection of the underlying determinate idea of organization.

In special cell studies connected with the subject of generation, however, as we have seen, these doctrines of teleology find their most unequivocal support.*

this makes the matter any more clear? You are obliged, on this supposition, to admit the existence of *latent forces* in the iron, and this is "unphysical," according to his own admission. Mulder says: "A weak force is strengthened; it is impossible to imagine the transference of a force from one material mass to another." But, is it not truly "unphysical" to suppose that a weak force can be strengthened except by the *addition* of new force?

* It may be urged that in all cases it conduces much more to the object of science to strive, at least, to adopt the *physical* instead of the *teleological* explanation of the phenomena of organic life. This is so because it leads us in our investigations rigorously to exclude the admission of those various hypothetical forces and conditions, which ultimately serve as a universal refuge in the instances of inexplicable phenomena. On the other hand, if we seek to base all phenomena on physical grounds, we tend to exclude the admission of conditions not having their corresponding expression in physical forms. This objection, therefore, rests rather upon our liability to error in interpreting natural phenomena, than upon real grounds. It must be regarded in the

In conclusion, I think it may be truly said that one of the tendencies of the most intimate physiological, microscopical studies of the present day, is to show the inadequacy of microscopic and chemical investigations to detect physical differences in structures essentially dissimilar. Once appreciating this fully, we are prepared for the ulterior doctrine that, in organization, matter is but the vehicle for the phenomenal manifestation of determinate ideas, which last only, therefore, have an objective reality.

II. The Direct Agency of Cells in the Production and Manifestation of Nervous Power, the Intellectual Processes, &c.

The subject which we have just discussed—that of a determinate teleological idea running through each organized form—is certainly one of a very high and important character, for, upon its admission, depends not only that of the doctrine of Final causes, but also many of our highest and grandest conceptions of nature as the work of an Almighty hand.

However this may be, it does not exceed in importance or magnitude the one which is now before us; for this last involves the question of the relations of the Thinking principle itself.

On a preceding page, when discussing in a general manner the subject of nerve-cell function, I briefly alluded to the present point, but I purposely deferred taking it up in a more complete manner, until the present time.

The point in question has nothing to do with the nature of the immortal Psyche—the immaterial mind; these are, by a wise Providence, beyond the pale of all scientific discussion. Our business is a humble inquiry into the conditions of the evolution of nervous power, and the manifestation of intellect, as referable to cell life and action.

In the first place, it cannot be denied that all nervous and mental phenomena are, in some way, directly connected with vital changes in nervous matter; this nervous matter is the nerve-cells which, exclusively, compose the potential part of the nervous substance. I would repeat, therefore, that whenever there is a manifestation of nervous power or intellect, its physical source lies with nerve-cells or their analogues, whether it be high or low in the animal kingdom.

light of a necessary *caveat* connected with the fallibility of our intellect. There is, however, as we have seen, a danger that, in endeavouring to be strictly *physical*, we may become highly "unphysical," not to say unphilosophical.

Another point, equally well sustained, is, that there is a direct correlation between the capacity for such manifestation, and the physical amount of these nervous cells. So far the subject is clear. It next remains for us to inquire if there are sufficient grounds for the supposition that there is a difference in the quality of the forces resulting *after* (I do not say *from*) the action of these cells; in other words, is all nervous manifestation the same in essential character, the apparent difference being due to mere form of its expression or conditions of its mode of action? As a purely physiological question, I should say that the tendency of all our best studies is to answer it in the negative. It is true that nerve-cells, wherever found, have exactly the same physical appearance, but then the wide difference of their capacities cannot be well referable to merely different forms of the same essential function. I would, therefore, make a distinction between that portion of the nervous substance which is connected with the individual, voluntary life of the being, and the remaining portion, which is subservient to the mere economical preservation of the life of the animal. The action of the former ends in what is called intellectual manifestation; and that of the latter in the evolution of simple nervous force. Now, with the application of the usual physiological criteria, we may regard the nervous force as a power *sui generis* which belongs to the nervous cells in virtue of their constitution, exactly as belongs muscular force to muscle. This force is the resultant functional effect of these cells, as vital, ever-changing parts. Its evolution, therefore, must be attended with organic changes in these cells; which changes are included in that cycle of actions—waste and repair—which aggregately constitute their life. Whatever causes, under the form of stimuli, induce an increased functional activity of these cells, this increased action is followed by an evolution of a larger amount of nervous power.

The relations, therefore, of nerve-cells to nervous power or force seem to be, potentially, as much those of cause and effect as any connected with the manifestation of organic forces elsewhere; and, in one sense, the nervous power may be regarded as the *product* of the nervous cells—the function of these last being to eliminate this force in virtue of their organic constitution. But we cannot use the word *eliminate* here in the same sense as it is used when speaking of the mammary cells eliminating milk; for the latter is an elaborated, metamorphosed product, whereas, as to the former, physiology has not yet decided—nor can I see how it ever can decide—whether it is a distinct entity, or whether it is a manifestation of what may be termed mere conditions of matter. It is quite sufficient that our recognition and perception of it is *active*—that is, as a *force;* and, considered as such, we may, finally, regard it as indissolubly connected with matter under an organized condition. In conclusion, it may be added that the action of the nervous cells, for the elimination of simple nerve force, may be, in one sense, spontaneous (though it may be questioned if, even here, it is not the result of irritation), as in the splanchnic nervous centres; or it may be due to stimuli: these stimuli are either internal, the will; or external, through irritation.

But with that portion (cerebrum) of the nerve-cells concerned in the intellectual act, other, if not wholly dissimilar relations of action occur; and the capacity of these cells for those vital processes which terminate in intellectual manifestation, must belong to them *per se*, in contradistinction to those of the other nervous centres, such as the spinal cord or its analogue, &c., belonging to them in virtue of their organization. The primary intellectual act must always be referable to an external stimulus acting on the brain; this stimulus is an object, which is brought in connection with these cells by means of the senses. The primary intellectual act, therefore, is one and the same in character (exactly as in the mere nervous force) whenever occurring, and the mere perception of an idea must always be the same as to simple quality.

In this respect, then, the intellectual conditions of man would not differ in mere quality from those of any of the lower animals; and in the perception of an object, constituting the naked idea, man would have no advantage over the brutes in the lucidity of that act: thus, the idea of a tree, merely as an external object of a peculiar form, &c., is probably just as lucid and clear in the mind of a dog as in that of a man. This simple intellectual act is no more mysteriously connected with the agency of the cells of the cerebrum than is the manifestation of nervous power with the nerve-cells of other nervous centres. The nerve-cells which form the outer layer of the retina receive the optical image of all objects, and, as such, this image is perceived by the consciousness; but, truly, in itself, this act is no more incomprehensible than our perception that an object is round or square by feeling it with the hand.

This direct agency of the nerve-cells in the simple intellectual act, appears to be the only ground that can be taken, if we consider the character of many mental phenomena; and, thus adopted, it would seem to accord with, or, indeed, in one sense explain, some of the conditions of mental action, which, with the purely *spiritual* view of the mental relations, seem wholly irreconcilable with observed facts. All who have watched the physical conditions of their intellect, know that after severe and protracted mental occupation, there is just as sensible a feeling of weariness about the brain, as there is with the muscles of a limb after the latter has been severely used. Mental action, then, whether it be in the simple act of perception of external objects, or in the combination and colligation of previously acquired ideas—constituting abstraction—must invariably be attended with vital organic changes in the cells of the cerebrum.

This conclusion, thereby closely connecting the conditions of intellectuality with mere material forms, is one which we must accept according to the laws of physiology. It may be asked, how can such doctrines be reconciled with the never-failing wonders of Memory—that voluntary substitution of the past for the present, and that, too, with the truth and beauty it had when really the present—when the agents (cells) of the intellectual conceptions are ever changing and passing away. To this deeply interesting question, I think physiology has an adequate answer. Let me make some explanation. An intellectual act occurs by the perception of an external object, the material structure of a nerve-cell or cells has thereby been changed, and this change, whatever it may be, is one and indivisible, and, in my opinion, is never completely effaced. Like the cicatrix of a wound, it persists to indicate the occurrence of a past physical effect. It is not overgrown or removed because of the exactness of the nutrition of these cells; the physical impressions made are preserved, and when afterwards, by means of association or combination of ideas, they are visited, as it were, by the consciousness, they represent the pictures of the past. It is not necessary to continue this topic farther, or to point out the relations of this exquisite nutritive act to the phases of memory in youth, manhood, and old age; they all accord with the more or less organic plasticity of the brain at these different epochs.

But this simple intellectual act we have been considering, and which we have intimately linked with the physical condition of nerve-cells, is the lowest form of mental action. The higher forms of mental activity belong to the colligation and comparison of previously acquired ideas, and the deriving of conclusions from such method. This is accomplished under the superintendence of that incomprehensible power we ever feel—the Will, a kind of metaphysical Ego. These mental processes constitute what is called abstraction, which seems to be the distinguishing feature of man's intellectual condition from that of the brutes, as far as we are able to judge; for there is no reason to believe that animals ever have a true mental abstraction. It is true that they sometimes act as though they really possessed such a power, but these instances may all, perhaps, be referable to the laws of association and combination in the simple intellectual acts. Man's nobility, then, lies in his exclusive possession of a deeply mysterious power, inseparably connected with, yet quite different from, his consciousness. A power to do or not to do as may be; a power which may induce mental action in this or that direction without limit, and which, therefore, is the supreme of stimulants. This is the Will—the immortal Psyche—which the innermost depths of our consciousness tell us holds no relations with matter, for it never grows weary or becomes borne down with labor, but even when the intellectual acts have ceased to become distinct, it retains that freshness of action belonging to its unearthly nature. Physiology and Psychology, therefore, are not only two distinct sciences, but they even do not pass into each other. By Physiology, in this connection, we learn the necessary relations of cell life and action to the conditions of the simple intellectual act; whereas, by Psychology, we seek to learn the ever-varying changes of this intellectual act due to the agency of the immortal will. Physiology, therefore, as connected with cells, would, indeed, be sadly deficient, did it not indicate to the humble student, as he stands upon the confines of its domain, the necessity of conditions and springs of action connected with our mental life, which lie above and beyond any mere conditional relations of ever-changing, decaying matter.