We learn in the preface that the observations on scurvy formed part of the journal of Dr. Armstrong's practice while serving in the *Investigator*, which obtained for him the honour of the Blane Gold Medal, an award of the adjudicators which we feel assured would have met the fullest approbation of the venerable founder himself, had he lived to the present day.

Besides the other undoubted merits of the journal, Sir Gilbert would have found in it another and most satisfactory proof that he had not exaggerated the prophylactic and curative power of lemon-juice in scurvy, when he stated in the 'Select Dissertations' that it was "peculiar and exclusive, when compared to all other remedies"—that it was "*sui generis—nil simile nec secundum.*"*

Review VIII.

1. *On the Archetype and Homologies of the Vertebrate Skeleton.* By Richard Owen, F.R.S.—*London*, 1848. pp. 172.
2. *Principes d' Ostéologie Comparée, ou Recherches sur l' Archétype et les Homologies du Squelette Vertébré.* Par Richard Owen.—*Paris.*
Principles of Comparative Osteology; or, Researches on the Archetype and the Homologies of the Vertebrate Skeleton. By Richard Owen.
3. *On the Nature of Limbs.* A Discourse delivered on Friday, February 9th, at an Evening Meeting of the Royal Institution of Great Britain. By Richard Owen, F.R.S.—*London*, 1849. pp. 119.

Judging whether another proves his position is a widely different thing from proving your own. To establish a general law requires an extensive knowledge of the phenomena to be generalized; but to decide whether an alleged general law is established by the evidence assigned, merely requires an adequate reasoning faculty. Especially is such a decision easy where the premises do *not* warrant the conclusion. It may be dangerous for one who has but little previous acquaintance with the facts, to say that a generalization is demonstrated; seeing that the argument may be one-sided: there may be many facts unknown to him which disprove it. But it is not dangerous to give a negative verdict when the alleged demonstration is manifestly insufficient. If the data put before him do not bear out the inference, it is competent for every logical reader to say so.

From this stand-point, then, we venture to criticise some of Professor Owen's osteological theories. For his knowledge of comparative osteology we have the highest respect. We believe that no living man has so wide and detailed an acquaintance with the bony structure of the vertebrata. Indeed, there probably has never been any one whose information on the subject was so nearly exhaustive. Moreover, we confess that nearly all we know of this department of biology has been learnt from his lectures and writings. We pretend to no independent investigations, but merely to such knowledge of the phenomena as he has furnished us with. Our position, then, is such that, had Professor Owen simply enunciated his generalizations, we should have accepted them on his authority. But he has brought forward evidence to prove them. By so doing he has tacitly appealed to the judgments of his readers and hearers—has practically said, "Here are the facts; do they not warrant these conclusions?" And all we propose to do, is to consider whether the conclusions *are* warranted by the facts brought forward.

Let us first limit the scope of our criticisms. On that division of comparative osteology which deals with what Professor Owen distinguishes as "special homologies," we do not propose to enter. That the wing of a bird is framed upon bones essentially parallel to those of a mammal's fore-limb; that the cannon-bone of a horse's leg answers to the middle metacarpal of the human hand; that various bones in the skull of a fish are homologous with bones in the skull of a man—these and countless similar facts, we take to be well established. It may be, indeed, that the doctrine of special homologies is at present carried too far. It may be that, just as the sweeping generalization at one time favoured, the embryonic phases of the higher animals represent the adult forms of lower ones, has been found untrue in a literal sense, and is acceptable only in a very qualified sense; so the sweeping generalization that the skeletons of all vertebrate animals consist of homologous parts, will have to undergo some modification. But that this generalization is substantially true, all comparative anatomists agree.

* Select Dissertations on Medical Science, by Sir Gilbert Blane, Bart., vol. i. p. 27.

The doctrine which we are here to consider is quite a separate one—that of "general homologies." The truth or falsity of this may be decided on quite apart from that of the other. Whether certain bones in one vertebrate animal's skeleton correspond with certain bones in another's, or in every other's, is one question; and whether the skeleton of every vertebrate animal is divisible into a series of segments, each of which is modelled after the same type, is another question. While the first is answered in the affirmative, the last may be answered in the negative; and we propose to give reasons why it should be answered in the negative.

In so far as his theory of the skeleton is concerned, Professor Owen is an avowed disciple of Plato. At the conclusion of his 'Archetype and Homologies of the Vertebrate Skeleton,' he quotes approvingly the Platonic hypothesis of ἰδέαι, "a sort of models, or moulds in which matter is cast, and which regularly produce the same number of diversity of species." The vertebrate form in general (see diagram of the *Archetypus*), or else the form of each kind of vertebrate animal (see p. 172, where this seems implied), Professor Owen conceives to exist as an "idea"—an "archetypal exemplar on which it has pleased the Creator to frame certain of his living creatures." Whether Professor Owen holds that the typical vertebra also exists as an "idea," is not so certain. From the title given to his figure of the "ideal typical vertebra," it would seem that he does; and at p. 40 of his 'Nature of Limbs,' and indeed throughout his general argument, this supposition is implied. But on the last two pages of the 'Archetype and Homologies' it is distinctly alleged that "the repetition of similar segments in a vertebral column, and of similar elements in a vertebral segment, is analogous to the repetition of similar crystals as the result of polarizing force in the growth of an inorganic body;" it is pointed out that, "as we descend the scale of animal life, the forms of the repeated parts of the skeleton approach more and more to geometrical figures;" and it is inferred that "the Platonic ἰδέα or specific organizing principle or force, would seem to be in antagonism with the general polarizing force, and to subdue and mould it in subserviency to the exigencies of the resulting specific form." If Professor Owen's doctrine is to be understood as expressed in these closing paragraphs of his 'Archetype and Homologies'—if he considers that "the ἰδέα" "which produces the diversity of form belonging to living bodies of the same materials," is met by the "counter-operation" of "the polarizing force pervading all space," which produces "the similarity of forms, the repetition of parts, the signs of unity of organization," and which is "*subdued*" as we ascend "in the scale of being;" then he implies the somewhat questionable belief that the properties which the Creator has given to matter have hindered the realization of His designs. If, on the other hand, Professor Owen holds, as every reader would suppose from the general tenor of his reasonings, that not only does there exist an archetypal or ideal vertebrate skeleton, but that there also exists an archetypal or ideal vertebra; then he carries the Platonic hypothesis much further than Plato does. Plato's argument, that before any species of object was created, it must have existed as an idea of the Creative Intelligence, and that hence all objects of such species must be copies of this original idea, is tenable enough from the anthropomorphic point of view. But while those who, with Plato, think fit to base their theory of creation upon the analogy of a carpenter designing and making a table, must yield assent to Plato's inference, they are by no means committed to Professor Owen's expansion of it. To say that before creating a vertebrate animal, God must have had the conception of one, does not involve saying that God gratuitously bound Himself to make a vertebrate animal out of segments all moulded after one pattern. As there is no conceivable advantage in this alleged adhesion to a fundamental pattern—as for the fulfilment of the intended ends it is not only needless, but often, as Professor Owen argues, less appropriate than some other construction would be (see 'Nature of Limbs,' pp. 39, 40), to suppose the creative processes thus regulated, is not a little startling. Even those whose conceptions are so anthropomorphic as to think they honour the Creator by calling Him "the Great Artificer," will scarcely ascribe to Him a proceeding which, in a human artificer, they would consider a not very worthy exercise of ingenuity.

But whichever of these alternatives Professor Owen contends for—whether the typical vertebra is that more or less crystalline figure which osseous matter ever tends to assume in spite of "the ἰδέα or organizing principle," or whether the typical vertebra is itself an "ἰδέα or organizing principle"—there is alike implied the belief that the typical vertebra has an abstract existence apart from actual vertebræ. It is a form which, in every endoskeleton, strives to embody itself in matter—a form which is potentially present in each vertebra; which is manifested in each vertebra with more or less clearness; but which, in consequence of antagonizing forces, is nowhere completely realized. Apart from the philosophy of this hypothesis, let us here examine the evidence which is thought to justify it.

And first as to the essential constituents of the "ideal typical vertebra." Exclusive of "*diverging appendages*" which it "may also support," "it consists in its typical completeness of the following elements and parts:"—A *centrum* round which the rest are arranged in a somewhat radiate manner; above it two *neurapophyses*, converging as they ascend, and forming with the centrum a trianguloid space containing the neural axis; a *neural spine* surrounding the two neurapophyses, and with them completing the neural arch; below the centrum two *hæmapophyses* and a *hæmal spine*, forming a hæmal arch similar to the neural arch above, and enclosing the hæmal axis; two *pleurapophyses* radiating horizontally from the sides of the centrum; and two *parapophyses* diverging from the centrum below the pleurapophyses. "These," says Professor Owen, "being usually developed from distinct and independent centres, I have termed 'autogenous elements.'" The remaining elements, which he classes as "exogenous," because they "shoot out as continuations from some of the preceding elements," are the *diapophyses* diverging from the upper part of the centrum as the parapophyses do below, and the *zygapophyses* which grow out of the distal ends of the neurapophyses and hæmapophyses.

If, now, these are the constituents of the vertebrate segment "in its typical completeness;" and if the vertebrate skeleton consist of a succession of such segments, we ought to have in them representatives of all the elements of the vertebrate skeleton—at any rate, all its essential elements. Are we then to conclude that the "diverging appendages" which Professor Owen regards as rudimental limbs, and from certain of which he considers actual limbs to be developed, are typically less important than some of the above-specified exogenous parts—say the zygapophyses?

That the meaning of this question may be understood, it will be needful briefly to state Professor Owen's theory of 'The Nature of Limbs;' and such criticisms as we have to make on it must be included in the parenthesis. In the first place he aims to show that the scapular and pelvic arches, giving insertion to the fore and hind limbs respectively, are displaced and modified hæmal arches, originally belonging in the one case to the occipital vertebra, and in the other case to some trunk-vertebra not specified. To give a colour to this assumption of displacement, carried in some cases to the extent of *twenty-seven* vertebræ, Professor Owen cites certain acknowledged displacements which occur in the human skeleton to the extent of half a vertebra—a somewhat slender justification. But for proof that such a displacement *has* taken place in the scapular arch, he chiefly relies on the fact that in fishes the pectoral fins, which are the homologues of the fore-limbs, are directly articulated to certain bones at the back of the head, which he alleges are part of the occipital vertebra. This appeal to the class of fishes is avowedly made on the principle that these lowest of the Vertebrata approach closest to archetypal regularity, and may therefore be expected to show the original relations of the bones more nearly. Simply noting the facts that Professor Owen does not give us any transitional forms between the alleged normal position of the scapular arch in fishes and its extraordinary displacement in the higher Vertebrata; and that he makes no reference to the embryonic phases of the higher Vertebrata, which might be expected to exhibit the progressive displacement; we go on to remark, that in the case of the pelvic arch, he abandons his principle of appealing to the lowest vertebrate forms for proof of the typical structure. In fishes, the rudimentary pelvis, widely removed from the spinal column, shows no signs of having belonged to any vertebra; and here Professor Owen instances the perennibranchiate *Batrachia* as exhibiting the typical structure: remarking that "mammals, birds, and reptiles show the rule of connexion, and fishes the exception." Thus in the case of the scapular arch, the evidence afforded by fishes is held of great weight, *because* of the archetypal regularity; while in the case of the pelvic arch, their evidence is rejected as exceptional. But now having, as he considers, shown that these bony frames to which the limbs are articulated are modified hæmal arches, Professor Owen points out that the hæmal arches habitually bear certain "diverging appendages;" and he aims to show that the "diverging appendages" of the scapular and pelvic arches respectively, are developed into the fore and hind limbs. There are several indirect ways in which we may test the probability of this conclusion. If these diverging appendages are "rudimental limbs"—"future possible or potential arms, legs, wings, or feet," we may fairly expect them always to bear to the hæmal arches a relation such as the limbs do. But they by no means do this. "As the vertebræ approach the tail, these appendages are often transferred gradually from the pleurapophysis to the parapophysis, or even to the centrum and neural arch."* Again, it might naturally be assumed that in the lowest vertebrate forms, where the limbs are but little developed, they would most clearly display their alliance with the appendages or "rudimental limbs" by the similarity of their attachments. Instead of showing this, however, Professor Owen's drawings show that whereas the appendages are habitually attached to the pleurapophyses, the limbs in their earliest and lowest phase, alike in fishes and in the lepidosiren, are articulated to the hæmapophyses. Most anomalous of all, however, is the process of development. When we speak of one thing as being developed out of another, we imply that the parts next to the germ are the earliest to make their appearance, and the most constant. In the evolution of a tree out of a seed, there come first the stem and the radicle; afterwards the branches and divergent roots; and still later the branchlets and rootlets; the remotest parts being the latest and most inconstant. If, then, a limb is developed out of a "diverging appendage" of the hæmal arch, the earliest and most constant bones should be the humerus and femur; next in order of time and constancy should come the coupled bones based upon these; while the terminal groups of bones should be the last to make their appearance, and the most liable to be absent. Yet, as Professor Owen himself shows, the actual mode of development is the very reverse of this. At page 16 of the 'Archetype and Homologies,' he says:—

"The earlier stages in the development of all locomotive extremities are permanently

* Arch. and Hom., p. 98.

retained or represented in the paired fins of fishes. First the essential part of the member, the hand or foot, appears: then the fore-arm or leg; both much shortened, flattened, and expanded, as in all fins and all embryonic rudiments of limbs: finally come the humeral and femoral segments; but this stage I have not found attained in any fish."

That is to say, alike in ascending through the vertebrata generally, and in tracing up the successive phases of a mammalian embryo, the last-developed and least-constant division of the limb is that basic one by which it articulates with the hæmal arch. It seems to us that, so far from proving his hypothesis, Professor Owen's own facts tend to show that limbs do not belong to the vertebræ at all; that they make their first appearance peripherally; that their development is centripetal; and that they become fixed to such parts of the vertebrate axis as the requirements of the case determine.

But now, ending here this digressive exposition and criticism, and granting the position that limbs "are developments of costal appendages," let us return to the question above put—Why are not these appendages included as elements of the "ideal typical vertebra?" It cannot be because of their comparative inconstancy; for, judging from the illustrative figures, they seem to be as constant as the hæmal spine, which is one of the so-called autogenous elements, and in the diagram of the 'Archetypus,' the appendage is represented as attached to every vertebrate segment of the head and trunk, which the hæmal spine is not. It cannot be from their comparative unimportance; seeing that as potential limbs they are essential parts of nearly all the Vertebrata—much more obviously so than the diapophyses are. If, as Professor Owen argues, "the divine mind which planned the archetype also foreknew all its modifications;" and if, among these modifications, the development of limbs out of diverging appendages was one intended to characterize all the higher Vertebrata; then surely these diverging appendages must have been parts of the "ideal typical vertebra." Or, if the "ideal typical vertebra" is to be understood as a crystalline form in antagonism with the organizing principle; then why should not the appendage be included among its various offshoots? We do not ask this question because of its intrinsic importance. We ask it for the purpose of ascertaining Professor Owen's method of determining what are true vertebral constituents. He presents us with a diagram of the typical vertebra, in which are included certain bones, and from which are excluded certain others. If relative constancy is the criterion, then there arises the question —What degree of constancy entitles a bone to be included? If relative importance is the criterion, there comes not only the question—What degree of importance suffices? but the further question—How is importance to be measured? If neither of these is the criterion, then what is it? And if there is no criterion, does it not follow that the selection is arbitrary?

This question serves here to introduce a much wider one:—Has the "ideal typical vertebra" any essential constituents at all? It might naturally be supposed that though some bones are so rarely developed as not to seem worth including, and though some that are included are very apt to be absent, yet that certain others are invariable; forming as it were the basis of the ideal type. Let us see whether the facts bear out this supposition. In his "summary of modifications of corporal vertebræ" (p. 96), Professor Owen says:—"The *hæmal spine* is much less constant as to its existence, and is subject to a much greater range of variety, when present, than its vertical homotype above, which completes the neural arch." Again he says:—"The *hæmapophyses*, as osseous elements of a vertebra, are less constant than the pleurapophyses." And again:—"The *pleurapophyses* are less constant elements than the neurapophyses." And again:—"Amongst air-breathing vertebrates the *pleurapophyses* of the trunk segments are present only in those species in which the septum of the heart's ventricle is complete and imperforate, and here they are exogenous and confined to the cervical and anterior thoracic vertebræ." And once more, both the *neurapophyses* and the *neural spine* "are absent under both histological conditions, at the end of the tail in most air-

breathing vertebrates, where the segments are reduced to their central elements." That is to say, of all the peripheral elements of the "ideal typical vertebra," there is not one which is always present. It will be expected, however, that at any rate the centrum is constant: the bone which "forms the axis of the vertebral column, and commonly the central bond of union of the peripheral elements of the vertebra" (p. 97), is of course an invariable element. No: not even this is essential.

"The centrums do not pass beyond the primitive stage of the notochord (undivided column) in the existing lepidosiren, and they retained the like rudimental state in every fish whose remains have been found in strata earlier than the permian æra in Geology, though the number of vertebræ is frequently indicated in Devonian and Silurian ichthyolites by the fossilized neur- and hæm-apophyses and their spines." (p. 96.)

Indeed, Professor Owen himself remarks that "the neurapophyses are more constant as osseous or cartilaginous elements of the vertebræ than the centrums.' (p. 97.) Thus, then, it appears that the several elements included in the "ideal typical vertebra" have various degrees of constancy, and that no one of them is essential. There is no one part of a vertebra which invariably answers to its exemplar in the pattern-group. How does this fact consist with the hypothesis? If the Creator saw fit to make the vertebrate skeleton out of a series of segments, all formed on essentially the same model—if, for the maintenance of the type, one of these bony segments is in many cases formed out of a coalesced group of pieces, where, as Professor Owen argues, a single piece would have served as well or better; then we ought to find this typical repetition of parts uniformly manifested. Without any change of shape, it would obviously have been quite possible for every actual vertebra to have contained all the parts of the ideal one—rudimentally where they were not wanted. Even one of the terminal bones of a mammal's tail might have been formed out of the nine autogenous pieces, united by suture but admitting of identification. As, however, there is no such uniform typical repetition of parts, it seems to us that to account for the typical repetition which *does* occur by supposing the Creator to have fixed on a pattern vertebra, is to ascribe to Him the inconsistency of forming a plan and then abandoning it. If, on the other hand, Professor Owen means that the "ideal typical vertebra" is a crystalline form in antagonism with "the idea or organizing principle," then we might fairly expect to find it most clearly displaying its crystalline character and its full complement of parts in those places where the organizing principle may be presumed to have "subdued" it to the smallest extent. Yet in the Vertebrata generally, and even in Professor Owen's *archetypus*, the vertebræ of the tail, which must be considered as, if anything, less under the influence of the organizing principle than those of the trunk, do not manifest the ideal form more completely. On the contrary, as we approach the end of the tail, the successive segments not only lose their remaining typical elements, but become as uncrystalline-looking as can be conceived.

Supposing, however, that the assumption of suppressed or undeveloped elements be granted—supposing it to be consistent with the hypothesis of an "ideal typical vertebra," that the constituent parts may severally be absent in greater or less number, sometimes leaving only a single bone to represent them all; may it not be that such parts as *are* present show their respective typical natures by some constant character: say their mode of ossification?

To this question some parts of the 'Archetype and Homologies' seem to reply, "Yes;" while others as clearly answer, "No." Criticising the opinions of Geoffroy St. Hilaire and Cuvier, who agreed in thinking that ossification from a separate centre was the test of a separate bone, and that thus there were as many elementary bones in the skeleton as there were centres of ossification, Professor Owen points out that, according to this test, the human femur, which is ossified from four centres, must be regarded as four bones; while the femur of birds and reptiles, which is ossified from a single centre, must be regarded as a single bone. On the other hand, he attaches weight to the fact that the skull of the human fœtus presents "the same ossific centres" as do those of the embryo kangaroo and the young bird.* And at p. 104 of the 'Homologies,' after giving a number of instances, he says:

"These and the like correspondences between the points of ossification of the human fœtal skeleton, and the separate bones of the adult skeletons of inferior animals, are pregnant with interest, and rank among the most striking illustrations of unity of plan in the vertebrate organization."

It is true that on the following page he seeks to explain this seeming contradiction by distinguishing

"between those centres of ossification that have homological relations, and those that have teleological ones; i. e., between the separate points of ossification of a human bone which typify vertebral elements, often permanently distinct bones in the lower animals; and the separate points which, without such signification, facilitate the progress of osteogeny, and have for their obvious final cause the well-being of the growing animal."

But if there are thus centres of ossification which have homological meanings, and others that have not, there arises the question—How are they always to be distinguished? Evidently independent ossification ceases to be a homological test, if there are independent ossifications that have nothing to do with the homologies. Add to which, that there are cases where neither a homological nor a teleological meaning can be given. Among various modes of ossification of the centrum, Professor Owen points out that "the body of the human atlas is sometimes ossified from two, rarely from three, distinct centres placed side by side" (p. 89); while at p. 87 he says:—"In osseous fishes I find that the centrum is usually ossified from six points." It is clear that this mode of ossification has here no homological signification; and it would be difficult to give any teleological reason why the small centrum of a fish should have more centres of ossification than the large centrum of a mammal. The truth is, that as a criterion of the identity or individuality of a bone, mode of ossification is quite untrustworthy. Though, in his "ideal typical vertebra," Professor Owen delineates and classifies as separate "autogenous" elements, those parts which are "usually developed from distinct and independent centres;" and though by doing so he erects this characteristic into some sort of criterion; yet his own facts show it to be no criterion. The parapophyses are classed among the autogenous elements; yet they are autogenous in fishes alone, and in these only in the trunk vertebræ, while in all air-breathing vertebrates they are, when present at all, exogenous. The neurapophyses, again, "lose their primitive individuality by various kinds and degrees of confluence:" in the tails of the higher Vertebrata they, in common with the neural spine, become exogenous. Nay, even the centrum may lose its autogenous character. Describing how, in some batrachians, "the ossification of the centrum is completed by an extension of bone from the bases of the neurapophyses, which effects also the coalescence of these with the centrum," Professor Owen adds:—"In *Pelobates fuscus* and *Pelobates cultripes*, Müller found the entire centrum ossified from this source, without any independent points of ossification." (p. 88.) That is to say, the centrum is in these cases an exogenous process of the neurapophyses. We see, then, that these so-called typical elements of vertebræ have no constant developmental character by which they can be identified. Not only are they undistinguishable by any specific test from other bones not included as vertebral elements; not only do they fail to show their typical character by their constant presence; but when present, they exhibit no persistent marks of individuality. The central element may be ossified from six, four, three, or two points; or it may have no separate point of ossification at all: and similarly with various of the peripheral elements. The whole group of bones forming the "ideal typical vertebra" may severally have their one or more ossific centres; or they may, as in a mammal's tail, lose their individualities in a single bone ossified from one or two points.

* Nature of Limbs, p. 40.

Another fact which seems very difficult to reconcile with the hypothesis of an "ideal typical vertebra," is the not infrequent presence of some of the typical elements in duplicate. Not only, as we have seen, may they severally be absent; but they may severally be present in greater number than they should be. When we see, in the ideal diagram, one centrum, two neurapophyses, two pleurapophyses, two hæmapophyses, one neural spine, and one hæmal spine, we naturally expect to find them always bearing to each other these numerical relations. Though we may not be greatly surprised by the absence of some of them, we are hardly prepared to find others multiplied. Yet such cases are common. Thus the neural spine "is double in the anterior vertebræ of some fishes" (p. 98). (And we may parenthetically remark that, joining this duality existing in the lower Vertebrata with the facts that in the higher Vertebrata the neural spine "may be developed from two lateral halves," and that where there is arrest of development, as in *spina bifida*, these lateral halves continue separate, Professor Owen might, had it suited him, have argued that the neural spine consists of two vertebral elements which usually coalesce; the evidence would have been much the same as that which leads him to class the parapophyses as separate elements from the centrum.) Again, in the abdominal region of extinct saurians, and in crocodiles, "the freely-suspended hæmapophyses are compounded of two or more overlapping bony pieces" (p. 100). Yet again, at p. 99, we read—"I have observed some of the expanded pleurapophyses in the great *Testudo elephantopus* ossified from two centres, and the resulting divisions continuing distinct, but united by suture." Once more "the neurapophyses, which do not advance beyond the cartilaginous stage in the sturgeon, consist in that fish of two distinct pieces of cartilage; and the anterior pleurapophyses also consist of two more cartilages, set end on end" (p. 91). And elsewhere referring to this structure, he says:—

"Vegetative repetition of perivertebral parts not only manifests itself in the composite neurapophyses and pleurapophyses, but in a small accessory (interneural) cartilage, at the fore and back part of the base of the neurapophysis; and by a similar (interhæmal) one at the fore and back part of most of the parapophyses." (p. 87.)

Not only is it, however, that the neural and hæmal spines, the neurapophyses, the pleurapophyses, the hæmapophyses, may severally consist of two or more pieces; but the like is true even of the centrums.

"In *Heptanchus* (*Squalus cinereus*) the vertebral centres are feebly and vegetatively marked out by numerous slender rings of hard cartilage in the notochordal capsule, the number of vertebræ being more definitely indicated by the neurapophyses and parapophyses. . . . . In the piked dog-fish (*Acanthias*) and the spotted dog-fish (*Scyllium*) the vertebral centres coincide in number with the neural arches." (p. 87.)

Is it not strange that the pattern vertebra should be so little adhered to, that each of its single typical pieces may be transformed into two or three.

But there are still more startling departures from the alleged type. The numerical relations of the elements vary not only in this way, but in the opposite one: a given part may be present not only in greater number than it should be, but also in less. Thus in the tails of homocercal fishes, the centrums "are rendered by centripetal shortening and bony confluence fewer in number than the persistent, neural, and hæmal arches of that part"—that is, there is only a fraction of a centrum to each vertebra. Nay, even this is not the most heteroclite structure. Paradoxical as it may seem, there are cases in which the same vertebral element is, considered under different aspects, at once in excess and defect. Thus, speaking of the hæmal spine, Professor Owen says:—

"The horizontal extension of this vertebral element is sometimes accompanied by a median division, or in other words, it is ossified from two lateral centres; this is seen in the development of parts of the human sternum; the same vegetative character is constant in the broader thoracic hæmal spines of birds; though sometimes, as e.g., in the struthionidæ, *ossification extends from the same lateral centre lengthwise—i.e., forwards and backwards, calcifying the connate cartilaginous homologues of halves of four or five hæmal spines, before these finally coalesce with their fellows at the median line.*" (p. 101.)

So that the sternum of the ostrich, which according to the hypothesis should, in its cartilaginous stage, have consisted of *four or five transverse* pieces, answering to the vertebral segments, and should have been ossified from four or five centres, one to each cartilaginous piece, shows not a trace of this structure; but instead, consists of *two longitudinal* pieces of cartilage, each ossified from one centre, and finally coalescing on the median line. These four or five hæmal spines have at the same time doubled their individualities transversely, and entirely lost them longitudinally!

There still remains to be considered the test of relative position. It might be contended that, spite of all the foregoing anomalies, if the typical parts of the vertebræ always stood towards each other in the same relations—always preserved the same connexions, something like a case would be made out. Doubtless, relative position is an important point; and it is one on which Professor Owen manifestly places great dependence. In his discussion of "moot cases of special homology," it is the general test to which he appeals. The typical natures of the "alisphenoid," the mastoid, the orbito-sphenoid, the prefrontal, the malar, the squamosal, &c., he determines almost wholly by reference to the adjacent nerve-perforations and the articulations with neighbouring bones (see pp. 19 to 72): the general form of the argument being—This bone is to be classed as such or such, *because* it is connected thus and thus with these others, which are so and so. Moreover, by putting forth an "ideal typical vertebra," consisting of a number of elements standing towards each other in certain definite arrangement, this persistency of relative position is manifestly alleged. The essential attribute of this group of bones, considered as a typical group, is the constancy in the connexions of its parts: change the connexions, and the type is changed. But the constancy of relative position thus tacitly asserted, and appealed to as a conclusive test in "moot cases of special homology," is clearly negatived by Professor Owen's own facts. For instance, in the "ideal typical vertebra," the hæmal arch is represented as formed by the two hæmapophyses and the hæmal spine; but at p. 91 we are told that

"The contracted hæmal arch in the caudal region of the body may be formed by different elements of the typical vertebra: e. g., by the parapophyses (fishes generally); by the pleurapophyses (lepidosiren); by both parapophyses and pleurapophyses (*Sudis, Lepidosteus*), and by hæmapophyses, shortened and directly articulated with the centrums (reptiles and mammals)."

Add to which that, in the thorax of reptiles, birds, and mammals, "the hæmapophyses are removed from the centrum, and are articulated to the distal ends of the pleurapophyses; the bony hoop being completed by the intercalation of the hæmal spine" (p. 82). So that there are *five* different ways in which the hæmal arch may be formed—four modes of attachment of the parts different from that shown in the typical diagram! Nor is this all. The pleurapophyses "may be quite detached from their proper segment, and suspended to the hæmal arch of another vertebra;" as we have already seen, the entire hæmal arch may be detached and removed to a distance, sometimes reaching the length of twenty-seven vertebræ; and, even more remarkable, the ventral fins of some fishes, which theoretically belong to the pelvic arch, are so much advanced forward as to be articulated to the scapular arch—"the ischium elongating to join the coracoid." With these admissions it seems to us that relative position and connexions cannot be appealed to as tests of homology, nor as evidence of any original type of vertebra.

In no class of facts, then, do we find a good foundation for the hypothesis of an "ideal typical vertebra." There is no one conceivable attribute of this archetypal form which is habitually realized by actual vertebræ. The alleged group of true vertebral elements is not distinguished in any specified way from bones not included in it. Its members have various degrees of inconstancy; are rarely all present together; and no one of them is essential. They are severally developed in no uniform way; each of them may arise either out of a separate piece of cartilage or out of a piece continuous with that of some other element; and each may be ossified from many independent points, from one, or from none. Not only may their respective individualities be lost by absence or by confluence with others; but they may be doubled, or tripled, or halved, or may be multiplied in one direction and lost in another. The entire group of typical elements may coalesce into one simple bone representing the whole vertebra: and even, as in the terminal piece of a bird's tail, half-a-dozen vertebræ, with all their many elements, may become entirely lost in a single mass. Lastly, the respective elements, when present, have no fixity of relative position: sundry of them are found articulated to various others than those with which they are typically connected; they are frequently displaced and attached to neighbouring vertebræ; and they are even removed to quite remote parts of the skeleton. It seems to us that if this want of congruity with the facts does not disprove the hypothesis, no such hypothesis admits of disproof.

Unsatisfactory as is the evidence in the case of the trunk and tail vertebræ, to which we have hitherto confined ourselves, it is far worse in the case of the alleged cranial vertebræ. The mere fact that those who have contended for the vertebrate structure of the skull, have differed so astonishingly in their special interpretations of it, is enough to warrant great doubt as to the general truth of their theory. From Professor Owen's history of the doctrine of general homology, we gather that Duméril wrote upon "la tête considérée comme *une* vertèbre;" that Kielmeyer, "instead of calling the skull a vertebra, said such vertebra might be called a skull;" that Oken recognised in the skull *three* vertebræ and a rudiment; that Professor Owen himself makes out *four* vertebræ; that Goethe's idea, adopted and developed by Carus, was, that the skull was composed of *six* vertebræ; and that Geoffroy St.-Hilaire divided it into *seven*. Does not the fact that different comparative anatomists have arranged the same group of bones into one, three, four, six, and seven vertebral segments, go far to show that the mode of determination is arbitrary, and the conclusions arrived at unworthy of confidence? May we not properly entertain great doubts as to any one scheme being more valid than the others? And if out of these conflicting schemes we are asked to accept one, ought we not to accept it only on the production of some thoroughly conclusive proof—some rigorous test showing irrefragably that the others must be wrong and this alone right. Evidently where such contradictory opinions have been formed by so many competent judges, we ought, before deciding in favour of one of them, to demand a clearness of demonstration much exceeding that required in any ordinary case. Let us see where Professor Owen supplies us with any such clearness of demonstration.

To bring the first or occipital segment of the skull into correspondence with the "ideal typical vertebra," Professor Owen argues, in the case of the fish, that the parapophyses are *displaced* and wedged between the neurapophyses and the neural spine—removed from the hæmal arch and built into the upper part of the neural arch. Further, he considers that the pleurapophyses are *teleologically compound*. And then, in all the higher vertebrata, he alleges that the hæmal arch is *separated* from its centrum, taken to a distance, and transformed into the scapular arch. Add to which, he says that in mammals the displaced parapophyses are mere processes of the neurapophyses (p. 133): these vertebral elements typically belonging to the lower part of the centrum, and in nearly all cases confluent with it, are not only removed to the far end of elements placed above the centrum, but have become exogenous parts of them!

Conformity of the second or parietal segment of the cranium with the pattern vertebra, is produced thus:—The petrosals are *excluded* as being partially ossified sense-capsules, not forming parts of the true vertebral system, but belonging to the "splanchno-skeleton." A centrum is *artificially* obtained by sawing in two the bone which serves in common as centrum to this and the preceding segment; and as it is admitted that in fishes these two hypothetical centrums are not simply coalescent, but connate, it follows that this bisection is unwarranted, save for convenience. Next, a similar *arbitrary bisection* is made of certain elements of the hæmal arches. And then, "the principle of *vegetative repetition* is still more manifest in this arch than in the occipital one:" each pleurapophysis is double; each hæmapophysis is double; and the hæmal spine consists of six pieces!

The interpretation of the third and fourth segments being of the same general character, need not be detailed. The only point calling for remark being, that in addition to these various modes of getting over anomalies above instanced, we find certain bones referred to the *dermo-skeleton*.

Now it seems to us, that even supposing no antagonist interpretations had been given, an hypothesis reconcilable with the facts only by the aid of so many questionable devices, could not be considered satisfactory; and that when, as in this case, various comparative anatomists have contended for other interpretations, the character of this one is certainly not of a kind to warrant the rejection of the others in its favour, but rather of a kind to make us doubt the possibility of all such interpretations. The question which naturally arises is, whether by proceeding after this fashion, groups of bones might not be arranged into endless typical forms. If, when a given element was not in its place, we were at liberty to consider it as *suppressed*, or *connate* with some neighbouring element, or *removed* to some more or less distant position;—if, on finding a bone in excess, we might consider it now as part of the *dermo-skeleton*, now as part of the *splanchno-skeleton*, now as *transplanted* from its typical position, now as resulting from *vegetative repetition*, and now as a bone *teleologically compound* (for these last two are intrinsically different, though often used by Professor Owen as equivalents);—if, in other cases, a bone might be regarded as *spurious* (p. 91); or again as having *usurped* the place of another;—if, we say, these various liberties were allowed us, we should not despair of reconciling the facts with various diagrammatic types besides that adopted by Professor Owen.

When, years ago, we attended a course of Professor Owen's lectures on Comparative Osteology, beginning though we did in the attitude of discipleship, our scepticism grew as we listened, and reached its climax when we came to the skull: the reduction of which to the vertebrate structure, reminded us very much of the interpretation of prophecy. The recent delivery at the Royal Society of the Croonian Lecture, in which Professor Huxley, confirming the statements of several German anatomists, has shown that the facts of embryology do not countenance Professor Owen's views respecting the formation of the cranium, has induced us to reconsider the vertebral theory as a whole. Closer examination of Professor Owen's doctrines, as set forth in his works, has certainly not removed the scepticism generated by his lectures: on the contrary, that scepticism has deepened into disbelief. And we venture to think that the evidence above cited shows this disbelief to be warranted.

There remains the question—What general views are we to take respecting the vertebrate structure? If the hypothesis of an "ideal typical vertebra" is not justified by the facts, how are we to understand that degree of similarity which most vertebræ display?

We believe the explanation is not far to seek. All that our space will here allow, is a brief indication of what seems to us the natural view of the matter.

Professor Owen, in common with other comparative anatomists, regards the

divergences of individual vertebræ from the average form, as due to adaptive modifications. If here one vertebral element is largely developed, while elsewhere it is small—if now the form, now the position, now the degree of coalescence, of a given part varies; it is that the local reqirements have involved this change. The entire teaching of comparative osteology implies that differences in the conditions of the respective vertebræ necessitate differences in their structures.

Now, it seems to us that the first step towards a right conception of the phenomena, is to recognise this general law in its converse application. If vertebræ are unlike in proportion to the unlikeness of their circumstances, then, by implication, they will be like in proportion to the likeness of their circumstances. While successive segments of the same skeleton, and of different skeletons, are each in some respects more or less differently acted on by incident forces, and are therefore required to be more or less different; they are each, in other respects, similarly acted on by incident forces, and are therefore required to be more or less similar. It is impossible to deny that if differences in the mechanical functions of the vertebræ involve differences in their forms, then community in their mechanical functions must involve community in their forms. And as we know that throughout the vertebrata generally, and in each vertebrate animal, the vertebræ, amid all their varying circumstances, *have* a certain community of function, it follows necessarily that they will have a certain general resemblance—there will recur that average shape which has suggested the notion of a pattern vertebra.

A glance at the facts at once shows their harmony with this conclusion. In an eel or a snake, where the bodily actions are such as to involve great homogeneity in the mechanical conditions of the vertebræ, the series of them is comparatively homogeneous. On the contrary, in a mammal or a bird, where there is considerable heterogeneity in their circumstances, their similarity is no longer so great. And if, instead of comparing the vertebral columns of different animals, we compare the successive vertebræ of any one animal, we recognise the same law. In the segments of an individual spine, where is there the greatest divergence from the common mechanical conditions? and where may we therefore expect to find the widest departure from the average form? Clearly at the two extremities. And accordingly it is at the two extremities that the ordinary structure is lost.

Still clearer becomes the truth of this view, when we consider the genesis of the vertebral column as displayed throughout the ascending grades of the vertebrata. In the first embryonic stage, the spine is an undivided column of flexible substance. In its early fishes, while some of the peripheral elements of the vertebræ were marked out, the central axis was still a continuous unossified cord. And thus we have good reason for thinking, that in the primitive vertebrate animal, as in the existing *Amphioxus*, the notochord was persistent. The production of a higher, more powerful, more active creature of the same type, by whatever method it is conceived to have taken place, involved a change in the notochordal structure. Greater muscular endowments presupposed a firmer internal fulcrum—a less yielding central axis. On the other hand, for the central axis to have become firmer while remaining continuous, would have entailed a stiffness incompatible with the creature's movements. Hence, increasing density of the central axis necessarily went hand in hand with its segmentation: for strength, ossification was required; for flexibility, division into parts. The production of vertebræ resulting thus, there obviously would arise among them a general likeness, due to the similarity in their mechanical conditions, and more especially the muscular forces bearing on them. And then observe, lastly, that where, as in the head, the terminal position and the less space for development of muscles, entailed a smaller lateral oscillation, the segmentation would naturally be less decided, less regular, and would be lost as we approached the front of the head.

But, it may be replied, this hypothesis does not explain all the facts. It does not tell us why a bone whose function in a given animal requires it to be solid, is formed not of a single piece, but by the coalescence of several pieces which in other creatures are separate: it does not account for the frequent manifestations of unity of plan in defiance of teleological requirements. This is quite true. But it is not true, as Professor Owen argues respecting such cases, that "if the principle of special adaptation fails to explain them, and we reject the idea that these correspondences are manifestations of some archetypal exemplar, on which it has pleased the Creator to frame certain of his living creatures, there remains only the alternative that the organic atoms have concurred fortuitously to produce such harmony." This is not the only alternative: there is another, which Professor Owen has overlooked. It is a perfectly tenable supposition that all higher vertebrate forms have arisen by *the superposing of adaptations upon adaptations.* Either of the two antagonist cosmogonies consists with this supposition. If, on the one hand, we conceive species to have resulted from acts of special creation, then it is quite a fair assumption that to produce a higher vertebrate animal, the Creator did not begin afresh, but took a lower vertebrate animal, and so far modified its pre-existing parts as to fit them for the new requirements; in which case the original structure would show itself through the superposed modifications. If, on the other hand, we conceive species to have resulted by gradual differentiation under the influence of changed conditions, then it would manifestly follow that the higher heterogeneous forms would bear traces of the lower and more homogeneous forms from which they were evolved.

Not only, then, do we find that the hypothesis of an "ideal typical vertebra" is irreconcilable with the facts; but we see that the facts are interpretable without gratuitous assumptions. The average community of form which vertebræ display, is explicable as necessarily resulting from natural causes. And those typical similarities which are traceable under teleological modifications, would obviously exist if, throughout creation in general, there has gone on that continuous superposing of modifications upon modifications which is displayed in every unfolding organism.

Review IX.

Transactions of the Pathological Society of London; including the Reports of the Proceedings of its various Sessions from 1846–7 *till* 1856–7. Eight Volumes, 8vo.—*London.* Printed for the Society.

The study of pathology, for its own sake, commends itself to every thoughtful physician; and in the belief that union is strength, a Society devoted to the cultivation of this interesting branch of medical science could not fail to secure the active co-operation of a very numerous body of medical men, especially in the metropolis of Great Britain. Institutions for the special cultivation of pathology have now been established in most of the metropolitan and in many provincial towns of Great Britain, America, France, Germany, and Italy; but to Dublin, in this country, in 1830, must be assigned the merit of having been the first city in which a Pathological Society was organized. Encouraged by the success which appeared to attend the proceedings of such institutions, and invited by the peculiar interest which invests the topics discussed at the meetings of such societies, several medical men of London met together in the month of February, 1846, and agreed upon the issue of a circular to such members of the profession as were known to be more particularly interested in pathological studies. Having received ample encouragement to proceed in this praiseworthy undertaking, a provisional committee elaborated a plan for the organization of the Pathological Society of London, as it is now constituted and named. They invited the support and

co-operation of the profession at large, not only in London, but throughout the kingdom, in prosecuting the science of pathology in every possible way, and by all means that could increase and advance our knowledge regarding the nature of diseases. At the first meeting of the Society, held on the 20th October, 1846, there were enrolled *one hundred and six* members. It now numbers no fewer than *three hundred* hordinary and *nine* onorary members. Its popularity as a society, therefore may fairly be considered to be increasing; and when we look at the list of those who have been its presidents and office bearers, and at the list of the officers and council elected at the general meeting in January, 1857, and finally to the members of the society as a whole, we cannot fail to perceive names the most distinguished in the ranks of our profession—of world-wide reputation—men, moreover, of the largest practice, the very busiest of doctors, who nevertheless find time to devote their attention to the highest pursuits of the science of medicine, and to work hand and hand with their younger brethren, often less favoured by the emoluments of an extensive practice.

The Pathological Society of London having been in active operation during the last ten years, it may not be considered premature if we institute some inquiry as to the results which have accrued to the science of medicine, or which are likely to accrue, from the operations of this society, as exhibited in the volumes of their published Reports. In so doing, we may perhaps succeed in giving an indication of the progress of pathological science, as set forth in the 'Transactions' before us. In them we ought to find expression given to the matured opinions which are held by the most advanced British school of pathology; and as the work of the Society mainly deals with the nature of disease as exhibited in the records of morbid anatomy, we expect to find the fullest details of all morbid appearances embracing the chemistry and microscopy of morbid products, associated with lucid clinical histories of the cases which have furnished the morbid specimens exhibited to the Society, the results of the bedside investigation of disease. On a foundation such as this we might hope to see the science of pathology, in the widest acceptation of the term, elucidated and advanced by the active co-operation of the members of the Pathological Society of London.

Whatever opinion may be arrived at regarding the work done by this Society, and the general results so obtained, there can be no doubt, when we examine the records of the past ten years, that the zeal and assiduity of the members of the Society have not diminished, but rather increased. Year after year the volumes of the 'Transactions' have deservedly acquired an increasing reputation. In demonstrating the practical usefulness of this Society, there is one fact in its history which strikes us as highly significant—namely, that at the first meeting of the Society for the winter of 1855–56, on Tuesday, the 16th October, the permission of the Society was sought for by the printers of its 'Transactions,' to reprint and republish the early volumes of its Reports, then out of print. A permission was of course most willingly granted, and we quote the circumstance to show the value in which the recorded works of the Society have been held.*

Those only who have prepared and arranged pathological records can appreciate the labour implied in the preparation and publication of these volumes. The chief burden of this labour has been borne by Dr. Quain, and the Pathological Society cannot be too grateful to him for his exertions to hand down to posterity an accurate account of the work that has been done. The care and labour bestowed at an early period in selecting and arranging the material of the 'Transactions' had an immediate, and has also had a progressively beneficial effect upon the exertions of the Society. The very appearance of the records of the material brought before the Society stimulated the members to select their cases, and to give the descriptions and histories of them with more care than at first was bestowed upon them. The evidence of this will be obvious to any one who takes the trouble to compare the first volume of the 'Transactions' with the

* Medical Times and Gazette.