

XLVIII.—On the Minute Structure of certain Brachiopod Shells ;
and on Vegetable Cell-Formation. By WILLIAM B. CAR-
PENTER, M.D., F.R.S., F.G.S.

To the Editors of the Annals of Natural History.

GENTLEMEN,

University Hall, London, May 19, 1856.

PROF. KING having introduced into his "Notes on Permian Fossils," in the 'Annals' for April last, certain comments upon former statements made by me respecting the intimate structure of the shells of Brachiopods, which must, if unnoticed, tend to diminish the value attached to them by those who have hitherto relied upon my assertions, I must beg you to admit the following reply, which shall be as little personal as the tone taken by Prof. King will permit me to make it.

In the 'Annals' for December 1843, I first published the fact, which had been nearly a year previously communicated to the Royal Society, that the shells of many Brachiopods are traversed by large perforations, passing from one surface to the other, the external orifices of which may be detected as minute punctations ; and I mentioned that this character presents itself in all the recent *Terebratulæ* which I had examined, with the exception of the *T. psittacea*, which, as is now well known, has been since separated as one of the two recent types of the genus *Rhynchonella*. In the 'Reports of the British Association' for 1844, I entered much more fully into this point, embodying the results of more extended examinations into the structure of the shells of fossil Brachiopoda, and giving thirteen figures of the minute organization of recent and fossil shells of this group, drawn under magnifying powers varying from 75 to 250 diameters, by that very accurate microscopic draughtsman, Mr. S. W. Leonard. Save for a want of perfection in the printing-process, these figures could scarcely be surpassed at the present time.

In his 'Monograph of the Permian Fossils of England,' published by the Palæontographical Society in 1850*, Prof. King took upon himself, upon no other evidence than that of the examination of the *surfaces* of various Brachiopods with a Stanhope lens, to throw discredit upon my previous statements ; asserting that punctures, though much more minute than those in the *Terebratulidæ*, occur in every species of *Rhynchonella* which had passed under his notice ; and adding, "I doubt their absence in any Brachiopod whatever."

* I am obliged to call attention to this date, which I take from the title-page, for a reason which will presently appear. The work was issued as the publication of the Pal. Soc. for 1849 ; but (according to the practice of the Society) it was not delivered to the members until the following year.

Having been requested by Mr. Davidson to contribute a Memoir on the intimate structure of the Shells of Brachiopoda to his admirable Monograph of that group in course of publication by the Palæontographical Society, I re-entered upon the investigation with no desire but that of contributing to the establishment of the truth; and made microscopic sections of many additional specimens, with which Mr. Davidson kindly supplied me,—the total number of sections examined (nearly all of which are preserved in my cabinet) being about *three hundred*. In the course of this inquiry, the *presence* of perforations in *Terebratulidæ*, and their *absence* in *Rhynchonellidæ*, was established as the character of so large a number of species of both tribes, that I thought myself justified in stating these as distinctive characters of the shells of these two groups respectively. A remarkable confirmation of their validity, and an important lesson as to the fallacy of *superficial* observations upon this point, were afforded by the apparently-exceptional cases of *Stringocephalus* and *Porambonites*. The former had been previously regarded as a non-perforated genus, and had been associated on other grounds with the *Rhynchonellidæ*; examination of microscopic sections, however, satisfied me that its shell was perforated; and the letter in which I communicated to Mr. Davidson this at first sight anomalous fact, was crossed by one from him to me, mentioning that he had been led by the researches of Prof. Suess to consider the affinities of *Stringocephalus* as being rather with the *Terebratulidæ*, and inquiring as to the presence or absence of perforations; so that both sets of characters came again into complete harmony. The place of *Porambonites* being undoubtedly among the *Rhynchonellidæ*, the existence of perforations (which had been thought to be unmistakeably indicated by the very regular punctations of the surface) was an apparent anomaly of no small importance; this, however, was at once removed by the examination of microscopic sections of the shell, since it was found to be as destitute of perforations as any true *Rhynchonella*. The case of *Trematis* was one of the same kind, the punctations being there also quite superficial; constituting, in fact, a peculiar kind of ‘sculpture.’

I thought it right, in stating these and similar facts, to give an emphatic warning against *superficial* observations upon this point, and to express my surprise that Prof. King should have ventured, upon such evidence, to affirm the universal existence of perforations in the shells of Brachiopoda; especially without having examined one of the most common of the recent types of the group, namely *Rhynchonella psittacea*, in which the absence of perforations, as described and figured by me in 1844, can be verified without the slightest difficulty. “To myself personally,”

I added, "it is a matter of entire indifference whether Prof. King does or does not admit the correctness of my observations; but I would submit, that the interests of science are not very likely to be promoted by this easy setting-aside of observations made with every advantage of first-rate instruments and careful preparation of specimens, in favour of glances with a hand-magnifier at shells whose surfaces are peculiarly liable to present deceptive appearances."

As Prof. King made no reply to these observations at the time they were published, I hoped that he acquiesced in their justice, and that the question between us might be regarded as settled. It now appears, however, that I was premature; since, after the lapse of two years, Prof. King returns to the charge; not so much, however, to maintain his former assertions, as to justify himself for having discredited mine. He now admits the non-existence of perforations in *Rh. psittacea*, and, by implication, in other *Rhynchonellidæ*; but he considers the case of *Rh. Geinitziana* to be an unquestionable exception to the universality of non-perforation in that genus,—both valves of this species being "as distinctly and regularly perforated as those of any *Terebratulidæ*." By the kindness of Mr. Davidson, I have had the opportunity of examining one of Prof. King's own specimens, as well as an authentic specimen of this species which he has received from Baron von Schauroth; and I am bound to admit that both these specimens bear out Prof. King's statement, so far as can be judged by external appearance. I have not felt at liberty, however, to damage these specimens to the extent necessary for determining the question whether the superficial pittings extend through all the layers of the shell, and are therefore the homologues of the perforations of *Terebratulidæ*. Supposing, however, this should prove to be the case, it would still have to be determined whether, in spite of its external characters, this species be a true *Rhynchonella*, or whether it should be separated as a sub-type of that genus, which, like *Spirifer*, may contain both perforated and non-perforated species, or whether, like *Stringocephalus*, it should be found to be more related in its internal structure, as well as in the texture of its shell, to the *Terebratulidæ*.

The question of the accuracy of my observations on this point is one quite distinct from that of the accuracy of my generalizations. I have given, in my Memoir, the evidence on which the latter seemed to me to be at least provisionally established; but I myself remarked at its conclusion, on the necessity of a far more extended examination of species than I had been myself able to make, before these generalizations could be regarded as established. I shall be, therefore, as ready as any one to with-

draw them, when they shall be proved to have been premature ; but until the structure of the species now in question shall have been fully investigated, I must claim a suspension of the verdict.

Prof. King attempts to justify his scepticism as to my former statement of the non-perforation of certain Brachiopods, on the plea that "fossilization had so obliterated the tissue of many shells, as to render the detection of it an impossibility ; and it was also conceived, that some shells were more prone than others to become thus altered." This argument is, of course, quite inapplicable to the case of *Rh. psittacea*, which I had described as the type of the non-perforated group. Further, it will be seen on reference to pars. 36 and 44 of my "Report" for 1844, that I distinctly recognized the existence of this metamorphic action as obscuring the structure of certain shells of this group ; and I have never spoken confidently about the presence or absence of perforations, save where the intimate structure of the shell was so perfectly preserved as to leave no possible doubt about the matter. Where the place of the passages which exist in Prof. King's imagination is found to be occupied, not by fossilizing or metamorphic substance, but by the peculiarly characteristic structure of the Brachiopod shell, I venture to affirm that there can be "no mistake."

The greater part of Prof. King's note, however, seems intended to turn the tables upon me, by showing that my original account of that structure was so incorrect, as tested even by my own subsequent description of it, that no confidence whatever was to be placed in it ; and also, to claim for himself the merit of setting me right. I shall not occupy your space by a detailed justification of myself as to this matter, but shall simply draw attention to the following points.

In my original "Report" I did not minutely describe the peculiar microscopic *appearances* of these Brachiopod shells, considering that my figures spoke for themselves ; but the special object of that "Report" being to establish the organic structure of Shell, I offered an *interpretation* of them (based on the idea of plications in the shell-membrane), which at that time seemed to me to be borne out by the facts I had ascertained by the decalcification of recent shells and examination of the organic residue. Subsequent examination having led me to doubt the validity of this interpretation, I did not reproduce it in my "Memoir" of 1854, but confined myself to a description of the *appearances*, which will be found to be accordant in all essential particulars with my figures of 1844. As I never saw the Memoir of Vicomte D'Archiac referred to by Prof. King, it is not to that accomplished palæontologist that my abandonment of my former heresy is attributable. And that Prof. King has

no ground for charging me with adopting *his* corrections without acknowledgment, will appear from the following quotation from the article "Shell," which I contributed to the 'Cyclopædia of Anatomy and Physiology' (vol. iv. pp. 563, 564):—"When thin sections are microscopically examined, they present a very peculiar texture (shown in the figure), which might be referred either to long flattened cells, or to plications in the shell-membrane. . . . The cells, if cells they be, must be excessively flattened, and no vestige of them can be traced in the decalcified shell; whilst, on the other hand, *the membranous residuum does not give any distinct indication of having been plicated with the regularity necessary to produce such a remarkable appearance.*" Now this passage was written in 1848 or early in 1849, consequently long before the publication of Prof. King's Monograph.

I must trespass a little further upon your space, for the purpose of requesting your readers to suspend their judgment upon the question on which Prof. Henfrey has pronounced (in your last Number, p. 417) a very positive opinion in opposition to mine,—namely, the value to be attached to Mr. Wenham's observations on the process of cell-development in plants. No one has a higher estimate than myself of Prof. Henfrey's acquirements in vegetable physiology; but since I happen to know that Mr. Wenham's conclusions are borne out, as to certain important particulars, by the testimony of other independent observers, who will probably ere long make public the facts they have witnessed, I venture to believe it possible that Prof. Henfrey may be mistaken. What I considered to be the essential point in Mr. Wenham's observations was this,—that a mass of protoplasm may resolve itself into cells by a process of vacuolation in the parts which are to be the cell-cavities, and of consolidation in those which are to become the cell-walls, essentially corresponding with that which takes place in the development of a single cell from a "gonidium" or any other isolated particle of protoplasm. That this doctrine does not agree with Prof. Henfrey's general ideas of the process of cell-formation, is no more proof that it is wrong, than the denial of the sexual nature of the antherozoids of Cryptogamia by Prof. Schleiden proved that doctrine to be invalid. When Mr. Wenham's observations shall have been *shown* to be incorrect *as to the essential point just mentioned*, I shall be quite ready to retract the "endorsement" which I gave to them.

I am, Gentlemen,

Yours sincerely,

WILLIAM B. CARPENTER.



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