

Mr. Owen's rejoinder to Mr. Nasmyth / [Richard Owen].

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MR. OWEN'S REJOINDER TO
MR. NASMYTH.

To the Editor of the Medical Gazette.

SIR,

IN the case of a scientific discovery or theory, enuntiated in simple and intelligible terms, nothing seems easier to prove than a priority. To ascertain the dates of publication of the respective works—to determine the degree of correspondence in the definitions or descriptions in question—are the simple processes by which the case is settled.

Whether, besides the undoubted merit to be awarded to the earlier author, the stain of plagiarism is to be affixed to the later writer, must be a more complex and difficult question for the upright and just mind to determine upon. It must depend on the degree of correspondence in the terms in which the discovery is narrated; on the intellectual character and pursuits of the later author, with reference to the probability or otherwise of his originating independently the same idea or theory. Lastly, on his known moral character and habits.

With respect to the theory of the "formation of dentine or ivory by calcification of pre-existing and pre-arranged cells in the substance of the pulp," the right of the author of the memoir communicated to the French Academy, 16th December, 1839, was called in question in two distinct communications, which appeared in the Gazette and Lancet for June 5th and 6th, 1840; and the priority being assumed to be proved against that author, his relation as a plagiarist from the earlier one seemed in one of the journals to be taken for granted, and coarse abuse was lavished upon him.

It was urged in defence, that, of the works quoted as plagiarised from, the one offering the nearest approach to a correspondence with the new theory could not possibly have been in possession of the author accused, being posterior in date to his work; and it was proved that the passages quoted for the same purpose from the accessible or anterior works were incorrectly quoted, as well as the professed parallel passages in the work of the accused author.

But it was assumed by the accusers that the earlier and later works of the author whose interests they defended

were essentially the same, being exponents of the same views, and, in short, different forms of the same memoir. This assumption led to a demonstration of an important discrepancy in regard to the theory in question, and to the conjecture of the existence of many others, between the earlier and the later forms of the memoir alleged to be plagiarised from.

Moreover, the later memoir, quoted as containing the statement "that the ivory was an organic deposition of ossific matter in the pre-existing cells of the pulp*," being in the condition of a privately circulated pamphlet, and no copy of it having been transmitted to the accused author, he was compelled to take the best means he could to obtain a sight of it, that he might compare its tenour and amount of resemblance with the earlier and only published forms of the (alleged) same memoir.

And now, having stated the case in question as it stood after my answer to the first charges against me, I may observe that my letter to the officer superintending the publication of the Transactions of the British Association, of which the privately circulated memoir professed to be a part, was a necessary step and consequence of the unfair manner in which I had been attacked—as it were, in the dark. I thought it fair to Mr. Nasmyth to print that letter, and at the same time, however strongly suspicion might point to his having had a hand in the simultaneous and anonymous attacks, yet the private and altered memoir might have come into the possession of a third person, without Mr. Nasmyth having any cognizance of or consent in the abuse, to which he must otherwise have known that the new paragraphs in that altered memoir were to be applied.

The least offensive suggestion as to the motive of such alterations was therefore, obviously, that the memoir about to appear should be *au courant*. And I was glad to read the explicit negation of any concernment in the attacks in question, with which Mr. Nasmyth commenced his letter in your number for June 26th, p. 545: although many of your readers, like myself, must have been at a loss to understand how Mr. Nasmyth's forbearance should have been particularly trespassed upon during the operation of

* Lancet, June 6, p. 378.

the corrected memoir, you will be able to vouch for the accuracy of my quota-

tions; and you will observe that the italics are Mr. Nasmyth's.

Version of August, 1839.

"He hoped, by a few facts, which he thought his investigations had placed beyond doubt, to pave the way for a satisfactory explanation of the formation of dental bone. On the surface of the pulp, he observed, are found innumerable detached cells, with central points."—*Literary Gazette*, Sept. 21, p. 598.

The description of cells with central points, or nuclei, lying on the surface of the pulp, is taken from Schwann. Their detached condition is an idea of Mr. Nasmyth's. How came a closely-packed layer of detached cells to be situated between the thin ossific layer of tooth and the formative surface of the pulp? Were they excreted, or exuded, or evolved from the surface below, or deposited on that surface by the ossific layer above? Perhaps Mr. Nasmyth will explain. I have shewn that these detached nucleated cells are *not* a natural state of things, but the result of a violent abrogation of the relation which subsists between the calcified and uncalcified portions of the pulp of the tooth. My observations and arguments to this effect have not been wholly lost on Mr. Nasmyth; yet he is unwilling quite to abandon Schwann's cells, which he describes as detached. Nevertheless, I affirm, in accordance with my theory of December last, that no part of the ivory of a tooth is at any period due to the calcification or transition of detached cells on the surface of the pulp, and that the restriction of such a condition of the pulp cells *to the young tooth at the*

Diffuse Memoir of August 1839.

"A comparison between the superincumbent perfect ivory and the formative surface of the pulp beneath is always easy."—*Literary Gazette*, l. c.

The succeeding paragraphs in the old version, descriptive of the deposition of the ivory in thin ossific layers on the surface of the pulp, and the consequent facility of their removal, are omitted.

Uncorrected Memoir, August 1839.

"It appeared to him that the framework of the reticulations or cellulæ of the pulp is constituted by the fibres of the

Version of June, 1840.

"He should confine himself to a few facts which he had established, and which he hoped would throw some light on the subject. In the young tooth, he said, at the period of the formation of the first layer of ivory, there are found on the surface of the pulp innumerable detached cells, with central points."—*Unpublished proof.*

period of the formation of the first layer of ivory, although it may bring Mr. Nasmyth's Memoir of June 1840 something nearer to mine of last December, is yet, so far as it goes, as remote from the truth as the statement of August 1839, in which this detached condition of the nucleated cells on the surface of the pulp is applied, without reservation, to teeth of every age, and through the whole period of the formation of the ivory.

When Mr. Nasmyth, following Purkinje and Schwann, instead of nature, described the formative surface of the pulp as presenting a general cellular arrangement, which he illustrates by the simile of skeletons of desiccated leaves, he did not know that he was stating what was the result of artificial and violent laceration, instead of describing a natural formative surface. But, as if some suspicion of this fact had occurred to him in the course of his investigations of my Memoir, he, contrary to the assertion of your reviewer (*l. c.* p. 593), does drop the word "formative" in the following passage in the new or corrected memoir, as I suspected he would.

Concise Memoir of June 1840.

"A comparison of the superincumbent perfect ivory with the surface of the pulp beneath is always easy."—*Unpublished proof.*

(Compare *Lit. Gaz.* p. 598, with the private copy, or proof now standing in type.

The next statement is retained, but altered.

Corrected Memoir, June 1840.

"The *fibres of the tooth*, as it appeared to Mr. N., *are derived from the framework of the reticulations.*"—*Un-*

The unsuspecting reader might not esteem this transposition of cause and effect of much moment; not so, however, Mr. Nasmyth; he has craft enough to perceive that the alteration of this passage is so important for his design upon my theory of dental development, that he prints it, as garbled for that purpose, in italics, at least in the proof of his memoir as it now stands in type, and, I suppose, in the private copies which he has already distributed.

If, in fact, the cellular reticulation which the torn surface of the pulp presents when the superincumbent cap of ivory is displaced were a natural formative surface, and if the ivory were deposited upon it, as all writers preceding me, and among them Mr. Nasmyth, declared, either in the form of thin ossific layers, or spiral fibres, or by the transition of nucleated cells lying detached on the pulp's surface, then the ivory so deposited would impress the plastic surface of the pulp with its characteristic sculpturings or inequalities; and, so deeming, the writer would be correct in saying, as Mr. Nasmyth stated in August last, that the reticulations or the frame-

work of the cellules of the pulp were constituted by the fibres of the tooth. But on the theory of conversion or ossification of pre-existing cells in the pulp's substance the proposition must be reversed, as in the new version.

No one will believe it to be an accidental coincidence that the anonymous reviewer in the *Lancet* (June 6, p. 378), though quoting the paragraph in question from the *Literary Gazette*, and with conviction of his transposition inevitable, whenever the quotation should be compared with the original—I say, no one can believe that this hypothetical personage should, in order to establish a plagiarism for the benefit of Mr. Nasmyth, garble the passage precisely as Mr. Nasmyth, for the same purpose, has himself garbled it in his corrected proof of the forthcoming memoir, and, I suppose, in the private copies.

To prove that the reticular impressions of the (so called) formative surface of the pulp were due to the fibres of the superincumbent ivory, Mr. Nasmyth originally adduced their correspondence in diameter; and the paragraph assumes the following form in the new version:—

Old Version, August 1839.

“It appeared to him that the framework of the reticulations or cellules of the pulp is constituted by the fibres of the tooth, which, while in this state, are spirally coiled, and fit into one another. At all events, the diameter of these fibres of the reticulations is precisely that of the fibres of the ivory.”—*Literary Gazette*, p. 598.

New Version, June 1840.

“The fibres of the tooth, as it appeared to Mr. Nasmyth, are derived from the framework of the reticulations; at any rate, the fibres bounding the reticulations are precisely analogous in diameter and direction to the subsequent fibres of the tooth.”—*Unpublished proof*.

Now, one of the arguments by which I supported my theory of dental development in December last, was the correspondence of the linear series of pre-existing cells in the pulp's substance, in direction, with the subsequent tubes of ivory, into which they were converted. The anonymous reviewer in the *Lancet*, of June 6th, whom I take to be Mr. Nasmyth, seemed to think that ‘a correspondence of direction between given parts and those into which they were to be converted,’ and ‘a correspondence of size between certain impressions and the parts producing them,’ were identical propositions. But, in order to insure an agreement between his memoir and mine, Mr. N., now, not only reverses the relation of cause and effect, but also the order of appearance of the parts com-

pared: the fibres of the ivory, now, not only do not constitute the framework of the reticulations, but they are subsequent to them in existence; how they should also be parallel to them in direction, Mr. N. does not explain. According to my observation, the calcigerous tubes of the ivory stand at right angles to the direction of the lines which form the reticulations or cellules which the lacerated surface of the pulp presents. In Mr. Nasmyth's proof, however, as it now stands, and doubtless in the private copies, the writer states that he exhibited in August last at Birmingham, “diagrams in confirmation of this view”!

As to the paragraphs which next follow in the *Literary Gazette* report, and which include the illustrations of Mr. Nasmyth's notions of dental develop-

ment by deposition of thin ossific layers of ivory, or the transition of peripheral layers of detached cells with central points on the pulp's surface, and which illustrations he adduces from the known facts of the laminar decomposition of the mammoth's tusks, and the alternate strata of red and white produced by Hunter's experiments with madder, and which Mr. N. asserts to be a result incompatible with any other theory—as these paragraphs, I say, are too stubborn to be made to tell the new story by any process of transposition and mutilation, they are wholly omitted in the proof now in type: in the published abstract, prepared, despite his quibble, by Mr. N. himself, they occupy one column and a half of the closely printed page of the *Literary Gazette*, l. c. p. 598: thus we have the curious phenomenon of the abstract being longer than the memoir; the part greater than the whole.

I thought my conjecture, in my letter to Professor Phillips, as to the requisite extent of mutilation to produce con-

Inconvenient Memoir, August 1839.

Schwann regards the dental substance as the ossified pulp, whilst Mr. N.'s observations lead him to conclude that the cells of the ivory are altogether a distinct formation.—*Literary Gazette*, p. 598.

To render this sudden conversion as decent as possible, Mr. Nasmyth looks about in the original memoir, as printed in the *Literary Gazette*, for the best point from which to jump to the new conclusion, and finds it in the proposition enuntiating the correspondence of size and appearance between the cells of the pulp, previously defined as lying detached on the surface, and the cellular fragments of ivory. And here again the new light is blazoned forth by Mr. N. in italics: he is unwilling to leave the discovery of his last adopted opinion to the chances, perhaps remote, of a regular perusal of his corrected memoir, but with all the tact of a quack advertiser, he takes care it shall catch the eye. But he is hasty: in August last, the cellular ivory was anything but the ossified pulp; now it is neither more nor less. My memoir of December last teaches, however, that the ivory is in some respects more than the ossified pulp, and in others less; and that its formation obeys peculiar laws, which differ from those of ordinary ossification. So that before the

formity, somewhat hazardous; but the quantity of unsound parts in the original memoir which the author has found it essential to excise, surpasses all anticipation.

From the passage enuntiating the correspondence in size between the detached peripheral cells (cells of the pulp, in Mr. N.'s present proof) and those of the ivory, he proceeds at once to discuss the relation between the formation of ivory and ossification of pulp. All that Mr. N. advances respecting detached cells on the surface of the pulp, and the part these cells play in the formation of ivory, is taken from Schwann; but there were ideas broached in that excellent observer's treatise, which ran counter to Mr. N.'s notions of dental development in August last, and not anticipating the new light that was so suddenly to break in upon him, I find the following amount of correspondence between his paragraph touching the ossified pulp in the memoir of August, 1839, and June, 1840:—

Convenient Memoir,* June 1840.

“He concluded, therefore, *that the ivory is neither more nor less than the ossified pulp, and that it can in nowise be considered as an unorganized body.*—*Unpublished proof.*”

new version goes to press, Mr. Nasmyth must again refer to my memoir, and again correct his proof, in order, in the disguise of an anonymous reviewer, to substantiate his charge of plagiarism. In that character, in the farce of the review of my *Odontography*, again performed in your 41st number, (July 3d,) Mr. Nasmyth still harps upon the analogy which exists between his memoir, as printed in the *Literary Gazette*, for Sept. 21, and mine of the December following. Formerly he illustrated this analogy in the propositions respecting the organized structure of the teeth, and he has since offered no explanation or excuse for the unprincipled manner in which he misquoted my memoir to establish this analogy. He now cites another example in regard to the internal structure of the pulp. Need your readers be informed that I referred in my memoir to the anatomist Purkinje, to whom we are indebted for our knowledge of the granules

* “It was the most convenient and concise, as well as the only corrected document for me to refer to on the subject.”—Nasmyth, *Lancet*, July 11th, p. 567.

or cellules of the pulp, and that my business was to show, not the structure of the pulp, but how the pulp was concerned in the formation of the ivory. If Mr. Nasmyth can delude any one into the belief that he discovered either the cells of the ivory or the pulp, then let him represent me to that person as doing Mr. Nasmyth great injustice by not acknowledging him as the discoverer.

Nr. Nasmyth now asserts, "that in his papers, as reported in the Literary Gazette and Athenæum, he described the teeth to be formed by a process of ossific 'transition,' and consequently as not being inorganic in any sense of the word."—(Lancet, July 11th, p. 569.) I never said that he, or any writer like him, had denied the teeth to be organic. My memoir is asserted to be borrowed from those reports of September last, because it treats of the formation of ivory by transition. But the question is, transition of what? Of cells in the pulp's substance, or of cells transuded in layers from the pulp's surface, and lying detached on that surface? Oh! but says Mr. Nasmyth, I never used the word "transuded." Well, then, "evolved" from the surface of the pulp. These detached cells must have been secreted or excreted either from the surface below or the surface above, between which they are said to be detachedly situated. If Mr. N. said "that the cellular fragments found upon the surface of the pulp are in size and appearance perfectly accordant with the cellules of the pulp," he could be supposed to mean no other cellules than those which he had defined in the preceding paragraph. But the transition of detached cells, belonging to the pulp, in no other sense than as being transuded from it, is not ossification of the pulp itself. No; Mr. Nasmyth takes care that the readers of his Memoir of August last should not fall into that mistake; and he, therefore, expressly states, "that the cells of the ivory, instead of being the ossified pulp, are altogether a distinct formation." They were so far distinct in Mr. Nasmyth's mind in August last, that they formed a loose and detached layer on the pulp's surface, and were there converted into ivory; which is nothing more than a combination of Schwann's doctrine of the development of tissues by the metamorphosis of cells, with the old ideas of the transudation of the ivory in thin ossific layers from the surface of the pulp. If Mr. Nasmyth re-

ported in August last, that the ivory was formed by "ossific transition or transformation of the pulp's substance," why does he not settle the matter, and refer to any one passage containing that statement, which your readers might verify in either the Literary Gazette or Athenæum, or in any other work, whence it would have been possible to have borrowed it from him before the December following?

But the reviewer, well knowing the amount of Schwann's treatise which is smuggled into the memoir, which Mr. Nasmyth calls his own, is sure that there must be some correspondence between it and mine, and that injustice must be done to his friend by my passing him over in silence. Besides, Mr. Owen himself calls it an *able* memoir. Granted; but what is the nature of the ability which it displays? Ability to an unexampled extent in the amount of plunder engrafted upon it. Let me, according to my custom, give proof of this assertion.

Mr. Nasmyth, as is generally known, abstracted largely, in 1839, from a work by Schwann published in that year, and which I did not see till the following one. But then he abstracted, I find, only those passages which appeared to favour the ideas of dental development which he entertained in August last. These passages enunciate Schwann's notions of the formation of the cellular ivory by the transition or calcification of cells on the surface of the pulp, and which Mr. Nasmyth defines as detached cells: these by-Nasmyth-plagiarised observations, occupy the whole of page 125, and part of the preceding and succeeding pages of Schwann's "Mikroskopische Untersuchungen Ueber die Uebereinstimmung der Thiere und Pflanzen, 8vo. 1839." In Mr. Nasmyth's memoir of August last the wholesale plunder loads more than a column of the 598th page of the Literary Gazette (Sept. 21.) From the passage beginning with "According to Purkinje," and ending with "and of the dental bone," the report of Mr. Nasmyth's memoir, excepting that where Schwann asserts "Mr. Nasmyth observes," and that where Schwann believes "Mr. Nasmyth presumed," is a coarsely literal translation of the German author; including the only fallacious observations perhaps which could have been picked out of his book. The following are specimens of Mr. Nasmyth's originality:—

Schwann, l. c. p. 125.

"Diese in die Länge gezogenen Kugelchen sind nun offenbar cylindrische Zellen."

"Da sie auf der anderen Seite doch mit der Zahnschubstanz fester zusammenhängen als mit der Pulpa, und an der ersteren hängen bleiben *so vermuthet* ich, das hier ein Uebergang statt findet."

But if he had observed for himself, instead of plagiarizing from Schwann, Mr. Nasmyth might have escaped this error, the nature and source of which my observation on the development of the shark's teeth enabled me to demonstrate.

Then again, when Schwann admits

Schwann, l. c. p. 126.

"Gegen die Ansicht, dass die Zahnschubstanz der verknöcherte Theil der Pulpa ist, hat man die leichte Trennbarkeit beider von einander eingeworfen, und ich erkenne das Gewicht dieses Einwurfs wohl an."

It might be here pleaded that the reporter mistook Mr. Nasmyth, and thought he was speaking of his own sense of the force of this objection, when he merely meant to quote Schwann's; but what does Mr. Nasmyth ascribe to himself, after having read my memoir proving the dentine to be in a certain sense the ossified pulp? Why the plagiarism in the *MEDICAL GAZETTE* (Jan. 3d, 1840, p. 541.) then assumes the following form:—"Gegen die Ansicht," is now rendered "Against *my* theory;" and Mr. Nasmyth makes himself address the Medical Section at Birmingham as follows:—"Against my theory," said Mr. Nasmyth, "that the dental substance is the ossified portion of the pulp, the facility with which the one is separated from the other has been adduced, and he allowed the force of this objection." And then Mr. Nasmyth proceeds, nevertheless, literally to plagiarise the remark which Schwann offers as invalidating the force of this objection, suggested by the well-known fact of the facility with which the ossified separates from the unossified portion of bone-cartilage. Now I had myself invalidated Müller's argument drawn from the slight cohesion of the ivory to the pulp by the same obvious analogy of growing bone, but in other terms, long before I had

Literary Gazette, l. c. p. 598.

"These longitudinally drawn out globules, Mr. Nasmyth observed, are plainly cylindrical cells."—*Also Medical Gaz.*, Jan. 3d, p. 540.

"As they cohere more firmly with the dental substance than with the pulp, and remain attached to the former, Mr. Nasmyth presumed that here a transition takes place."—*Literary Gazette*, p. 598, and *Medical Gazette*, p. 541.

the validity of an objection to the theory of the ossification of the pulp, which I prove to have no weight at all, Mr. Nasmyth likewise, in August last, admits its force in the words of his author, and again with a modified version in January 1840.

Literary Gazette, l. c. p. 598.

"Against the theory that the dental substance is the ossified portion of the pulp, the facility with which the one is separated from the other has been adduced; and he (Mr. N.) allowed the force of that objection."

seen Schwann's excellent work. It is quite natural, therefore, that a dull-witted plagiarist, who never originated an illustration or idea in his life, should set down at once such a coincidence as necessarily standing in the relation of cause and effect; but I am much mistaken, if Mr. Nasmyth, by the time he has reached the end of this letter, does not discover that I am not the man who needs to be indebted to any one for an apt illustration.

And now such of you readers as have not been favoured with a private copy of Mr. Nasmyth's correct memoir, and know it only through Mr. Nasmyth's version, printed entire in the *Literary Gazette* for September 21, 1839, may be curious to learn how he disposes of the closely printed column of consecutive plunder from Schwann, since the new theory in the "*Comptes Rendus*" rendered this acquisition useless and unvendable. Here was a new difficulty; the plagiarized column would not tally with the new views. And what was to be done with this large and important share of the memoir of August 1839, when it came to be converted into the memoir of June 1840, became, doubtless, the subject of anxious cogitation. Mr. Nasmyth might have put a bold face upon the matter, and averred, as he before did in the case of similar difficul-

ties, that it was all a mistake of the careless reporter for the Literary Gazette; and that, instead of Mr. Nasmyth's 'observing' and 'presuming' in the borrowed wisdom of Schwann, at the Medical Section at Birmingham, to the extent of sixty consecutive lines of close print, he was actually quoting and refuting Schwann's paragraphs touching the transition of the superficial cells; but then he must have charged the reporter for the MEDICAL GAZETTE with precisely the same blunder. And, more provoking still, there was the unalterable text of the Literary Gazette ready at all times to prove that Mr. Nasmyth had exaggerated Schwann's doubtful leanings to the true doctrine—ossification of the pulp—in order to refute them.

What, then, was to be done? Why, he quietly drops the column of plunder from Schwann, when he finds it no longer worth keeping; and the new memoir, as it appears in the proof sent to me, is thus shorn of another third of its unfair proportions, as printed, in abstract, in the Lit. Gaz. So now the discovery of the formation of ivory is enunciated in the clearest and briefest terms; and instead of the reader being troubled with any of the inductive processes by which it was established, he is at once put into possession of it by the attractive artifices of type: "*The ivory*," says Mr. N., "*is neither more nor less than the ossified pulp.*"

Acquainted now with the mode in which Mr. Nasmyth's original memoir was fabricated, let us examine the validity of one of the statements which he makes in his recriminatory epistle in your forty-second number. He there disowns the report in the Literary Gazette; if we are to believe him, it is a *bonâ fide* report, subject to the ordinary inaccuracies of such productions. Yet, in sixty consecutive lines of the abstract of Mr. Nasmyth's communication to the Medical Section of the British Association, the supposititious reporter attributes to Mr. Nasmyth, observations, beliefs, and presumptions, without deviating a tittle from the text of Schwann, otherwise than in decorating the daw with his pilfered plumes. And again, either you, Mr. Editor, must have had the services of the same hypothetical reporter, or else we have the marvellous fact of two *bonâ fide* reporters reporting discoveries as narrated by an English soi-

disant discoverer in the accidental form of a literal German translation: for the same page of Dr. Schwann gives occasion to the same observations and presumptions of Mr. Nasmyth in your pp. 540 and 541, of the Gazette for Jan. 3d, 1840, as in p. 598 of the Lit. Gazette.

But if Mr. Nasmyth has thus unscrupulously plundered Schwann, he ought, in consistency, to abuse and damage him as far as he is able. And this he accordingly does, for before the plagiarism begins he informs the Medical Section at Birmingham that "Schwann acknowledges, that he is ignorant of the process of transition, and he regards the dental pulp as a simple cartilage. In fact, "he starts with a ready-made hypothesis, and founds his opinion rather on the observations of others, and on the inferences he draws from them, than on his own actual researches: with respect to what he himself gives as his own, it accords for the most part with the details Mr. Nasmyth had first communicated." So poor Schwann is obliged to get even his hypothesis ready made to his hand. Modest plagiarist! your fate has been a just one, to steal whatever was apocryphal, and to reject those ideas of your author, which, if understood, might have conducted you into the right track to the theory of dental development.

But being opposed altogether to the doctrine of ossification of the pulp in August last, Mr. Nasmyth exaggerates every statement of Schwann's which leans towards that doctrine, *in order that he may refute him*. Thus he makes Schwann "regard the dental pulp as a simple cartilage." But, if Mr. Nasmyth had procured the whole of Schwann's treatise to be translated, he must have known this to be a false statement; and your reviewer, so far as he is not Mr. Nasmyth, and who, to that extent, must be presumed to possess the requisite learning for his craft, must know it to be untrue, although he quotes it, at p. 594 of your number for July 3d, as if it were not doing a great injustice to Schwann so to represent his statement. Schwann's word, are "The pulp agrees with all the other organs of the fœtus, and with cartilage, inasmuch as it is cellular. It differs in consistence from mammalian cartilage, inasmuch as the quantity of cyto-blasts (nucleated cells) which gives to cartilage its hardness, is very small, whilst there are numerous cylindrical cells lying

close together upon the surface of the pulp.

Again, Mr. Nasmyth drags Schwann's dubious expression of his inclination towards the ancient doctrine of the tooth being the ossified pulp, from a remote part of Schwann's treatise, converts it into a positive affirmation, and places it in juxta-position with the similarly falsified statement of Schwann's ideas of the relation between the dental pulp and cartilage; whereby the readers of the *Literary Gazette* are led to infer, that Schwann regarded the formation of tooth and bone to be identical processes, which Mr. Nasmyth accordingly refutes and denies, in August last, and now admits and affirms. And your reviewer, to reconcile these discrepancies, assures us (July 3d, p. 596.) that, when Mr. Nasmyth says the cells of the ivory are altogether a distinct formation from the ossified pulp, he means no part of the dental substance at all; and yet the title of his memoir, in the new proof, is, on the cellular structure of the ivory, &c.; and the conclusion to which Mr. Nasmyth arrives at the end of his (?) observations on dental development, is thus recorded by himself in the *Athenæum*, (Sept. 14th, p. 707.) "But in whatever respect," said he, "we view the formative organs of the tooth, and the dental tissues themselves, and whether we examine the latter during the process of their development, or after their formation has been completed, we are every where met by appearances which denote a cellular or reticular arrangement."

The case is this: Mr. Nasmyth, in August last, steals Schwann's account of the nucleated cells on the surface of the pulp, and so much of the theory of their transition into ivory as he can comprehend: he sees these observations only through the distorted medium of his old notions of the deposition of the ivory by successive thin layers upon the formative surface of the pulp; he accordingly makes the superficial cells "detached," and strangely jumbles together the doctrines of transudation and transition. Wherever he finds Schwann verging towards the true theory, that of conversion of the pulp's substance, he gives him a lift. He makes Schwann, *e. g.*, whilst indicating the difference between cartilage and dental pulp, assert their identity: and where Schwann modestly expresses his tendency towards

the ancient doctrine, "Ich möchte mich zu der älteren Ansicht hinneigen, dass die Zahnsubstanz die verknöcherte Pulpa ist," *l. c.*, p. 124, — this Mr. Nasmyth metamorphoses into an asseveration "that the dental substance is the ossified pulp," in order that he may display his superior knowledge, in August last, by its refutation.

The only passages on the development of the teeth attributed to Schwann by Mr. Nasmyth in the *Literary Gazette*, are quoted either to receive Mr. N.'s refutation, or to expose Schwann's ignorance: with regard to the details in which they agree, Mr. Nasmyth asserts that he first communicated them! and then Mr. N. proceeds to observe and to presume, for a whole column of closely printed type, in the very words of Schwann. And if he still affirms all this to be the mistake of the reporter, why did he not rectify it, divest himself of the borrowed plumes, and give Schwann his due in the next number of the *Literary Gazette*, instead of perpetrating the same dishonesty towards Schwann, aggravated by the additional spoliation of my memoir, in the *MEDICAL GAZETTE* of the January following?

Had Mr. Nasmyth consulted Nature instead of Schwann, he would have escaped the influence of Schwann's errors, and might have advanced further into the track of truth, instead of retreating from it; and he might have saved himself from the necessity of now tumbling awkwardly into this track, affirming vehemently what he denied a few months before I had demonstrated its truth.

That the dentine or ivory is the ossified pulp, is, as Schwann observes, an old opinion; but an opinion is not a theory. Almost every true theory has been indicated, with various degrees of approximation, before it was finally established: but the best philosophers agree "that he discovers who establishes the true theory." Whether the laws of dental development by centripetal calcification of pre-existing and pre-arranged cells in the substance of the pulp were discovered by Mr. Nasmyth or myself, physiologists will not be slow to discover. I should never have taken up the pen in defence of my right to that discovery, had it not been made the occasion of plotting and aiming at me certain slanderous bolts, which it will

now be seen have fallen wide of their mark, or to have glanced back on the shooter.

So much I have thought it requisite to premise in illustration of the character of the author who has foolishly sought to fasten that suspicion upon me, and of the mode in which his memoir of August last was manufactured. I have, likewise, given sufficient evidence in proof of the same author's tamperings with this same memoir, as he proposes it to appear in the Transactions of the British Association, vol. viii.

How Mr. Nasmyth has managed to anticipate the publication of the Transactions of the Association, at whose expense the memoirs of the contributors are printed, he will have to explain to the Council of the British Association. Such an act, in reference to the publication of either the Geological or Linnean Societies, would be attended with the expulsion of the anticipator, if even the surreptitious forestalling of the information, on the freshness of which the sale of the volume depends, had been motivated only by impatient vanity, and not perpetrated with a view to literary theft and anonymous slander.

It is in vain that Mr. Nasmyth, who referred, without reservation, to the accurate Reports of the Athenæum and Literary Gazette, when they were to establish a charge of plagiarism against another, now repudiates them, when he finds they bring that disgraceful act home to himself. He must be content that his views of dental development, in August last, be gathered from those journals with no other allowance than for such inaccuracies as he may be able to prove to the satisfaction of an impartial person. His *assertion*, though shouted in italics, will fail in stunning any one into the belief that his memoir "*given in the Transactions of the Association contains no interpolations whatever, nothing which was not contained in the papers themselves.*" Even CAPITALS would not convert the explicit negation of a theory into its emphatic adoption, unless, indeed, Mr. Nasmyth means now to deny that the ivory is cellular, and that cells form any part of its substance.

But it is plain that Mr. N. feels that the verdict of guilty is about to be returned against him, and, like too many unhappy men in the same predicament, he adopts the recriminatory line

of defence. He takes my memoir, as published in the *seventh* (he still harps upon the *eighth*) volume of the Trans. Brit. Association, compares it with the abstract printed in the Athenæum, &c. and discovers that the two differ. He finds that my abstract contains less than the whole of which it was a part; and amongst the additional matter, he seizes upon a specification of certain discoveries of two industrious German investigators, which I had acknowledged only in general terms in the previously published abstracts: he further finds that the special testimony which I bore to the accuracy of the discovery, and the extension of its application, of which no notice appears in the abstract, are mentioned in the memoir. Here's a flagrant case of interpolation, cries Mr. N. See how flagitiously this practice was carried on in 1839, says your reviewer! Strange, that in illustration of the odious system, they should be able to select, out of the additional matter that my memoir in the Transactions may contain, that passage only which illustrates a disposition and purpose the very reverse of those of which Mr. Nasmyth has been convicted: that these two persons, if they be two, should take the trouble to prove that my additional paragraph goes to ascribe to a contemporary the merit of his rightful discovery, instead of to deprive him of it.

I suppose the passage was selected because Mr. Nasmyth thought he had proof that it must have been a subsequent interpolation; and, reckless of the forfeiture of caste and character which follows gross violation of the confidences of social life and gentlemanly intercourse, he prints, without my knowledge or consent, my note dated April 1st, 1839. And what does he gain by it? He is thus able to prove that, as soon as I had it in my power to lend a copy of Purkinjé and Fraenkel's Treatise, I lent it to Mr. Nasmyth. This was, perhaps, an act of folly; but more foolish things have been done, even on the first of April, than rendering a civility to an ill-conditioned and thankless person.

I had studied, Mr. Editor, and was well acquainted with the discoveries of Purkinjé and Fraenkel, more than a year before I had a copy of my own to lend to any body. Your readers will find admirable notices of all their discoveries in dental anatomy in the

Jahresbericht, for 1836, of "Müller's Archiv. für Physiologie, 1837," and in "Froriep's Notizen," both which reached the Library of the College in December, 1837, and which I immediately, as is my custom, perused, extracted therefrom the new points, and went to work to confirm or otherwise by direct observation. With regard to the Thesis itself, I immediately took means to obtain it, and Mr. Nasmyth correctly establishes the date of its arrival. It is a work the interest of which is not exhausted by a first perusal, and I shall frequently, and always with pleasure, look into it again.

But what feeling does Mr. Nasmyth expect to excite in the minds of the unprejudiced readers of your journal by his attempt to recriminate the charge of interpolation upon me, and the kind of evidence which he brings forward in support of it? I give credit to Purkinjé and Fraenkel for a discovery; I lend Mr. Nasmyth their book; he takes advantage of the act to damage my character. He thinks it enables him to prove that I could have known nothing of their discoveries at the time I professed to acknowledge them. But Mr. Nasmyth only proves that when I had a copy of the work to lend, I lent it him.

Where shall we look for a parallel to this attempt to turn a civility into a weapon of attack? It is as if you were to overtake a cripple on the road, and, willing to help him on his journey, lend him your stick, the which he no sooner obtains, than with it he aims at you an impotent blow. But to imagine such a case is almost a libel on human nature. The actual parallel of Mr. Nasmyth's attack can only be found in the inferior species. You may see, *e. g.*, any day at the Zoological Gardens, an ill-favoured red-visaged baboon (*Simia rhesus*, L.); you may distinguish it from its quadrumanous congeners by the pertinacity with which it follows and stretches out its hand to the visitor who may possess an attractive dainty. Wearied by the beast's importunities, and amused, perhaps, by its supplicatory grimaces, he gives it what it wants; but, no sooner is the coveted apple acquired, than the ingrate ape flies at its benefactor, shakes the bars of his prison, and grins and chatters impotent malice with such vehement anger, that bystanders, ignorant of the brute's nature, might suppose that it had received an affront instead of a favour.

"*Simia quam similis turpissima bestia n. .*". To finish the simile would be unjust both to the species and the poet, who meant to indicate the ape's resemblance to man in physical conformation only, not in psychical deformity.

But even Mr. Nasmyth seems to think that the additional matter which he quotes from my memoir might be deemed by some to reflect credit rather than disgrace on its author; and he dives again into his escudoir, to fish out another weapon by which my motive may be assailed: and he prints, through your aid and co-operation, my note of December 11th, 1838.

He is conscious that the additional passage, quoted from the Seventh Vol. of the Transactions of the British Associations, as an instance of flagrant interpolation, simply renders to Purkinjé and Fraenkel the merit due to them for their discovery of the coronal cementum of the human teeth, and establishes their accuracy. Mr. Nasmyth, therefore, proceeds to show that the motive of this act of justice was highly reprehensible. "It can have been no other," writes Mr. N., "than that of appropriating to himself the discovery of the enamel capsule." (*Gazette*, June 26th, page 516).

Does Mr. N., then, mean to tell us that the new organ, his persistent capsule, or enamel-capsule, of which the discovery has been trumpeted forth in such various forms and places, is nothing more than the gelatinous constituent of the cementum of the crown of the human tooth, described by Purkinjé in 1835, and acknowledged long before by comparative anatomists as investing the crown of the teeth of the calf, and of all herbivora? If this be the case, I was deceived by Mr. N.'s description of his discovery in the MS. referred to in my note of Dec. 11th, 1839.

I revised that MS. with much pleasure, looking upon it as the first-fruits of the advice which I had tendered to Mr. Nasmyth five months before, *viz.* to apply the discoveries of Purkinjé and Retzius to the elucidation of the pathology of the teeth, and I again repeat, as in my note of July 25th, 1838, that it would be "a source of great credit, and a matter of importance, to whoever, practising in the line of dental surgery, should combine these discoveries with the practical or remedial part of the

subject." Now, in the manuscript which I recommended Mr. Nasmyth to present to the Medico-Chirurgical Society, I understood that the author's object was to elucidate the "eruptive stage" of the development of the teeth; I found him stating that he had investigated that stage very attentively, and was convinced that the capsule of all teeth was persistent, and that, instead of being deciduous, it was a membrane whose functions continued throughout life. The influence of this persistent membrane in preserving the enamel, and its relation to decay of the teeth, were then treated of."

This is the note that I took of the new point, as it appeared to me, in Mr. N.'s manuscript; and when the paper was afterwards referred to me to report upon, by the Council of the Medico-Chirurgical Society, I recommended it for publication, on the ground of its relation to dental pathology, and believing that Mr. Nasmyth meant what he wrote. I suggested to Mr. N. the possibility of his being deceived by the process he had adopted to demonstrate the so-called "persistent capsule," and that it might only be Purkinjé's coronal cementum, under another form and title; but my suggestion was not well received. In December 1838, the persistent capsule was, to Mr. Nasmyth, anything but the membranous part of Purkinjé's coronal cementum: his MS. then contained not the slightest allusion to that estimable author, or to his discoveries. As to what Mr. Nasmyth's new organ, or persistent capsule, really was, I thought it his business to satisfy the physiological world. So far as it might be shown to explain pathological phenomena, Mr. Nasmyth's remarks were worthy the attention of the profession; but it was my duty, as a teacher of physiology, to take care that no industrious investigators of dental anatomy should suffer either through Mr. Nasmyth's ignorance of their writings, or his unscrupulous appropriation of their discoveries. *Hinc illæ lachrymæ!* Assuredly the rancour of mortified vanity knows no bounds, as Mr. Nasmyth's attacks, and Mr. Tomes's misrepresentations, testify.

Prompted by recent experience, I perused, the other day, Mr. Nasmyth's memoir, as it appears in the volume of the Medico-Chirurgical Transactions, published in September 1839, and I

compared it with the reports of the same memoir, as read in January 1839, which appeared in the Gazette and Lancet for February 2nd, 1839. Such of your readers as may be disposed to make the same comparison, will find that Mr. Nasmyth has not only profited by the suggestion which I made to him, relative to the probable relationship which his new discovery bore to Purkinjé's coronal cementum of the human tooth, but likewise, and largely also, by my discovery of its presence in the simple teeth of many of the lower mammalia. (See *Trans. Brit. Association*, Vol. vii. p. 136.)

In January 1839, "the author begins by observing that of the three stages into which the period of the growth of the teeth has been divided, namely, the follicular, the saccular, and the eruptive, it is his intention, in the present communication, to allude more particularly to the eruptive stage only. Having been induced to investigate this stage very attentively, he is convinced that the capsule of all teeth is persistent." *Medical Gazette* and (*ipsissimis verbis*) *Lancet*, Feb. 2nd, 1839. And here, by the way, may be observed another instance of curious coincidence in the admiring reporters of Mr. Nasmyth's discoveries. But in the memoir, as printed in the volume of the Medico-Chirurgical Transactions, the author professes a loftier aim: finding that anatomists acknowledged that the crown of the teeth is coated with cementum only in the complex teeth of certain animals, he is led, by the philosophical desire to trace and illustrate the laws of conformity in Nature's works, to demonstrate the existence of the coronal cementum in all.

Now, between the period of the publication of the report of Mr. Nasmyth's paper "On the persistent Nature of the Dental Capsule," in February, and the publication of the memoir in September 1840, there appeared my memoir on the Structure of the Teeth, in the seventh volume of the Transactions of the Brit. Association, in which, at p. 136, after quoting Purkinjé's discovery of the coronal cementum in the human tooth, I confirm it, and show that it likewise exists in the simple teeth of many of the lower mammalia. Mr. Nasmyth's future contribution to any Publishing Society may henceforward be regarded as a "trap" for any discovery that may

fall in between the time of reading his memoir, and the publication of the Transactions, only he must be more chary of his abstracts; for they lead to conviction of the tamperings with the text, which the capture of the discovery necessitates.

In January, Mr. Nasmyth was convinced that the membrane he had discovered was the persistent membranous capsule. In September he says, "It is either a production of the capsule, or the entire capsule itself, or a part of it in a state of atrophy, ossified, and adhering to the enamel by means of the ossific matter deposited in it."—*Med. Chir. Trans.*, p. 313. He thus, in September, begins to make a gradual approach to the truth, and when he has quite arrived at it, he will find that the persistent capsule is neither more nor less than the gelatinous constituent of the coronal cementum, previously discovered in the simple teeth of the mammalia by Purkinjé and myself. Yet this person, since his cognizance of Purkinjé's work, and my Memoir on the Structure of the Teeth, sends to the Hunterian Museum preparations of human and calves' teeth, demonstrating nothing more than the gelatinous constituent of the coronal crusta petrosa, and has the effrontery to inscribe them as the "Enamel Capsule discovered by Alex. Nasmyth."

And now I come to the third of the complaints preferred against me by Mr. Nasmyth, by way of reply to my refutation of his first charges. This complaint is founded upon the facts of my having had temporary possession of a Translation of Retzius' Memoir, which Mr. N. asserts he had previously prepared for the press; of my having disowned any desire or intention of publishing any general observations on the structure of the teeth, *in which I had been anticipated* by the Swedish anatomist; and thirdly, by the fact that I had "published certain new general observations on the structure of the teeth, with practical deductions," though not, I hope, all that I am capable of making, as Mr. N. gratuitously asserts. These three allegations I admit, and have, first, to reply,—that with respect to whatever novelties in dental anatomy I have published, it will be found that the discoveries of Retzius and other anatomists are alluded to with no more detail than was essential to the reader's compre-

hension of the new matter I had to treat of. Secondly, I affirm that I have been in no wise indebted to, or dependent upon, the translation of Retzius, which Mr. Nasmyth lent to me in the course of the summer of 1838; but that I acquired my knowledge of the Swedish anatomist's discoveries, from a study, six months before, of Dr. Creplin's German Translation of Retzius' Treatise, published in Muller's *Archiv.* for 1837, which reached me early the following year. The translation which Mr. Nasmyth had procured was sent to me, not as prepared for the press, but rough from the hands of a translator evidently ignorant of the subject and of its technical terms: I returned it to its owner, corrected by means of a comparison with the text of Dr. Creplin, and with the proper technical words added.

There are still, however, some odd blunders in it, which would almost justify a suspicion that it had not received the benefit of a zoologist's revision; but these are due to subsequent interpolations of Mr. Nasmyth. Take an example: Mr. N. found in his corrected translation, as returned by me, the Swedish word for alligator rendered *Croc. Lucius*. This seems to have puzzled him, being as much an adept in zoology as Swedish. However, he goes to the Latin Dictionary, and finds *lucius*, a pike: and with this additional information he proceeds to stultify the amended text in preparing it for press. If your readers will turn to p. 99 of the published translation, vended under the title of Mr. Nasmyth's *Researches, &c.* they will find Retzius made to say—"The teeth of the *crocodile* and of the *pike* have a covering of enamel on the crown, and a tolerably thick coat of cortical substance on that part which is situated within the alveolus," &c.; and a little further on, at page 104, where Retzius is actually speaking of the *Esox Lucius*, Mr. N. says, "In the common *pike*, as in most fishes, are found a number of unequally developed teeth, from the smallest loose points to the teeth immoveably fixed to the jaw by a solid osseous base. On none of these could Retzius, by means of the microscope, perceive any covering of enamel."

Now these two contradictory propositions respecting the teeth of the pike are contained in the same sheet (H 2),

which may have passed two or three times through Mr. Nasmyth's hands before it went to press. Yet this obtuse individual seems to have been as little struck with the pleasant inconsistency of a pike's tooth having and not having a covering of enamel on the crown, as your reviewer with that between the statements that the ivory is neither more nor less than the ossified pulp, and that the cells of the ivory are altogether a distinct formation.

By a similar but more reprehensible, because wilful, obtuseness, Mr. N. having recourse again to a confidential note, adduces therefrom *the negation of an intention to publish Retzius's discoveries*, and the facts of my subsequent publication of discoveries, not Retzius's, as evidence of a versatile disposition. He converts the expression of an absence of desire to publish observations on the structure of the teeth, previously made by another person, into the denial of an intention of bringing before the public any general observations at all on that subject.

Now, as I have thought it worth my while to notice any of the calumnies of my protean traducer—for I am not ignorant that I might now, as on other slanders, with the least temporary loss of reputation, have used no other defence than silence and sufferance, and honest deeds set against dishonest words—yet as I have striven to say clearly what might wipe off any blemish which my adversary has sought to leave on my good name, I may again state that, in my published accounts of novelties on the structure or development of the teeth, it will be found that the discoveries of Retzius, and other anatomists, are alluded to with as much brevity and succinctness as was consistent with the intelligibility of the new matter I had to treat of: the translation of Retzius and Purkinjé's General Observations on the Teeth I left to Mr. Nasmyth, than whom no one has profited more by the additional advance in dental physiology, which is due to my own proper investigations and discourses.

But he informs us that he was "naturally vexed, and that not a little," at these contributions of mine; and then, as deluded with respect to his own importance and attractive qualities as ever was the honest Malvolio under the influence of his crossed garters and yellow stockings, he vapours forth "his

indignation that a person, calling himself his friend," should presume to give to the world any novelty in dental anatomy at all. He says he was *naturally vexed*, as though it were in human nature to be otherwise. But when Mr. Nasmyth reveals so much of his nature, he, perhaps, did not think that he might be reminded that, in the human species, the naturalness of the act is according to the nature of the individual, and, in the lower animals, according to the nature of the beast. As, *e. g.* Craft is natural to the fox. To steal precious things is the nature of the daw and magpie. It is natural to the hog to wallow in the mire. To receive a favour, and jabber impotent malice at the donor, is the nature of a baboon.

And so, when a man of science devotes the little leisure and cash he may have to spare to the advancement of dental anatomy, and instead of reserving his discoveries for the benefit of one, publishes them, without eye to profit, for the benefit of all, while other dentists are grateful and applaud the act, to be enviously vexed and indignant is *the nature of a Nasmyth*.

But Mr. Nasmyth's revelations of his own nature do not stop here: it appears to be likewise natural to him to dissimulate this his envious vexation for a considerable period, and he tells us 'that, nevertheless, he remained silent on the subject, hoping that my interference' with his plagiarisms of better men's discoveries, as they successively came into his 'trap,' would cease.

Here, however, his assertion is as little consistent with the fact, as that of his having taken no part whatever in the simultaneous calumnies aimed at me in the *Lancet* and *Gazette*, for June 5th.

Instead of nursing his no-small wrath* in silence, he hypocritically volunteers upon my 'reports' his fulsome praise, with the reservation that I, like Schwann, had only arrived at the same conclusions, which he had *first* announced.—(See '*Researches on the Teeth*,' of A. Nasmyth, p. 123.) Then, again, when, writhing under the exposure of the unprincipled means to which he had resorted, in order to sustain his charge of plagiarism, and regretting that he had exposed himself to that exposure,

* "The appearance of his 'voluminous' reports naturally vexed me not a little."—*Nasmyth*, in *Gazette*, June 26th, p. 548.

he is not ashamed to own that 'his motive for so doing was very natural.' No doubt; but very disgusting nature, at the same time.

There is one statement of mine that Mr. Nasmyth has thought fit to publish, which I regret to see in print, as I never meant it for the public eye. It is, that I once thought myself a discoverer of the tubular structure of the teeth. As, however, I have represented myself in that light to others, besides Mr. Nasmyth, to whom I exhibited my preparations of the teeth, between the months of May and December, 1837, I will state briefly how I fell into that belief. I received from Mr. Darwin many fragments of the teeth of the *Megatherium*, *Megalonyx*, *Myiodon*, and *Toxodon*, soon after his return from his travels in South America. Some of these fragments were in a state of incipient decomposition. Instead of being resolved, like the fossil mammoth's tusks, into parallel superimposed lamellæ, they separated into fine fibres, running at right angles to the plane of the layers, of which, at that time, it was believed a tooth was composed. I exhibited the most characteristic of these specimens at my lectures on the teeth, at the College of Surgeons, in May, 1837, and stated that the appearances they presented were inexplicable on the (then as I supposed) only known doctrines of dental structure; I said that I should investigate the subject further, and endeavour to reconcile the apparent anomaly before the following session.*

At the conclusion of that course, I had sections of these fragments prepared for the microscope; and stimulated by the amount of clearly defined and beautiful structure which they exhibited, I proceeded to examine similar sections of the human teeth, and of those of many of the lower animals. The excitement of the research became heightened as I advanced, and I had collected extensive materials for a treatise on the general structure of the teeth, when the number of Müller's Archives, containing that part of his 'Jahresbericht' analysing Purkinjé's treatise, came into my hands,

* In Hunterian lectures, for 1837, I treated of the teeth in their relation to the skeleton; in 1838, as parts of the digestive system, when I detailed fully the discoveries of Purkinjé and Retzius, and my own additional observations; in 1839, I considered the teeth in their analogies with the epidermoid system, and pointed out the characteristic differences in their mode of development, illustrated by diagrams of my observations on the teeth of the foetal shark.

in December, 1837, and awoke me from the fair dream of discovery in which I had been indulging: I soon after received Dr. Creplin's translation of Retzius, upon which I abandoned 'my intention of bringing before the public any of the general observations on the structure of the teeth which I once thought were new, but since found to have been mainly anticipated by Purkinjé and Retzius.' I thenceforward devoted the leisure, which I had to spare for that purpose, to new applications of the principle of the tubular structure of the dentine, to its investigation in animals not before examined, and more especially to researches into the laws of development of the dental tissues, which, as then taught, were greatly at variance with the new views of the structure of the teeth. These researches have been published in the 7th volume of the Transactions of the British Association; in the Proceedings of the Geological and Microscopical Societies; in the Comptes Rendus de l'Académie des Sciences; and in so much as has yet appeared of my Odontography. Mr. Nasmyth asserts that he has anticipated me in the application of the microscopic structure of the teeth to the elucidation of fossil remains; and Mr. Tomes says there is great reason for believing that I got the idea of working at the subject from him and his preparations: I affirm that I was incited to, and did labour in this department of anatomy, long before I had the misfortune to see either of these persons.

With respect to the application of the micrography of the teeth to fossil remains, let me state that, hitherto, the instances on record are as follows:—

The *Basilosaurus* of Harlan has been thus proved to be a cetaceous mammal, not a saurian.

The *Saurocephalus* has been shown to be, not a saurian, but a sauroid fish.

The *Megatherium* has been demonstrated to be, as Cuvier contended, more nearly allied to the sloths than, as De Blainville supposed, to the armadillos.

The *Glyptodon* has been proved to be a giant armadillo, and to have the rightful claim to that colossal bony armour which before was placed upon the shoulders of the *Megatherium*.

A character has been elicited, which Agassiz despaired of, whereby the

genera *Lepidotus* and *Sphærodus*, rarely represented save by detached teeth, precisely similar in outward form, can now be distinguished from each other.

Which of these discoveries will Mr. Nasmyth contend I plagiarised from him? Why, if any thing else had been wanting to fix upon him the authorship of the slanderous review in the *MEDICAL GAZETTE* for June 5th, it was his assertion that he had anticipated me in this application; for the instances of such application still remain hidden in his own breast. No one, save himself, as yet knows of his discoveries in this field.

And now I come to notice two other attacks in your journal, by which it is attempted to divert my attention, and that of your readers, from the vulnerable points of Mr. Nasmyth's.

The first professes to come from a "M.R.C.S. residing at a distance from London," complaining of the miserable state of the arrangement, display, and catalogue, of the Hunterian Museum, which I shall leave to speak for itself: the onus of the charge being, that the distant member was refused access to the microscopic preparations of osseous tissues, &c. with which I had illustrated my lectures. Now as it happens that Mr. Nasmyth is the only person who, upon application to the Museum Committee, to use these specimens, has been refused—such specimens not forming part of the collection, but being my private property—I conclude that the 'distant resident from London,' like the editorial 'we,' and the editorially-discarded reviewer, is but another of the disguises in which Mr. Nasmyth has indulged his *natural* indignation in raking up malicious slander against me.

But crimes, like diseases, are contagious, and no sooner does Mr. Nasmyth—one set of charges preferred against me being refuted—rake up another from the contents of private letters, than his friend Mr. Tomes, recollecting conversations and events that took place in May 1838, next volunteers his version of them to prove that I have no claim to priority in making known to an English audience, or to English readers, the tubular structure of the teeth.

When Mr. Tomes was introduced to me as a discoverer of this structure, I had been working at the subject eleven months, and the number of preparations which I then possessed exceeded a hundred. Backed by the authorities, who had assured him of the novelty of his observations, I found him very difficult to be convinced of their anticipation by Purkinje and Retzius. He received from me the best information I could give him. I showed him as many of my preparations as my time would allow, especially those of the *Megatherium* and *Acrodus*, which best illustrated the analogy of ivory and bone. It was the interest he expressed at seeing these specimens that induced me to furnish him with fragments of the portions of teeth from which they were prepared. He told me he did not understand the German language; but I shewed him the illustrations appended to Creplin's German Translation of Retzius, in Müller's Archives, and the diagrams I had copied from them to illustrate Retzius's discoveries. This fact it does not suit Mr. Tomes to remember; it would not square with Mr. Nasmyth's assertion that I first became acquainted with Retzius's discoveries through the medium of the translation which he, Mr. N., had prepared for the press. And such are the men whose envious carpings and calumnies my time is to be wasted in refuting.

That mortified vanity should impel a man to revenge himself by deliberate misrepresentation, after the instance of Mr. Nasmyth, needed no further illustration by Mr. Tomes.

It is a very unpleasant task to undeceive a man who comes to you under the delusion that he is a discoverer; it is still more disagreeable when such a person, persisting in his delusion, obtrudes his anticipated observations on the Royal Society, and compels you to reject them; and I can assure Mr. Tomes, whose anger on both accounts appears to have been waiting for a vent since 1838, that to no one can these duties be more painful than to, sir,

Your very obedient servant,

RICHARD OWEN.

July 12, 1840.

When Mr. Turner was introduced to me as a discoverer of this structure, I had been working at the subject eight months, and the number of preparations which I then possessed exceeded a hundred. I had by the assistance of Mr. Turner secured the discovery of the structure, and I had secured him the priority of the discovery. I found that Mr. Turner had been working at the subject for some time, and I had secured him the priority of the discovery. I found that Mr. Turner had been working at the subject for some time, and I had secured him the priority of the discovery. I found that Mr. Turner had been working at the subject for some time, and I had secured him the priority of the discovery.

... I have no claim to the discovery of the structure, and I have no claim to the priority of the discovery. I have no claim to the discovery of the structure, and I have no claim to the priority of the discovery. I have no claim to the discovery of the structure, and I have no claim to the priority of the discovery.

Richard Owen