

Three memoirs on the development and structure of the teeth and epithelium : read at the ninth annual meeting of the British Association for the Encouragement of Science, held at Birmingham, in August, 1839 : with diagrams exhibited in illustration of them / by Alexander Nasmyth.

Contributors

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THREE MEMOIRS

ON THE
DEVELOPMENT AND STRUCTURE
OF THE
TEETH AND EPITHELIUM,

READ AT THE NINTH ANNUAL MEETING OF THE BRITISH ASSOCIATION
FOR THE ENCOURAGEMENT OF SCIENCE, HELD AT BIRMINGHAM.

IN AUGUST, 1839;

WITH DIAGRAMS
EXHIBITED IN ILLUSTRATION OF THEM.

Second Edition.

TO WHICH IS ADDED A LETTER

TO THE

RIGHT HON. LORD FRANCIS EGERTON,

PRESIDENT ELECT OF THE ASSOCIATION,

CONTAINING OBSERVATIONS ON STATEMENTS MADE BY ITS OFFICERS IN THE
VOLUME OF THEIR TRANSACTIONS, PUBLISHED IN JUNE, 1842.

BY ALEXANDER NASMYTH, F.L.S., F.G.S.

MEMBER OF THE ROYAL COLLEGE OF SURGEONS, &c.

LONDON:

JOHN CHURCHILL, PRINCES STREET, SOHO.

1842.

THREE MEMOIRS

OF THE
REVOLUTIONARY AND
REPUBLICAN

TEETH AND DENTISTRY

BY
JAMES W. PALMER, D.D.S.,
OF NEW YORK.

IN TWO VOLUMES.

WITH
DIAGRAMS

ILLUSTRATING THE
TECHNIQUE OF DENTISTRY.

NEW YORK:

JOHN W. PALMER, 100 NASSAU ST.

1854.

THE
REVOLUTIONARY AND
REPUBLICAN

TEETH AND DENTISTRY

BY
JAMES W. PALMER, D.D.S.,
OF NEW YORK.

IN TWO VOLUMES.

WITH
DIAGRAMS

LONDON:

PRINTED BY G. J. PALMER, SAVOY STREET, STRAND.

ADVERTISEMENT

TO

THE SECOND EDITION.

Since the following Memoirs were first published in the beginning of July 1841, the Council of the Association have at length replaced my abstract in the volume of the Transactions; but they have unfortunately accompanied it with statements which, instead of affording me any satisfaction for former infractions of justice and courtesy, have aggravated these injuries to myself individually, and completely compromised the character of the Association. Under these circumstances, I have felt it necessary to address, to the President of the Association, a Letter, containing a detailed account of the transactions between the Council and myself, and as this letter involves all that was stated in the Introduction to the first edition of these Memoirs, with much additional matter, the reader is referred to the letter, which immediately follows, and requested to regard it in the light of an Introduction to the present edition of the Memoirs.

A LETTER, &c.

TO THE RIGHT HON. LORD FRANCIS EGERTON,
PRESIDENT ELECT OF THE BRITISH ASSOCIATION FOR
THE ADVANCEMENT OF SCIENCE.

13 A, George Street, Hanover Square,
June 7th, 1842.

MY LORD,

It is with much reluctance and regret that I lay before your Lordship the following exposition of facts. I do so, however, in the full conviction that, as President of the British Association for the Advancement of Science, to whom all its members may confidently look for an unbiassed consideration of every point relating to the general welfare, and the moral and scientific character of the institution, your Lordship will concede to me the attention which the circumstances of the case require. The necessity for the appeal I now make has arisen from my feeling myself aggrieved by the officers and ruling body of the Association. I am warranted in this feeling, because no charge of irregularity on my part has been brought forward till the recent exhibition of a document which, now that I have an opportunity of examining it, proves to be *altogether unfounded in fact*. During a protracted correspondence

with the officers and Council, I have offered every facility and means of information in my power. Instead, however, of any attempt at redress, the injury to myself has been greatly aggravated, and the interests of the Association at large completely compromised, by the publication of what professes to be a faithful narrative of these, but which I am sorry to say is filled with misrepresentations, contains a letter in which there is an interpolated passage, and is drawn up in the most disingenuous manner,—recourse being had to the double unfairness of suppressing most important evidence in my favour, and producing at the same time, documents which have never before been seen by me, or formed any part of the transactions of the Council or its officers with me.

As soon as a copy of this statement reached me, I proceeded to examine it, but found that it was quite impossible to make any progress without information derived from the records of the Association, to which I was unable to get access. Such being the case, I resolved rather to wave my rights of publication than attempt to reply to a document of such a character, which resolution I urged on the Council without any satisfactory result. Some correspondence ensued between us, and the following is the last letter I addressed to them through their Secretary, which never obtained a reply, or even the common courtesy of an acknowledgment.

*To Col. Sabine,
General Secretary to the British Association for the
Encouragement of Science.*

13 A, George Street, Hanover Square,
3rd Dec. 1841.

Sir,

Your letter of the 30th ult. containing the resolution of the Council of the British Association, has surprised

me very much, as most certainly there have not been any laches on my part. I have over and over again offered to produce all the original papers, documents, and evidence, to prove the correctness of my statements, but I have never been allowed to do so. After I received the printed pamphlet (a great part of the contents of which were perfectly new to me) in the middle of August last, which came in a blank envelope, without any communication whatever, I wrote to three of the officers of the institution, without being able to obtain the explanations and information I required respecting it, nor have I yet received any; and the fourth officer I applied to, stated that he had resigned his situation. Had the Council, upon receipt of Mr. Owen's letter, called upon him, as I conceive they ought to have done, to verify his statements, they would at once have discovered the fallacy of them, and all trouble and delay would have been avoided. As it is, you are only now talking of printing irregularly the abstract which ought to have been done in its simple form two years ago, the report having been obtained from me under that express condition alone. Under these circumstances, and after this lapse of time, I must again protest most strongly against the Council having the same now published at all, and also against their right to do so, more especially with the addition of a statement and documents appended to it which, as I have already informed the Council, are in most material parts erroneous, and not founded in fact, and the documents themselves not correct copies of what they pretend to be, but even apparently altered to suit some purpose. I have done all I can, and have always been most anxious and ready to produce everything necessary for satisfying the judgment of the Council, but without effect; and I can only, therefore, repeat my most decided protest against their

making any use whatever of my abstract now, or in any way sending the same forth to the public, more especially with a statement annexed which at the least must be considered personally libellous as regards myself.

I have the honour to remain, Sir,

Your very obedient servant,

ALEX. NASMYTH.

The only knowledge of the determination of the Council which I have been able to gather, is from the appearance of a volume of the Transactions, containing what is entitled an "Addendum to the Report of the Transactions of the Sections in 1839;" being a literal copy of this objectionable statement. It is, therefore, now formally before the public under the sanction and approval of the Council.

I now also find, that previously to my protesting against such a committal of the Association, this statement had been strenuously objected to by Mr. Yates, the very officer through whom the correspondence had been carried on, but whose respect for justice and propriety has prevented him from acquiescing in the course which has been adopted by the majority of the Council. I find, moreover, that a lengthened protest and remonstrance had been presented by one of its most upright and honourable members—Dr. Hodgkin. The opinions of these gentlemen, authoritative as they are from their high personal character, must carry with them conviction, when it is known that they have examined with the utmost care every particular of the case, and have attentively perused the entire original documents in my possession, from which alone a just estimate of its merits could be formed. I have reason to believe also, that other members of the Council, on the same principle of justice, have been at the trouble of exercising their individual judgment on the

merits of the case, taking an interest in it from the beginning, and constantly urging on the Council the necessity of doing me justice.

Notwithstanding such admonitions, the weight of which it was impossible not to acknowledge, the publication of this statement, still more injurious to the Association than to myself, has been persisted in.

This has imposed on me a most disagreeable and irksome task, and places me in a most anomalous position. Although I am fully persuaded that the individual noblemen and gentlemen who compose the Council are men of high honour and integrity, the proceedings of the aggregate body appear to indicate the operation of very opposite principles; and it is extremely painful to me to be compelled to act in a manner so discrepant from my conviction of individual merit. It must be quite evident to the most superficial observer that the controversy in which the Council of the British Association has involved itself with me, arising out of the arbitrary proceedings of its Editorial Secretary, has now merged in a simple endeavour on the part of the Council to exhibit some excuse for its conduct. It would have given me the greatest pleasure to have assisted in the accomplishment of so desirable an object at any and every stage of the proceedings; but a very unaccountable resistance has all along been offered to such aid, and the result is the dilemma in which the Council is now unhappily placed.

As the circumstances of the case may possibly be new to your Lordship, I think it right briefly to recapitulate them, and in so doing it will be necessary to refer to events which have found their way into the discussion as matters of history, though somewhat antecedent to the main transaction to which I solicit your Lordship's attention.

Being professionally much interested in researches connected with Odontology, and having been personally engaged in such pursuits, my attention was directed to the microscopic investigations of Professor Retzius, of Sweden. I obtained the loan of a copy of his original account of these researches, which, from their appearing to me to form a new era in Odontology, I immediately had translated into English, with a view to publication. In the course of my own inquiries, I applied to Mr. Owen, the conservator of the museum of the College to which I belong, to point out to me such preparations as might throw light on the subject of my pursuit. Mr. Owen having come to the knowledge that I was in possession of the translation above mentioned, borrowed it of me. Seeing that Mr. Owen was interested in the subject, I wrote a letter to him, inquiring whether he had any intention of publishing upon it, and saying that, if he had, I would relinquish my arrangements, and assist him to the utmost of my power. He sent me, in answer, a letter in the following terms:

“ July 25, 1838.

“ DEAR NASMYTH :—Many things have intervened to
 “ prevent my returning you the translation of Retzius
 “ earlier, but as you have not sent for it, I hope without
 “ inconvenience to you. As I have before said, I have
 “ neither desire nor object in bringing before the public
 “ any of the general observations on the structure of the
 “ teeth, which I once hoped were new, but now perceive
 “ to be mainly anticipated by the industrious and sharp-
 “ sighted Swede. It will obviously, however, be a source of
 “ great credit, and a matter of importance, to whoever prac-
 “ tising in the line of dental surgery should combine
 “ these discoveries with the *practical* or *remedial* part of
 “ the science. And I have only again to add, how happy

“ I shall be to testify my sense of the obligations I owe
 “ to you and your skill, by revising every line of such
 “ a work as you propose; only, for some good reasons,
 “ you must accept this assistance, without acknowledg-
 “ ment, and in perfect confidence.

“ Believe me, dear Nasmyth,

“ Ever yours,

“ RD. OWEN.

“ Al. Nasmyth, Esq.”

Time was not allowed me to accept of the gratuitous offer contained in the last sentence of this letter; and it will scarcely be believed, but it is no less a fact, that, in spite of this letter, and of my having, in consequence of Mr. Owen's repeated assurances, advertised a work on the subject, of which the translation above alluded to was to form an important part, that gentleman, within about one calendar month after the date of the above epistle, not only published all the “ new ” “ general observations “ on the structure of the teeth,” but also the “ practical “ or remedial ” deductions from them; that is to say, besides the anatomical details, he communicated to the Geological Section of the British Association, met at Newcastle, the deductions from them bearing on natural history, which I had already alluded to in my advertisement; and before the Medical Section, he treated of the practices adopted “ in the line of dental surgery,” thus professing to “ combine these discoveries “ with the practical or remedial part of the science.” It is still more remarkable, that he professed to give an “ analysis of the laborious and accurate microscopical “ observations of Professor Retzius, as related in the “ original Swedish memoir of that author,” (vide Athenæum,) a memoir with which he was only acquainted through the translation he had borrowed of me, which he had never asked permission, and had,

therefore, no right to make use of, and which he had returned to me, as the reader has seen, with an assurance, that he had "neither desire nor object in bringing before the public any of the general observations on the structure of the teeth."

Not feeling much interest in the kind of explanation which Mr. Owen might give of such incomprehensible conduct, I determined to avoid discussion, though perfectly aware of the real character of the transaction. Presuming that his interference, therefore, was at an end, I proceeded with my projected publication, and circumstances having occurred which induced me to send him a proof of the preface, he, to my astonishment, informed me in return, that he also was in the press with an extensive work on the same subject! Still determined to keep at peace, I quietly pursued my course, notwithstanding his persistence in this strange and unlooked-for opposition, set up on the ruins of confidence between man and man.

Having conceived a very high opinion of the uprightness and integrity of the British Association for the advancement of Science, and not being aware at the time that Mr. Owen was one of the ruling body of that Association, I selected from among the drawings I had accumulated in the course of my investigations, a certain number, illustrative of my own notions of the intimate structure and development of the different tissues of the teeth. I had made, at no small cost, fifty large drawings of these, for the purpose of illustrating two memoirs on that subject, which I offered to the Association, accompanied with a separate communication on the intimate structure and development of the epithelium. I took with me also to the meeting the preparations from which these drawings were made, and about one hundred and fifty small drawings. These contributions were accepted,

the papers read, and drawings exhibited, accompanied with demonstrations of the subjects, to two different sections of the Birmingham meeting. Mr. Owen himself was present at this meeting of the Association, acting in his capacity of councilman, and producing reports which the Association had employed him to make. These transactions took place in the beginning of September, 1839.

The Editorial Secretary, Mr. Phillips, after some delay, requested me to make an abstract or consecutive report of all my three memoirs, comprehending a description of the diagrams. This was done, and the report transmitted by me to Mr. Phillips. He retained it in his possession for a considerable time, and returned it to me with proposed abbreviations, which being acceded to by me, I saw no more of it till I received from the printing-office a proof, under cover of a note from Mr. Phillips, by direction of which I carefully corrected the press, under the express condition guaranteed by him, that he "gladly" acceded to its appearance in the forthcoming volume of the Transactions. He referred me to the printer for the private copies which I required. These were delivered to me by the printer, and on the title-page were inserted by Mr. Phillips or his agent,—certainly not by me—these words, "Reprinted from vol. 8th of the Transactions." There being no restriction as to the use I was to make of these private copies obtained by me under sanction and authority, I immediately applied them to the purpose for which they were intended, namely, circulation. Thus I conceived that my first transaction with the British Association had terminated in a manner satisfactory to all parties, and that I was about to reap some share of the benefit held out by that Association as an inducement to contribute the fruits of years of laborious investigation on a very difficult subject, to say nothing of the pecuniary sacrifice

of many hundred pounds expended by me in preparations and illustrations. This hope, however, proved illusory, and a series of incomprehensible proceedings on the part of the Council has terminated in an act which I think cannot meet with the approbation of right-thinking men, either in or out of the Association.

In the *Compte Rendu* for the 16th of December, 1839, less than four months after the presentation of my memoirs to the British Association, Mr. Owen is reported to have presented one to the *Academie des Sciences* of Paris, "Sur la Structure et la Formation des Dents," &c. This was one of the branches of the different subjects which I had confided to his care as a councilman of the British Association, and the absence of all mention of my memoirs on the same subject, and exactly to the same purport, is extremely difficult to reconcile with the character of an upright guardian. The preparations which afforded materials for the observations detailed in that Memoir were, I believe, taken from the shelves of the Museum of which he was conservator. From whatever source, however, the preparations were obtained, the illustrations derived from them were transported beyond seas, to claim for their possessor the credit of being the first to demonstrate the self-same theory which it was the sole object of my memoirs and illustrations on that particular subject, to demonstrate to him and the public at Birmingham, *not four months* before this foreign promulgation of his opinions! As conservator of the Museum of the College of Surgeons, he was guardian of the preparations which formed the materials of my studies; as a Councilman of the Association, he was guardian of the fruit of those studies: how far he has fulfilled these trusts, I leave it to your Lordship to determine from the history of the transaction.

Shortly after this, he transfused these opinions into the

work which he stated to me he had in the press, and gave them to the world; which proceeding, however, did not escape the animadversion of the Medical Journals.

The peculiar manner in which Mr. Owen conducted himself throughout the public discussion which ensued between him and his reviewers, precludes me from expressing personally any opinion on the subject, and nothing but the necessity of self-defence, and the calls of duty to my fellow-associates, could have induced me to allude to it at all. The feelings connected with the question of priority of discovery discussed in the journals has merged in others of a far more serious nature. These, however, I must excuse myself from expressing, and will therefore simply quote the sentiments of some of the journalists themselves.

So violent was Mr. Owen's language, that the *Lancet* rejected one of his articles with the following remarks:—
 “To a simple rejoinder from Mr. Owen we should have
 “offered no objection, for that might be read, and would
 “have suited our space; but into another dozen pages of
 “solid octavo print we will not convert a bushel of abuse
 “and criticism, containing one grain of matter to the
 “point. That we decide with propriety in not suffering
 “our pages to be occupied by this fresh correspondence,
 “leading to endless counter-statements and reviews of
 “the questions in dispute, will appear from a single
 “extract, taken from such part of the manuscript as most
 “readily allows an isolated quotation to be made.”* Then follows a passage, abounding in coarse personalities, which it is unnecessary to quote.

The *Medical Gazette* animadverted on Mr. Owen's peculiarities of diction in the following terms:—“As
 “if conscious of the weakness of his cause, he resorts

* Vide *Lancet*, vol. ii. 1839-40, p. 623.

“at once to angry words, and such reckless abuse, as no prudent controversialist, much less a man strong in the consciousness of truth and in the justice of his position, would ever have recourse to.”—*Med. Gaz.* vol. ii. 1839—40, p. 596.

The editor of this journal, as he had inserted the allegation against Mr. Owen, felt bound to admit his reply also, violent as it was; but he took care, for the credit of his journal, to print it as an appendix, so that it need not be bound up with it, and, to use his own expression, it was not without “pain” that he published it even there.

The Dublin Medical Press, after discussing at length the point at issue between Mr. Owen and his reviewers, and decidedly condemning the unjust conduct of that gentleman towards me, thus speaks of his mode of diction in the *Lancet*:—“Professor Owen, indeed, has not remained silent, but his clamorous exclamations only seem to us calculated to make his castigation the more ludicrous.”

* * * * * After quoting some of his abuse, it says —“This is undoubtedly the vernacular slang of Billingsgate, and of the costermongering fraternity; or, it may be, of a man seeking to divert by ridicule the weight of a heavy accusation; but we beg to assure Professor Owen that it is neither the language of philosophy nor of good society, and that such expressions cannot fail to fall back with redoubled violence to the source from whence they came.”*

Mr. Owen was also convicted by the Reviewer in the *Medical Gazette* of having fabricated a statement favourable to his object, and then attributed it to me; and also of having made assertions with respect to facts which were directly at variance with truth: and to these charges he has never made any defence. I shall content myself with

* *Dub. Med. Press*, vol. iv. p. 122.

quoting the reviewer's remarks on the first of the above-mentioned delinquencies, which will serve to explain some of the documents and observations which follow.—

“ At p. 507 of the last Number of this Journal, he (Mr. Owen) manufactures what he is pleased to call Mr. Nasmyth's ‘*emphatic statement*’ of September, 1839, “ that so far from being the ossified pulp, it (the dental substance) was altogether a distinct formation.’ In making this fabrication, he appears to have been assisted by a friend; and, in order to give it greater weight, he takes the trouble to inform us that the character of that friend ‘is the very antipodes of impudence and duplicity.’ Now of this passage, which he gives as quoted from the ‘*Lit. Gazette*,’ p. 598, only *the last four words* will be found, on reference to that journal, to be correctly copied: *all the first part of the passage is the composition of Mr. Owen*. Mr. Nasmyth, by this perversion of his report, is made to say that the dental substance is altogether a distinct formation, whereas he never mentions the dental substance at all; and his real meaning evidently is, as we have shown above, that the cells of the ivory have undergone a distinct formative process in their transition from the cells of the pulp.”—*Med. Gaz.* vol. ii. 1839—40, p. 596.

Such is shortly the character which this affair was made to assume by the party with whom it originated, and in whose behalf the Council of the Association has thus involved itself. To the credit of Mr. Owen, however, it must be remarked, that he sought to expiate these offences by a public apology, wherein he states that “ so far as I am concerned the controversy ” “ is at an end,” and adds, “ but I owe it to myself to express my regret that the irritation produced by the charges so pertinaciously advanced against me, should have led me, in some passages, into the employment of language which

"my calmer judgment disapproves." This apology, however, contains the following sentence: "I have asserted nothing in my published observations upon this subject which I do not know to be true."* Mr. Owen's repentance would have worn an air of greater sincerity, if the sentence last quoted had been omitted.

Strange to say, the editorial officer and Council of the British Association espoused such a cause when it had arrived at such a stage, and commenced a series of arbitrary measures, ending in a most remarkable deviation from moral rectitude. It is to the extraordinary character of these measures that I shall now take the liberty of craving your Lordship's attention, although I most reluctantly apply myself to the task of making such an exposure, after having exercised forbearance to the utmost limit—protested and appealed in every conceivable way—and expressed my desire to wave my rights of publication altogether in order to avoid it.

In the excitement of the moment, Mr. Owen, as an individual, requested the Editorial Secretary, Mr. Phillips, to annul the deliberate acts which he had officially concluded with me. That gentleman, without any orders, or any communication with those for whom he was acting, and on his own responsibility alone, withdrew the report which had been inserted in the volume, he having obtained it from me in trust for that specific purpose. In doing this, I conceive that he inflicted on me a gross personal affront, and an injustice without law or precedent. The abettors of this transaction in the Council, by means of allegations which turn out to be utterly void of foundation, obtained the approval of a majority to Mr. Phillips's conduct, and got the matter committed for investigation *without ever citing me, or examining original*

* See Med. Gaz. vol. ii. 1839—40, p. 783, as also Lancet.

evidence at all! To justify such extraordinary proceedings, arguments of no common texture were required; and accordingly the "Addendum to the Report" of my transactions with the Association now published is as singular as the conduct which it is intended to justify is anomalous. Evidence is withheld, and documents never before seen are brought forward; allegations utterly unfounded are assumed as true; and, what is worst of all, *evidence is actually fabricated* to give some show of fairness and liberality to these proceedings. The last of these charges, a grave one certainly, it is most painful to me to make. The desire to save myself from this trial formed one of my principal reasons for proposing to wave my rights of publication altogether, a course which I preferred to the discharge of so disagreeable a task. I have here to remark that the main excuse held out for Mr. Phillips's conduct, was an alleged appeal to the Council by Mr. Owen; and what I have particularly complained of is, that the original document forming the ground of the charges against me had never been exhibited, nor in fact the charges themselves in any intelligible shape. In lieu of this, however, a set of "allegations" is now, *for the first time*, brought to light, and these are put prominently forward in the "Addendum," as forming the original basis of the transaction with me, whereas *I never saw one word of them* before they were prepared for exhibition as such to me in common with the public at large. The totally groundless nature of these "allegations," which will shortly be demonstrated, will at once explain why this document has been kept out of sight.

Mr. Phillips and the other actors in this scene must be well aware of the foregoing facts; but in addition to this unfairness, and presuming, as they must have done, that the original letter of the 10th of August, 1840, (p. 5 of the Addendum) had not

been preserved by me, they modify this letter throughout, and alter the concluding paragraph. Unfortunately for these gentlemen, the original letter is still in my possession. In order not to occupy space unnecessarily, I will merely quote the concluding sentence, and place it in apposition with the corresponding portion of the printed document, pretending to be a faithful transcript of it :

Concluding sentence of Mr. Phillips's original manuscript letter in my possession.

"Should you however,
"instead of this easy and ob-
"vious mode of correcting
"any error of mine, resolve
"to appeal to another tribu-
"nal, the public, I can make
"no other objection than
"that I fear you will not have
"chosen the course most ad-
"vantagous to your views.

"I am, Sir,

"Yours very obediently,

"JOHN PHILLIPS.

"Alex. Nasmyth, Esq."

The same sentence as given in its altered form in the "Addendum," at p. 5.

"Should you, however,
"instead of this easy and ob-
"vious method of correcting
"any error of mine, resolve
"to appeal to another tribu-
"nal, the public, I will give
"you the only proof in my
"power to offer of an unbi-
"assed mind, by transmit-
"ting copies of all the let-
"ters I have received from
"you, to render any state-
"ment you may think pro-
"per to make as complete
"as possible.

"I am, &c. &c.

"JOHN PHILLIPS.

"A. Nasmyth, Esq."

If professions such as those now set forth in the printed letter had ever been made to me, in sincerity, and acted upon, the matter would have been very speedily brought to a conclusion. I should of course have at once requested a little extension of this generosity of feeling, by a production of the original document on which the whole of the matter was founded; this would certainly have been granted; and the ungrounded nature of the allegations contained in such document would immediately have been made apparent, by simply requesting *an exhibition of evidence*. The non-existence of any evidence rendering such exhibition impossible, the matter must have expired at the threshold of the inquiry. In direct opposition to the sentiments here professed, I could not but remark, that throughout the protracted correspondence between the officers and Council of the Association and myself, an entire want of candour and openness had been evinced in all their proceedings, and a fixed resolution to afford me no information, but to bring the affair to an arbitrary conclusion, no matter on what pretences or by what means, provided they could exclude my Report. It must have been with a view to deceive the reader into an opposite belief that the concluding sentence of Mr. Phillips's letter has been tampered with.

A copy of this "Addendum," however, having found its way to me under a blank cover, I was willing to test the sincerity of this liberal feeling, and finding it impossible thoroughly to understand the statement which had thus come into my possession, without reference to certain papers therein alluded to, and minutes of Council regarding these transactions, I first applied to Mr. Yates, under the impression that he still held the office of Secretary to the Council, and I received from him the following answer;

" 49, *Upper Bedford Place, August 12th, 1841.*

" MY DEAR SIR,

" In my letter addressed to you from Plymouth, I explained the reason why I had not laid your printed letter of June 10th before the Council when I first received it, and I informed you that on receiving your subsequent communications addressed to me at Plymouth, I put them into the hands of Col. Sabine, the Acting General Secretary, in order that he might decide whether it would be proper to call a meeting of the Council to consider them; he has returned them to me, and all that I can do is to return them to you. So far as I have understood, the matter is considered by the Council as finally settled by their proposed publication of the statement agreed on at Plymouth.

" My correspondence with you on the subject must also cease with this letter, as I never concerned myself in the question, except as it was pressed upon me by the office I held as secretary to the Council, and that office I have been unwilling to retain on account of the want of accordance between the majority of the Council and myself, in regard to this subject. As in my official communications with you I have always thought it necessary to avoid as much as possible all expression of my opinion on the question at issue, you may have inferred that I participated in the views and feelings which actuated others, and which you have considered as of a hostile or partial character against yourself; I think it right and fit, therefore, being now able to express my private opinion, to conclude our long correspondence with the assurance of my high esteem for your character and talents. I cannot say that I greatly interest myself about the originality or the importance of your discovery in regard to the development of ivory. Of such questions I do not pretend to be a judge; but of the far more momentous question of the charge upon your character as a man of honour and

“ integrity, I think myself as well able to judge as the most
 “ eminent physiologist. This question might have been
 “ settled by any honest and sensible school-boy in five
 “ minutes. The charge never had the slightest foundation,
 “ as no evidence worth a straw was ever brought to prove
 “ it. I therefore think that it ought not to have been enter-
 “ tained for a single day in any quarter, and it is my utter
 “ aversion to be employed as an agent, or to appear in any
 “ way in such a case, that has principally induced me to
 “ decline the office which for ten years I held with unal-
 “ loyed satisfaction.

“ Believe me to remain,

Dear Sir,

“ Yours always, with great regard,

“ JAMES YATES.”

“ Alr. Nasmyth, Esq.”

The proceedings of the Council having forced me to enter into an explanation of the case, I wrote to Mr. Yates for permission to make use of the foregoing letter, which he gave me in the following terms:

49, Upper Bedford Place, April 8th, 1842.

“ MY DEAR SIR,

“ Having read my letter of the 12th of August, I see
 “ in it nothing that I should now be disposed to retract,
 “ and have therefore no objection to the publication of it.
 “ On the contrary, I had rather that my name should not
 “ appear, as it now does, in the Report of the Plymouth
 “ Meeting, without something to show my dissent from
 “ the course pursued by my colleagues in the Council.

“ Yours always,

“ JAMES YATES.”

About the same time that I received Mr. Yates's letter of the 12th Aug. 1841, I applied to Mr. Phillips, whose name is

attached to the sophisticated letter quoted above, and subsequently to Colonel Sabine, who, I was informed, was acting as General Secretary, and also to the President, Professor Whewell. My applications, however, met with nothing but neglect or evasion, and I am to this day without the information which I required, and which the "unbiassed mind," professed by Mr. Phillips in his letter, as now altered, might have led me to expect. Being thus unable to obtain from the officers any satisfaction as to particulars, I applied for information to personal friends in the Council, none of whom seem to have been present during the discussion, or studied the circumstances of the case sufficiently to afford me any assistance, except Dr. Hodgkin, who has favoured me with a copy of the Protest which he made against the publication of the statement in question. This will be found appended to the present letter. The well-known honour and integrity of this gentleman, and of Mr. Yates, make me proud of the very unequivocal testimony they have borne in my favour. That of Mr. Yates acquires increased force from his having informed me, at our very first interview, that as he was an officer of the Association, he felt it his duty to carry into effect the wishes of the Council, and to correct any mistake into which I might have fallen, more especially when, by so doing, he could vindicate or support the Council. I required little of him, but he took every pains to make himself master of the subject, by a simple consideration of the documents in their entire and original state, a task which Dr. Hodgkin also took upon himself, although declined by Professor Phillips and the judicial delegates of the Council.

After exposing such deviations from rectitude on the part of the compiler or compilers of this "Addendum," and producing these attestations, which must be perfectly satisfactory to all who are acquainted with the character of strict integrity universally accorded to the gentlemen by

whom they are made, it might appear unnecessary to say another word on the subject.

Several powerful considerations, in addition to those I have already mentioned, disinclined me for the prosecution of this subject, involving, as it necessarily would, a perpetuation of such disgraceful scenes in the bosom of an Association, which professes to be the honourable arena of scientific emulation. In the *first* place, I was anxious to avoid the appearance of want of generosity to one who, throwing himself into an adverse position towards me, had unfortunately committed himself by intemperate language, but who had tried to expiate the outrage by a public apology, and who added to that apology the declaration that, "so far as" he was "concerned, the controversy" was "at an end." In the *second* place, I was unwilling to foment any further the injudicious zeal of Mr. Owen's friends, who sought to perpetuate, contrary to this public avowal on his part, a discussion which could tend to nothing but the prejudice of science, and still further disclosures of individual delinquency. *Lastly*, I was especially desirous to avoid the necessity, even in self-defence, of accusing the compilers of this document of want of rectitude in the discharge of the trust reposed in them, since this would apparently have implicated, in the same charge, the whole Council of the British Association, the greater part of whom, although formally "approving" these transactions, as now exhibited, I conscientiously believe gave their individual sanction to the "Addendum" in entire ignorance of the untruths it contained.

Influential as these considerations were, I have nevertheless determined, after mature reflection, to extend my remarks into an exposure of a few more of the peculiar features of this statement; and this for two reasons:—first, that by letting the matter drop I might leave an impression on the public mind, that although I had convicted the

compilers of the statement of these serious improprieties in the principal points, there might be other points on which I shrunk from inquiry; and secondly, that I feel it right to let the whole Council, many of whom have thus innocently been committed in an act of flagrant injustice, understand precisely the position in which they have been placed.

In the performance of this ungracious task, I am under the necessity of reviewing the productions of Professor Owen, in a light by no means favourable—a necessity which I regret the more, because he has himself publicly declared that, so far as he was concerned, he considered the matter at an end.

The principal excuse pleaded by the movers and conductors of these strange proceedings is founded, as I have already mentioned, on an alleged “Appeal to the Council” by Mr. Owen, which has been incessantly talked of, and as incessantly called for by me, as in it I presumed I should find the distinct charge which the Council had to prefer against me, and be thus enabled to meet it with a direct answer. Nothing in the shape of such an appeal, however, has ever been exhibited to me till now that a letter designated as such has been produced, addressed by Mr. Owen to Mr. Yates, and dated “Royal College of Surgeons, July 18, 1840.” (See Addendum, p. 4.) This document, moreover, consists of mere “*allegations*,” into the truth of which no inquiry has hitherto been instituted by the Council—so that to this day a regular charge professing to be founded on evidence is altogether wanting;—surely a very strange anomaly in the proceedings of a court of justice, into which the Council in this case has erected itself.

This letter contains statements which the writer in the hurry and excitement of the moment, may have thought himself warranted in making, but of the ungrounded nature of which I cannot doubt that he has long ere

this become convinced. Since, however, this document has been so injudiciously brought to light by the friends of Mr. Owen, I have no alternative but to expose its fallacies. The first duty of the Council most certainly was to have called the party forward who thus sought their protection against assailants from without the Association, over whom neither they nor I had any control, and to have immediately demanded of him distinct proofs of his "allegations;" instead of doing so, however, they took the truth of his statements for granted, and never so much as inquired whether I had anything to bring forward in opposition to them. If this letter be the only document which has regulated the proceedings of the Council, it is very strange that the measures adopted against me, have little or no connexion with it, and that the several actors in these transactions seem not to have attended to it.

But now for the appeal itself. In brief, then, the allegations contained in it are, I am constrained to say, *totally false*. The first of these allegations is, that my abstract, or report, "has been altered by the author, both in "the way of substitution of new matter and omission "of old, in order to include a discovery of" his. The subjects connected with this "allegation" have been so thoroughly discussed by the public journals, that it is only needful for me to quote their conclusions. These are stated in the introduction to the first edition of my Memoirs, which, despairing of any satisfactory arrangement with the Council, I published in their original and entire form, in the beginning of July, 1841, and from which I make the following extracts:—

"In fact all inquiry on the subject, leaving out of the "question its evident irregularity, was quite superfluous, "for the accuracy of my own condensed report had been, "some time before the Council entertained the ques- "tion, satisfactorily vindicated in the *Lancet*, *Medical Gazette*, and *Dublin Medical Press*, by a reference

“ exclusively to the unauthenticated reports of my
 “ communications published in the Literary Gazette and
 “ Athenæum. In the Lancet for 1839-40, vol. ii. p. 644,
 “ the editor states, ‘ Mr. Nasmyth has demonstrated in
 “ the clearest manner that he was acquainted with, and
 “ had published, an account of the organised nature of
 “ the teeth long before the month of December, 1839;
 “ and, furthermore, that he considered the teeth to be
 “ developed by a transition of the pulp cells (*at*, not *on*
 “ the external surface) into the cells of the ivory.’ ” . . .
 “ ‘ The essence of the theory evidently is a *transition of*
 “ *pulp cells into ivory cells*; and this essential element is
 “ developed in the papers of Mr. Nasmyth, which were
 “ published fourteen weeks before the presentation of Mr.
 “ Owen’s memoir to the Academy of Sciences.’ ”

“ In the Medical Gazette for 1839-40, vol. ii. p. 593, is
 “ the following passage. ‘ He (Mr. Owen) will never
 “ succeed in convincing a single rational being that Mr.
 “ Nasmyth, in the papers from which we have quoted
 “ above, (*viz.* the reports in the Literary Gazette and
 “ Athenæum,) teaches the old doctrine of secretion or
 “ exudation of the ivory from the bulb, in contradistinc-
 “ tion to Mr. Owen’s pretended “ *nouvelle théorie* ” that
 “ the ivory is formed by the transition of the pulp to an
 “ ossified state.’ ”

“ The Dublin Medical Press says, in its No. for August
 “ 19, 1840, p. 121,—‘ We have now given the substance of
 “ Mr. Nasmyth’s researches and discoveries, as related in
 “ the report of his papers published in the Literary Ga-
 “ zette of Sept. 21, 1839, and in the Athenæum of Sept.
 “ 14, 1839. The most careless reader cannot cast his eyes
 “ over these reports without perceiving at once that they
 “ attempt to establish the vesicularity or cellularity of the
 “ pulp, and its conversion into the cells and fibres of the
 “ ivory by the deposition of osseous matter.’ ”

“ As a specific and direct confirmation of the above public

“ testimonies to the consistency of my opinions, it was with
 “ great pleasure that I was able to submit to the Council the
 “ following documents, which contain an account of the
 “ nature of my communications drawn up by three eminent
 “ physiologists, who saw them *before* I took them to Bir-
 “ mingham, and consequently, before there could be any
 “ motive for misrepresentation ; but I am sorry to say that
 “ even the receipt of them, as was the case with the other
 “ gratuitous aids to inquiry which I have afforded, has
 “ never been acknowledged :

“ 6, *Holles Street*, March, 17, 1841.

“ ‘ MY DEAR SIR,

“ As you have asked me to state my recollections of a
 “ visit to your house for the purpose of seeing your dia-
 “ grams, and hearing your demonstration of them prior to
 “ your visit to Birmingham in 1839, I can have no hesita-
 “ tion in complying with your wish.

“ On the afternoon of the day immediately prior to your
 “ starting for Birmingham, where you were engaged to read
 “ some papers at a Meeting of the British Association, on
 “ the minute structure of the teeth, I availed myself of your
 “ invitation, and met several gentlemen, amongst whom I
 “ well remember to have seen the late Sir A. Cooper,
 “ Mr. Liston, Mr. Burrowes, Mr. Gulliver, the Rev. Mr.
 “ Daniell, and one or two others. On the floor of your room
 “ were laid a great many very beautiful diagrams on various
 “ subjects connected with the development of ivory, enamel,
 “ the pulp of the tooth, &c., as well as some on the micros-
 “ copic characters of epithelium. You explained your
 “ views of these drawings, and read various extracts from
 “ the papers you were about to communicate to the Associa-
 “ tion, and this meeting I considered as a private view
 “ to your friends, none of whom I believe were able to
 “ be present at the Association.

“ I distinctly remember being struck with one particular
 “ diagram, which I then considered as demonstrative of the
 “ views you then expressed as to the conversion of the
 “ cells of the reticulations of the pulp into the cells of the
 “ ivory. In answer to several questions of Sir A. Cooper,
 “ who told us he had many years before attentively studied
 “ this subject with Mr. Fox, you controverted his opinions
 “ as to the formation of the ivory being simply an ossific
 “ secretion from a vascular pulp, and showed him the cor-
 “ respondence in figure and size between the cells of the
 “ ivory and those of the reticulations observed on the sur-
 “ face of the pulp. I recollect, also, most distinctly observ-
 “ ing, in the particular diagram alluded to, that there were
 “ several leaf-like processes, which I could not help com-
 “ paring, in my own mind, with some of the fan-like coral-
 “ lines, which were coloured partly blue and partly yellow :
 “ you observed that the artist had in all the diagrams pre-
 “ served the same colour for similar structures ; thus, in
 “ all, the yellow represented the ivory, and the blue the
 “ pulp. Upon being struck with the exact resemblance
 “ between the yellow and blue portions of this drawing,
 “ we were informed that in the lighter parts (yellowish) the
 “ pulp cells were in a transition stage of change into ivory ;
 “ and in those more deeply tinted yellow, that change had
 “ been completed ; and that the original curiously-arranged
 “ pulp cells had been converted into bone. There were
 “ several other diagrams more or less demonstrative of this
 “ fact, but I well remember to have considered, at that time,
 “ that this particular diagram distinctly demonstrated the
 “ facts you then tried to impress upon us, because of its
 “ representing in one view the successive changes from
 “ original pulp cells into the complete ivory of the tooth.
 “ My memory does not serve me as to whether the drawing
 “ was made from the human or some animal’s tooth ; but I
 “ satisfied myself of the correctness of the artist as to the
 “ arrangement of the cells and the reticulations, by an ex-

“amination of a preparation which was placed under the
 “microscope in another part of the room, which I examined
 “immediately after Sir Astley Cooper, who expressed
 “himself satisfied of the fidelity of your artist.

“Yours faithfully,

“(Signed)

JOHN DALRYMPLE.

“A. Nasmyth, Esq.’ ”

“ ‘ I have read Mr. Dalrymple’s statement, and am able
 “to confirm it in every particular ; indeed, my recollection
 “of the transactions in question coincides precisely with
 “Mr. Dalrymple’s narration.

“(Signed)

GEORGE GULLIVER.

“*Hyde Park Barracks, March 17, 1841.*’ ”

“ ‘ I recollect perfectly the meeting at Mr. Nasmyth’s
 “house above referred to, and I have the same impression
 “of what then passed as Mr. Dalrymple and Mr. Gulliver
 “have expressed.

“(Signed)

ROB. LISTON.”

“*5, Clifford Street, March 17, 1841.*’ ”

In a printed letter, addressed to me by the Secretary,
 in the name of the Council, dated 30th of March, 1841,
 it is expressly stated “that the Association has avoided,
 “and, consistently with its objects, must carefully avoid
 “erecting itself into a tribunal which shall judge of the
 “value and priority of scientific researches. All such

“ questions must be discussed by other modes of publication.” Yet, in the face of this declaration, they on the one hand, show no respect for the unanimous public opinions just quoted, nor the private opinions of men whose authority is looked up to in this particular branch of study ; neither do they acquiesce in Mr. Owen’s desire that the controversy should be considered at an end ; but, on the contrary, they adopt every conceivable device to deprive me of my rights.

The next “allegation” is that “the author has made use of the proof of his memoir, as so altered, to found a charge of plagiarism against” Mr. Owen. There is here an equivocation which it is proper to point out. A few lines above the sentence just quoted, Mr. Owen talks of my “paper in the forthcoming volume ;” but now he talks of the “memoir” itself, which, he says, has been altered in the proof. By order of Mr. Phillips, a paper was certainly drawn up by me to include all the three original memoirs, with the drawings, descriptions, and demonstrations ; and, in this state, it was approved of by Mr. Phillips, and printed in place of the original communications. With respect to the assertion that I made use of the “proof of my memoir” for the purpose of charging Mr. Owen with plagiarism, I have no other expression to apply to it than that it is a *pure invention*. The proof was used by me for its legitimate purpose and no other, namely, that of correcting the press ; and I defy Mr. Owen to bring forward any scrap of a printer’s proof of my report which has been put by me to the use asserted, in extenuation of such ungrounded exculpatory “allegations.”

If I had desired to bring forward a charge of plagiarism against Mr. Owen, I had the originals of my own communications in my possession, containing all that I had exhibited, said, and thus published to the world at

Birmingham, under the guardianship of Mr. Owen himself. I had also the reports by others of what they knew to have been said and shown, contained in the journals. But the unpleasant task of charging Mr. Owen with plagiarism did not remain for me to perform. It had been already accomplished by the public reviewers, and if Mr. Owen has failed in getting any of the medical journals of this kingdom to withdraw that charge, the failure must be attributed to the lack of argument on his part, and not to the use of my printer's proof. If I had considered it worth my while to enter the lists of controversy with Mr. Owen, I had plenty of materials previously in my possession relating to other subjects, without having recourse to the disgraceful expedient of purloining that which the editor had entrusted to me.

To give greater effect to his tale, Mr. Owen speaks of the public criticisms of his book as being "anonymous"—a very silly observation, seeing that the editors of all the journals are perfectly well known, and incur the responsibility of everything contained in their pages, no name being ever appended to any criticism.

Mr. Owen makes this assertion respecting the printer's proof truly ridiculous, by admitting and stating in the same letter, that I "obtained *private copies*." He dares not insinuate that these copies were obtained by me in any improper manner, and they having been put into my possession in the regular way without any restriction whatever, I, of course, proceeded at once to distribute them. I immediately sent copies to two journals and to the Institute of France. The others would all have very shortly been circulated had I not received Mr. Phillips's communication of the 24th June, 1840, on the receipt of which, even the two copies which I had sent to the journals were withdrawn, and no others were allowed to leave my possession till some were made use of for the information of the Council.

Mr. Owen again exercises his inventive powers by stating, that I transmitted copies to "other societies;" to which all I have to say is, that it is not true.

Having been reluctantly drawn into the necessity of "explaining and justifying my views of dental development" by the editor of the *Lancet*; I, in order to demonstrate that there was no occasion for raising a clamour about the private copies of my own Report that were delivered to me, made use only of those materials which were open to all the world—namely, the reports in the public journals, *throwing aside my own report altogether*. Of this, Mr. Owen is quite aware.

When a man brings forward serious charges, with the deliberate intention of injuring a brother-associate in the eyes of his fellows, for his own protection against opinions expressed out of the Association, the chronology of all events connected with such a charge ought to be rigidly accurate. So far is this from being the case with Mr. Owen's statement, that it contains not a single correct date. Whether these errors were intentional, or arose from carelessness, they are altogether unpardonable in such a case. Mr. Owen twice states that his "discovery" was published December 16th, 1839, whereas it was only read on that day, and published on the 23rd. He also speaks of the *Literary Gazette* containing a "verbatim" report of Mr. Nasmyth's Memoir, as read at Birmingham, August 12, 1839." All which is entirely incorrect, the fact being that, of three memoirs, one was read on the 1st of September, and the others on the 2nd of September. It must be added that the use of the word "verbatim" is a misrepresentation in so far as regards the memoirs in which he was concerned, it being the memoir on the epithelium alone, which is stated by the Editor of the *Literary Gazette* to have been given "verbatim." Any one reading the Report, in that Journal,

knows this, as it is expressly stated without the possibility of mistake. On this flagrant misstatement the whole excuse for Mr. Owen's conduct rests.

Such are the materials of which this memorable production is composed! In the whole document, there is not one allegation that is true, and the reason is now quite apparent for keeping it out of sight, till the business was sought to be considered at an end. Often as the alleged appeal or specific charge was called for by me, I never saw these "allegations" till I saw them in print forming part of the transactions which are pretended to have taken place between the Association and myself. I must remark, also, that the compilers of the "Addendum" have been guilty of an attempt to deceive the reader into the belief that I was cognisant of the contents of Mr. Owen's letter; for they have not only given it a leading place in the "Addendum," but they have said at the middle of page 7, that "On the 5th of September, Mr. Nasmyth enclosed to Mr. Yates the following letter, to be laid before the Council at the same time with Mr. Owen's appeal." Thus endeavouring to imply that I, being acquainted with the precise tenor of Mr. Owen's appeal, desired that my letter should accompany it.

The foregoing statements have, I trust, enabled your Lordship to estimate Mr. Owen's "allegations" at their just value.

I now proceed to make a few remarks on the conduct of Mr. Phillips, and in the first place I would observe generally, that there seems to be no connexion between that gentleman's proceedings and the wishes of Mr. Owen, by which he professes to have been regulated; for instead of giving me the option as proposed by Mr. Owen, of "substituting the original memoirs" read at Birmingham, he proceeds at once to "suppress" my report, not only with regard to what Mr. Owen calls the "question at issue," but also with regard to *all other subjects*, although

entirely unconnected with it, and forming nine-tenths of the entire substance of my memoirs.

In noticing Mr. Phillips's conduct at this stage of the business, I must request the reader to keep in mind Mr. Owen's method of talking of the "alleged alterations;" and then notice the novel turn which the Editorial Secretary gives to the charge, in accusing me of making what are now called "additions." This accusation is contained in a letter dated June 24th, 1840, which is given at page 3 of the "Addendum." He does not deny that he "forwarded the MS. to be set up," but pretends to be "concerned to find" that I "have made additions to it since." My answer to this letter which the compiler of the "Addendum," with a show of justice, presents to the reader, might, I think, at all events have drawn from Mr. Phillips a more explicit avowal of the particulars of these alleged "additions," and a statement of the heinous offence committed in making such "additions," which were of course, notwithstanding the attitude of "concern" he assumes, as much under his control as the acceptance of the original manuscript, the sending it to be printed, the transmission of proofs to me in his own name, and the delivery of the perfect copies with the words "Reprinted from vol. 8 of the Transactions of the Association," put in the title-page by him or his agent. He pretends, however, to be "concerned" at all this having occurred, although he himself wrote to me, referring me to the printer, from whom I should obtain the private copies, and admits at page 3 that I was not to "be regarded as at all responsible for irregularities," if such had occurred.

The manner in which this discussion had already terminated, and the apology made by the person who advanced the allegations now given as an excuse for the conduct of the Council, ought surely to have satisfied Mr. Phillips of the unfounded nature of the charges against me.

It will, perhaps, be necessary to make a few remarks in addition to those contained in my letter to Mr. Phillips, dated 27th June, 1840, regarding the charge of making "additions" or "alterations," which Mr. Owen and Mr. Phillips, each in their own fashion, have advanced against me, without furnishing the least shadow of proof.

No observation nor theoretical opinion was added by me to the Report in its passage through the press, nor, in fact, any new matter at all in connexion with the subject which Mr. Owen alludes to in his letter to Mr. Yates.

The whole tenor of the Report on the subject of the development of the teeth, which is that on which Mr. Owen arrogates to himself a "*nouvelle théorie*," is simple and uniform, in exact accordance with the original communications, and tending distinctly to the establishment of the theory expressed at page 19 of the "Addendum," namely, that "the ivory is neither more nor less than the ossified pulp."*

This received the sanction of the Editorial Secretary, after many weeks' deliberate consideration, and there being no deviation from it throughout the whole paper, it ought to have been respected by him, and he should never have lent himself to such an impropriety as he committed by obeying the mandate of any individual member to "suppress" what he had so deliberately "approved,"—what he had guaranteed to "insert in the forthcoming volume," and had, in fact, positively inserted, furnishing me, through his agent the printer, with private copies.

I allow there was one distinct "addition" which I made to the Report while passing through the press. It is contained in a foot-note, and relates to the investigations of Schwann, of which I gave a particular and rather lengthened notice in the original Memoir, but as

See the original Memoirs and Drawings, *passim*.

Mr. Owen professes his total ignorance of Schwann as an investigator of this subject, notwithstanding the lengthened notice of his labours which accompanied my Memoir, was read at Birmingham as forming part of it, and was partly reported in the journals of the day, it seems very inconsistent in Mr. Phillips to consider it "grievous" that I, on detecting a deficiency in my report, should have hastened to do justice to such an accomplished investigator, whose labours being avowedly unknown to Mr. Owen, could not be supposed to interfere with him in any way.

Another portion of Mr. Phillips's letter, in which he says, "What astonishes me more, is to learn, that without my knowledge you have received copies of the paper," contains a misrepresentation, the evidences of which are furnished by the compiler himself in the preceding page of the Addendum; but are placed at such a distance from each other, that the reader cannot without care connect them. It is there stated, that "when the proofs were finally corrected, Mr. Nasmyth applied to the printer for 100 private copies, and was informed that without an order his request could not be complied with until the volume was published. Mr. Nasmyth then enclosed, or brought to the printing-office, a note from Mr. Phillips to himself, the exact purport of which Mr. Taylor cannot now recall, but which appeared to him at the time to authorise a compliance with Mr. Nasmyth's request." (Addendum, p. 2.) This contains another of those misrepresentations in which the narrative abounds. I beg to state that I never requested, either of Mr. Phillips or Mr. Taylor, that copies should be delivered to me before the volume was published. On the contrary, I distinctly intimated to Mr. Taylor, in speaking of the copies, that I wished for nothing but what was regular. The above extract, however, will evince, by the showing of the compilers themselves, that

Mr. Phillips, previously to his making the assertion in his letter of the 24th of June, had sanctioned the delivery of private copies to me. Moreover, he has elsewhere declared, that I "could not be regarded as at all responsible for these irregularities;"—pretending to assume as such the delivery of private copies to me. The printers' evidence, in my possession, corroborates my statement, as will be seen from the following letter:

"Red Lion Court, Fleet Street, June 25, 1840.

"SIR,—I have a perfect recollection of seeing Mr. Phillips's letter to you, authorising us to print you separate copies from the Report of the British Association; but I saw it only for a few minutes, and have searched repeatedly in vain for it since. I still hope it may turn up from among our numerous papers, and if so, you may depend on its being returned to you.

"I am, Sir,

"Your obedient Servant,

"C. GYDE,

"For R. and J. E. Taylor.

"To Alex. Nasmyth, Esq., &c. &c. &c."

Whatever subterfuge may be had recourse to, the admissions which Mr. Phillips himself makes are quite sufficient to have warranted that gentleman in refusing to interfere at the instance of an interested party, seeing that, if any irregularity had occurred, it arose entirely from my being subjected to the influence of the errors or negligence of others.

At this stage of the business, I must say I was very much chagrined and disappointed to find such a decided resistance made to affording me the least satisfaction by the officer whose duty it was to arrange these matters. I certainly conceived that he had no right to address himself to me at all on the subject, and that I ought not to have been obliged to defend myself from such irregular attacks. If anything had been done it ought to have

been by a new transaction, grounded on the exhibition of evidence. But instead of this obvious course, the records of the Association were mutilated, and I was left to defend myself against a concealed enemy, who dared not even exhibit his charge against me. Notwithstanding this ungenerous hostility on the part of Mr. Phillips, I offered to begin the transaction anew with him, to take the trouble of going over the whole of it from the original communications, and even to repair personally to York for that purpose. My surprise was great when I found that this offer, which I considered so liberal, was treated with contemptuous silence. I was fearful at the time that this was the harbinger of measures such as those which have been adopted, yet I could not but think, that however the Secretary had forgotten himself, the Council would be willing to make ample reparation, although I was suspicious that he must have powerful abettors to embolden him to adopt such a course. It turns out to have been so, and the secretary's excuse is now exhibited at p. 7 of the "Addendum" *for the first time*—like most of the other exculpatory documents contained in that narrative. It is in the following words:—"Mr. Nasmyth's offer to convey "his original memoirs to York, for Mr. Phillips's satisfaction, was declined, as an appeal having been made to "the Council, the case was removed out of his jurisdiction." Here is another prevarication, inasmuch as I not only offered to convey "my original memoir," but "the whole of my preparations, drawings, and original "manuscripts"—the memoirs being three in number. It should here be observed, that the foregoing excuse for Mr. Phillips's conduct, renders it evident that the Council did actually receive my offer from his hands. How that offer was treated the sequel will show.

The inconsistency of the above excuse must be obvious to any one. If the case was removed from his "jurisdiction" in one point, it must have been in all, and ought,

of course, to have been left in *statu quo*, until the "appeal" had been heard. Instead of which, although pretending to be precluded from exercising his judgment in a simple matter of fact, he does not consider himself precluded from exercising his power to the destruction of the property of the Association, and the infringement of my individual rights.

Without the smallest inquiry into the facts of the case, the Council approved of Mr. Phillips's precipitate and arbitrary proceedings; and notwithstanding they had thus assumed a serious judicial duty, they at once tried to divest themselves of responsibility, by devolving on a committee of gentlemen resident in different parts of the kingdom, an inquiry quite unconnected with the correctness of my conduct, or the authenticity of my report. The most incongruous opinions, amounting to an entire mistake of the subject of my researches by that Committee, were the result of the inquiry.

The conductors of this affair, and the framers of the "Addendum," not content with sacrificing the interest of their friend Mr. Owen, by forcing his fabrications into view, and acting in a manner diametrically opposed to his publicly-declared wishes, proceed to record the above transaction, and place the actors in it in an unfavourable point of view, in order to keep the truth in the shade, and gain a colour for a charge against me. In their resolution of the 15th of September, 1840, a committee is requested, "to decide whether the Report of "Mr. Nasmyth's paper, as published in the Literary "Gazette and Athenæum, or in either of those periodicals, "or the Report of that paper sent by Mr. Nasmyth to "Mr. Phillips for publication, in the Report of the "Ninth Meeting of the Association, held at Birmingham, "is more correct in regard to the points under discussion "between Professor Owen and Mr. Nasmyth." (Addendum, p. 8.)

It is here strange, in the first place, that such a minute should contain the words "under discussion," when the discussion had already terminated. In the second place, the Council, instead of acting on my offer, conveyed to them through Professor Phillips, of submitting to examination the original documents in their entire state, or directing their delegates to do so, make no mention whatever of my original documents in their instructions to the Committee quoted above. The Committee, thus installed, acts up to the very letter of these strange instructions, and never even intimates its existence to me at all, nor asks for the legitimate evidence.

Thus Mr. Owen, as Conservator of the Museum of the College of Surgeons, and Councilman of the British Association—Mr. Phillips, in his character of Assistant-General Secretary and Editor of the Transactions of that Association—the aggregate body of the Council of the Association convened at Glasgow—and the Committee of Delegates appointed by the Association—all these, as well as the compiler or compilers of the "Addendum," while professing to act with one accord, commence hostilities, each by their separate and distinct mode of attack, against a single member of the Association, not one of these modes of attack having any direct reference to what is *now* brought forward as the groundwork of the whole, namely, the letter of Mr. Owen to Mr. Yates, of July 18, 1840.

It is quite evident that the novel tactics prescribed to this committee of judicial delegates had nothing to do with the real points at issue between the Association and myself, namely, whether my conduct was honourable or not, and my abstract faithful to the original communication or otherwise. Their only effect was to keep alive a mischievous dispute, which I had not commenced, and never willingly interfered with, and which the other party had publicly declared "at an end."

However, it seems three gentlemen out of the six, residing in separate and distant parts of the kingdom, were found willing to engage in such an ungracious task, and they proceeded to their work in perfect secrecy so far as I was concerned. I neither knew who had undertaken the business, where they had met for deliberation, or on what evidence they tried the question, till I received the laconic intimation contained in the following letter :

“ 49, Upper Bedford Place, Russell Square,
“ November 19th, 1840.

“ Sir,

“ The officers of the Medical Section of the British Association at the Birmingham Meeting, having returned
“ their answers on the question referred to them on the
“ 15th of September, the Council assembled this day, and
“ passed the following resolution, requesting me to communicate it to you ; viz :

“ ‘ That the Council do not consider it necessary to
‘ make any further publication of Mr. Nasmyth’s communication, than the notice inserted in the Report of the
“ Ninth Meeting of the Association held at Birmingham.’

“ I remain, sir,

“ Yours, respectfully,

“ JAMES YATES,

“ *Secretary to the Council.*

“ A. Nasmyth, Esq.”

An earnest remonstrance of mine against such injustice produced a revocation of this arbitrary edict, and a new order to the delegates ; but even a simple narration of this fact cannot be given without another deviation from truth. In mentioning the reception of my communication this phrase is used, “ Expressing his readiness and
“ desire to lay before the Council the engravings from
“ the drawings which had accompanied his papers read
“ at Birmingham.” (Addendum, p. 9.) The fact being that at this very date, fac similes of them were actually

in the possession of each individual councilman, appended to separate copies of the report which had been delivered to me. It will be seen, however, from what follows, that they did not choose to take these ocular evidences into consideration themselves, or even to remit them to their delegates, although, in the strong remonstrance which I presented to them, I stated that, "By their means you will at once be able to convince yourselves, indisputably, of the accuracy of my abstract; to see, at a glance, the original anatomical evidence on which my papers were founded; and to estimate the justice of the comments in which its suppression originated. The titles, even, of these engravings, have been copied from those of the drawings themselves, so that the whole may be regarded as the actual communication itself. They require no comment, and I respectfully demand your attention to them, confining myself to putting this simple question to any person who has examined these drawings—*How my report, in order to be accurate, could have been worded otherwise than it is?*"

Hemmed in by this unanswerable appeal to their common sense, and unable to resist the conviction which must have followed from an acquiescence in my request, the Council determine on overlooking altogether this original and principal evidence, which, if employed to illustrate any of the accounts given of my researches, must at once have cleared up every difficulty that could have been found or invented. It is therefore carefully kept out of sight in the "Addendum," that each of the councilmen had fac similes of the diagrams at that time in their possession. They try to get out of this dilemma by desiring "Mr. Yates to obtain from Mr. Nasmyth the original memoir or memoirs read by him to the Medical Section at Birmingham," "and to refer them to the authors of the report to the Council," "for the purpose of enabling them to decide upon the correctness of the

“abstract presented by Mr. Nasmyth for publication.” There is here another inaccuracy, inasmuch as this abstract was not “presented” by me. It was drawn up by me at the express desire of the Editorial Secretary, who also directed that it should contain a succinct and “consecutive” account, not only of the “memoir or memoirs read by me to the Medical Section,” but also of that read to the Geological Section, as well as that on the epithelium. It was, moreover, expressly required by the Editorial Secretary, that it should comprehend the diagrams and their descriptions. This committee, however, is “requested to report the result of their inquiries” from one single and isolated portion of the original communication, with a view to determine the truth of the abstract containing a “consecutive” account *of the whole*. It is really quite astonishing to me, that a number of gentlemen should commit themselves by such a strange incongruity, and at the same time suppose that I should acquiesce in it. The whole of my original communications were free and open to the inspection of every one, and of course to these delegates. This I intimated both verbally and by letter; but for a partial investigation I would afford no facilities; and also, in the absence of a specific charge against myself, I should certainly have objected to engaging in a conflict in the dark with a body of delegates, invested by the Council with the authority of umpires without my sanction, more especially as these gentlemen had already fallen into the extraordinary mistake of supposing that I was treating of “dentition,” whereas that subject was never alluded to by me. Even to these gentlemen, however, they were open for inspection, if taken in their entire state. But such an inspection not suiting their purpose, they found it necessary to invent means of avoiding it, and recourse was had to the disingenuous expedient of refusing to look at

them, because I had offered to present them personally, and point out to them the new and old portions of the manuscript and the drawings which had been subsequently placed in the folios.

In excuse and explanation of these abrupt and arbitrary proceedings, a letter signed P. M. Roget, and addressed to the Rev. James Yates, is *now for the first time* produced at p. 10 of the "Addendum," and is introduced there as if it had formed part of the transactions with me. This letter contains an account of certain scruples of Dr. Macartney's, concurred in by Drs. Roget and Rees, which are meant to serve as reasons for not accepting my offer of delivering to them the entire original communications as read and exhibited at the Birmingham Meeting, divested of the matter since added to them. I beg, however, it may be distinctly understood that the existence of this letter, like that of most of the other important documents contained in the narrative of the "Addendum," was entirely unknown to me till I saw it in print, and consequently, that no opportunity was afforded me of overcoming the scruples therein expressed.

Persisting in a refusal to consider the main portion of my communications, namely, the diagrams, and requesting the renewed assistance of the committee, to which they add the officers of the Geological Section, the Council, in a printed letter to me, dated March 30th, 1841, remark, "that if they were put in possession of a copy of the original memoirs, duly authenticated, they might find it proper to publish an abstract of them in a future volume." The Council could not suppose that such degradation would be submitted to; and as matters now stand, I cannot but express my indignation that such a proposal should have been made to me by the very body which had received such a document as Mr. Owen's

letter, without the smallest hesitation, and whose authorised compiler had been guilty of unwarrantable interpolation.

Not being cognisant, at that time, of the nature of the "appeal" now produced, I was not aware of the full extent of the indignity offered me; but I knew that a cause had been tried without any definite charge, without any citation of the accused, or demand for his evidence; that a verdict had been returned and received against him, which had no direct relation to the matters at issue between him and the Association; and that a uniform resistance had been offered to the consideration of the most conclusive evidence in possession of every member of the Council, and to the reception of the testimony of competent judges. On the back of all this it was expected that I, for the satisfaction of the gentlemen acting thus unhandsomely towards me, and in the vague hope "that they might find it proper to publish *an* abstract," should *authenticate* an isolated portion of my communications to be taken as proof of the whole. I cannot persuade myself that such an offer was ever made but for the purpose of being rejected, it being obviously impossible that at such a stage of the proceedings I could admit of any compromise, or consent to the publication of anything more or less than *my own* authentic abstract.

An explicit answer was given by me to the whole letter of the 30th March, and circulated to each individual of the Council, though all notice of that answer is omitted in the account of the transactions now published. The cause of this omission may be found in its containing a proposal, which, from its reasonable and comprehensive nature, was justly entitled to adoption, and could only be evaded by silence—namely, to refer the physiological part of the dispute between the Council of the Association and myself to the arbitration of the Royal College of Surgeons, a step which had become the more necessary, because the un-

equivocal and united testimony of three eminent physiologists in my favour had proved insufficient to convince the Council that truth was on my side.* I was additionally justified in proposing this arbitration, inasmuch as the Council had declared, in their printed letter of March 30th, p. 3, that "in a matter depending on delicate physiological considerations," "the Council did not consider themselves to be adequate judges." Declaring this incapacity, "they therefore thought they could not in any way further the ends of justice and of science so well, as by referring the question to the president and officers of the Medical Section." These gentlemen, however, must, at the least, have been very inattentive, since they entirely mistook even the subject of my researches, imagining it to be "dentition," a theme never once touched upon in my communications. All notice of this proposal, and the circumstances connected with it, is entirely kept out of sight by the compilers of the "Addendum," and the reasons for this remarkable omission must now be sufficiently obvious.

It was with an earnest desire to give every one concerned an unobjectionable and honourable opportunity of freeing themselves of all difficulty and embarrassment, that I respectfully but strongly urged this appeal, and I certainly thought that the utmost stretch of ingenuity could have devised no means of escape from a proposal so reasonable. The exhaustless tact at subterfuge was, however, again called into exercise, and a new set of antagonists brought up against me, who, like the former classes of combatants, take up an entirely new position, and come armed with new weapons. Limited as were the former demands for original documents, my new antagonists restrict them still further. Notwithstanding the former refusal to meet me personally, they order me into their presence, disarmed of everything but the paper which was

* See page 25.

read to the Geological Section at Birmingham, and thus they not only wish to deprive me of my means of defence, but make a strange attempt to drop the original subject altogether, and get me to acquiesce in the substitution of another, involving nothing more than a scrutiny as to the actual writer of a short report, a proof* of which was sent to me under cover of a note from Mr. Phillips, on the 2nd of April, 1840, wherein Mr. Phillips, the editor, and therefore the only person whom I knew in the matter, expresses himself exactly as I do in the report furnished by me. Mr. Phillips confessed himself responsible for that report, and yet, in the face of his written avowal, still in my possession, a story is trumped up in connexion with this, about a report made out by Dr. Lloyd, whose name had never before been heard of by me in the transaction, but in which the compilers pretend to find out that a certain sentence is not contained. They have not, however, the hardihood even to affirm that I had anything to do with this report which was sent to me under cover of a note from Mr. Phillips, and, therefore, the story is irrelevant and ridiculous. The compilers forget that it is acknowledged, at page 2 of the Addendum, that "all the other sheets of the volume," except those of my own reports, of course including this which was sent to me, "passed through Mr. Phillips's hands in their progress through the press;" and this being the case, he ought at once to have found and produced the manuscript of that report which was sent to me, or to have acknowledged the origin of the whole of this transaction. It is, however, of no consequence whether the original manuscript contained the particular sentence alluded to or not, since even this *treasure trove* of argument contains quite enough to frustrate the attempt which has been founded on the absence of one particular sentence. So far as it goes, it is conclusive of

* I call it proof, because Mr. Phillips, who sent it to me, calls it proof, and this notwithstanding the quibble that is now got up about the word "revise."

the character of my views of the "organised cellular nature" of the ivory. The pulp is said "to be observed "to be cellular throughout its internal structure, and "this structure is essentially concerned in the development of the ivory;" this doctrine being the whole and sole undeviating tenor of that portion of my communications.

The covert insinuation infused into the published account of this particular part of the transaction is really too ridiculous to require comment; and I leave it to confute itself, with the accompanying explanatory narrative of the now obvious purposes of the proposed conference with me at the rooms of the Royal Society. The pretended call for "the original paper" will be easily appreciated, and the abrupt refusal of the terms proposed by me will be as easily seen through.

In proportion as the exigences of the case were extreme, the tactics of the Council have been eccentric, and they have not only abandoned all ordinary principles of justice, but have overleaped those regulations and limits of conscience, which they prescribed to themselves in the consideration of this subject. Three of the delegates, be it remembered, are the self-same gentlemen who professed such a conscientious delicacy only a very short time before, as recorded in the second page of the "Addendum," in these words:—"We have thought it right to protect "ourselves from all suspicion of being biassed in our "judgment by ex parte statements or representations; "and we have accordingly scrupulously avoided having "any communication with Mr. Owen, either directly or "indirectly, on the matters in dispute. For the same "reason we must decline the proffered interview with Mr. "Nasmyth." What was the real weight of these scruples may be estimated from the fact, that the very gentlemen in question, being powerfully reinforced by a reserve sent by the Council to "assist the Committee in forming their

judgment," become suddenly relieved of their scruples, and have no objection to meet me, provided I bring forward only one portion of the original documents, and allow them to change altogether the subject of discussion.

In the printed letter of the Council of March 30th, 1841, it is stated that "in a matter depending on delicate physiological considerations," "the Council did not consider themselves adequate judges," the geological delegates now added, being, of course, included in this category. Yet, by the 5th of May, these same gentlemen had acquired such a delicate knowledge of physiology, as to become in their own opinion competent judges of the matter at issue, and wished me to recognise them, and bow to their authority as such! Any comment on these facts would be superfluous.

This strange narrative ends in a manner worthy of its commencement and course, namely, by the following assertion:—"Mr. Nasmyth having thus made no reply to their request to be allowed to see his original papers, your committee cannot proceed further in the inquiry." Trifling as this little terminal sentence appears to be, it is a genuine specimen of the whole composition. It is another of the *ex-post-facto* documents, given with all the air and semblance of a portion of the transactions with me. It is moreover at variance with fact, though put forth with no small hardihood, since evidence to the contrary glares in the eyes of the reader from the opposite page of the Addendum, where reference is made to two letters on this subject, dated 17th and 27th of April. In fact, my original *papers* were not in the smallest degree required for the matter then under discussion, and if required could not have been disjoined from the rest of the communications. The Council had been often told so, and yet whenever any allusion is made to my original communications, it is always to an insulated portion of

them. In the instance to which the word "thus" applies, it was only one *paper* which was asked for, namely, the Geological one; this being another of those cunning prevarications in which the Addendum abounds.

Thus the plausible story respecting my not giving up the original communications resolves itself into this:—My offer, "to convey the originals of my communications to York," was declined by the Editorial Secretary, because, forsooth, "an appeal having been made to the Council, the "case was removed out of his jurisdiction." These are the words in which the excuse is now for the first time put forth in this "Addendum." The Council having received this business from Mr. Phillips's hands, instead of taking up the correspondence with me where that gentleman professes to have dropped it, and immediately acting on my offer, proceed to entrap three gentlemen, as above explained, to sit in judgment on an entirely irrelevant question, *without the slightest reference to my original communications*, and ultimately refuse these original communications, offered to them, in the exact and entire state in which they were delivered to the meeting at Birmingham; whilst the main portion of these original communications, namely, the drawings with their explanations, although brought before the notice of every individual member of the Council, by fac similes of them being transmitted to each by me, were excluded altogether from their consideration. They make attempts to evade a faithful consideration of the question, by asking for insulated portions of the communications; first for the "Memoirs read at the Medical Section" alone; abandoning that position, they next ask for the "Geological" one alone. The compilers of the statement sanctioned by the Council, at the close of these transactions, adopt similar manœuvres, and refuse even to read or satisfy themselves from the original communications, in their entire form, which had been before

the public nearly ten months previous to the appearance of the volume containing the "Addendum."

In the 23 pages of letter-press, which are occupied by this "Addendum," pretending to be a history of the transactions which have taken place between the Council of the Association and myself, I have also shown that there are four documents over which the compilers had a control, and from each of these an attempt has been separately made to derive a shadow of exculpation. The discretion exercised, and the spirit in which this task has been performed, may be gathered from the following brief recapitulation.

The first of these documents, namely, the letter from Mr. Owen addressed to Mr. Yates, is here advanced as the source from which all these proceedings have flowed. This document, so far as I am concerned, has never found a place in the transactions at all, and, still worse, it is made up of allegations totally devoid of foundation.

The second document, Mr. Phillips's letter addressed to me, August 10th, 1840, is set forth to excuse that gentleman's unjust and illiberal conduct towards me, and his interference with the rights of the Association. This document, besides being modified by the compilers, contains a deliberate interpolation.

The third document, namely, the letter from Dr. Macartney to Mr. Yates, dated the 12th of January, 1841, as well as that in which it is contained, of the 21st inst., is intended to form a loop-hole for the escape of the gentlemen who had been entrapped into the position of judges in a trial where no charge was exhibited, no defendant cited, and no original evidence called for; the decision arrived at by them, being in strict accordance with these unjust and ridiculous proceedings. This is also another of the *ex-post-facto* exculpatory documents, which have never formed part of the transactions with me.

The fourth document is exhumed on purpose for this occasion from among the literary sketches of Dr. Lloyd, and also now introduced for the first time to my notice. I have no objection to admit that it is well written, and somewhat to the point, although redundant. The only thing to be deduced from this evidence is, the demonstration of an insufficient acquaintance with my researches on the part of the compilers of the Addendum themselves. In fact, it is in itself totally inert, though its production now is another indication of the uniform spirit which the whole of the compilers' statements manifest. It cannot be of the least avail in forming a judgment of the case, whether or not Mr. Phillips attempts to repudiate this as an illegitimate production which is fathered on him, and he has acknowledged, or is the offspring of any one else, it being quite immaterial as to the affiliation of such opinions by that gentleman, because in the consecutive abstract or Report which I drew up for him, and which he received, retained in his possession, analyzed, and modified, printed and recorded, in the Transactions of the Association, and copies of which he furnished me with for private circulation—the self-same epitome of the theory of dental development, derived from the investigations, observations, and delineations presented to the meeting at Birmingham by me is contained. It will be seen as thus recorded by him near the bottom of page 19, and is in these words:—"He concluded, therefore, that the ivory "is neither more nor less than the ossified pulp."

Under all this mass of opposing evidence, and in the face of their own productions and statements to the contrary, the compilers of this document have yet the hardihood to assert that "the Council having failed in their "frequently-repeated endeavours to obtain Mr. Nasymth's "original papers, or assured copies of them, are unable, on "the one hand, to publish the latter authenticated as a "faithful report of the papers read by that gentleman, at

“ Birmingham, or, on the other hand, to decide that it is
 “ not a fair report of those papers.” (p. 13.)

This assertion is also made to follow the narration of a trial made to entrap me into a single-handed discussion with three gentlemen who had been induced to commit themselves in the expression of an opinion without evidence, to whom were added the members of the Council forming the officers of the Geological Section; *it now appearing that the subject into which I was to be drawn, and which they would have discussed, was one in which I was not at all interested, with which I had nothing to do, and which involved merely the conduct and merits of their own officers.*

I now leave it, my Lord, to yourself and to the dispassionate portion of the Council, to judge whether the preceding facts afford materials for gratulation regarding the truth of this document or the integrity of its authors; and your Lordship will now doubtless be able to estimate the degree of aversion with which I have entered on a subject, the details of which are so repugnant to all honour and justice.

That those honourable and upright gentlemen who compose the Council of the Association have been deceived, and are not individually conscious participators in the erection of such an unseemly fabric, I am fully persuaded. The case has been designedly too much involved for them to unravel. Mr. Owen even may have misunderstood, or not attended to my investigations, and if such were the case I wish he had at once avowed it, as he did his unacquaintance with the labours of Schwann, although detailed by me at the same time with my own. Had he pursued this course, there would have been no need of the inventions to which he and his abettors in the Council have so unhappily resorted.

As the matter now stands, however, an individual member of the Association is paramount to the whole, break-

ing down its regulations, and scattering to the winds the leaves of its recorded transactions. He has also influence to induce an officer paid by the members at large to obey his arbitrary mandates, and, what is more strange, gets a majority of his brother councilmen to approve of these disorganizing measures, as well as some person or persons with hardihood sufficient to brave the public obloquy of detected deviations from truth.

No apology is necessary on my part for at once laying this letter before the public, seeing that copies of the statement to which it is a reply, were *privately circulated* so early as June 1841, in exactly the same form as contained in the volume of the Transactions itself, published, I believe, in April 1842. Although printed copies of the statement referred to had thus been circulated early in June 1841, these words are used by the Council on the 28th of July 1841: "That copies of the statement be transmitted, *as soon as they can be printed*, to Mr. Nasmyth." This expression being used about two months after they had *not only actually been printed, but circulated*. On these grounds, I consider myself quite justified in doing *avowedly*, what was done *privately* by the Council, and thus endeavouring to apply, as soon as possible, some direct antidote to the baneful influence of such erroneous statements as contained in the "Addendum."

I conclude by respectfully requesting your Lordship to lay this letter before the ruling body of the Association, in any manner which your Lordship may think most appropriate, and I trust that, as the Council have given insertion in the Transactions to the statements contained in the Addendum, they will see the justice of inserting also my counter-statements.

Allow me to subscribe myself,

With great respect,

Your Lordship's very humble servant,

ALEX. NASMYTH.

LETTER FROM DR. HODGKIN TO PROFESSOR PHILLIPS.

Lower Brook Street, 20, 7, 1841.

RESPECTED FRIEND,

I have received from Taylor and Co.'s printing-office, a copy of Alexander Nasmyth's abstract of the papers read by him at the Birmingham meeting of the British Association, to which is prefixed the draft of a report which it is intended to publish along with it, with a request that they should be returned, with any comments which the perusal of them may suggest as important. I regret I have been more tardy in complying with this request than I could have wished. I have throughout felt this to be a very painful and trying affair, and my regard for both the parties concerned has throughout increased the anxiety which, in common with every member of the Council, I have felt that the affair should be satisfactorily and amicably arranged. I am sorry to say that a very careful examination of the document put into my hands, in which I have compared part with part, as well as reverted to the circumstances as they have from time to time been brought before the Council, leads me to fear that Alexander Nasmyth will only feel himself additionally aggrieved, since, although it is very much made up of actual copies of minutes and letters, the whole is made to present accusations, direct or implied, which would not be the case were the statement complete. It is not surprising that the author of a paper should feel himself hurt by a letter like thine of the 24, 6, 1840, although I am fully persuaded that it was not thy intention to wound his feelings. He, however, immediately replied by an offer to prove that he had inserted nothing new in his abstract, but it does not appear that either this offer, or that subsequently made by him to convey his proofs to York, were accepted, whilst great stress is laid

upon his not complying with the request to deliver up his original paper, without its being made evident what were the valid grounds for his refusal, viz. that it had been so employed, in the recording of further researches, as to render it altogether unsuitable to go out of his own possession. The suggestion made by Dr. Macartney, for overcoming this difficulty by obliterating all interlineations, is what few authors would submit to, although he might secure a copy of them, as proposed by the Professor.* To render the narrative complete, it should have been stated that the original MSS. were seen by the secretary of the Council, James Yates; and it should likewise appear in the report, that the original diagrams, which are as much a part of the original communication as the MSS., were never asked by the Committee or the Council, or taken notice of when offered† by the author. On the other hand, much stress is laid on the two reports published in the Literary Gazette and Athenæum, both of which, though traced to the author, and therefore certainly of great importance, are not without essential defects, though even these appear to me to afford no valid foundation for the charge brought against the author. An author, in furnishing a hasty abstract, such as that given to the Athenæum, can scarcely fail to omit some part of his matter, or to produce it indistinctly, which he will the more readily submit to in the consciousness that a more ample account will be ultimately given; but it has been stated by Alex. Nasmyth, that that report was published without his having finally

* The reader will please to refer to page 34, where a further explanation connected with this letter of the Doctor's is given, it being one of those interpolations into the transactions which, so far as I am concerned in the affair, has no place, the account of it never having been seen by me till after the transaction is sought to be considered as finished.—A. N.

† They were not only *offered*, but fac similes of them were positively transmitted to the dwelling-place of each individual member of the Council.—A. N.

approved of it, and when he had expressly desired to retouch it, whereas the report, as it now stands, makes him give it his entire sanction.* The report in the Gazette, though more extensive, was given from a rough copy; and on reference to the journal I find that the part in which the article is defective, is precisely that in which the editor admits his difficulty, in a note printed at page 4,† but which appears to me to be in a great measure supplied even by Dr. Lloyd's abstract, as given at pages 17 and 18. I think that, as the subject is to be brought before the public in the forthcoming volume, the report in the Gazette, which is the more extensive, is as necessary as that given in the Athenæum. The report of the committee named at the Glasgow meeting, announces the ambiguity in the two reports referred to, and it does not appear that they were supplied with by the Council, or sought for themselves, any document to elucidate the question. They admit, however, that the formation of the proper substance of the teeth is effected by the addition of ossific matter in the original structure of the pulp, which is precisely the view contained, *without ambiguity*, in the printed abstract objected to, and

* When a proof of the report published in the Athenæum was sent to me, I noticed that there was a whole leaf wanting, where my theoretical opinions were more especially detailed. That proof being hurriedly called for, I particularly desired a revise, hoping that this portion was still to come; at all events I set about preparing to supply the deficiency ready for the revise, but I was astonished to find it inserted as it was, in the published number, two days after. At the time I attached no importance to it, nor is it now of much consequence.—A. N.

† This is the portion, I presume, where Mr. Owen unfortunately tampered with evidence by fabricating a passage. There is also a word left out in the article in the Literary Gazette, and a mistake has crept into the report, although drawn up with great ability, causing a confusion betwixt the researches of Schwann and my opinions. But when it is considered that the Report was drawn up from a rough MS. by another party, and not seen by me previous to publication, this is not to be wondered at; but on reference to the originals, all is very evident and easily understood.—A. N.

which, as it met with successive reductions at thy requests made in October, and at some period between the 24th of 2nd month and 10th of 4th month, it is not surprising that a change in the wording should have been effected, both for clearness and abbreviation. In looking to these dates, I find that Dr. Lloyd's abstract, and also that furnished by Alexander Nasmyth, must have been at the printer's together, which may account for the passage which makes the difference between Dr. Lloyd's original and the proof, as sent to Alex. Nasmyth, and at once rejected by him. The report of the committee refers to a passage in one of the journals, as expressly disclaiming the view of the ossification of the pulp by A. N., and on reference to the paper, for the grounds of this assertion, I find it evident that this disclaimer is not given by him in the statement of his own opinions, but as a comment upon Schwann's opinions, and relates to the mode in which the transition of the pulp is effected. I think that no doubt on this head can remain, if the concluding paragraph of the report in the *Athenæum*, and Dr. Lloyd's own abstract, are properly considered; but, with the unfavourable bias which the draft of the report is calculated to inspire, this conclusion may escape the notice of the casual reader. When it is determined to print documents and correspondence, from which the public are to suppose that they have the opportunity of judging for themselves, it is very important that the whole evidence should come before them, and for the removal of the confessed ambiguity, it seems but due to the author, when his own explanation is rejected, at least to accept that of three most respectable and competent witnesses, who were made acquainted with the author's views even before the meeting at Birmingham had taken place, and whose written statements were transmitted to the Council.*

* The gentlemen here alluded to were Mr. Liston, Mr. Dalrymple, and Mr. Gulliver, whose evidence is given at page 24.—A. N.

It is much to be regretted that Alexander Nasmyth has not furnished to the Council a verbatim copy of the original document, when it had consented to receive this evidence of his actual opinions, as mentioned at p. 12,* but it is probable, that independently of the difficulty of making such transcript from the papers in their actual state, he may have felt that he had done quite enough in offering the originals to the inspection of the General Secretary, and in really submitting them to the Secretary of the Council. I must observe that I urged the expediency of producing this copy upon the author more than three months ago, and I am glad to be able to add that since the proof report has been sent to me from Taylor's, it has happened that, *without the smallest communication having taken place between us*, I received from the author a printed copy of the original paper, which presents a view of it as it was actually taken to the meeting of the Association at Birmingham, and which he is about to publish in vindication of himself.† As a friend to peace, as well as to both the parties concerned, I cannot help strongly expressing the opinion that it will be to their interest, as it certainly will be for that of our Association, to keep out of view in our Transactions any trace of this unpleasant affair. It will then appear, before the scientific world, that both of these original and laborious inquirers have been engaged in the same research, and

* Dr. Hodgkin, I see, has taken for granted that the assertions brought forward in this statement by the Council are correct. On this head I have at page 40 stated where they are incorrect, in so far that there is not a word of the willingness to "publish" *my own abstract on receipt of "a copy of the original memoirs."* The circumstances regarding the rejection of my offers to give access to the entire original communications have been sufficiently detailed in the preceding pages.—A. N.

† Yet neither the delegates nor the Council at large accept this, which they have pretended to call for, although thus pointedly brought under their notice by a man of such probity and sincerity as Dr. Hodgkin, and one who has proved himself a friend of peace and good will to all parties.—A. N.

arrived, by somewhat different steps, at very nearly the same conclusions,—I cannot say *precisely the same*, for I think there are shades of difference sufficient to stamp the originality of each, and which future labours of the same or other inquirers may hereafter settle. I regret that I cannot refer to the *Comptes Rendus*, and I think that the reader will feel the same difficulty, and must be sensible that whilst the report of the Council studiously brings forth matter to convict a contributing member on the authority of somewhat irregular evidence, there is not a sentence of the Memoir from which it has been suspected that plagiarism has been made. The report will further record an unconfirmed and unanswered accusation regarding attacks on Professor Owen, and the distribution of separate copies of the Memoir. These attacks have been publicly and privately disclaimed by A. Nasmyth, and the Council has been informed that the copies of the abstract were received by the author without any restriction as to distribution, but that their further circulation was suspended, and the further circulation of those delivered put a stop to, when scarcely half-a-dozen copies had been given out.

I sincerely regret that my observations on the report are necessarily given so much in the character of an advocate, as it places me in a position with reference to my two friends in which it is distressing to be situated, but I feel that I should not be doing justice to either of the parties or to the Association which has placed me on its Council, were I to act differently.

I am thine, respectfully,

THOMAS HODGKIN.

Structure of the Teeth.

Pl. C. 5.

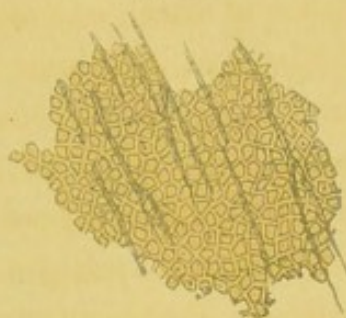
Fossil Ivory.

No. 8.



Megalichthys.

No. 10.



Lamna.

Ivory Fibres.

No. 3.



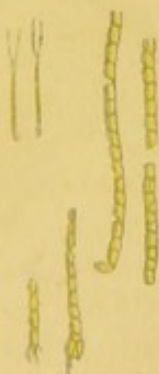
Loris.

No. 4.



Orang Outang.

No. 5.

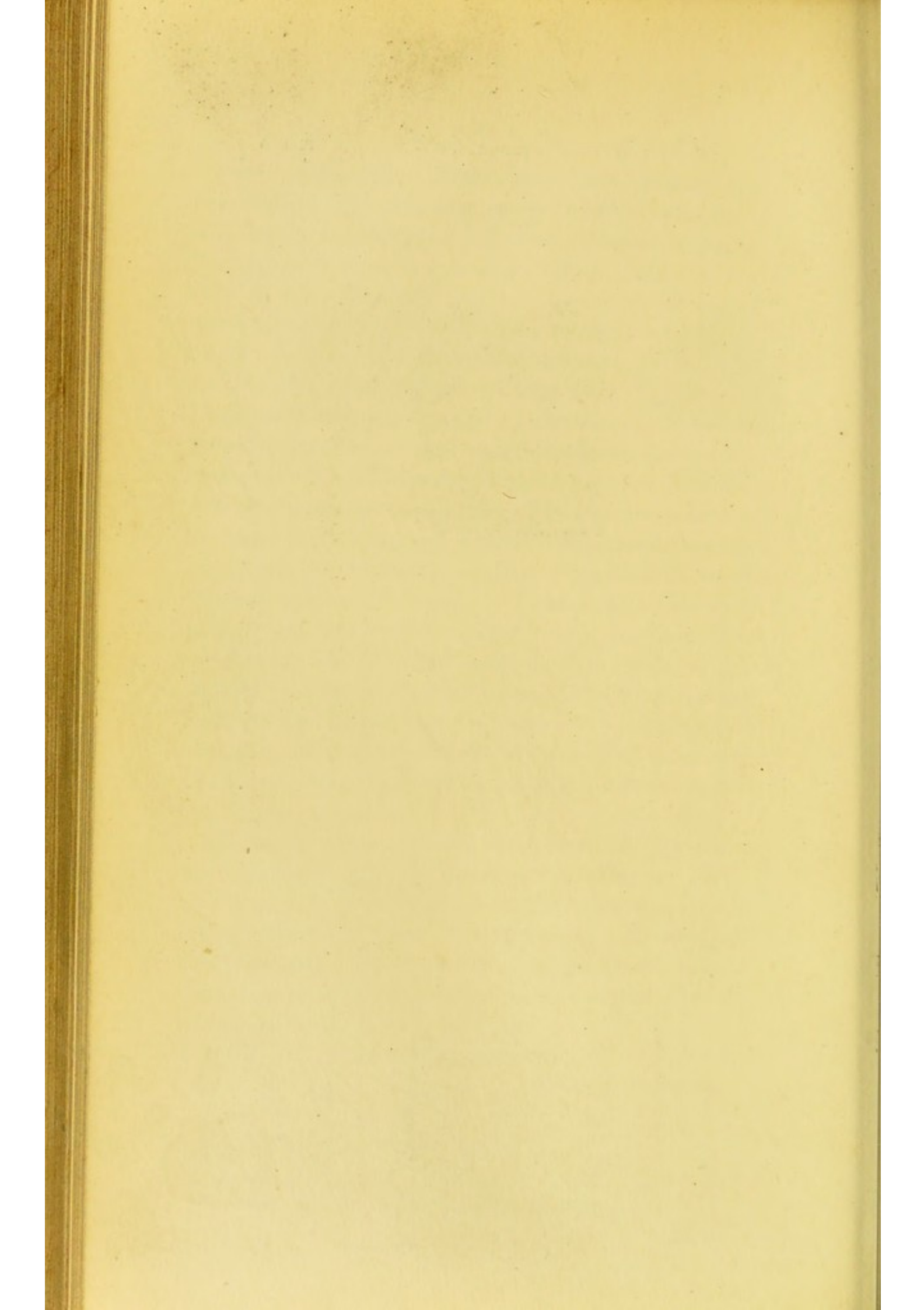


Mandril.

No. 6.



Cynocephalus.



Structure of the Teeth.

Ivory deprived of Earth.

No. 1.



No. 2.



No. 3.



Human.

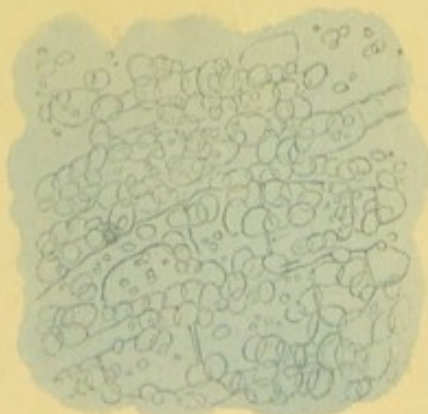
No. 4.



Elephant.

Structure of the Pulp.

Pulp, No. 1.

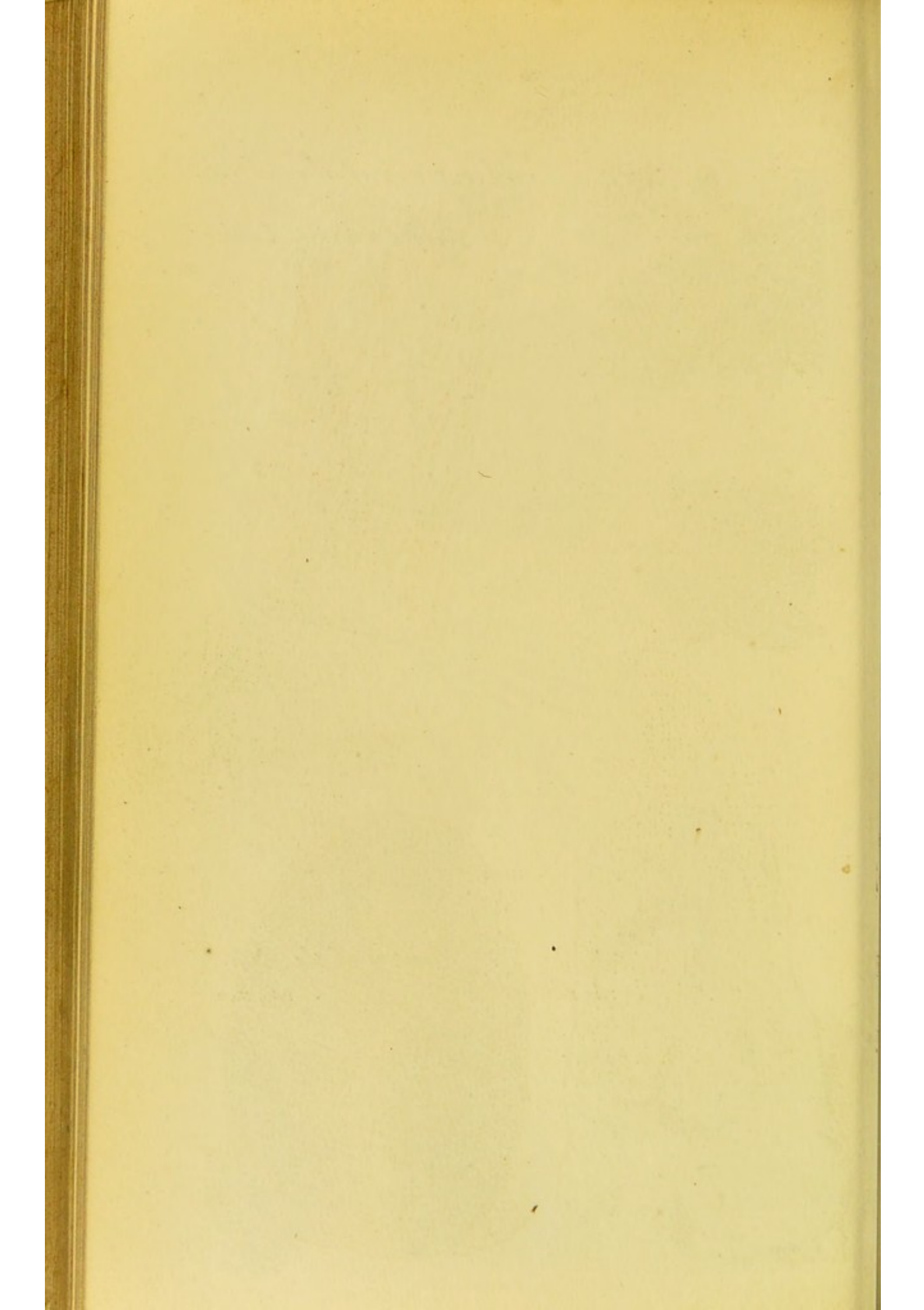


Vesicles.

Pulp, No. 2.



Vesicles.



Structure of the Pulp.

Pulp, No. 3.



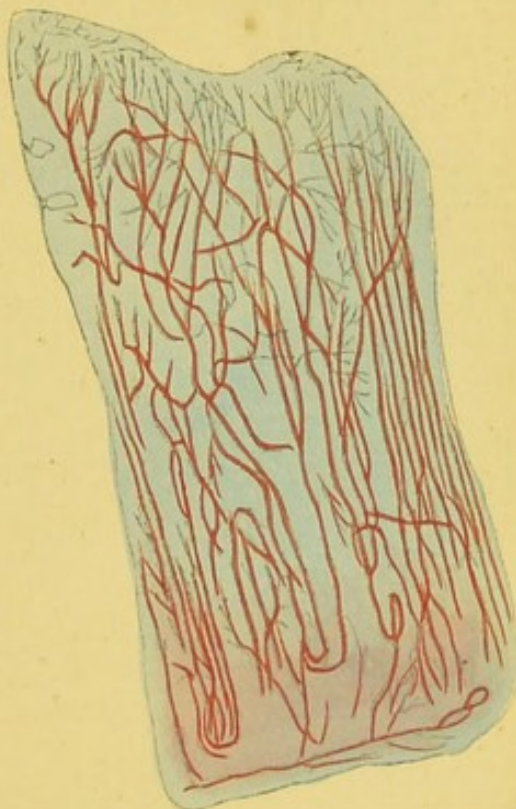
Cells.

Pulp, No. 4.



Cells.

Pulp, No. 5.

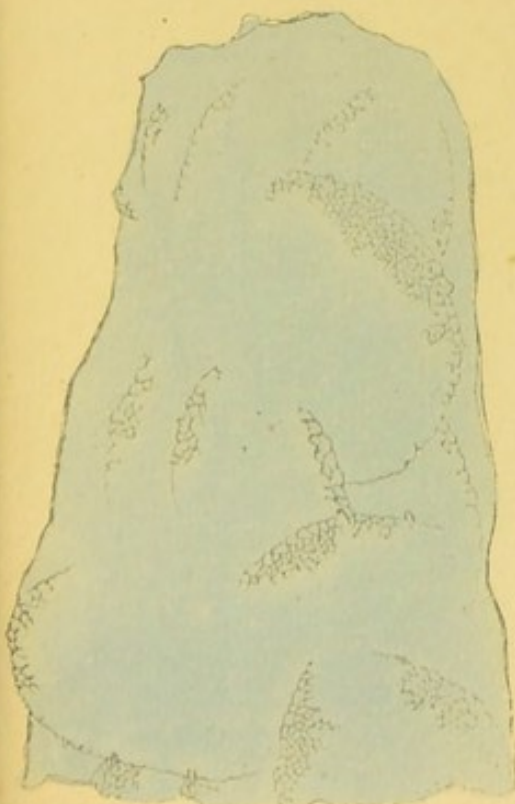


Human.

Vascularity.

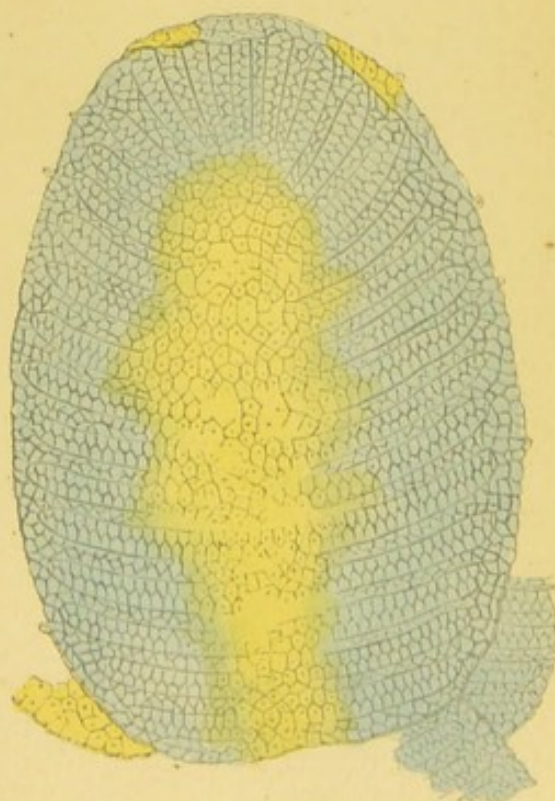
Structure of the Pulp, and Developement of Ivory.

Pulp, No. 6 A.

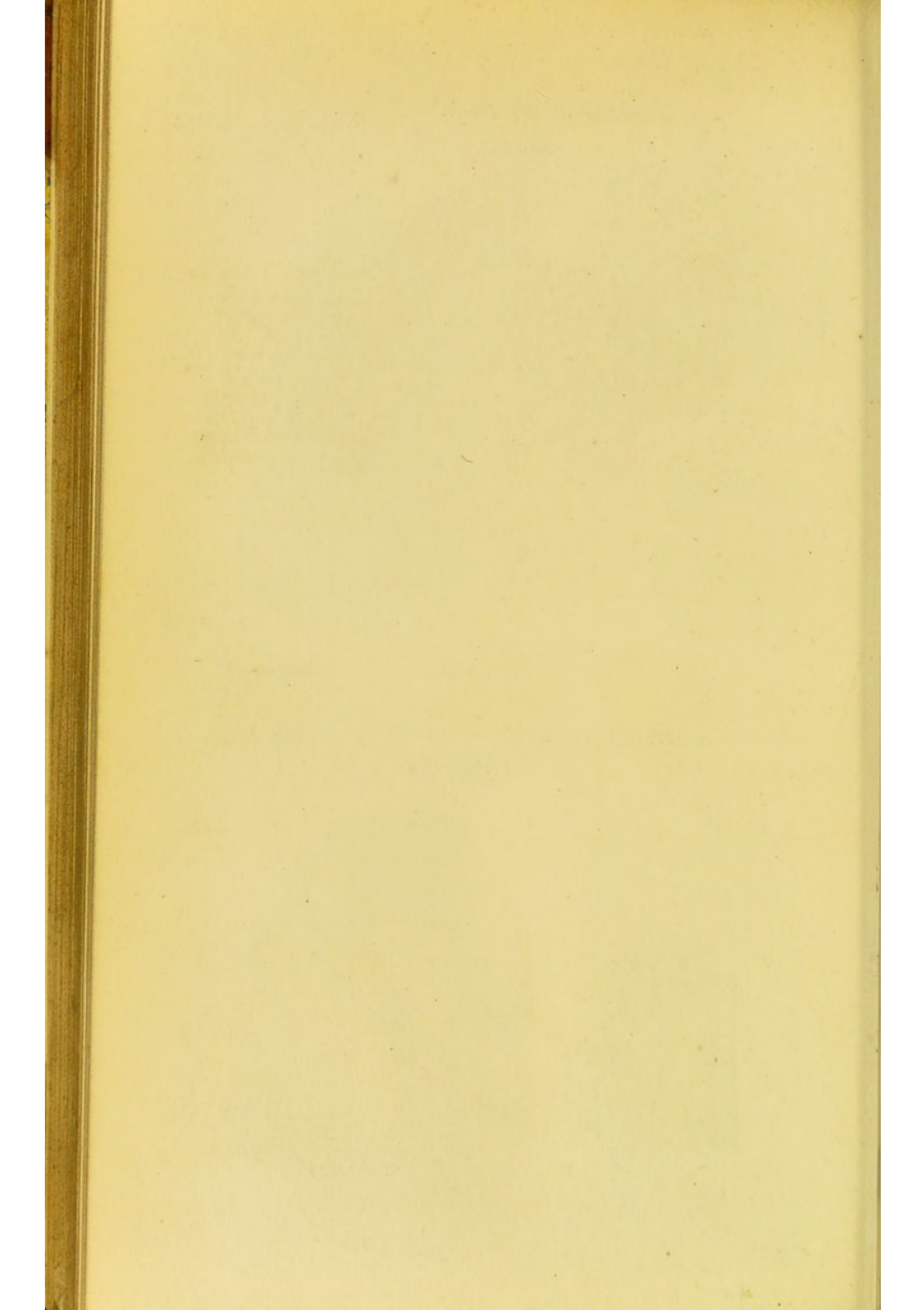


Reticulation.

Pulp, No. 6 B.



Reticulation.

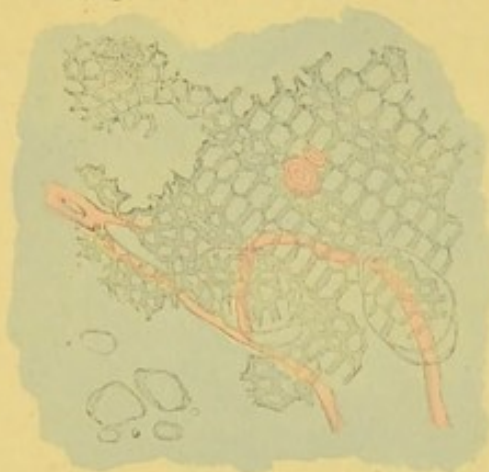


Structure of the Teeth.

Pl. C. 8.

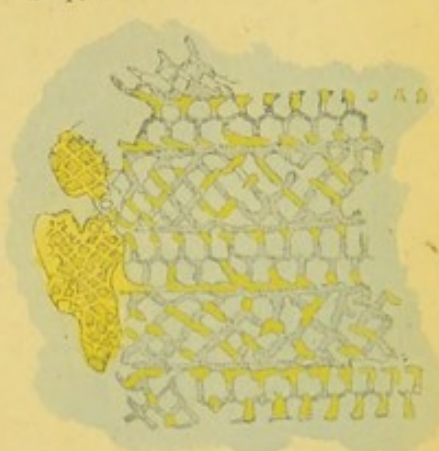
Structure of the Pulp, and Developement of Ivory.

Pulp, No. 8.



General View.

Pulp, No. 9.



Ossification, or Developement of Ivory.

Pulp, No. 10.



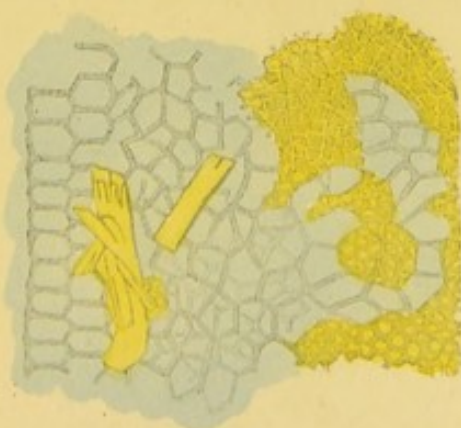
Ossification, or Developement of Ivory.

Pulp, No. 16.



Ossification, or Developement of Ivory.

Pulp, No. 11.

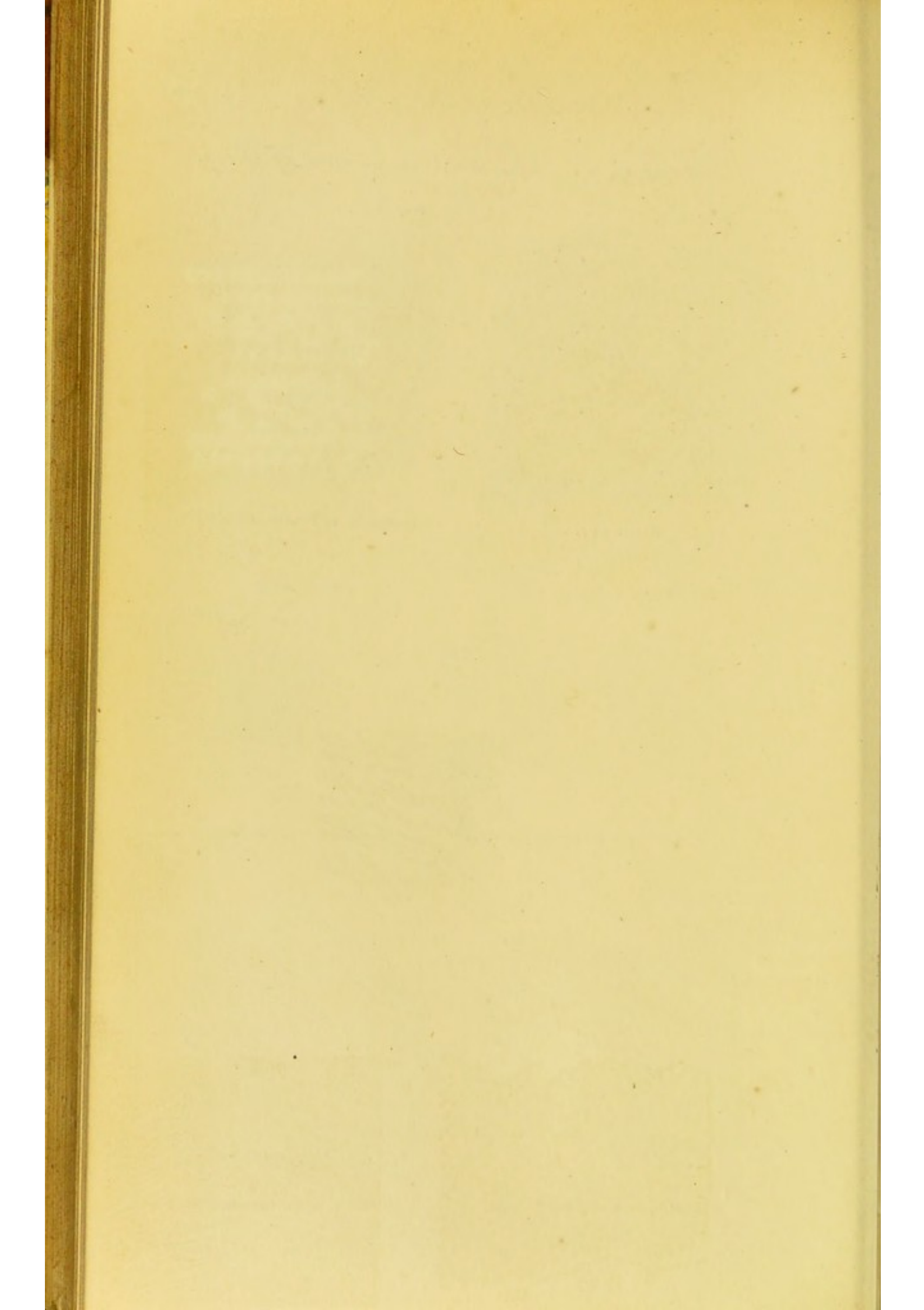


Ossification, or Developement of Ivory.

Pulp, No. 12.



Ossification, or Developement of Ivory.



Structure of the Teeth.

Pl. C. 9.

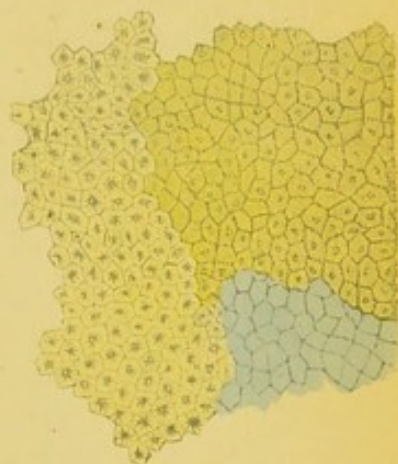
Structure of the Pulp, and Developement of Ivory.

Pulp, No. 13.



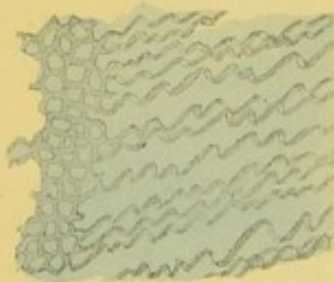
Ossification, or Developement of Ivory.

Pulp, No. 14.



Ossification, or Developement of Ivory.

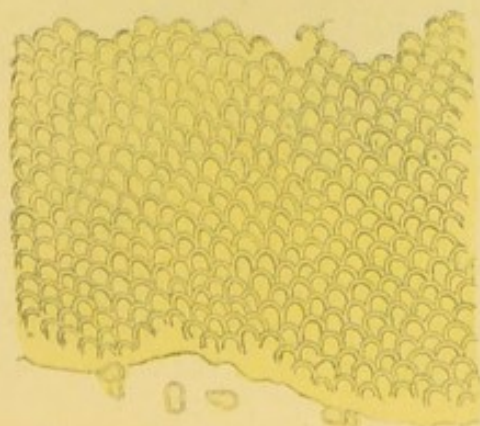
No. 15.



Human Fibres of Ivory in connexion with Pulp.

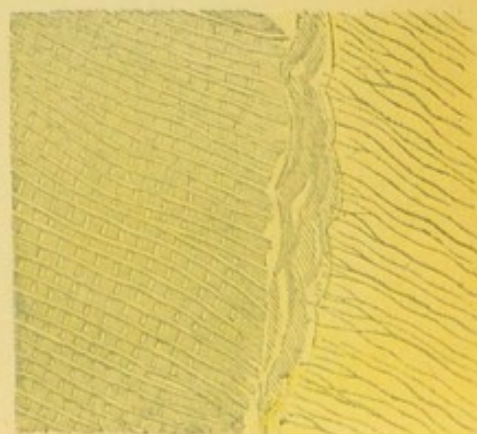
Structure of the Enamel.

No. 1.

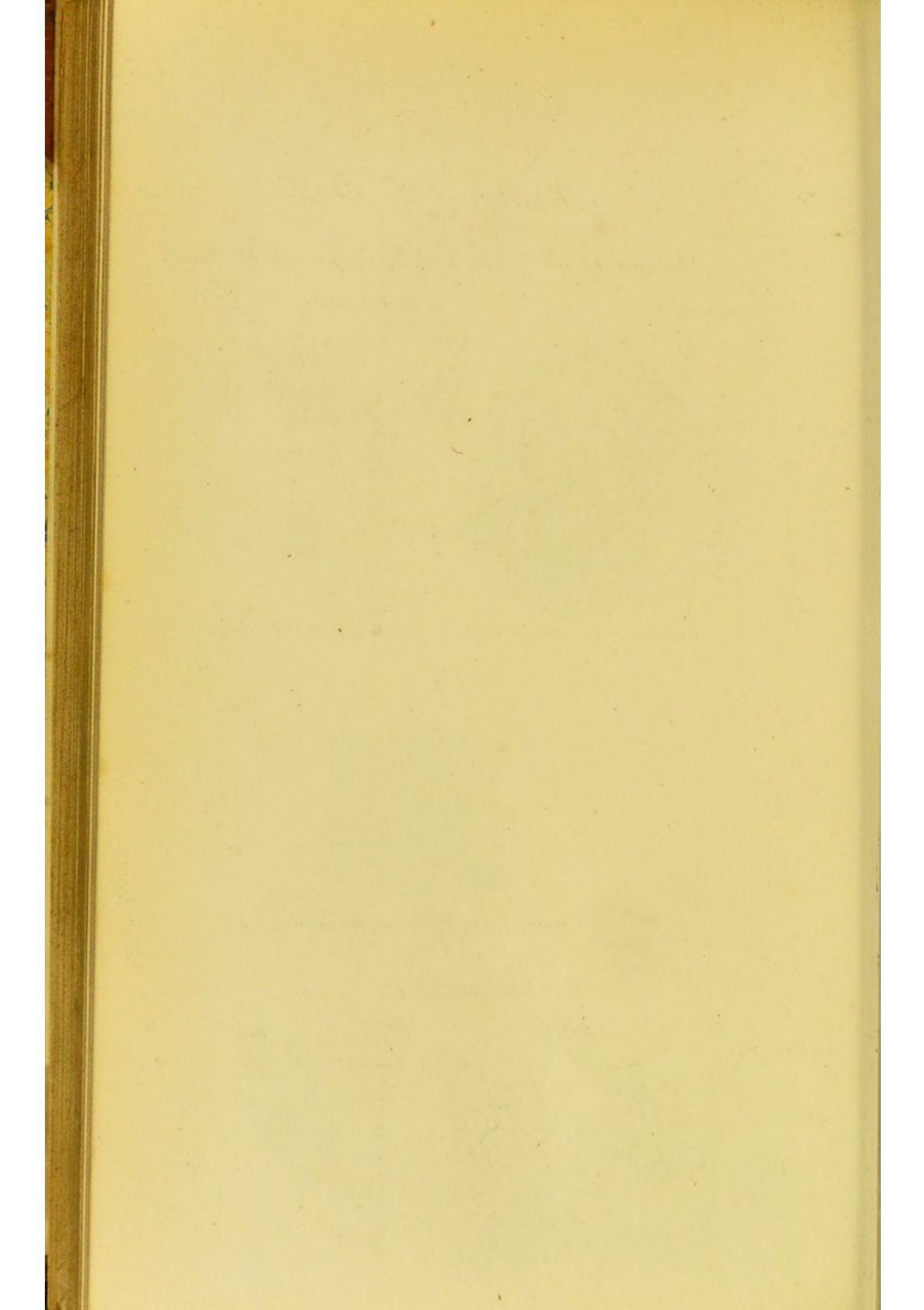


Human Enamel with Surface

No. 2.



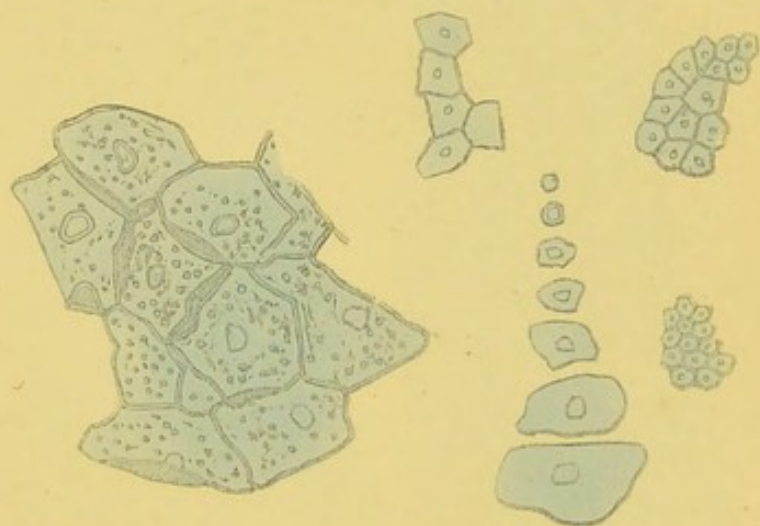
Human Vertical



Structure of the Epithelium.

Pl. C. 10.

No. 2.



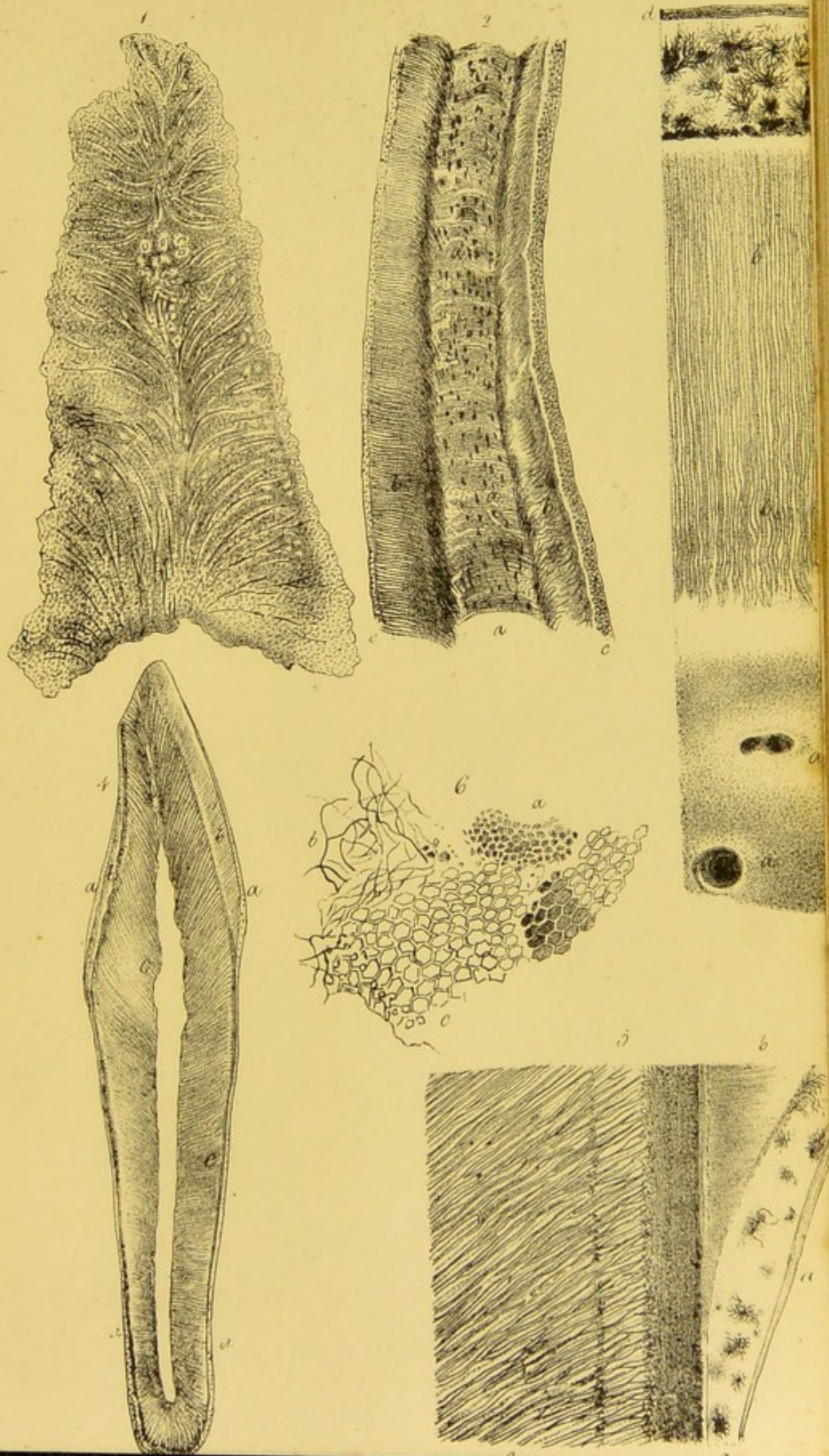
No. 3.

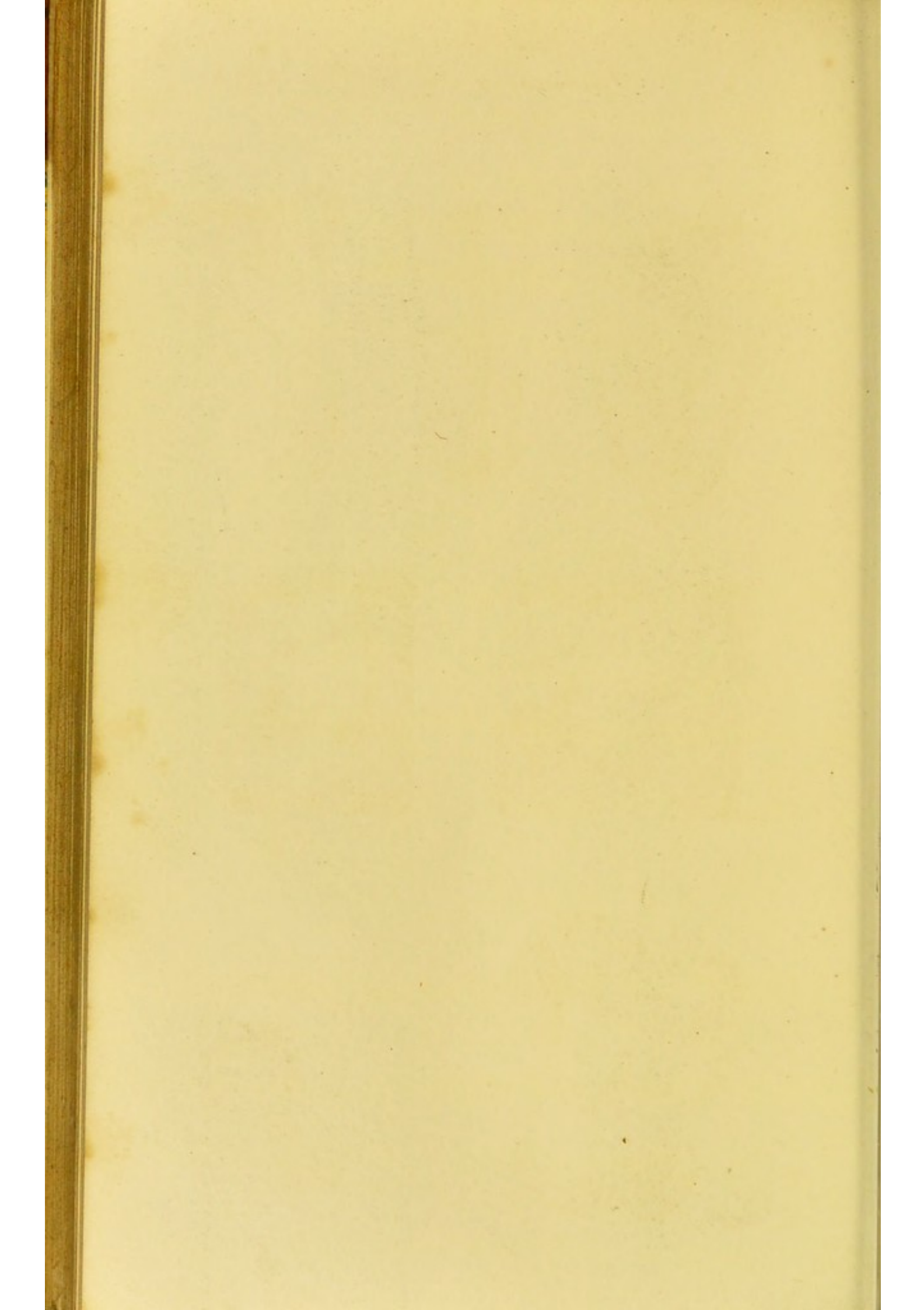


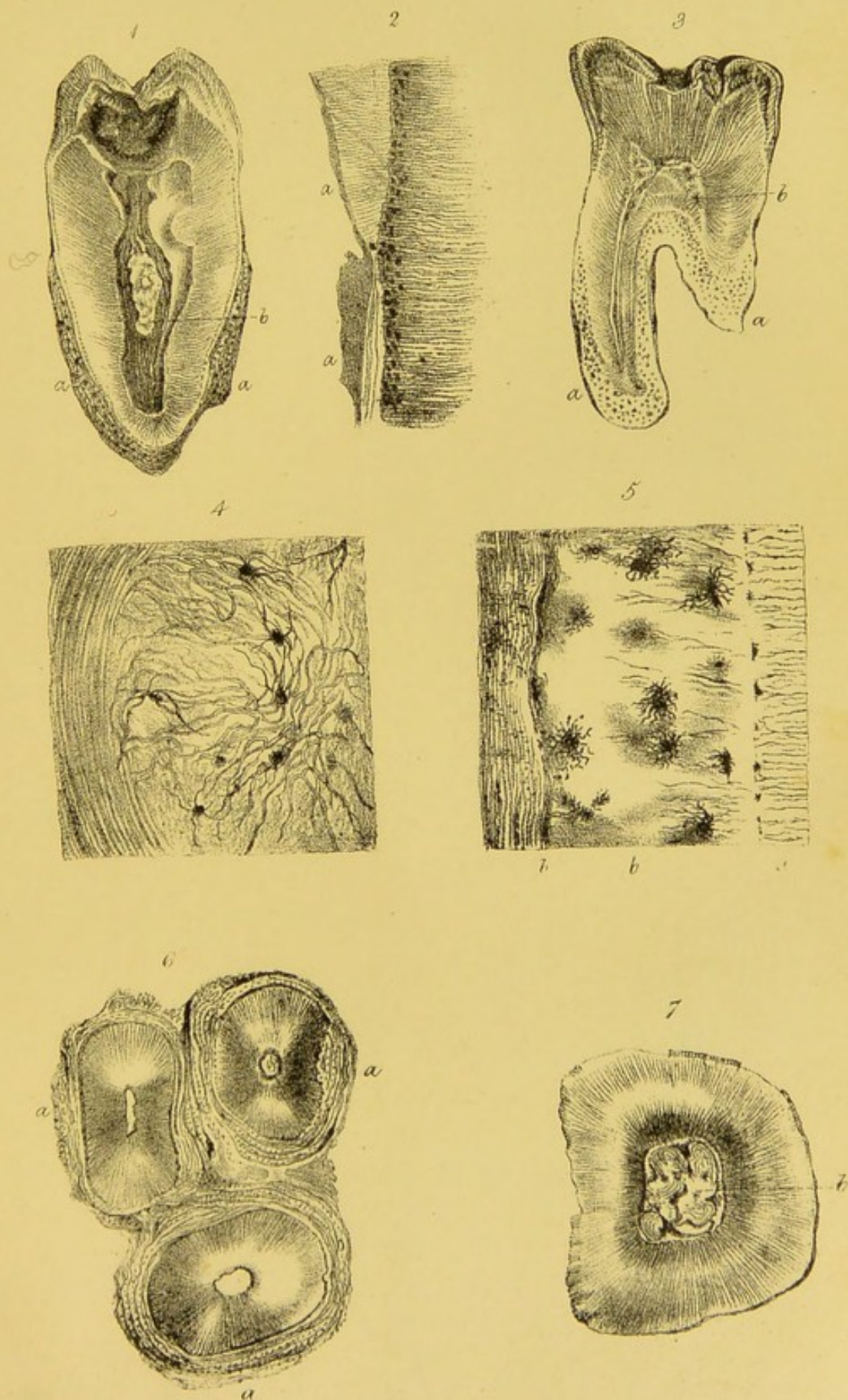


Structure of the Teeth.

Pl. C. II.







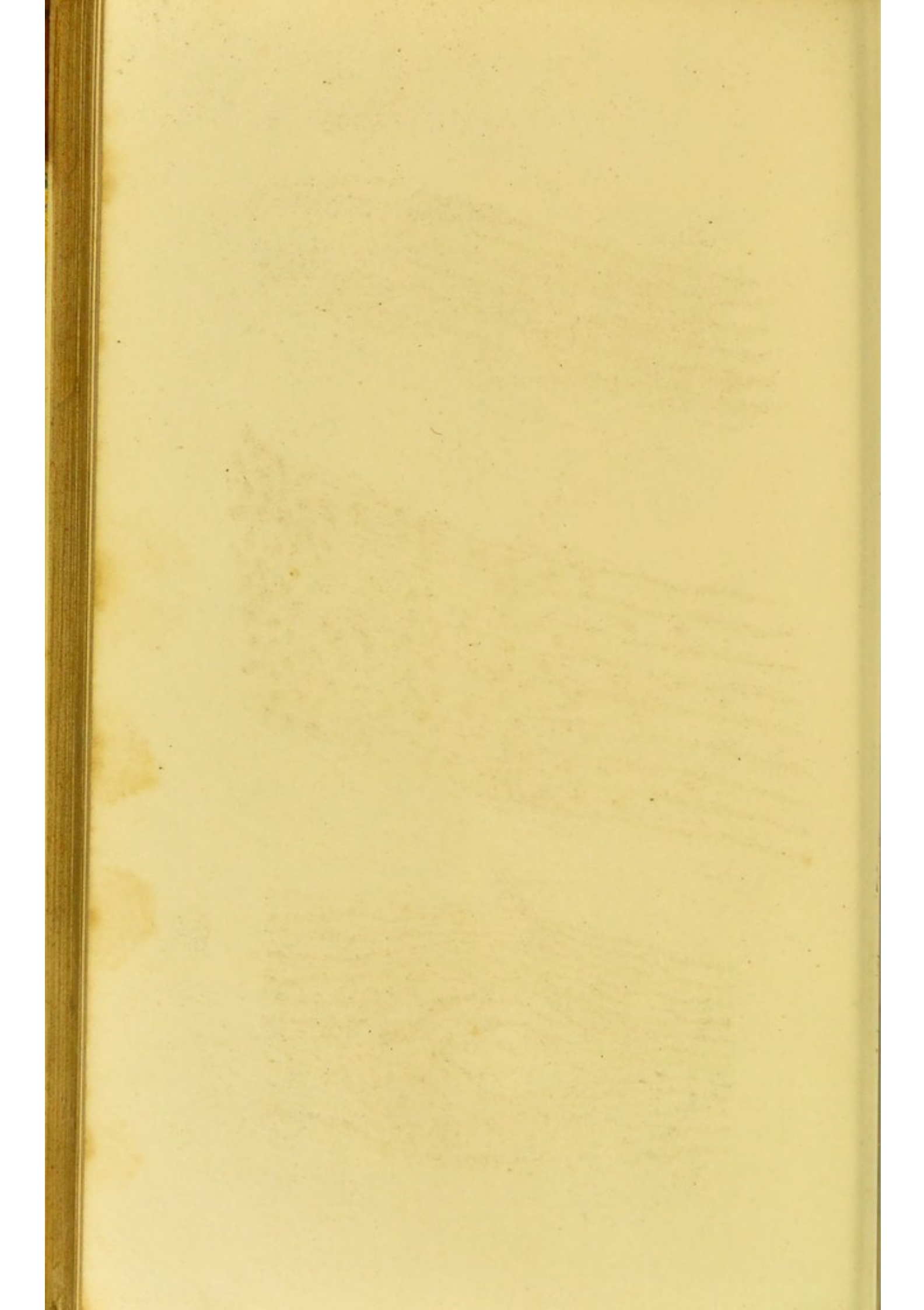


Fig. 5.

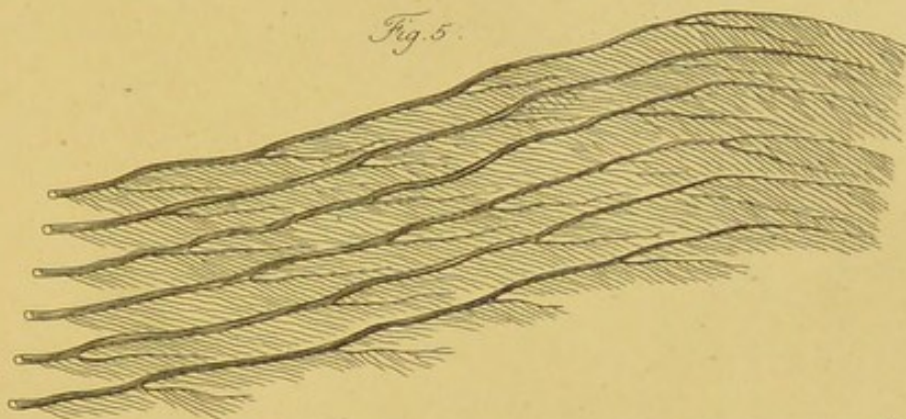


Fig. 6.

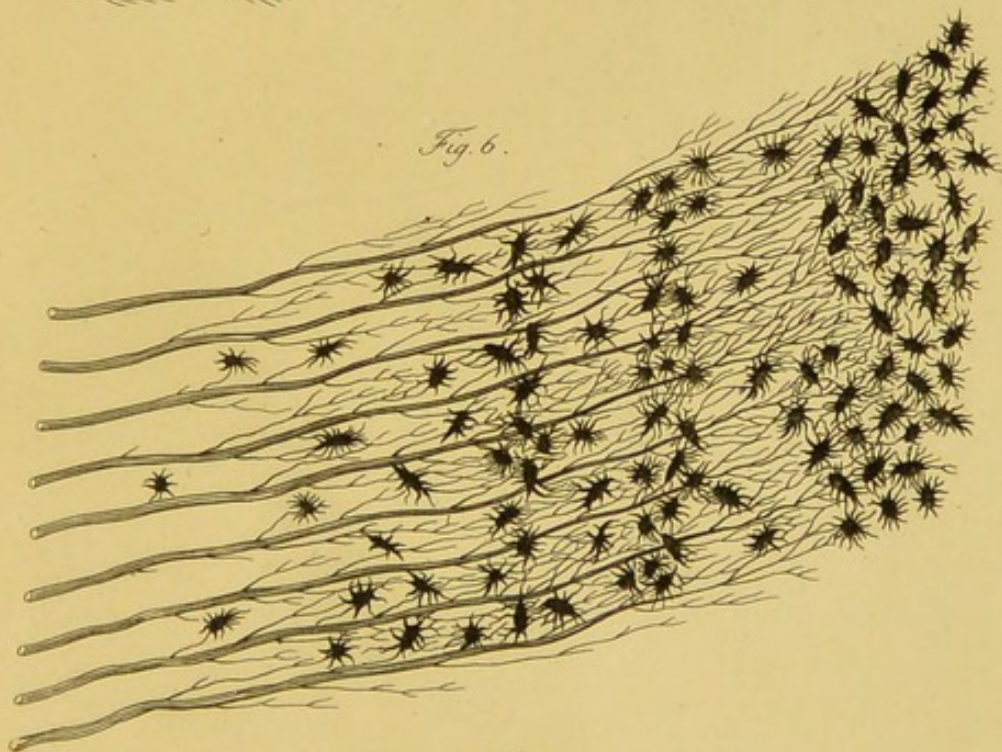
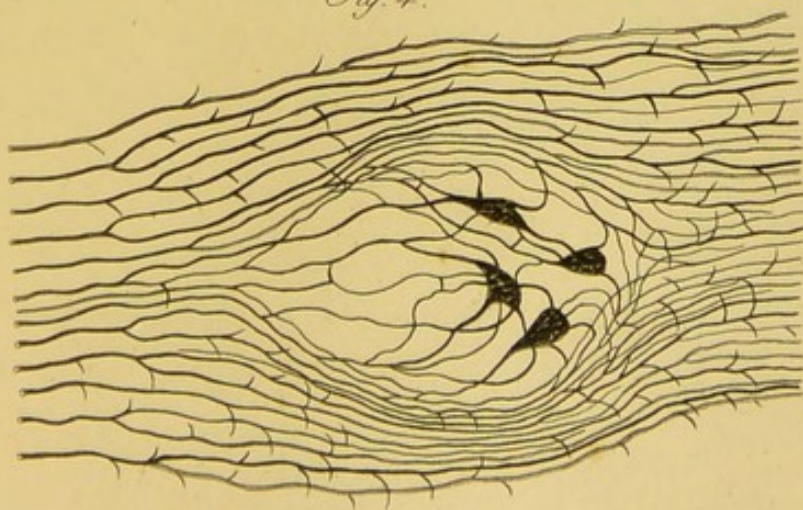
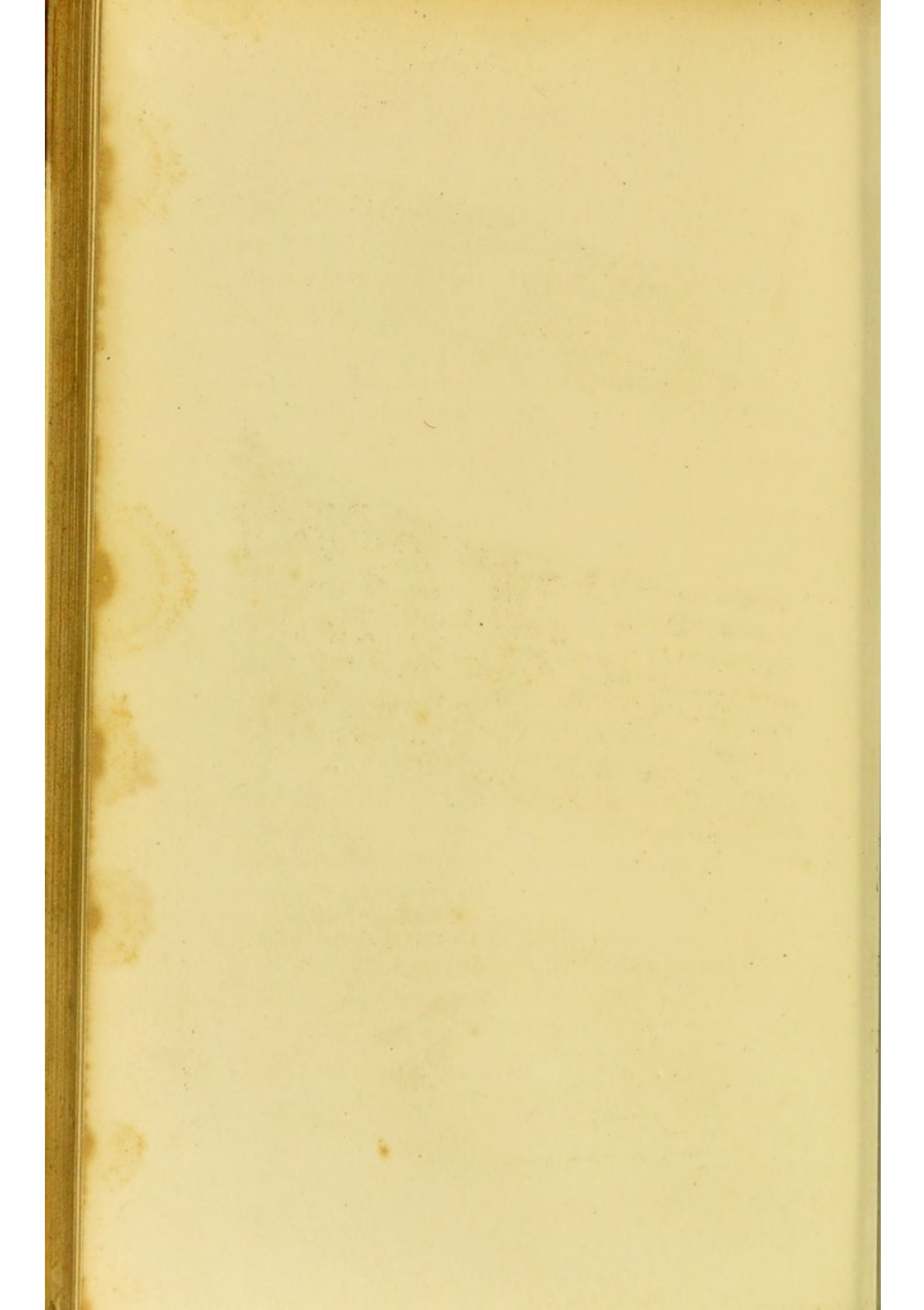


Fig. 4.





THREE MEMOIRS.

* * * The subjoined extracts from the correspondence of Professor Phillips, the Editorial Secretary of the British Association for the Encouragement of Science, contain the instructions in strict conformity with which I drew up a report of the following communications for insertion in the Transactions of the Association. The passages to which I wish more particularly to call attention, I have placed in Italics.

On the 10th of February, 1840, I received from the Editorial Secretary the following note:—

“ Professor Phillips begs to inform Mr. Nasmyth, that the arrangements for printing the volume of the British Association, require the very earliest possible transmission of abstracts of Memoirs read, to Professor Phillips. There can only be an *abstract* of Mr. Nasmyth’s paper, *not requiring plates*, but *Mr. N. is at full liberty to publish elsewhere, and at any time, the whole paper illustrated as he pleases*. Professor Phillips encloses the MS. as read in the Geological Section, for Mr. Nasmyth’s guidance in making his abstract.

There will be, of course, only one abstract, though the subject of the paper was treated in two sections.”

In a subsequent letter, dated Launceston, Cornwall, April 4, 1840, written after I had transmitted to him my own report, he informed me, that this report “is to appear in the Proceedings of the Medical Section. On looking carefully over it,” he continues, “there seem to be a few parts capable of abbreviation; and as *our rule of publication requires very close condensation of the communications to the sections*, I return you the M.S. with pencil-marks, proposing to you the omission of these few passages, *to save space*. They do not affect the substance of the Memoir.”

MEMOIR No. I.

*Investigations into the Structure of Fossil Teeth, &c.
presented to the Geological Section of the British
Association for the Encouragement of Science, at
the Meeting held at Birmingham in August, 1839.
By ALEXANDER NASMYTH, Member of the Royal
College of Surgeons, London, &c. &c.*

THE modern science of Geology may be considered the most comprehensive of all the branches into which human knowledge resolves itself. No progress can be made in any department of physical philosophy, which does not throw light upon its vast and wondrous domain, and exalt the brilliancy of its truths. Though it exacts a knowledge of the perishable portions of the animal frame, it is obvious that its advance chiefly depends on accessions of information respecting permanent animal relics which remain embedded in the crust of our globe, unaffected by the powerful chemical agents contained in the laboratory of nature. The skeleton,

scales, and teeth of animals being the only parts which are found in a fossil state, it is evidently of paramount importance that the general character and internal structure of these should be clearly understood. Their external forms have already been well established, and the combined labours of eminent naturalists have left little to be desired in this department of science ;—but natural revolutions, slowly but surely taking place, during the long march of time, whose steps the geologist is summoned to trace back to a period incalculably remote, have occasionally so mutilated these otherwise permanent remains of animals, that they retain none of the characteristic peculiarities of the species to which they belong. Here micrography comes to our aid ; and its able application to odontology by Retzius and others, so ably described to you last year by Professor Owen, has opened a new field of inquiry, which already promises the most brilliant results. Presuming that any additional researches in this direction cannot but be of interest to the members of this section of the Association, I hasten to lay before you a succinct account of my recent researches on the microscopic structure of the teeth. In endeavouring to form a classification of the animal kingdom, having for its basis the peculiar disposition and arrangement of the internal organization of the teeth, I was led to the discovery of a structure, which has hitherto not been noticed. Purkinje and Fränkel state that “the proper dental substance consists of a uniform structureless sub-

stance, and of fibres passing through it." The writings on the subject by Retzius, Müller, and others, leave us to conclude that the interfibrous substance does not present any traces of peculiar conformation:—but I am disposed to believe that it is not only organized, but so differently and characteristically so in different animals, as to be capable of affording valuable aid to the naturalist in classifying the animal kingdom.*

[In illustration of this subject, I beg to refer to the drawings, showing the characteristic varieties of this conformation [two of which, marked Fossil Ivory, No. 8 and 10, are engraved in Pl. C. 5, at the end of the present papers]. The beautiful characteristic varieties of this cellular conformation in different classes of animals, show the delicate nature of the process of transition by which it is produced].

* It has been absurdly attempted to show that, in my papers delivered at Birmingham, I supported the old theory of the formation of ivory by "excretion," or "exudation," from the pulp. Neither of those words, however, occurs at all in my papers; and the corresponding ideas were equally absent from my mind. Moreover, the slightest reflection will convince even a tyro in anatomy, that the above simple announcement of a characteristic and in every case beautiful cellular organization of the interfibrous substance of ivory, is diametrically opposed to such a theory. How could this organization be the result of a process of exudation or excretion? In short, to speak of the excretion or exudation of ossified cells, is a ridiculous absurdity. When, on first entering upon my researches into the structure and formation of ivory, I discovered, by an examination of extremely thin sections of the latter, the cellular arrangement of its inter-

My attention was first drawn to the structure of the interfibrous substance on examining a delicate section of the fossil tooth of a rhinoceros, by the aid of a very high magnifying power; and I must here remark, that those who repeat these investiga-

fibrous substance, I was compelled to conclude that this could only be produced by a process of transition which I then proceeded to trace. The result of my further inquiries will be given in the course of this memoir, but at greater length in the next. In connexion with this subject may be examined Pl. A. 3, Fig. 6, at the end of the appended series of plates: it is copied from Retzius, and shows in a tooth, which he says is that of the adult horse, the corpuscles, or cells, as he calls them, in which, and on which, according to him, the dental tubes terminate. These corpuscles or cells are the dark spots with lines radiating from them, in a manner similar to the corpuscles of true bone. They are only to be found, he states, towards the periphery of the tooth. It will be seen at once that they are altogether distinct from the interfibrous cells which I have described, though one writer on Odontology has, from unaccountable ignorance of the subject, or from some other cause, confounded them with each other; and has accused me of announcing, as my own discovery, that which had been previously published by the distinguished Swedish anatomist. Neither in connexion with this plate, nor in any other part of his work, does Retzius ascribe any structure to the proper interfibrous substance: he only alludes to the corpuscles above mentioned, which he says are extremely minute, and scarcely demonstrable *in human teeth*, and to what he terms the ramifications of the tubes, as occupying a portion of the interfibrous spaces. How the corpuscles of Retzius can have been mistaken for the cells of the interfibrous substance which I have described, I think any one who compares the former, as delineated in Pl. A. 3, Figs. 4 and 6, at the end of the series appended, with the latter as represented in Figs. 8 and 10 of Pl. C. 5, will be at a loss to imagine.

tions, the results of which I am about to detail, will find it necessary to make use of a magnifying power of one-tenth of an inch focal distance, and of the most perfect kind, with an achromatic condenser of the light. The instrument with which I have conducted my researches, and upon the accuracy of which I place the greatest reliance, is that of Mr. Powell.*

In the section of the tooth of the rhinoceros to which I have just alluded, will be observed an appearance of cells or compartments. This I at first imagined might arise from fractures of the material, but on examining other sections of fossil teeth, examples of which are now exhibited, and at the same time seeking whether this cellular appearance could be observed in recent teeth, I was enabled to demonstrate that in every case this peculiar appearance was that of real structure.

The examples of this appearance in the ivory, both of fossil and recent teeth, which I now place

* All the anatomical observations detailed in these memoirs were frequently repeated, were verified by the eyes of others, and the preparations were faithfully copied by my talented artist. It may require some practice, and a certain degree of proficiency in manipulation, to produce and examine such preparations of the pulp and ivory in the human subject; and some persons may perhaps be found, who, having been unsuccessful in making them, at once deny that it is possible for others to be more expert or fortunate; but the fallacy of this will soon be evident to all persons of tact and perseverance who devote themselves to the subject.

before you, are brought forward merely with a view to establish the general principle, that the organization of the dental bone is cellular, and they have been selected more from convenience at the moment, and as demonstrating the variety of cellular conformation existing in the teeth throughout the wide range of the animal kingdom, than with any intention of establishing at once a complete and systematic view of the subject.

I have also made researches into the structure and composition of the fibres of different teeth, and have generally found that these present an interrupted or baccated appearance, as if they were made up of different compartments,—an obvious concomitant of the cellular structure of the inter-fibrous material. The size and relative position of these portions or divisions of a fibre differ in various series of animals. In the human subject, for instance, each compartment of the fibre is of an oval shape, and its long small extremity is in apposition to the one next adjoining. The long axis of the oval corresponds with the course of the fibre. In some species of the monkey tribe the fibre appears to be composed of two rows of compartments parallel to each other. In the orang utan the fibre is composed of rhomboidal divisions, and in the baboon, they are oval like those of the human subject, and the surfaces of the long axes are in apposition. In fact, each class of animals seems to have a distinct characteristic appearance, but all are similar in respect to the general baccated

appearance. A few examples of this structural arrangement are seen [at Pl. C. 6,] in the diagrams marked "Ivory deprived of earth," Nos. 1 to 4, showing its appearance after the earthy matter has been removed.

Among the gigantic remains which are brought under the consideration of the geologist, there is one which to me is particularly interesting—I allude to the tusk of the mammoth. As found in our museums, it will be observed to present a laminated, concentric structure, being apparently composed of layers gradually increasing in size from the centre to the circumference. These layers in numerous instances are separated from each other by considerable spaces. The experiments of Hunter on animals fed on madder also lead to the conclusion that the structure of the teeth is actually laminated; and the workshop of the mechanic, called by Professor Leslie the School for Philosophers, furnishes numerous facts in support of the same position: for instance, the circumstance that ivory is found to possess considerable strength if cut parallel to the long axis of the tooth, and that it is weak if cut at right angles. If we suppose that ivory consists only of fibres surrounded by a structureless material, passing from the centre to the circumference, we must conclude that at right angles to their course it would be most difficult of fracture; but the contrary is the fact, as is exemplified by the natural separation of the lamellæ of the tusk of the mammoth in a direction contrary to the

course of these fibres, as well as by the mode adopted by ivory cutters of making sections corresponding to the vertical, and not to the transverse diameter of the tooth. Many other facts might be here adduced, as well as phenomena accompanying the decay of these organs, were this necessary or relevant to the business of this section. All systems of dental structure which have hitherto been propounded have failed, I think, to explain facts of daily occurrence, but they may be accounted for, I venture to assert, by the cellular organization of the interfibrous substance which has been improperly termed structureless, and by the peculiar baccated arrangement of the fibres.

According to the views of Retzius, Purkinje, and the recent investigators of the structure of the teeth by the aid of the microscope, the enamel consists of fibres running in a direction from the centre to the circumference of the tooth. Both Retzius and Purkinje have given delineations of the course of these fibres. On making a section of the enamel, in a direction parallel to the transverse diameter of the tooth, the appearance as described by these writers is observed, and they are said to be seen to terminate in an hexagonal form beneath the investing crusta petrosa. If, however, a different section of the enamel of the human tooth be made; for instance, one near the surface, parallel to the vertical direction or long axis of the tooth, an appearance presents itself which has induced me to take a different view of the nature of the struc-

ture of the enamel. The appearance to which I now allude is represented at [Plate C. 9] figure marked Structure of the enamel. No. 1. It will there be found that the section of the enamel presents compartments or divisions, but of a different character from those I have already spoken of as existing in the interfibrous substance of the ivory. Each compartment of the enamel is of a semicircular form, and the convexity of the semicircle or arch looks upwards towards the free external portion of the tooth. The vertical section, [Plate C. 9,] enamel No. 2, gives the appearance of these cells as seen in that direction.

In the sections both of enamel and ivory, there will be always observed near the margins isolated cells, which admit of their form and appearance being carefully studied.

All the analyses hitherto made of the chemical composition of the enamel lead to the conclusion that the structure contains only a very small portion of animal matter. But when I detected the cells or compartments just mentioned, I could not but infer that each of these had for its basis and support a frame-work of animal tissue; and I immediately requested my friend Dr. Thomson of Glasgow to favour me with a complete analysis of the teeth, and their different component structures, in their various states of health and disease. The results of this analysis, so far as it has been proceeded with, are highly interesting; I present them to you in a tabular form, and you will observe how

fully accordant they are with the position that the enamel is provided with an animal basis—a view my microscopic investigations had already corroborated by various appearances.*

Having thus uniformly found appearances denoting cells or compartments in all these states and stages, I became doubly eager to investigate the structure of the pulp, with a view to discover the nature of the process by which this cellular structure is produced, and the source from which it is derived. I might now enter upon a description of the organization of the pulp, and its relation to the process in question; but as this is a subject of purely anatomical detail, I consider that it falls more particularly under the cognizance of the medical section of the association. Suffice it here to state, that the pulp is cellular throughout its entire structure, as may be seen from the series of diagrams placed before you, marked Structure of the Pulp, and Developement of Ivory, Nos. 1 to 16, (and contained in Plates C. 6, C. 7, C. 8, & C. 9.)†

I at first experienced great difficulty in submitting to minute examination the surface of the pulp; that is to say, that portion of it where its transition into ivory takes place; but I at length

* To this table considerable additions have since been made, which are not yet completed; and I therefore, in justice to its eminent author, withhold it, until I am enabled to publish it in a perfect state.

† To avoid repetition, I beg to refer to the next Memoir, p. 27, for a description of these diagrams.

succeeded in obtaining a clear view of it, and found it to present the beautifully interesting reticulated cellular appearance which is represented in the series of drawings marked Pulp, Nos. 6 to 16, [Pl. C. 7, 8, and 9,] where the cells of the pulp in a state of transition are coloured blue, and those ossified are coloured yellow.

Having ascertained that this reticular conformation was constant in all the pulps which I examined, I proceeded to study it with confidence, and soon found that it was essentially concerned in the process of the developement of the ivory, and in the production of the fibres of the latter, as well as of the interfibrous substance. Of this part of my subject I intend to treat more at length before the medical section, but some idea of the different stages of the formation of dental bone may be gained from an examination of the series of diagrams above alluded to, showing the transition of the cells of this reticular surface into the cells of the ivory.

Conceiving that there existed a great analogy in the productions of the capsule and the pulp, I searched for the appearances displayed by the internal or productive surface of that membrane, and found them analogous to the external or productive surface of the pulp, as shown in the drawing, [Pl. C. 11,] No. 6.

The membranous investment which I lately discovered as investing the enamel of the teeth of man and other simple teeth, displays a similar arrangement.

Thus, there is a remarkable uniformity in the structure of the formative tissues of the tooth, and of the dental substance itself; for not only is the interfibrous material cellular, but the surface of the pulp, which is the organ for the production of the ivory, and the internal or productive surface of the capsule, also uniformly present a reticulated or cellular appearance.

My researches have, I venture to hope, also established a new and beautiful instance of the harmony of the laws of nature in demonstrating the fact of the uniformity of the products of the capsule; for not only is the enamel uniformly provided with an external covering, but there is also a membranous investment of the crusta petrosa itself. I think, also, that we must be compelled to allow the uniform presence of a fourth tooth-bone substance, the existence of which is more constant in all animals, either normally or anormally, than any of the other three hitherto recognized textures. But as all these subjects belong properly to the province of the medical section of the Association, I think it unnecessary to allude to them more fully here.

MEMOIR No. II.

Investigations into the Developement and Organization of the Dental Tissues, &c., presented to the Medical Section of the British Association for the Encouragement of Science, at its Meeting held at Birmingham, in August, 1839. By ALEXANDER NASMYTH, M.R.C.S., &c. &c.

AT the last meeting of this Association, the subject of the structure of the teeth was entered into by Professor Owen, who detailed before this section more especially the investigations which have of late years been conducted in this branch of science. I must confess that I felt dissatisfied with some of the results of these investigations, contained in the writings of Retzius, Purkinje, and Müller; and at the time that paper was read, I was endeavouring to arrive at a confident conclusion on the subject, by prosecuting a series of researches which I had commenced several years before, and which I have pursued uninterruptedly to the present time.

I will in the first place briefly place before you the conclusions to which I have arrived on the fol-

lowing subjects: 1st, on the Covering of the Enamel: 2ndly, I will communicate my own views on the Structure of Teeth; and 3rdly, I will treat succinctly of the Structure of the Pulp, and its relation to the developement of the ivory.

The researches of Retzius and Purkinje, so faithfully detailed to you last year, have established that there enters into the composition of the simple and compound teeth of man and mammalia generally, not only ivory and enamel, but a third substance, the *crusta petrosa*, which had been noticed previously on the free surface of the compound teeth of graminivorous animals. The *crusta petrosa*, as existing on simple teeth, was described to you, after Retzius, Purkinje, and Fränkel, as a layer external to the ivory of the fang, but as not present on these simple teeth as a covering to the enamel. I should here mention, however, that Purkinje and Fränkel, state, that they had once noticed it coating partially the enamel on the tooth of an old man. My researches, however, have led me to the conviction, that the enamel itself possesses in all instances a distinct envelope or coating. On the incisor of the calf and several other simple teeth, I have also distinctly traced in this layer of *crusta petrosa*, superimposed on the enamel, the corpuscles of Purkinje, analogous to those found in bone. I possess preparations of teeth of the human subject, and simple teeth of the herbivora and carnivora, showing this structure in a clear and unequivocal form. The details of its discovery and of its anato-

mical description may be found, by those who feel anxious to inquire further into the subject, in a paper of mine in the forthcoming volume of the Transactions of the Med. and Chir. Society, accompanied by drawings.*

At present, in order not to make inordinate demands on your limited time, I hasten to the next branch of my subject. In a paper which I have had the honour of submitting to the notice of the geological section of the Association, I have treated of the structure of the substance which in the dental ivory occupies the space between the fibres. I have proved, by a careful examination both of fossil and recent teeth, that the interfibrous substance is not structureless, but that it presents a character so remarkably well defined as to furnish a most important and interesting accession to the odontographic basis for a classification of the animal kingdom.

I have selected specimens of these appearances, (seen in the diagrams, marked Recent and Fossil Ivory, two of which I have given at [Pl. C. 5] Nos 8 and 10,) not with any reference to the individual peculiarity of their arrangement, but merely because the drawings of them are in a more convenient and advanced state.

I have also made researches into the structure and composition of the fibres of different teeth. I have generally found that these present an in-

* Here were exhibited and described the drawings contained in Pl. C. 11 and 12 of the appended series.

interrupted or baccated appearance, as if they were made up of different compartments. The size and relative positions of these portions or divisions of a fibre differ in various series of animals. In the human subject, for instance, each compartment of the fibre is of an oval shape, and its long, small extremity is in apposition to the one next adjoining. The long axis of the oval corresponds with the course of the fibre. In some species of the monkey tribe the fibre appears to be composed of two rows of compartments parallel to each other, and a trace of the same appearance is evident even in some of the principal ramifications of the fibres. In the orang utan the fibre is composed of rhomboidal divisions, and in the baboon they are oval like those of the human subject, and the surfaces of the long axis are in apposition. Some of the appearances are seen in the diagrams [Pl. C. 5,] marked ivory fibres, Nos. 3 to 6, where the appearances of the fibres of the orang utan, loris, mandril, and cynocephalus, are shown.

When teeth are submitted to the action of acid, for a period long enough to allow the earthy matter to be all taken up, I find that the animal residue consists of solid fibres, and if the decomposition be allowed to continue, these fibres present a peculiar baccated appearance. The drawings marked, *Ivory deprived of Earth*, [Pl. C. 6,] Nos. 1 to 4, show these appearances.

[No. 1 shows the appearance of the ivory when the earthy matter has been almost entirely removed

by acid, but where the cells still retain their position, general appearance, and connexion with each other.

Diagram No. 2, represents a more advanced stage of decomposition, where there seem to be attached to each fibre minute lateral filaments, which I presume to be the remaining portions of the emptied cells.

Diagrams No. 3, and No. 4, depict the appearances presented when decomposition has so far advanced as to have rendered the fibre interrupted or baccated. No. 3, represents the fibre of a human tooth, and No. 4, that of the elephant in this state.

It may be useful to compare these cells in No. 1, after they have been deprived of their earthy contents, with their state previous to the reception of the earthy matter as delineated in pulp No. 6, A. &c. [Pl. C. 7 and 8.] In the reticulations they are collapsed, lying one above another, but after having been deprived of earth, they will be observed to be rigid, and to retain the erect distended form which they acquired by the deposition within them of ossific matter.*]

* To indicate the true theory of the formation of ivory, nothing more is required than the display of these appearances. No "excreted" or "exuded" substance can possibly present an animal tissue arranged in regular connected cells. It is quite evident that these cells, whilst receiving a supply of earthy matter during the process of transition, must remain in connexion with, and indeed continue to form part of, the pulp. It would be absurd to suppose that a regularly cellular structure can be "excreted;"

The general appearance of the fibres thus treated is exactly similar to that of the fibres of cellular tissue generally, and the diameter of each corresponds exactly to the diameter of the calibre of the tube, which, according to Retzius, is pervious, although at the same time he says that it is always more or less filled with earthy matter. In fact, the tubes have been said to be principally visible by means of their contents, the reason of which appears to me obviously to be, that these contents are the only part of them which actually exist.

In order to separate the animal matter from the osseous substance of the tooth, I submitted thin slices of many different kinds of dental bone to the action of a solution of caustic potash, for a period sufficient to dissolve and remove the organic tissue; but the brittle nature of the residue, the difficulty of washing it without breaking down its structure, and the great opacity of the sections which had been thus treated, deprived this experiment of any striking results illustrative of the internal organization of teeth: but the appearances presented in its progress were all such as to favour the conclusion that the structure of the ivory is essentially cellular.

but it would be still more ludicrous to maintain, that after such cells have been excreted, that is to say, after all connexion between them and the pulp has ceased, they still possess the power or means of deriving from the blood the materials requisite for their transition into ivory, and of carrying on that process in their isolated state.

Having convinced myself of the existence of the peculiar cellular structure of the tooth, I entered with great interest on an examination of the organ by which it is produced, viz. the pulp.

On examining the internal structure of the pulp generally, the number of minute cells presenting themselves in a vesicular form is very remarkable; they seem to constitute indeed the principal portion of its bulk. These vesicles vary in size from the smallest perceptible microscopic appearance, probably the ten-thousandth part of an inch in diameter, to one-eighth of an inch, and are evidently disposed in different layers throughout the body of the pulp. They are of various shapes, as is shown at [Pl. C. 6], Pulp Nos. 1 and 2.

[No comparison can properly be instituted between these vesicles and the cells of the ivory, for it is only at the surface of the pulp that these vesicles are prepared by some peculiar change in their form and arrangement for the reception of earthy matter.]

When thin layers of macerated pulp are examined, they present an irregular reticular appearance, and are found to be interspersed with granules. The parenchyma is traversed by vessels of which the direction is generally vertical, as seen in [Pl. C. 7], Pulp No. 5. Pulp Nos. 3 and 4, [Pl. C. 7], show the appearance of these cells in sections of the pulp which have been thus macerated.

I have frequently been struck with the rapidity with which the pulp diminishes in volume, and

with the extent of this diminution. Sometimes, indeed, it would appear in a short space of time to be almost annihilated, and this seems to take place more decidedly, when the tooth has been in a healthy state, and more frequently in adult than in temporary teeth. This shrinking, or almost total disappearance, may, I think, be accounted for by a peculiar collapse or change in the congeries of cells of which we find the pulp to be made up. The use of this peculiar arrangement, and the purpose which it serves in the economy of the part, will furnish curious subjects for future inquiry. A subject also highly worthy of investigation is the nature of the contents of these cells. They must evidently be filled either with air or fluid, but they are so extremely minute that I have not yet been able to ascertain which.

After repeated and careful investigation, I have convinced myself that they constantly exist on the surface of the pulp which is in apposition to the ivory, and which is essentially concerned in its developement.

[By comparing, however, the diagram, Pulp No. 4, with Pulp No. 6 B, it will be seen that these vesicles are present on the surface of the pulp in a modified, more regular, and more distinctly cellular form than in its interior: and with this additional difference, that throughout the substance of the pulp they present ill-defined layers, whereas, at its surface, they are arranged in reticular leaflets, to be hereafter described.]

Much diversity of opinion has always existed respecting the connexion of the pulp with the ivory of the tooth ; and as to whether the ivory be simply a product of the pulp, or a transformation of its substance. Although this is by far the most interesting point in dental physiology, and involves the grand question of the manner in which the tooth is formed, as well as that of its arrangement and conformation, it is notwithstanding less understood, has been less studied, and is consequently more obscure, than any other part of the subject. The vague style in which authors discuss, or rather dismiss this topic, shows how little has been really done to elucidate it. I must confess that I devoted myself to its examination for a long time before I was fortunate enough to obtain any light wherewith to guide my steps to the discovery of its true bearings, nor am I yet certain to what extent this knowledge of the structure of the transition surface of the pulp will be found to facilitate our comprehension of the whole complicated process by which the ivory is developed.*

* Although, from an examination of the diagrams, Pulp No. 6 A. and No. 6 B., Pl. C. 7, of all the diagrams in Pl. C. 8, and of the first three in Pl. C. 9, it is distinctly demonstrated that the interfibrous substance of the ivory is formed by the deposition of osseous matter in the cells of the reticular surface of the pulp, I cannot boast of being able fully to unveil that interesting process. The cells for the reception of the earthy matter are displayed, but—how is this matter arranged in those cells? How is it, in the first place, derived from the blood, and introduced into them? What are the causes of the characteristic varieties of the interfibrous cellular substance in different classes

The formative surface of the pulp displays a regular cellular arrangement, which I have denominated reticular, and which may be described as resembling a series of skeletons of desiccated leaves. (See [Pl. C. 7], Pulp No. 6 A. and B.) It is not easy to obtain a preparation where the appearance is so perfect as to allow of a clear sketch of the consecutive parts of it being taken. The drawing I now present to your notice is the most perfect I have been able to obtain, and is from the tooth of a calf. The compartments of the reticulation are seen to be oval, and overlap one another. On insulating one of these compartments or leaves, (see [Pl. C. 7], Pulp No. 6 B,) we find that its structure is curious and regular. These beautiful reticulations have peculiar diversities in different animals. I first observed them in the human pulp, and soon found them in all other animals which I had an opportunity of examining, varying, as I have said, in size and arrangement in different cases. I next extended my observations to the capsule, and to the capsular investment of the enamel, and found in

of animals? What is the precise degree of importance of the reticular cellular organization observed on the surface of the pulp with regard to the process of transition, besides the fact that it presents cells into which the earth is deposited? Do these reticular cells form a system containing circulating fluids, from which the osseous material of the tooth is eliminated? How are these cells connected together, both in their transition state, and in the pulp, as well as when they have passed into the state of ivory? These, and many other similar questions, remain to be solved before our comprehension of the process of the formation of ivory can be said to be complete.

these the same reticular disposition, though with characteristic variations, as seen in diagrams marked "Capsule" and "Enamel Investment." [One of these is seen at Pl. C. 11, No. 6.]

These leaves of reticulation are surrounded by a well-defined scalloped border, from which occasionally processes are observed to project at regular intervals, (as may be seen at [Pl. C. 7] Pulp, No. 6 B.)

Having thus demonstrated the cellular texture of the pulp, throughout its entire extent, I next proceeded to inquire how its transition into ivory is effected. The researches which I have made on this point are as yet imperfect, and I approach the subject with diffidence, knowing the deceptive results to which novel experiments are liable; and well aware of the necessity of long study and deliberation, before judgment be positively given on a point hitherto undecided.

How does the fibre of the tooth originate? and how is the interfibrous substance, which must form the main bulk of the tooth, deposited? I will state how far my own observations allow me to answer these difficult questions. If I cannot at once satisfy the querist entirely on this subject, which has been seldom even approached, much less frankly entered upon, I may at least hope, by a few facts which I think my investigations have placed beyond doubt, to pave the way for a satisfactory explanation of the formation of dental bone.

On the surface of the pulp are found innumerable

detached cells, with central points. Generally, these cells form a regular and complete coating, studded with points, which are placed at intervals, corresponding in extent to those between the fibres: [Pl. C. 8], Pulp, Nos. 10, 16, 12, and [Pl. C. 9] Nos. 13 and 14, show the appearances of these ossified cells.

These points are rendered visible from the greater opacity of the intermediate material, and will be seen to reflect or absorb the light, according to the difference in the focal distance. A comparison between the superincumbent perfect ivory, and the formative surface of the pulp beneath, is always easy, because portions of the former, at an early stage at any rate, remain adherent to the latter, and fragments of the dental bone are found strewn over it, more especially in human teeth, as seen in the drawings alluded to above.*

* It is almost unnecessary to state that it is impossible to obtain a clear view of the surface of the pulp after the process of the formation of ivory has commenced without breaking away or forcibly separating in some manner, the superincumbent crust of ivory from the pulp beneath; and in this disruption the ivory is always somewhat shattered, and the surface of the pulp is found with separate cells and cellular fragments of all shapes, and in every stage of transition, strewn over it. In the diagrams marked Pulp No. 10, Pulp No. 16, Pulp No. 11, and Pulp No. 12 and No. 13, are seen fragments of this kind, coloured yellow. In No. 14, on the other hand, which is of a very young tooth, the pulp is shown at the period of the formation of the first layers of ivory; no disruption, consequently, was necessary in order to exhibit it, and hence the pulp is seen with its two superior layers in successive stages of transition into ivory. The fragments above alluded to can

The cellular conformation of these fragments is always evident, and in size and appearance they never be too minute for the purposes of study. Indeed, there is no more satisfactory and interesting method of examining all hard organized bodies, than by pounding them in a mortar: fragments thus produced present themselves under an infinite variety of aspects, and display, in the most various and complete manner, the structure of these tissues. Such particles will never be too small for observation in the focus of the instruments of the present day. With respect to the preparation of anatomical specimens like the above-mentioned, where the formative surface of the pulp is shown by the removal of the ivory, I may observe that considerable patience and perseverance, and an abundant supply of subjects of investigation, are indispensable before they can be properly made; and that even great proficiency and experience will not always insure success, which necessarily depends, to a certain extent, on fortuitous circumstances, such as the nature and state of the subject, the direction and degree of the fracture, &c. It sometimes happens that out of a great number of fresh-drawn teeth, not one available preparation of this kind can be made. The pulp and the ivory being in positive union, the separation at the proper point seldom occurs. When, however, the industry of the inquirer is at length rewarded by a successful preparation, he will be gratified by a view of phenomena of the highest general physiological interest, the importance and full bearings of which could only be glanced at in the present memoir.

It has been asserted that no preparations can be made from mammalia showing the transition of the pulp into ivory. Such a statement can only have originated in inexperience or in ill success, arising from causes hinted at above; but the number of such preparations of the most satisfactory kind which are in my possession, and which I am always glad to exhibit to any person who may feel a wish to see them, precludes the necessity for my wasting any of my own or my reader's time, by refuting such an inconsiderate and groundless assertion.

are perfectly accordant with the cells of the pulp. (See [Pl. C. 8] Pulp, 9 to 16, where the yellow colour shows the ossified cells or ivory.)

At an early stage of dental developement, the reticulated or cellular appearance of the pulp is particularly beautiful. When merely a thin layer of ossific matter has been deposited on its surface,* it may with great facility be drawn out entire,

* I am ready to allow that there is a certain degree of vagueness in this phrase, which had the paper been intended for publication, I should have rendered more definite and precise. It contains, however, not the slightest contradiction or incongruity; and it is only the most unfair and malevolent criticism which could avail itself of passages of this kind, written as they were for oral delivery, with the accompaniment of illustrative diagrams, and not for the press, to impugn the general tenor of my communications, written and graphic. "The fact is," as I have elsewhere observed, "that when the external layer of the pulp becomes ossified, it can no longer be regarded as pulp. It is then spoken of as a layer of ivory in apposition to and connexion with the surface of the pulp beneath. Thus the deposition of ossific matter on the surface of the pulp simply means its deposition in the cells of the reticular formative surface, which is undergoing the process of dentification. That the idea of exudation could not even have been floating before my mind, is proved by a subsequent passage, where I state *that the manner in which the osseous matter is deposited in the cells of the interfibrous substance I had not been able to discover;*" the concluding portion of even the above paragraph proves the same: but it is useless to waste more time in demonstrating the monstrous absurdity of the assertion that my two papers on the structure of the teeth, and the diagrams exhibited in illustration of them, were in support of the theory of excretion or exudation !

together with the former, laid on a glass, compressed a little, and then examined with the high powers of the microscope. The different layers of cells will be seen, and the transition into ivory may be observed, (as shown in [Pl. C. 9] Pulp No. 14, where the different gradations of ossification are marked by the shade of colour.)

The diagram marked Pulp, No. 6 A. [at plate C. 7] represents the general arrangement of the reticular leaflets of the surface of the pulp. In No. 6 B. is seen a beautifully-organized single leaflet of this reticular surface, in the pre-existing cells of which the earthy matter is in course of deposition, and the process of ossification is seen to be gradually extending from its centre towards its circumference. No. 8 [plate C. 8] shows the blood-vessels coursing beneath the reticular formative surface. This drawing is an exact copy of the preparation, in which, as is seen, the blood-vessels have been ruptured in two places, in separating the ivory from the pulp, owing to the close connexion between the reticular surface and the ivory already formed. Pulp No. 9, with the title *Ossification, or Developement of Ivory*, and the following drawings, [see Plates C. 8 and 9,] are a selection from the interesting preparations which I have made for the purpose of elucidating the process of the formation of ivory. They display under various aspects the different appearances, some constant, the others incidental, which present themselves on the removal of the superincumbent ivory

from the surface of the pulp beneath. The portions coloured blue are cells of the pulp as yet unossified, and the parts coloured yellow are either entire layers of ossified pulp-cells, or fragments of ivory which have remained adherent to the pulp, or extravasated fluid, containing probably osseous matter. (See [Pl. C. 8.] Nos. 11 and 12.) The whole appearances denote the existence of a peculiar system of cells external to the peripheral ramifications of the blood-vessels, the functions of which are highly interesting, and the study of which will ultimately lead, I think, to the unveiling of many vital processes connected with the growth of animal tissues, which are at present shrouded in obscurity, besides that which is rendered so clearly evident in the diagrams, marked [Pl. C. 7] Pulp No. 6, B.; [Pl. C. 8] *Pulp, Ossification, or Developement of Ivory*, Nos. 9, 10, 16, 11, 12; [Pl. C. 9.] *Pulp, Ossification or Developement of Ivory*, Nos. 13 and 14, and which is plainly neither more nor less than a process of ossification;—a view of it totally opposed to that which has been taken by previous inquirers into the formation of ivory.*

* To enter into a full description of the details of these drawings was impossible on an occasion like the meeting at Birmingham, and I was therefore compelled to trust in some measure to the ocular demonstration they afford for conveying to the audience an impression of the nature of the interesting process by which ivory is formed. Aware of the limited extent of time which could be granted me for the exposition of this complicated and comparatively uninvestigated subject, I, as far as was possible, made it speak for itself in the drawings above mentioned.

It appears to me that the framework of the reticulation, or cells of the pulp, constitutes 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; the points or projections rising from the framework correspond to the centres of the cells, and may be traced to belong to their structure. [The fibres composed of granules of animal matter, and which I describe as the framework of the reticulations, become, upon the deposition of ossific matter within the cellules of those reticulations, the fibres of the ivory. The only change which they appear to undergo during the process of transition, is, that they are then drawn out from the coiled-up state in which they exist between the collapsed cells of the reticulations into the more longitudinal but still spiral form in which they are found in the ivory. This will be fully understood by an examination of the diagram [Pl. C. 9. No. 15.] The fibres of the ivory are frequently very spirally curved, like those of the pulp, and as we should conclude they must be from the manner of their evolution, as seen in diagram, Ivory, No. 8.* Pulp No. 15, [Pl. C. 9], shows the appearances presented by a portion of a recent tooth, which has been submitted to the action of acid. Part of the pulp is visible in con-

* This I cannot give in the present publication: it is of no essential importance.

nexion with the ivory, and the spiral fibres are seen as they are evolved on the surface of the former. It appears to me, on microscopic examination, that this convoluted fibre is made up of single successive granules, which are developed one after the other from the body of the pulp, until the fibre is complete. I have, moreover, already shown that the fibre of decomposed teeth is resolved into separate granules or compartments, as is seen in the diagrams marked "Ivory deprived of Earth," [Pl. C. 6], Nos. 1 to 4.

The manner in which the osseous matter is deposited in the cells of the interfibrous substance, I have not been able to discover. It would appear, however, that these cells are subdivided into minute cellules, for they present the appearance of being filled with granules in certain progressive stages of developement, as is shown in diagrams [at Pl. C. 8], Pulp Nos. 9, 10, 11, 12, [and C. 9], No. 13.

In whatever aspect we view the formative organs of the tooth, and the dental tissues themselves, and whether we examine the latter during the process of their developement, or after their formation has been completed, we are everywhere met by appearances which denote a cellular or reticular arrangement.

I must allow that these views of the formation and structure of the teeth are both bold and novel, but I do not claim for them infallibility; I simply submit them to the Association as the results of actual observation. I fully recognize and respect the authorities ranged in support of very different

theories, though I still venture to think that, were not my limits confined, I could easily show how what I hold to be fallacies and incongruities have arisen. As, for instance, from want of practice in the manipulation of materials, the precise nature of which has not been attended to ; want of familiarity with microscopic appearances ; ocular deception ; the want of a well-defined magnifying power, and imperfect light in the microscope used, have been doubtless, also, causes of many of the conflicting conclusions to which almost all inquiries in this region of anatomy have arrived. I think that the view I have taken of the subject more satisfactorily explains that any other, facts of daily experience. The laminated arrangement of the osseous cells explains the concentric fracture of the tusks of the mammoth and of other teeth, when left to decompose spontaneously. The cells being in imbricated apposition, and held together by earthy salts, being moreover arranged in layers conformably with the periphery of the pulp, must be regarded as concentrically laminated. The existence of this structure explains the phenomena daily noticed by ivory-cutters, and also Mr. Hunter's experiments of feeding animals with madder, the result of which is incompatible with any other theory of the structure of dental bone.

No view hitherto taken of the structure of dental bone has afforded a satisfactory explanation of the ordinary morbid appearances of the tooth, but many of these I think may be explained, if we

regard the latter as cellular. Still I do not conceive that I have in any way exhausted the subject; far from it; I am quite alive to the imperfect nature of my researches, and am prepared for correction on many points, when more extended and varied investigations shall have been undertaken.

I find that Schwann, in a recent work, teaches that all the primary tissues of the animal frame are cellular, and has given to the world some remarkably interesting details on this subject. He says that he has remarked the characteristic "cellular nuclei," or elementary cells, on the enamel-membrane, that they are continued in minute fibres, and that these are similar to the epithelium cylinders in mucous membranes. He notices what he calls cylindrical cells on the surface of the pulp, and he supposes that these cylindrical cells of the pulp are the fibres of the tooth in their first stage, which does not at all coincide with my observations. He regards the dental substance simply as the ossified pulp, whilst my observations lead me to conclude that the cells of the ivory are altogether a distinct formation.* He acknowledges, however,

* To show the unwarrantable means which have been had recourse to in order to misrepresent my views of the formation of ivory, I beg to quote from an article in the Medical Gazette the following passage, exposing the manner in which the above statement has been tampered with and misrepresented:

"At p. 507, of the last week's number of this journal, he (Mr. Owen) manufactures what he is pleased to call Mr.

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 I have just communicated.

The following is the account contained in his work on the structure and function of the pulp.

“According to Purkinje and Raschkow, the pulp consists at first of nearly uniform globules, without vessels and nerves; afterwards, vessels

Nasmyth's *emphatic statement* of September, 1839, ‘that so far from being the ossified pulp, it (the dental substance) was altogether a distinct formation.’ Now of this passage, which he gives as quoted from the Literary Gazette, p. 598, only *the four last words* will be found, on reference to that journal,” (and to the original paragraph itself in the paper above,) “to be correctly copied: *all the first part of the passage is the composition of Mr. Owen.* Mr. Nasmyth, by this perversion of his report, is made to say that the dental substance is altogether a distinct formation: whereas he never mentions the dental substance at all, and his real meaning evidently is, as we have shown above, that the cells of the ivory have undergone a distinct formative process in their transition from the cells of the pulp. Such unfair and unworthy tactics must inevitably defeat the purpose for which they were adopted. Mr. Owen's unscrupulous violence, far from concealing, as he seems to calculate it may do, the weakness of his arguments, will only still further damage his cause in the eyes of every dispassionate reader.”—*Medical Gazette* for July 3, 1840.

arise in it, and at last nerves also. On the surface the globules are more regularly arranged, and more longitudinally extended, and turned in an external direction under right or slightly acute angles.

“ These longitudinally drawn out globules are plainly cylindrical cells. They contain very evidently in recent (fresh) teeth the characteristic cellular nucleus with its nucleus-corpuscles, and are very similar to the prisms of the enamel membrane. The interior of the pulp consists of round cells, also with a nucleus, and between these cells run vessels and nerves. If we draw the pulp of a young tooth out of its cavity, and then examine the dental substance, whether deprived or not of its calcareous salts by muriatic acid, we shall find on its internal surface, at any rate inferiorly, where the already formed dental substance is still thin and soft, a layer of the cylindrical cells of the pulp. These have about the same thickness as the solid fibres of the dental substance, and also the same course; and inasmuch as they, on the one hand, plainly belong to the pulp, on account of their conformity with the cylindrical cells adhering to the remaining surface of the pulp, and, on the other hand, as they cohere more firmly with the dental substance than with the pulp, and remain attached to the former, I presume that here a transition takes place, and that the cylindric cells of the pulp are only the fibres of the tooth in their first stage, and change into the latter by filling with organic substance,

becoming solid and ossifying. Sometimes these cylindriculi are not found on the dental substance, but then in their place are found a number of cellular nuclei. These are of a very pale colour, and are intimately connected with the dental substance, so that they are easily overlooked; but when the attention is once directed to them, it is impossible not to recognize them: the spaces between them are very narrow. 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 I allow the force of this objection. Nevertheless it is at any rate weakened by the circumstance that a portion of the pulp actually remains attached to the dental substance, and by the fact that in half ossified ribs, for instance, the cartilage can be easily separated from the ossified portion, and it must be remembered that in the tooth the separation must be easy in proportion to the difference between the consistence of the pulp and of the dental bone." *

* I consider it a duty to acknowledge with gratitude the obligation I am under to the editor of the Literary Gazette for the extended report of my communications which he inserted in his excellent journal. When I placed in that gentleman's hands a rough draft of my papers to assist him in drawing up his report of them, I did not expect that he would have given me the benefit of so extensive a report as that which made its appearance. I was not able, however, to see a proof of it, or I should certainly have endeavoured to explain more fully the allusions to the diagrams, and to introduce a few of the oral

observations in explanation of the latter, without which it is possible that some portions of the paper, referring as they do exclusively to the diagrams, may appear rather incomplete. An omission which was made in the Literary Gazette with respect to my account of Schwann's researches, has given rise to some ridiculous observations. The mistake consisted in the editor's omitting in his report the sentence immediately preceding my account of Schwann's researches.

MEMOIR No. III.

*Investigations into the Structure of the Epithelium,
presented to the Medical Section of the British
Association for the Encouragement of Science, at
its Meeting held at Birmingham in August, 1839.*
By ALEXANDER NASMYTH, M.R.C.S., &c. &c.

IN a former part of this communication I have endeavoured to prove that the pulp, the formative organ of the tooth, is composed of cells. I have also shown, that the character of the teeth themselves is more or less cellular; and the observations which I have made on the structure of the epithelium have led me to a conviction, that it also is composed of cells. Although some of the facts which I am about to relate, have, since the prosecution of my researches, been noticed by Henle and Schwann in Germany, I think it right, neverthe-

less, to state concisely all the results of my investigations.

Leeuwenhoek, who did so much towards the advancement of structural anatomy, was the first to give an accurate account of the structure of the epithelium. His researches on this subject are contained in letters to the Royal Society in the years 1674, 1684—85, which will be found in the third and fourth volumes of his collected works. He there states, that the human epidermis and epithelium are composed of scales, and of these he has left very accurate descriptions and delineations. He spoke of the scales of the epithelium as existing upon the mucous membrane of the mouth. The researches of subsequent observers tend to prove that these scales or cells exist in various forms upon the surface of all mucous and serous membranes, upon the inner membrane of the vascular system, &c. With respect to the existence of these scales on almost all these membranes, I agree with the authorities whom I have just quoted; and having premised thus much, I shall now proceed to treat, firstly, of the structure of the epithelium generally; and, secondly, of the epithelium as existing in the cavity of the mouth.

Structure of the Epithelium generally.—The epithelium is a layer of substance destitute of vessels, which covers the vascular surface of mucous membranes. Though destitute of vessels, it cannot, however, be considered as inorganic, as I shall presently show. If the surface of a mucous

membrane (for instance, the conjunctiva or the buccal) of a living animal be slightly rubbed, it will be found, on microscopic examination, that numerous small particles have been detached from it. At the first glance they present precisely the appearance of scales, for they are flat bodies with a thick portion or nucleus in their centre, and with very thin and transparent margins. It was Leeuwenhoek who first gave to these bodies the name of scales. They are found not unfrequently with a curved margin and without a central spot or nucleus, and their surface often presents numerous transparent points, with very fine lines. The nucleus of the scale generally contains a small body which has been termed the nucleus-corpuscle. But by this simple method of observation we do not obtain an insight into their true structure. If we remove the secretion from the surface of an irritated mucous membrane, we shall find another class of bodies which differ from these first mentioned, in being smaller and more globular. They have a nucleus of the same size as those of the so-called scales, and also a nucleus-corpuscle, but the surrounding structure is in the form of a cell, and is much smaller. Here and there may also be observed a nucleus with its accompanying corpuscle, lying in substance which presents no appearance of a cell. The structures here described may also be seen on a careful examination of a section of the epithelium and mucous membrane of a young subject. On the surface of, and

in immediate apposition to, the mucous membrane, are seen numerous nuclei, which more externally are surrounded by a cell ; and on approaching still nearer to the surface, we find this cell, from having increased considerably in size, and become compressed, assuming the appearance of a scale, which retains the nucleus, and its corpuscle of primitive size. The various stages of the developement of the epithelium may be satisfactorily traced, by removing, after a short maceration, the layer of epithelium from the under surface of the tongue of a young calf, and placing it upon a piece of glass, when, if the external surface of the object be brought into focal distance, large scales only with their central nuclei are observable ; but if the object be approximated to the glass so as to bring the internal part of it into the proper focal distance, numerous small scales are brought into view ; and if the object be still more approximated to the lens, so as to bring its internal surface into the proper focal distance, numerous rounded cells become apparent.

In the fœtus, the defined and well-formed scales of the epidermis are not unfrequently distinctly seen externally ; the *rete Malpighii* consists of newly-formed cells, and between the two may be observed other cells in a state of progressive developement. On the surface of the vascular mucous membrane minute cells are found with a nucleus in their interior, round which the cells grow ; and this, in short, is the process of developement of the mi-

nute bodies which constitute the epithelium. An interesting subject of investigation, and one which I believe has not been entered upon by those who have hitherto treated of this department of anatomical science, is the manner in which the component parts of the epithelium are connected. The cells on the surface of the mucous membrane are separated from each other by considerable spaces, which are occupied by a gelatinous substance, interspersed with minute granular bodies. But the scales forming superficial layers of the epithelium are separated by very minute linear spaces, but are still connected together by a translucent, gelatinous substance.

This latter displays considerable elasticity, as is easily rendered evident by an attempt to lacerate the epithelium in a moist state, if the latter be examined at the same time by the aid of a magnifying power. Each time that the laceration is attempted, the membrane yields, and the scales separate to a certain extent; but regain their original position, on the cessation of the effort to draw them apart. In some instances a fibrous structure is evident in the gelatinous substance between the scales, See Diagram, No. 3. (Pl. C. 10.)

The scales towards the free surface are distinctly observed to overlap. The gelatinous substance above alluded to, presents distinct granular bodies, which give to the epithelium, *en masse*, a rather dense aspect, the individual scales being sometimes covered by these granules; the latter can, however,

be separated from the scales by compression ; by which means, indeed, the granules themselves may be made entirely to disappear. In certain parts of the epithelium of the calf, distinct fibres are observed, which pass over the surface of the scales, and connect them together, thus forming a very delicate net-work. This appearance is most evident upon compression of the thick epithelium on the anterior part of the alveolar arch of the upper jaw.

In these cases, where the small scales, or small clusters of scales, are being continually thrown off, as on the surface of the body, and of the mucous membranes of man and animals generally, the scales composing the external layer will be found to overlap each other, and thus the gradual pressure of scales below, which are increasing in size, is the cause of the throwing off of these cuticular lamellæ. After these have been detached, their place is occupied by newly-formed scales. But there is another form in which the external layer of cuticle is removed, viz. in a continuous layer. The cuticle of the frog is composed of minute scales, the borders of which do not overlap, but are held in direct apposition, so as to form one lamina, which has a beautiful continuous tessellated appearance. This layer, of which I here display considerable portions covering the whole body, is thrown off entire by frogs and efts ; and I am disposed to believe that it is this covering which, according to naturalists, is swallowed by the animal after having been thrown off. As soon as this layer is

removed, another lamina of scales is seen on the surface of the animal's skin. If after the death of a frog it be immersed in water, this thin external translucent layer generally separates; but upon prolonging the maceration, another lamina is found to be gradually separating from the cutis, which is dense, and sometimes measures a quarter of a line in thickness. Internally it will be found to be composed of very numerous cells, while externally the regular series of scales is evident. The tessellated lamina alluded to above evidently takes its origin from this layer of cuticle. An examination of the specimens which I here present to your notice, and a consideration of the facts which I have related, cannot, I think, lead to any other conclusion than that the cuticle and epithelium are organised tissues. It would appear that they are formed from a fluid secretion on the surface of the vascular corion. The various stages of developement being, 1, the formation of nuclei and corpuscles; 2, that of cells; 3, the growth of the latter affected by vital imbibition; 4, their compression and gradual conversion into minute lamellæ or scales. [See Pl. C. 10. Nos. 2 and 3.] In short, it appears a rational conclusion that the component parts of the cuticle and epithelium have within themselves a power of growth; and it remains for pathologists to determine what share the derangement of this function has in the production of cutaneous diseases. Another argument in favour of the organic nature of the epithelium is derived

from the circumstance, that under certain modifications it presents various vital phenomena, among which may be mentioned the ciliary motions.

I now proceed to describe my researches on the structure and developement of that portion of the epithelium which lines the cavity of the mouth. In the foetal subject, previous to the extrusion of the teeth, it forms on the alveolar arch a dense, projecting layer, distinguishable from the surrounding membrane by its whiteness, and by the existence on its surface of ridges and sulci, having a waving course and a variable direction. The alveolar epithelium is thicker in proportion to the youth of the subject examined. It is most prominent where it corresponds with the molar teeth: its internal surface is concave, receiving the projecting mucous membrane. This portion presents various objects for investigation.

Firstly, as regards its composition:—It is made up of a mass of scales, lying one on the surface of the other. This disposition shows that the terms “dental cartilage,” or the “cartilage of the gum,” which have hitherto been applied to this structure, give an erroneous idea of its true nature, for cartilage always presents the corpuscle discovered and described by Purkinje. As in other portions of the epithelium, the external scales here are the larger, and this holds good generally, until we come to the surface of the vascular mucous membrane, which presents simple cells with their corpuscles.

In the interior of this alveolar epithelium, where it corresponds to the molar teeth, small vesicles may be frequently observed, varying in size from one-quarter to one-eighth of a line in diameter. They appear to the naked eye to be transparent; under the microscope their parietes are found to consist of attenuated scales, and their cavity to contain a fluid abounding in minute granules and cells.* The internal surface of the epithelium covering the alveolar arch frequently presents concavities or indentations which are from a line and a half to three or four lines in circumference: they correspond to projections from the mucous membrane formed by a larger species of vesicle. The latter is deeply implanted in the vascular mucous membrane. The parietes of these vesicles are composed of a very delicate membrane; they contain a transparent fluid which coagulates on the application of heat, or acid, or on immersion in spirit, and in this fluid float numerous globules and scales similar to those of the epithelium generally. The internal or attached surface of the alveolar epithelium also presents numerous fringed processes measuring from one line to one and a half lines in length, and half a line in breadth, which sink into the substance of the subjacent mucous membrane. Under the microscope these

* The vesicles here alluded to are most probably those which Serres describes as glands for the secretion of tartar: they are very numerous even after the extrusion of the incisor teeth of the calf, and are seen with great facility internally.

fringes are found to be composed of elongated scales connected together, forming masses which divide and subdivide until they attain such an extreme tenuity that the most minute terminations consist but of two scales in marginal apposition. If the epithelium be carefully separated from the surface of the mucous membrane corresponding to the unextruded molar teeth, and placed in water or in diluted spirit of wine for some little time, its internal or attached surface presents these fringes much enlarged and forming a mass more considerable in size than the dense epithelium itself.

The epithelium covering the mucous membrane of the palate presents transverse rugæ, corresponding to those of the mucous membrane. If these palatal rugæ of the epithelium of the calf be carefully examined from the internal surface with a magnifying power of one inch focal distance, each will be found to consist, or to be composed of, numerous depressions or cul de sacs which receive prolongations or pointed processes of the subjacent mucous membrane.

They are of extreme tenuity, and, when viewed by the aid of high magnifying powers, are observed to consist of distinct scales.

A question of much interest, and one to which I have paid considerable attention, is whether recent investigators are warranted in considering mucus and epithelium as identical; I am disposed to think that they are formations quite inde-

pendent of each other ; but my reasons for arriving at this conclusion are numerous, and the details of my investigations upon this part of the subject I propose to treat of at length elsewhere.



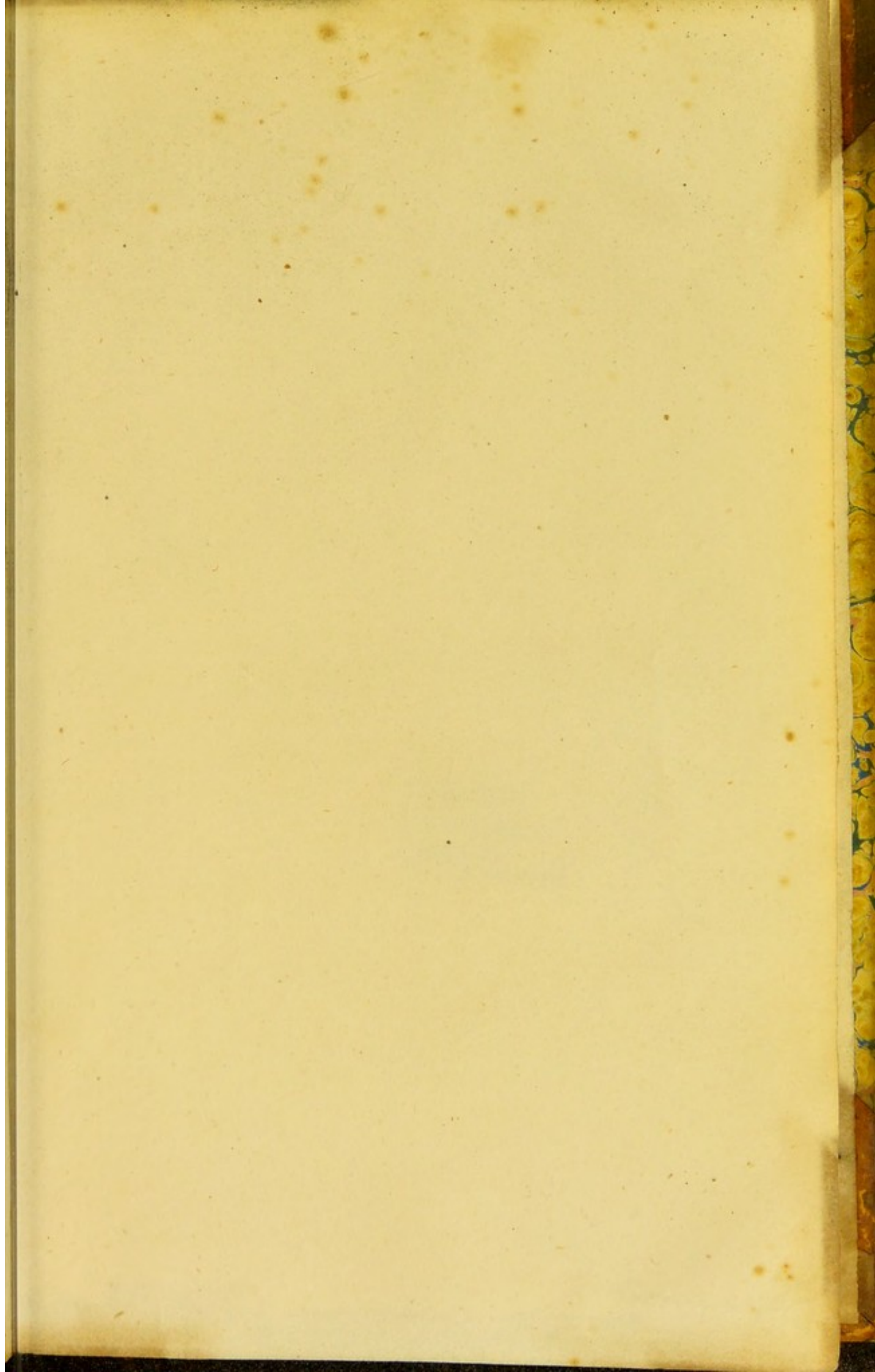
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