

**A probationary essay on necrosis : submitted by the authority of the president and his council to the examination of the Royal College of Surgeons of Edinburgh, when candidate for admission into their body, in conformity to their regulations respecting the admission of ordinary fellows / by James Syme ... June 1823.**

### **Contributors**

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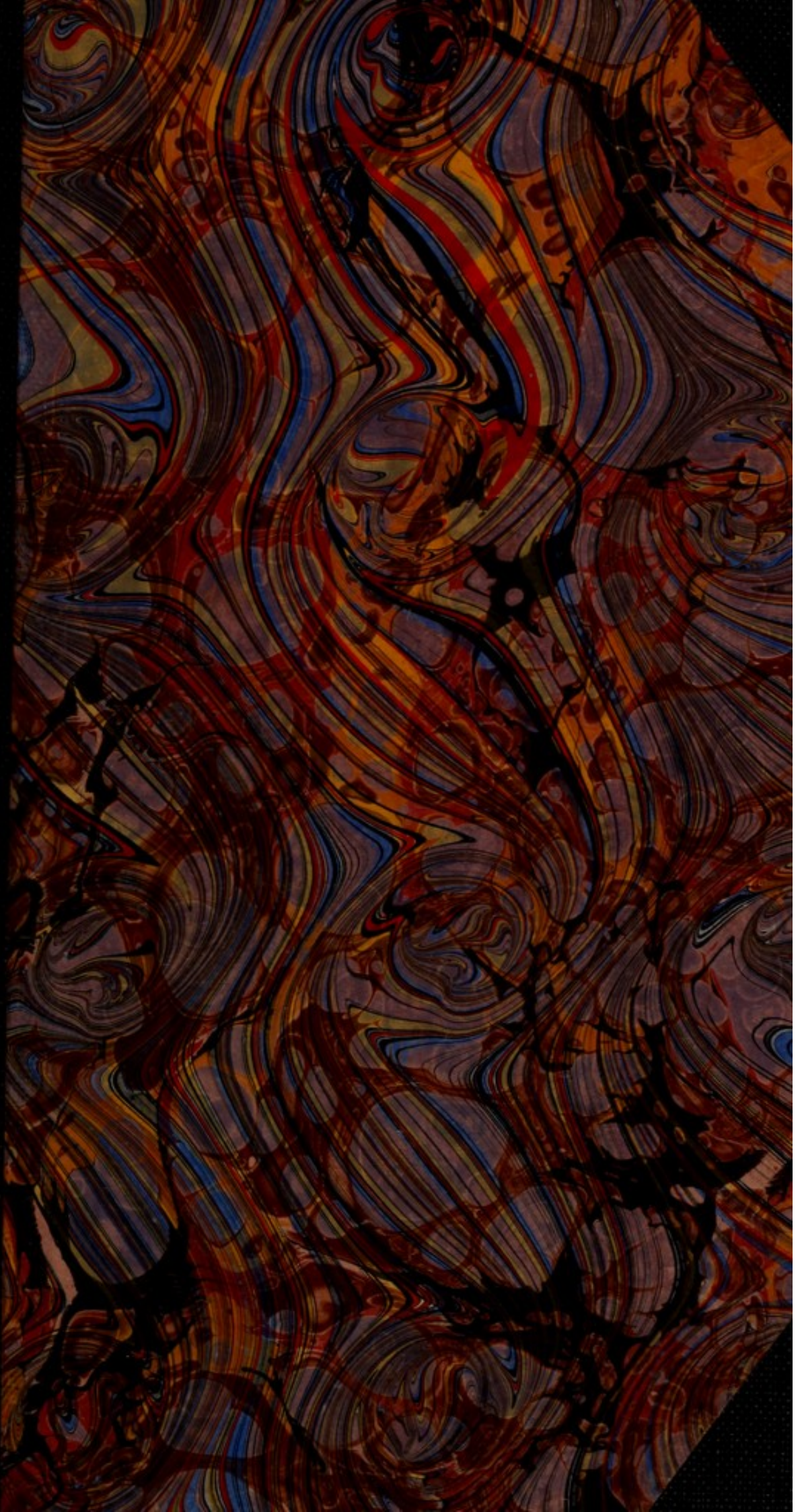
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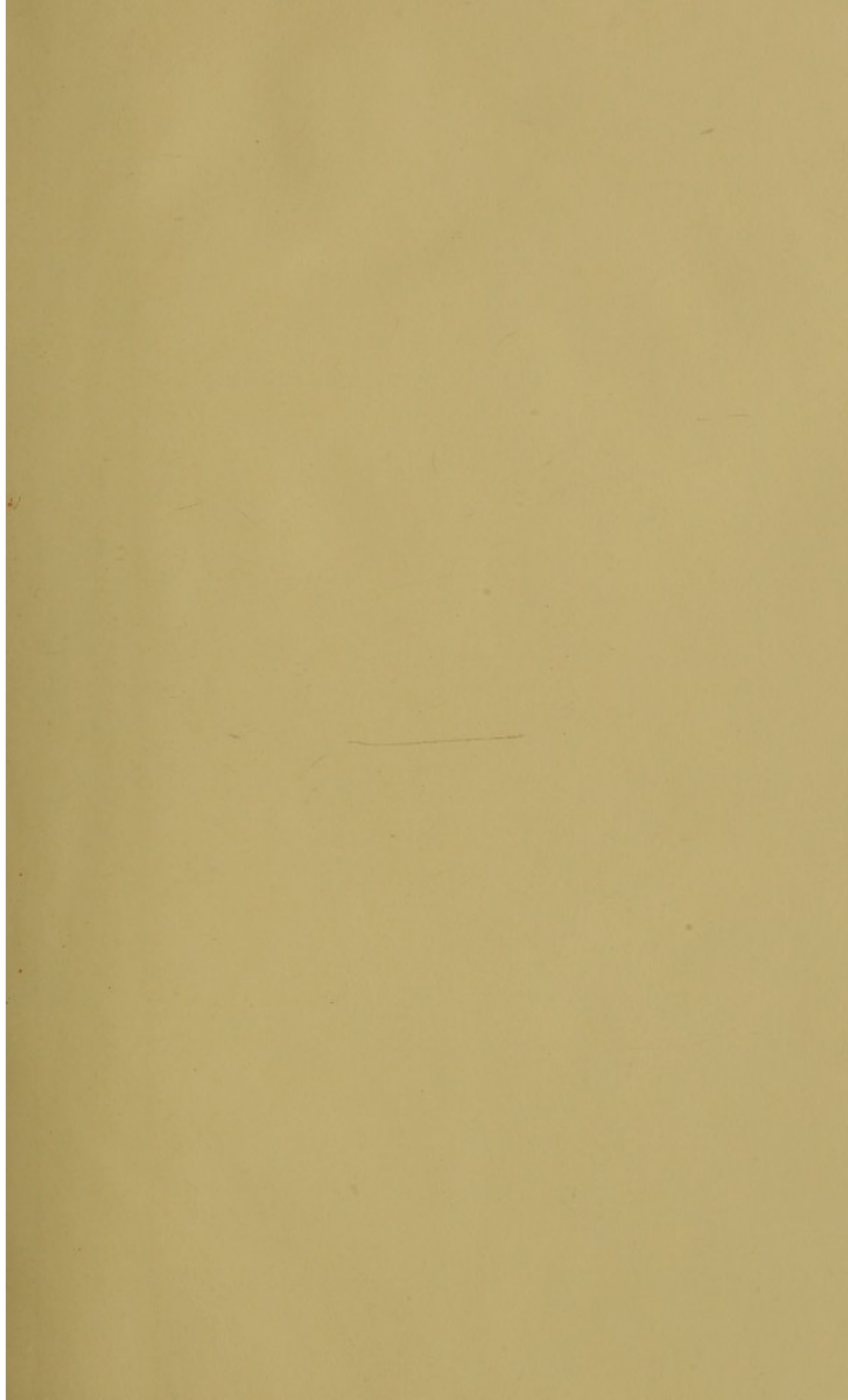


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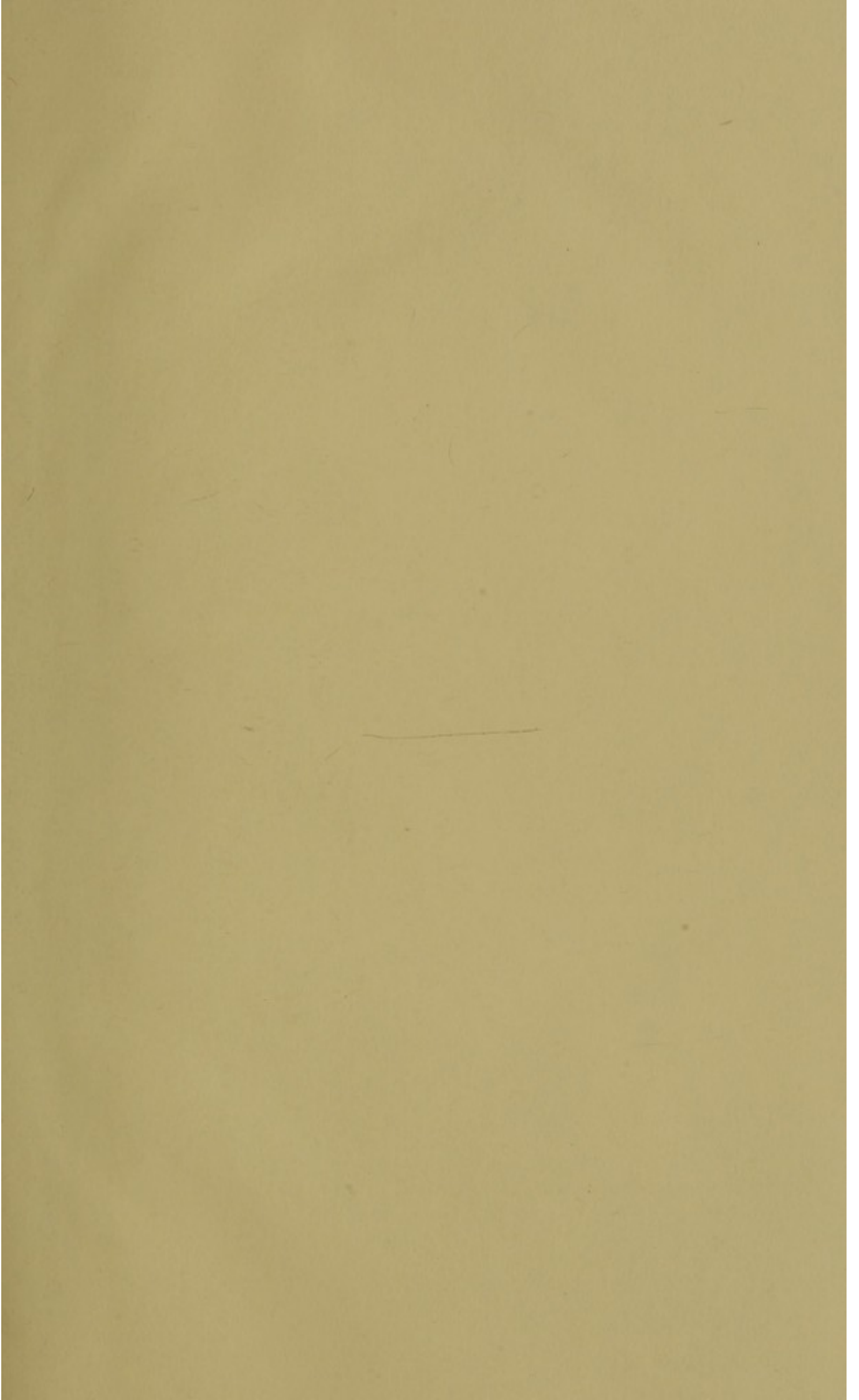


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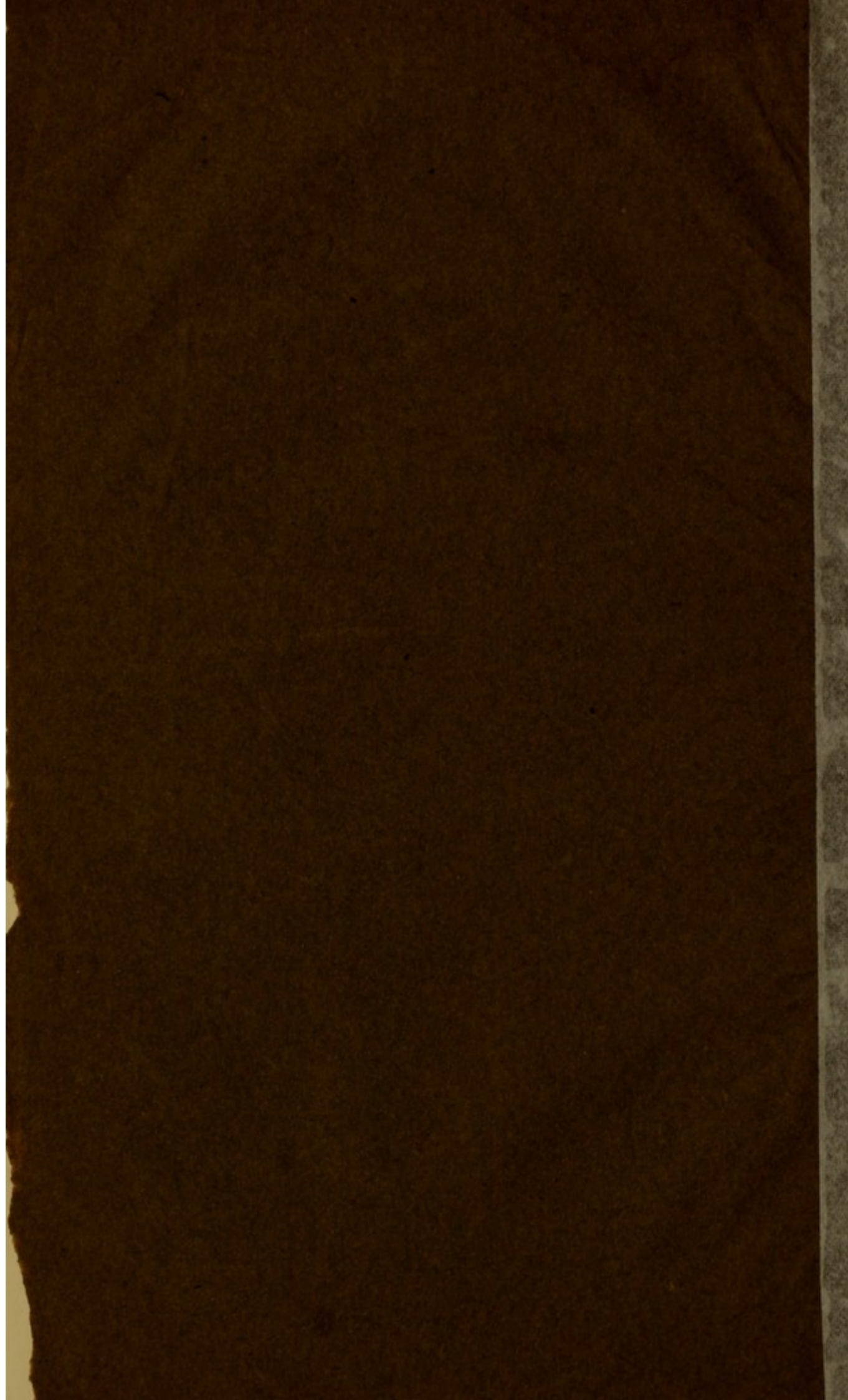














A  
PROBATIONARY ESSAY  
ON  
**NECROSIS;**

SUBMITTED,  
BY THE AUTHORITY OF THE PRESIDENT AND HIS COUNCIL,  
TO THE EXAMINATION OF THE  
**Royal College of Surgeons of Edinburgh,**  
WHEN CANDIDATE  
FOR ADMISSION INTO THEIR BODY,  
IN CONFORMITY TO THEIR REGULATIONS RESPECTING THE  
ADMISSION OF ORDINARY FELLOWS.

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BY  
**JAMES SYME,**  
MEMBER OF THE ROYAL COLLEGE OF SURGEONS OF LONDON.

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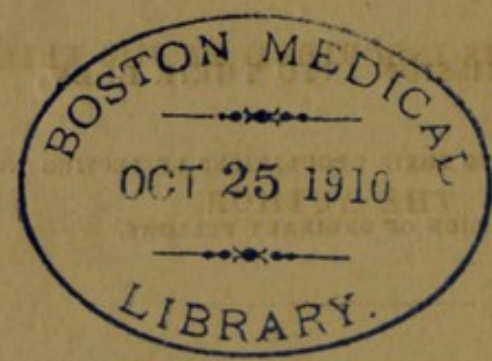
THEORY OF  
NECROSIS

BY THE AUTHOR OF THE THEORY OF THE COUNCIL

IN THE REVENUE OF THE

TO ROBERT LESTER, F.R.S.

WHEN COMPLETED



JAMES SYMM

JUNE 1853

EDINBURGH

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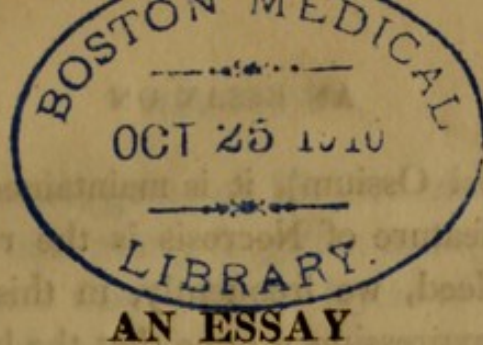
TO ROBERT LISTON, ESQ. SURGEON,

THIS ESSAY IS INSCRIBED BY HIS FRIEND,

THE AUTHOR.







ON

## NECROSIS.

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**S**URGICAL Writers have made many and very different applications of the term NECROSIS. LOUIS first used it to express the death of a bone throughout its whole thickness; DAVID, again, understood by it a process in the animal economy during which a bone died, and was replaced by a substitute; while WEIDMAN, perhaps the greatest and most complete authority on the subject, though little known, owing to the extreme scarcity, not only of his original work, but also its French translation, contends that neither of these definitions is sufficiently comprehensive. “Verum cum non semper, si aliqua ossa pars emortua et absoluta est, novum aliquod os succrescat, morbus tamen non indole, sed vel modo vel gradu tamen diversus sit, patet, sic necrosin angustioribus, quam fas est, limitibus præclusum fuisse. Vera demum necrosis semper est, si aliquod ossis ramentum, in quo vis vita extincta est, abscessit, vel proxime abscessurum est.”

Mr SAM. COOPER\* expresses his belief that all surgeons will agree with WEIDMAN as to the propriety of implying by this term nothing more than the death of bone. In the works, however, of Mr RUSSELL, Dr MACDONALD, and Mr ALLAN, (all of which are posterior to the excellent treatise of WEID-

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\* Surgical Dictionary, Art. Necrosis.



MAN, De Necrosi Ossium), it is maintained, that the great distinguishing feature of Necrosis is the reproduction of a substitute. Indeed, we frequently, in this country at least, hear surgeons expressing a hope that the bones of their patients are *necrosing*, and styling this a salutary process.

Though the ancients must have seen, and do indeed seem to have noticed the disease, now generally known by the name of Necrosis, professional men were first induced to consider it with care, by the recital of some remarkable cases published by LA MOTTE, RUYSCH, DUHAMEL, MOREAU, &c., in which the shaft of a bone was separated from the system in whole or greater part, without causing much lameness or inconvenience.

Researches in the vaults of the dead, and museums of the curious, soon seemed to throw much light upon these very extraordinary facts, by the discovery of many bones in which the process of reproduction appeared to have been going on at the time of death or amputation, since they consisted of a cylindrical osseous shell or case, containing the old shaft, which could be heard rattling on the wall of its prison, or even seen through the numerous openings by which it was perforated.

And it having been thus satisfactorily ascertained that it was possible for a bone to die and be regenerated, ingenuity was immediately employed to investigate the dark and mysterious process, by which so wonderful a change was effected.

The common idea, so keenly advocated by DUHAMEL, that bone in a healthy state is formed by its investing membrane or periosteum, naturally led to the idea that this organ was the instrument in the process of reproduction; and, if any doubts remained, the laborious, ingenious, and faithfully detailed experiments of TROJA, seemed altogether conclusive as to the truth of this opinion.

The explanation, indeed, given by DUHAMEL, of osseous regeneration, is somewhat different from that offered by



TROJA. The former, going on his favourite, and once celebrated, although now nearly forgotten analogy between bone and wood, supposed that the inner layers of periosteum were constantly being changed into an osseous, as were those of the inner bark into a ligneous state, and therefore concluded that nothing more was necessary for the formation of a new shell than the separation of the periosteum, in consequence of disease or accident. TROJA, on the contrary, believed that the layers of periosteum were not converted into bone, but merely suffered a great distention or separation, in consequence of osseous matter being effused between them.

But whatever might be the ideas entertained regarding the precise manner in which reproduction was performed by the periosteum, all surgeons who considered this membrane as the agent of that process, very naturally inferred, that the amount of destruction in the bone must always be indicated by the extent of swelling in the periosteum.

Thus the death of an entire bone, or shaft of a bone, came to be considered as a very common occurrence. For every surgeon could not but frequently meet with cases of general and hard deep-seated enlargement of limbs, which, after a tedious course, at last discharged some fragments of bone, and ultimately regained a nearly natural appearance. Now, in all of these instances, it was believed that the whole shaft died, and was reproduced by the periosteum.

For this new and remarkable disease it was deemed necessary to appropriate a distinguishing title, and Necrosis was accordingly adopted, as implying general or total death, while the old-established term, Exfoliation, still remained in use, to express partial destruction. It is true that the very rare actual appearance of an entire dead shaft seemed at first an objection to the argument just stated; but this difficulty was got rid of by attributing its removal to the pus and absorbent vessels. Such is the reasoning which it would appear has led to the definition of Necrosis given by LOUIS, DAVID, and other early writers; and that they should have



so reasoned is not very wonderful, considering the imperfect notions then acquired concerning the nature of the bones.

Now, however, that our knowledge on this subject has received such additions from the labours of HALLER, SCARPA, BICHAT, RICHERAND, &c., an obstinate adherence to these obsolete opinions can hardly be excused ; and in this opinion I am sure every zealous surgeon will concur.

LEVEILLE', in his very excellent treatise *Sur les Necroses*, thus eloquently and indignantly expresses similar sentiments :

“ Je le demanderai à ces vils détracteurs titrés, dont l'entêtement invincible demontre l'ignorance profonde, dans la science de leur profession ; je le demanderai à ces chirurgiens qui n'ont de places superieures que parcequ'ils ont beaucoup manié le couteau et la scie ; je le demanderai à ces hommes, qui n'ont aucune education première, qui ne sont susceptibles d'aucune instruction : peuvent-ils faire autorité ?

“ Leur ton d'assurance est-il capable d'en imposer ? Ecoutez les parler, discuter ; les fautes de Français sortent de leur bouche, comme la lave est projetée d'un volcan embrasé. Ils s'expliquent à tort et à travers ; ils *protégent* des élèves qui les abandonnent vite, lorsque leur instruction est avancée, et leur crasse ignorance reste à decouvert. C'est à ces fléaux de la chirurgie qu'ils peuvent rendre barbare, que je demande s'il est permis de reconnoître aujourd'hui dans la texture des os, une organisation lamellée, si l'on ose professer cette doctrine dans les leçons de physiologie ? Est-il reçu, dans ce moment que le perioste forme les os, que cette membrane s'ossifie dans les cas de maladie ? Prononce-t-on affirmativement sur les usages particuliers de cette enveloppe ? Si ces barbares connoissoient le Latin, je les renverrais à l'ouvrage de SCARPA ; s'ils lisoient passablement le Français, s'ils pouvoient entrer dans leur tête deux idées de suite qu'ils fussent capables de combiner, je les renverrais aux ouvrages de BICHAT, de RICHERAND, et à tous ceux qui ont paru depuis. Que peut-on esperer ? Ils n'offrent aucune ressource pour le bien, et les places qu'ils occupent les met-



tent dans le cas de faire le plus grand mal, de tuer la chirurgie, qui est aujourd'hui sans défenseur \*."

A complete revolution, indeed, has been established in public opinion, as to what regards the structure and actions of bones. The strange idea that the shafts of cylindrical, and the plates of tabular bones, consist of lamina or foliated layers, is now happily abandoned; and the fallacious arguments by which it was formerly supported are clearly exposed. Anatomists now make the due and necessary distinction between the decomposing and developing effects of heat, moisture, and disease. They allow that the action of fire, or of the weather, does occasionally give dense bones a scaly appearance, (though hardly to the extraordinary degree represented by GAGLIARDUS;) but they regard this change as owing altogether to the effect of decomposition; and they view it in the same light as they do the scaly oxidation of iron, the crusting of baked bread, the exfoliation of glass in a state of decomposition, or the well-known onion-like disintegration of stone, so frequently to be noticed in the fences of this country. They know well that bones in the living body frequently exfoliate, or cast off scales; but they do not forget that those which do so, expose an extensive surface to the operation of external causes, the cranial and shin bones, for example; while their museums contain abundant evidence that fragments detached from bones in other situations, as necrosed lower jaw, or unhealthy stumps, display every variety in external conformation.

In answer to the analogical reasoning of DUHAMEL, they hardly give themselves the trouble of entering upon any formal argument, believing that the experiments of that illustrious, though in this point certainly misguided philosopher, must afford a refutation sufficiently satisfactory to all, who with unbiassed minds take the trouble of studying them. So far from formally refuting his arguments, found-

\* Mémoires de Physiologie et de Chirurgie Pratique, par J. B. F. Leveillé. Paris, 1804, p. 265.



upon the tinging property of madder, they endeavour to forget, from regard to excellence in other respects, the force of prejudice which could blind a great man, and lead him into the absurdity of attributing the appearance of many successive red and white strata in bones, to the occasional intermixture of madder with the food of animals, at various intervals of days, and even weeks, when he himself had fully ascertained, and explicitly stated, that a few days were quite sufficient for either the complete tinging of a healthy bone, or the regaining of its natural colour.

They are willing to pass over in silence, the folly of believing that the formation of new layers proceeds with a rapidity which would very soon render the bones altogether disproportioned to the animal whose skeleton they constitute. They feel ashamed in being obliged to point out the distinction between wood, the organ of vegetable circulation, and bone, the lever of animal locomotion, or to explain that wood is laminated, because, being unable to perform, any more than the other instruments of organized beings, the important duty entrusted to it beyond a certain time, it requires a substitute, which is accordingly formed year by year from the inner bark, an organ similar in function and structure,—while there being no internal absorbents in plants for the removal of unserviceable parts, the old wood is necessarily piled up, layer upon layer, in the centre of the tree. It is only when compelled to do so by some reference to DUMAMEL's opinions as incontrovertible, that they appeal to these or any other of the many conclusive objections within their reach.

The tinging of bones by means of madder, is unquestionably a most important fact: and the rapid alternations of colour from white to red, and red to white, which attend its use and discontinuance, are well worthy of attention, in as much as they afford satisfactory proof that the bones are supplied, like other organised parts, with internal exhalents and



absorbents, which are continually employed in changing their component particles \*.

We no longer look upon the bones as dead unorganised effusions of earthy matter, similar to the enamel of the teeth, or as, in any respect, resembling the cuticle and nails, which, so far from possessing an organised structure of their own, are mere secretions from vascular subjacent organs, are capable of no action, and susceptible of no disease.

On the contrary, we thankfully avail ourselves of the researches of HALLER, of SCARPA, and of many other illustrious men, who have satisfactorily ascertained, and fully proved, that the structure of bone differs in no essential respect from that belonging to other organised tissues which compose the body; that it consists of a vascular cellular basis, containing earthy matter in its interstices; and that it is similar in every part of the skeleton.

Would it have been consistent with the simplicity of nature to have constructed one part of a bone reticulated, and the remainder foliated? Certainly not; and reason might have led us to infer what synthesis and analysis have so positively ascertained, that the minute structure of spongy and dense bone is the same.

Now that the membranes have been so carefully studied, and are comparatively so well understood, anatomists can

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\* Although the particles of matter submit for a while to the control which life exerts over the laws of chemistry, they sooner or later obey their natural affinities, and quit the organised system which they assisted to form. Hence the necessity of a constant supply of food even to adult organised beings. It would also appear that the rapidity with which the particles circulate corresponds directly with the vigour of health. Hence sick people are able to endure for days, and even weeks, a degree of abstinence which would speedily prove fatal to those, the functions of whose bodies are performed with activity. In disease there seems to be nearly a total stagnation of the particles which compose the body, as little emaciation can be discovered during the progress of a fever, while the first effect of convalescence is to produce it very remarkably, — a circumstance no doubt owing to the rapid departure of exhausted matter, in consequence of the absorbents suddenly regaining their healthy powers.



perceive no peculiarity which distinguishes the periosteum from the other fibrous coverings of organised parts.

They know that it agrees with them in affording protection against violence, and in assisting the establishment of connections with surrounding parts, but can discover nothing in its structure more than in that of any other, to qualify it for the secretion of the organ which it envelops.

The medullary membrane, or internal periosteum as it used to be called, ought, as it would appear, to be regarded rather as belonging to the marrow than the bone. But the decision of this question is of little consequence; and it is of more importance to know that innumerable branches of the nutritious artery which is ramified over this membrane, pass from its external surface to the bone.

The marrow, a substance so well known, yet the subject of such various opinion, differs in no essential respect from fat. It is no doubt more fluid, and of lower specific gravity than that generally distributed over the body, and, above all, than that which is to be found in the palms of the hands and soles of the feet; but there are other situations in which the difference is not so remarkable, I should rather say in which it is hardly distinguishable; as, for example, the soft adipose covering of the kidney and neighbouring viscera, or the nearly fluid substance which accommodates the round special cord to its triangular bony canal. On the whole, it may be said that the density of fat corresponds to the pressure to which it is subjected; thus it is firmest in the feet, and most fluid in the cavities of bones.

These considerations being kept in view, little reflection is necessary to determine the function of the marrow. I shall not use any arguments to prove that it cannot pass into the joints, there to constitute synovia,—that it can have no effect in preventing brittleness, or rendering the bones more tough,—and least of all shall I attempt to reason against the idea that it nourishes or feeds them. Instead of doing so, it will be better to state at once the object which may, with most



probability, be assigned as that which has induced nature to place it in the bones; and this I believe to be simply the plan of uniting strength with lightness, on which she seems to have proceeded regularly in contriving the skeleton.

Having constructed the bones of the extremities after a cylindrical fashion, which every one knows is more favourable to strength than that of a solid rod, she was under the necessity of filling their central cavities somehow or other; and being unable to make use of air for this purpose, except in some of the bones of birds, she has chosen the animal fluid, which stands next in order as to specific gravity; and if the marrow is subservient to any other function than that of filling up space like its congener fat, I believe that it is one also possessed in common by all the species of this substance, I mean giving support to blood-vessels.

Minute injection shows that the vessels of bone, before entering its proper substance, are minutely ramified over its external and internal surfaces; wherefore it may be naturally concluded, that these parts are most freely supplied with blood, while our acquaintance with the principles of arterial distribution, independently of the evidence afforded by disease, would lead us to imagine that the two sets of vessels meet together, and inosculate towards the centre of the bone.

If, then, the constant and insensible renovation of bone be effected by means of its own vessels, why should its occasional and obvious reproduction be still attributed to any other source? For no reason that I know of, unless it be the too common preference of marvellous to natural explanations.

Since the entire shaft is believed by many people to perish before the formation of the new one is commenced, it necessarily follows, according to their opinion, that the soft parts must afford the materials of regeneration; and, therefore, the periosteum, as being in the neighbourhood, is generally fixed upon as the one most immediately concerned.



Mr Russell, in his very excellent treatise, has used many ingenious arguments to prove the periosteum unworthy of the credit he at one time so generally allowed it, of secreting bone; but, as he also believes, that the death of the entire shaft is essential to the constitution of necrosis, or rather reproduction, he has attributed the process of ossification to all the soft parts of the limb indiscriminately.

Now that the bones are admitted to hold that rank in the scale of organization to which their structure and actions so well entitle them, their reproduction has been explained in a manner different from the one already mentioned, and certainly more agreeable to our present notions of their nature.

The bones themselves are now looked upon by many as the instruments of their own reunion when fractured, and reparation when injured by accident or disease.

But here a difficulty has occurred. For according to this view of the matter, the whole of a shaft can never be reproduced, since a portion must always remain to constitute the germ or rudiment of its successor. Hence the supporters of this opinion have been under the necessity of denying, as LEVEILLE has done, that the loss of an entire shaft ever was supplied either in man, or the lower animals, notwithstanding the confident assertions of TROJA, KOELLER, MACDONALD, &c. Or they object, as Dr KNOX has done, to all reasoning transferred from observations made on the brutes, to human pathology; an opinion with which I feel as little inclined to agree, as with another not unfrequently advanced, that all experiments on the lower animals are unjustifiable. For my own part, I can see little more cruelty or injustice in sacrificing animals inferior to ourselves in compliance with the demands of our mental, than in doing so at the instigation of our bodily appetites; and even granting that no consideration, but the latter, can excuse the taking of life, I still think that there is no harm in such pathological experiments, since the knowledge which results



tends directly to the welfare and preservation of our bodies. I therefore feel no shame in confessing, that I have made many experiments on various kinds of animals, from a wish to satisfy myself of the precise changes and appearances which have been so faithfully detailed. And my labour has not been in vain. For it was only after repeating some of the well-known operations of TROJA and his followers, that I felt able to form clear ideas on the subject of the present Essay.

I shall conclude these general remarks, and proceed to inquire more particularly into the Reproduction of bone; an expression much more convenient than the one commonly used to denote the same thing, I mean Necrosis, a term which invariably and necessarily leads to confusion, unless when strictly confined to its etymological meaning.

The osseous matter which serves for the union of fractured bones, and supplies the place of those that die in consequence of accident or disease, must, it is very evident, proceed either from the bone itself, or from the neighbouring organs; and much ingenious argument has been exercised in endeavours to ascertain from which of these sources it is in truth afforded.

DUHAMEL, TROJA, MACDONALD, RUSSELL, and Mr ALLAN, believe that the soft parts perform this important duty; but as the opinions of these gentlemen are distinguished from each other by certain individual peculiarities, I shall consider them one by one.

DUHAMEL supposed, that when the periosteum was separated from a bone, it speedily increased in thickness, and became converted into an osseous shell.

His reasons for thinking so rested chiefly upon an assumed analogy between wood and bone, not only as to structure, but also as to formation; whence he conceived, that the layers of periosteum suffering a constant successive and regular ossification during health, must of necessity form the case in question whenever they were prevented from applying themselves to the bone.



FOUGEROUX made some experiments with the view of confirming this theory, but very few writers on Necrosis have adopted it. Indeed, the fallacy of all DUHAMEL's osteological reasoning was so obvious, that his cotemporaries, however great their respect might otherwise be for the Philosophus Octogenarius, as some of them call him, could not but perceive it, and distrust the explanation of reproduction to which it served as the basis.

Having already, at some length, discussed the opinions of DUHAMEL, I shall not now recur to them, but proceed to the consideration of TROJA's theory, which met with more numerous supporters, and which still maintains its ground, pretty generally at least in this country.

TROJA found, that in no long time after he removed the marrow from pigeons' legs, and stuffed the cavity with cotton, a new bone was formed, surrounding the old one, like a cylindrical case or shell, and separated from it by a thin but distinct membrane. From this solitary fact he concluded, that the bone having died in consequence of injury inflicted on the marrow and medullary membrane, served as an irritation to the periosteum, between whose layers ossific matter was in consequence deposited, so as to constitute an osseous shell; the external covering of which was formed by one layer of the old periosteum, while its internal or medullary membrane was represented by the other.

Instead of waiting until he had performed those experiments, as yet unrivalled, whether we consider their number, or the time, labour, ingenuity, and expense, necessarily bestowed on them, before he came to any determination, TROJA commenced by forming an opinion, which plainly cast a strong shade on the light afforded by his after researches, and prevented him from making that progress towards the truth, so well merited by his genius and assiduity. In fact, the very first results obtained from the experiments which he instituted, with a view of ascertaining the steps of the re-



markable change he had discovered, were sufficient to disprove his early-formed conjectures, and rendered him, notwithstanding the force of prepossession, suspicious, or rather certain, as he himself tells us, that the membrane separating the old and new bones was not a part of the original periosteum, but ought rather to be looked upon as a new formation. In tracing the effects which followed the destruction of the marrow, he observed, that a quantity of lymph was effused in the first instance between the bone and periosteum,—that this gradually increased in thickness, assumed greater consistence, acquired the appearance of cartilage, and at last became converted into bone.

Now he found that until this final change was fully perfected, no appearance of internal membrane could be discovered. No wonder, therefore, that he says, "*Ejus absentia, aut melius intima curia et confusio cum gelatina primis temporibus, et differentiae, quas inter ipsam et periosteum observabam, ancipitem me reddebant et forsan certum fuisse de integro regeneratam* \*."

Instead, however, of altering his opinion in conformity with the result of his observations, and regarding the internal membrane as a new formation, he adhered to his first conjecture, and excused himself for doing so by relating several cases in which the new membrane entirely resembled the old periosteum: "*Hi characteres eam pro vera interna periostii lamina fecerunt agnoscere*."

In the course of his experiments many curious facts were ascertained. Thus, for example, he found, that, when he removed the external periosteum, the bone died, and was replaced by another in the situation formerly occupied by the marrow. This change he believed to be precisely similar to the one already described, as following injury of the marrow; the new bone being deposited between the layers

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\* *TROJA de Novorum Ossium Regeneratione*, p. 62.



of the medullary membrane, instead of the periosteum. He also noticed, that, occasionally, after he had destroyed the marrow, the bone did not die, and yet had its central cavity much contracted. In this case also, he supposed that the internal periosteum was chiefly concerned.

The opinions of TROJA were advanced and supported with such a weight of imposing facts, and had indeed so great an air of truth and sincerity about them, that little wonder need be felt at the celebrity which they speedily acquired and still retain. BLUMENBACH and KOELER made many repetitions and variations of his experiments, with the effect of satisfying themselves, and of establishing still more firmly the theory of TROJA.

In the year 1799, Dr MACDONALD published a Thesis here, in which he related many experiments that he had performed on the bones of the lower animals, and endeavoured to overturn the theory of TROJA, because it appeared to him that the explanation of ossification given by that laborious investigator did not agree well with the result of his observations.

In examining the progress of reproduction, Dr MACDONALD noticed that the interjacent membrane, which ultimately separated the old and new bones, could not possibly proceed from the original periosteum, since in the early stages it plainly appeared, that the basis of the future new bone was effused below the inner surface of the periosteum; and that, when the change was watched from beginning to end, it could easily be seen that the internal lining of the new bone, so far from being formed previously to its commencement (as TROJA believed) did not in truth exist until it attained perfection. The appearances in favour of this opinion, which might be discovered by an attentive examination of the changes occurring during ossification, were so decisive, that Dr MACDONALD took for granted that they must have been unknown to TROJA and his followers. "Idcirco sus-



picor viros claros non satis accurate tradidisse, quo tempore hanc internum membranum observarint \*."

But if Dr MACDONALD ever read TROJA's book with care, he must, while writing this opinion, have forgotten the passage quoted a little way above, in which, talking of the new membrane, he thus expresses himself: "Its absence, or rather intimate union, and confusion with, the gelatinous effusion poured out at an early period, and the differences which are to be observed then, between it and the periosteum, rendered me doubtful, I should rather say certain, it was a new formation."

It certainly was not owing to ignorance, carelessness, or want of observation, that TROJA maintained an erroneous idea, but rather in consequence of unfortunate prepossession in favour of a particular notion rashly adopted.

Dr MACDONALD himself seems to have been under the influence of prejudice; for, instead of drawing the conclusions which ought naturally to have followed the results of his experiments, he contented himself with shewing, that new bone was not formed between the layers of the periosteum, as TROJA imagined: and, instead of attributing its formation to the source so obviously determined by the facts which he had ascertained, could not divest himself of the idea that the periosteum must be the organ of reproduction. He therefore believed, that, although the new shell was formed neither by the conversion of the layers of the periosteum into osseous laminæ (as DUHAMEL imagined,) nor by the effusion of ossific matter between them (as TROJA supposed) it was nevertheless produced by the periosteum, and in the following manner:

The first step towards the formation of new bone, he conceived to be the death of the old shaft throughout its whole thickness. For he is careful in making a distinction between exfoliation, whether external or internal, and Necrosis.

The periosteum irritated by the presence of the dead bone,

\* Macdonald's Thesis de Necrosi et Callo. Edin. 1799.



which might now be looked upon as a foreign substance, poured out, he imagined, on its internal surface a thin gelatinous effusion, which gradually increased in quantity and consistence, so as at length to represent cartilage, or rather the cartilaginous basis of bone; (for here also he, not without reason, makes a distinction); and, finally, by the addition of earthy matter, became converted into an osseous shell surrounding the old shaft, and separated from it by the membrane which TROJA regarded as belonging originally to the old periosteum, but which he had proved to be a new formation; cotemporaneous with the completion of the ossifying process.

Yet it could not escape his notice, that, until the membrane was formed, the new bone adhered inseparably to the old; and his attention seems to have been particularly excited by the observation of numerous radiated fibres, distinguished by their superior hardness, which, shooting out from the surface of the old shaft, penetrated the surrounding soft mass. The appearance, he has often and very carefully mentioned; at the same time expressing his surprise at its presence, and inability to explain why the fibres should be radiated and not longitudinal.

These facts, it might reasonably have been imagined, were sufficient to shake his opinion as to the share of ossification performed by the periosteum. They ought indeed to have satisfied him, that the osseous shell was derived, not from it, but the old bone; and it is difficult to see how he could refrain from adopting this idea, when he found that the periosteum could all along be separated not only easily, but with more than usual facility from the new bone, while it again could not be detached even at the earliest date of its appearance by any degree of violence from the old shaft.

Was it, I may ask, at all probable that newly formed parts should adhere so firmly to others, not only dead, but acting as foreign bodies in exciting irritation? Certainly not; and yet Dr MACDONALD retained his old opinion, that the bone



died before its substitute was reproduced, and argued in support of it, that when animals, in which the process of reproduction was going on, were obliged \* to swallow madder mixed with their food, the old bone never acquired the red tinge; whereas the new one shewed it from the very first period of its acquiring earthy matter, and asks triumphantly, "*Nonne hinc apparet, os vetus prorsus emortuum esse?*"

To this question I readily answer, Certainly not necessarily, for the bone may naturally enough be regarded as in a diseased state, in consequence of the liberties taken with its inner surface, though not actually dead, and, as I before stated, the circulation of particles composing morbid organs being much slower than during health, or rather as it would appear nearly arrested, it is easy to see why the diseased shaft does not acquire a red colour; since this change can happen only when the constituent particles are circulated with activity.

The only other opinion regarding the manner in which the periosteum forms new bone, is that of Mr ALLAN, and is thus unfolded at the 23d page of the second volume of his *System of Surgery*: "I have paid considerable attention to this disease in all its stages, and am convinced that the bone, so far as it is concerned in it, is merely passive. Necrosis is not preceded, as is generally imagined, by inflammation and swelling of the bone; but the inflammation is confined entirely to the periosteum. In consequence of external injury, in particular constitutions, or from a less obvious internal cause, the periosteum inflames and swells. This inflammation terminates in suppuration, which causes that membrane to separate from the bone. The nutritious artery, and the other vascular connections, are consequently destroyed; and

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\* All experimenters, in particular DUHAMEL, have observed, that animals shew the utmost repugnance to food mixed with madder, and not without reason; since it was regularly noticed, that, besides impairing their health in general, it rendered the bones soft and brittle: this pernicious effect might be offered as one objection to Mr LIZARS's very novel plan of giving madder for the cure of rickets. See his *Pathological Observations on the Bones*. Edin. 1823.



the marrow and internal periosteum being insufficient to support its vitality, the bone dies."

"As soon as the bone is separated from the periosteum, it begins to die. While it dies, but long before it parts from the sound epiphysis, the internal surface of the periosteum, which is now highly vascular, and in which there is considerable action, shoots out granulations that rapidly increase both in number and size. These granulations are not soft, like those of ulcers of the soft parts, but are firm and hard like the gum. They form an organized surface, which by its absorbent powers assists in removing the dead bone, and which soon begins to ossify."

The explanation here given of the manner in which Necrosis commences, agrees very well with the antiquated ideas of DUHAMEL and DAVID; but the description of the process by which the periosteum performs reproduction, belongs exclusively to Mr ALLAN. He alone, of all the writers on Necrosis that I am acquainted with, mentions the occurrence of granulations on the periosteum, *firm and hard like the gum*; and it is to be regretted that he has not made known the source from which he obtained information of a fact so extraordinary, and which has escaped all other investigators of this subject.

After what has been said of the observations made by Dr MACDONALD, and the numerous other experimenters who have added to our knowledge since the time of DUHAMEL, it will be unnecessary, I should imagine, to enter at length into any argument in refutation of Mr ALLAN's theory. It is evidently altogether incompatible with the phenomena afforded by disease, whether natural or artificial, but more particularly so with the close adherence of new bone to the old, which could hardly happen if a quantity of pus was interposed between them.

Mr RUSSELL, in his well-known and very excellent treatise on Necrosis, proves satisfactorily that new bone can be formed independently of the periosteum; and gives it as his opinion, that this membrane has little or nothing to do at any



time in the process of ossification, which he imagines is performed by the general mass of soft parts surrounding the bone, whose complete and entire death he considers essential to the constitution of Necrosis.

But Mr RUSSELL does not believe that the death of a bone is essential to the commencement of reproduction. On the contrary, he supposes that if the shaft which is about to be expelled from the system, does not remain in full possession of its powers during the gradual preparation of its substitute, it at least retains a certain portion of vitality, which he thinks is stimulated to the utmost. In one case, indeed, he particularly describes the existence of violent inflammation in the sequestrum, as proved by the extraordinary vascularity which it displayed when injected with size. The first step towards the formation of a new bone, is, according to his ideas, a thick and general effusion of a soft pulpy mass all about the bone, proceeding, however, neither from it, nor the periosteum, but from all the soft parts in general of the limb, stimulated to increased action by the same widely extended attack of inflammation, that is ultimately to destroy the bone.

To shew that the periosteum is not the agent of ossification, he remarks that new bone is often formed where this membrane does not exist, as in the union of compound fractures, and the reparation of injuries inflicted intentionally or accidentally, so as to cause its removal. He has also well noticed, that the substitute bone is much more rugged on its surface, and unequal in its thickness than it ought to be, if moulded between the two layers of a membrane.

On the other hand, by way of proving that the soft parts of a limb, independently of the periosteum, are adequate to the duty he assigns them, he appeals to the ossification, or rather earthy degeneration, so frequently to be noticed in almost all the animal tissues, and particularizes, as examples, the bony concretions, as he calls them, that are so often met with in the arteries, glands, scirrhus, and steatomatous tumours. But surely these unorganized unformed masses are



not to be confounded with real vascular active bone, and the appearance of the one is certainly no evidence of power to produce the other.

Mr RUSSELL concludes his arguments on the subject thus :  
“ Thus, from a due consideration of every circumstance, it appears, that the pulpy mass which extends from one portion of the bone to the other, and is itself at last converted into bone, is entirely a new creation, and has no dependence upon the original bone, or its periosteum.”

Now, however much I may feel inclined to agree with Mr RUSSELL in his opinions, that inflammation of a bone always precedes its death, that the periosteum has nothing to do in ossification, and though I think that the treatment of the disease, which he recommends, is exceedingly judicious, and deserving of the approbation bestowed upon it by the profession, I must here express my decided objection to any such conclusion ; and this for many reasons, of which I may at present mention only two. In the *first* place, if the periosteum does not afford the osseous matter, new bone, if formed by the soft parts, can never become contiguous, or rather adherent to the old. In the *second* place, all analogy is unfavourable to the supposition, that the vessels of muscle, fat, artery, or, in fine, any organ but bone can form bone, since we have no other example of a vascular tissue secreted by one different from itself.

Having thus discussed the various explanations which have been given of osseous reproduction by those who attribute this process to the periosteum, or other soft parts, I may remark, in general, that they are all liable to one grand objection, besides the many lesser charges of inconsistency and improbability already noticed. It is, that they all demand belief in complete and general death of the shaft, while we know that this separation of such a sequestrum very rarely, if ever, occurs in cases of human Necrosis. This difficulty has not escaped attention, but, on the contrary, has given much trouble to the supporters of osseous regeneration



being performed by any parts except the bones themselves. It was long confidently affirmed, that the pus and absorbents have the power of consuming and removing dead bone ; but this assertion is now as rarely made as believed. The pus has long been looked upon as a very harmless inactive fluid ; and Mr RUSSELL is, as far as I know, the latest author who believes in its possessing corrosive and solvent properties. As to the effect formerly, and still by some supposed to be exerted, by the absorbent vessels on dead bone, there are many very strong objections. We never, for instance, see sloughs of the soft parts removed by absorption ; even clots of blood remain altogether unchanged for years. PELLETAN mentions examples of their doing so within the peritoneal sac for twenty years and more. Portions, in the same way, of wood, leather, cloth, and such substances, lie within the body unacted upon for any length of time. Finally, we have daily proof that fragments of human bone, when once fairly detached from their connections, will remain without change for ever, if not removed or expelled. Every surgeon must have seen how much mischief is occasioned by the smallest splinter left after amputation, how effectually a sequestrum resulting from a compound fracture prevents union, in fine, how long such dead portions, or osseous sloughs, will remain within bones, when no means are employed for their removal. Dr WHATELY mentions instances of their doing so three, six, even eleven years ; and, in our collection, besides many specimens of the same sort, there is one bone which contains a sequestrum, that, in all probability, must have been exposed to the pus and absorbents for five-and-twenty years. From all of which considerations, I think it may be very safely concluded, that bone, when once separated from the system, suffers little or no change of shape and dimension. Consequently, the non-appearance of an entire dead shaft completely disproves the existence of such a sequestrum.

It is difficult to decide who should have most credit for establishing the theory of reproduction, which I have next



to examine. BRUGNONE and PENCHIENATI, the editors of BERTRANDI'S Work on Surgery, published it first in the year 1787. LEVEILLE', fully satisfied of his own originality, expressed similar ideas at greater length in the *Mémoires de la Société Médicale d'Emulation*, 1802, while RICHERAND, in his *Nosographie Chirurgicale*, tom. iii., 1808, details them still more diffusely, and at the same time declares his high satisfaction at finding LEVEILLE'S sentiments completely in accordance with his own.

The distinguished men whose names have just been mentioned regard the process of reproduction in very nearly the same light. The bone, they maintain, does not die from starvation, in consequence of being abandoned by the periosteum, but rather falls a victim to its own overactions, precisely as the soft parts do when inflamed, and therefore rarely or never perishes throughout any more than other organs do when attacked by inflammation, but sacrifices a part for the whole, and regains its health, after losing a portion of its substance, greater or smaller, according to the extent of the disease. The generation of an entire shaft is therefore an operation which they contend Nature never has to perform. They conceive, that when a portion of bone dies, whether in consequence of inflammation or external violence, the sound and healthy remnant swells out so as to compensate for the diminution of strength which attends its loss. Of course, when some internal portion alone of a bone is destroyed, while its external wall remains alive, the sequestrum cannot escape entire, and, being proof against the pus and absorbents, must continue in its place, keeping up, as they imagine, a constant irritation, the effect of which is, a general development of the osseous tissue, and consequent enlargement of the bone throughout. It is plain, that if this theory of reproduction be accurate, a portion of the old bone must be left in every case of Necrosis followed by regeneration of a new shell, whereas complete destruction of the entire shaft was considered as an inseparable attendant, or ra-



ther essential precursor of the same process, by those who assigned it to the soft parts. LEVEILLE', indeed, not only denies that any perfect human sequestra were ever renewed, but even distrusts the positive assurances of TROJA and his followers, that the tibias of pigeons and other animals are completely destroyed and reproduced after certain injurious operations are performed on the marrow and medullary membrane. He accounts for these remarkable relations by referring them to the prejudiced observation of their authors, who, having a theory to support, might, as he alleges, be suspected of believing, and therefore describing more than they actually saw. But in this sentiment I cannot agree with Dr LEVEILLE'; for TROJA, more than perhaps any man who ever published the results of an experimental inquiry, seems to have divested himself of all prepossession when examining and describing appearances. Hence, indeed, the value of his work, which, as a magazine of facts, is altogether invaluable. It was in drawing general conclusions, as already observed, that TROJA seems to have erred; for here, it would appear, he was all along guided by early formed and peculiar ideas.

Leaving the farther and more serious discussion of the question as to possibility of entire renovation to a future and more favourable opportunity, I cannot help here remarking the great obligations we are under to the editors of BERTRANDI, to LEVEILLE', and to RICHERAND, for throwing so much light on the history of Necrosis, for clearly referring the death of bones to its true source, and for explaining their reproduction in a manner consistent with the enlightened views of their nature, which we owe to the invaluable labours of HALLER, SCARPA, BICHAT, &c. Being delivered (thanks to the assistance of these great men) from the shackles which so long repressed investigation, and conducted so far towards the attainment of truth, little difficulty remains in overcoming the obstacles still to be encountered in explaining all the cir-



cumstances that occur during the process of osseous regeneration.

Dr KNOX has lately, through the medium of the Edinburgh Medical and Surgical Journal \*, explained in English this theory, with that ability which might be expected from a gentleman of such well-known talent. I cannot help remarking, however, that, considering the importance of the subject, he has hardly been sufficiently explicit. He tells us, that at the time he framed the opinions published in the essays already quoted, the general conclusion from all the works on Necrosis he had read was, that the principal agent employed by Nature in the formation of new bone was periosteum †.

From this it would appear that he had not read the works of HALLER, SCARPA, BICHAT, LEVEILLE', BERTRANDI, RICHERAND, or BELL, and has therefore the merit, though not the honour, of originality.

In describing the steps of the process by which a bone repairs the loss of a sequestrum involving only part of its thickness, Dr KNOX agrees entirely with Dr LEVEILLE', and indeed uses nearly similar expressions; but when the whole thickness is concerned, he believes that, instead of the bone swelling out, as LEVEILLE' supposed it to do in every case of reproduction, granulations arise on the surface round the sequestrum, and gradually become converted into bone ‡.

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\* January 1822, and April 1823.

† "Some of the works written on Necrosis and regenerated bone I know only by report, and at second-hand; but the general conclusion from all has uniformly seemed to be, that the principal agent employed by Nature in the formation of new bone, is the periosteum; at least this was the opinion of all the practical surgeons (and they are not few) with whom I have had an opportunity of conversing on the subject."

‡ "The manner in which new osseous matter shoots from old bone, appears to me sufficiently simple. The vessels supplying the remaining healthy old bone, whether proceeding to it from the periosteum, surrounding soft parts, or otherwise, become increased in size, and perhaps in number; granulations arise on the surface, which, by degrees, becoming firm, are afterwards converted into bone."



This description of the particular process of reproduction agrees so nearly with BICHAT's opinion \*, that I am surprised that Dr KNOX has expressed himself so disrespectfully of that very eminent anatomist and physiologist †. Indeed, considering BICHAT's genius, enthusiasm, and unlimited opportunities of study, under his partial and illustrious master DESAULT, he certainly does not appear a proper subject for the imputation of professional ignorance.

Before proceeding to the elucidation of my own opinions regarding the manner in which new bone is formed in cases of Necrosis, I think it proper to say, that I must distinctly disclaim the honour of originality, since the great principle on which they are founded was published long ago by Mr CHARLES BELL, in a Note to his brother's History of the Bones, in their System of Anatomy ‡.

The bones, as I have already said, are constantly undergoing a change of their constituent particles, by means of exhalent and absorbent vessels, the presence of which is proved by many observations.

The changes in colour produced by the use and discontinuance of madder, are unquestionably amongst the most important of these; but there is also another very well-known fact in their history that appears to be equally conclusive. I allude to the remarkable alterations of shape suffered by

\* " Rien de plus facile, d'après ce que a été dit jusqu'ici sur la nutrition osseuse, que de concevoir la formation du cal. On sait qu'elle presente trois periodes; 1. Le developpement des bourgeons charnus; 2. Leur transformation en cartilage; 3. Le changement de ce cartilage en os." Anatomie Generale, t. iii.

† " Hence we are necessitated to conclude, that being but little acquainted with the subject, he resorted to those sweeping analogies, which form at once the merits and defects of his writings."

‡ " In the experiments and observations which I have made, neither the periosteum nor marrow seemed to have formed the bone; and I conclude that nothing but bone can form bone, by the continuation of the natural actions; and that in the case of Necrosis the old bone inflames and begins the new formation, before the continued irritation in the centre kills it." Vol. i. page 33. 3d edition.



bones at different periods of life, and placed under different circumstances.

We are not unfrequently told, that the projections which render the bones of old animals so much more rugged than those of young individuals belonging to the same species, are caused by the action of the muscles, which, pressing on those parts only corresponding to their bellies, leave their point of attachment undiminished, and therefore, to appearance, prominent. As examples of this change, we see the skulls of old bears, tigers, and other animals, provided with strong masticatory muscles, contrasted with those of their youthful progeny. But every person must at once perceive the fallacy of this opinion; for, if processes were the result of pressure, what would become of the young bear's brain? And if the peculiar shape of a seal's head be owing to muscular action, how does it happen that the skull of one unborn in general character resembles that of the mother?

The formation of processes ought to be looked upon merely as an example of the beautiful relation so constantly established between all parts of the body. If the osseous muscular, or any other tissue, was perfected before the others, then it might be imagined that the other parts were suited to its accommodation. But we know, on the contrary, that the foundation of all the organs is laid at once, in conformity with a certain plan appropriate to each species, and contrived by Nature, who is too good an architect to be guilty of any disproportion standing in need of subsequent alteration.

Thus the humerus and humeral muscles of a foetal mole are equally remarkable for strength, and the bone, even then, is more strongly marked than that of an old dog. When the animal betakes itself to the habits of its kind, when it begins to use its thoracic extremities in digging, then, indeed, a conspicuous change may be discovered in the processus, which become infinitely larger; but it must be recollected, that at the same time the muscles, and, in short, the limbs throughout, acquire additional and corresponding strength.



An excellent illustration of the principles on which processes are formed, is afforded by the dissection of old dislocations which have never been reduced. In these cases the muscles acquire new attachments, and the bone shoots out into corresponding projections.

All the diseased actions of bone, (in other words, all those which alter the appearance and properties of their tissue), may be referred either to increase or alteration in the operation of the exhalents and absorbents.

When the absorbent system of vessels exert stronger action than what is consistent with that of their antagonists, an ulcer is the consequence. It is very rare that we find idiopathic ulceration in bone. This change happens more frequently in consequence of some local irritation, such as an aneurism or other large tumour stimulating the absorbents to increased activity, in order that accommodation may be afforded to the new and unusual neighbour. Yet in these cases the action of the vessels being merely increased, without being in any way altered, nothing more is necessary for the reparation of the ulcer than the removal of its exciting cause. I mean the tumour or whatever else pressed upon the bone.

Very different is the state of matters when the absorbents have their natural actions altered and depraved, as happens unfortunately so frequently in consequence of general or local irritation in all, but particularly young individuals, whose habit of body is weak.

The ulceration here, is of a most foul, obstinate and intractable kind. It persists long after its exciting cause has disappeared. It yields to no remedy, except removal of the bone in which it is situated. In short, it is that baneful disease termed caries.

I have often been asked what is the distinction between simple ulceration and caries; how we are to distinguish a disease which requires no treatment from one that demands the most active practice? When an ulcer in a bone shews no symptom of amendment, after every circumstance concerned



in its production has been removed, and the discharge is particularly dark, thin and foetid; in fine, when there is reason to suspect a general or local weakness of the patient, we may safely conclude that the ulceration is carious. It is not possible to discover, by examination of the bone in a fresh state, what is the nature of the ulceration, because in every case the appearance presented resembles very much that of half-dissolved loaf-sugar; but after maceration the difference is sufficiently obvious and distinct. It may then be seen that the simply ulcerated bone looks as if its cells had been merely exposed to view, while, on the contrary, that which has been the seat of caries appears as if it had been exposed to the action of fire, being hard, brittle, and of a perfectly pure white colour.

The various alterations produced in bones, by excited or perverted action of their exhalent vessels, are more numerous, and not less important than those depending on an unnatural state of the absorbents.

The most gentle degree of their excitement beyond the natural standard of health, seems to be that which affects the growth of bone. The first step of this process, as every surgeon knows, is an effusion round the edges of what is called the point or centre of ossification, of a gelatinous substance, which gradually acquires the consistence of cartilage, and is then transformed into bone by the addition of earthy matter deposited at first sparingly, and in such a way as to give the newly ossified surface a reticulated appearance, afterwards more plentifully, so as to render it solid.

Some people, though unable to deny that the cylindrical and spongy bones grow in the way just described, nevertheless maintain that those of a tabular kind are produced by the ossification of a double membrane. Thus, they attribute the existence of an internal and external table in the skull to the conversion of dura mater and pericranium into bone. But nature makes no exception in the present instance to her general rule of attaining the same ends by the same means; for there can be no doubt that the effusion of gelatine and



cartilage between these membranes is regularly antecedent to the growth of the bones which lie between them.

The degree of excitement which causes the effects just described, exists naturally in young and growing animals, whose organs of nutrition perform their functions with greater vigour than those of grown individuals. But the same sort of action is induced not unfrequently in adults, by constitutional or local irritations.

When arising from the former it occasions nodes, and perhaps exostosis. As a consequence of the latter, it gives birth to certain important results, which I have now more particularly to consider.

When a bone is broken across, without any wound of the soft parts, or displacement of its two portions, a gelatinous effusion is quickly poured out upon the fractured surfaces, so as to establish an union between them, as is particularly well described by TROJA. This connection is, of course, at first very weak, but gradually acquires strength as the interjacent substance becomes firm, cartilaginous, and at last bony. If the broken ends are not retained in situ, then the process is of necessity more difficult and extensive; but it still remains the same in kind. From the extremities of the bone a great effusion of cartilage ensues, so as to stretch from one portion to the other. In this soft and yielding bed earthy matter is gradually deposited, and perfect solid bone at length results.

This was termed callus by the ancients, and still retains the name. The extent of its effusion was believed to depend, in a great measure, upon chance, and to be regulated only by pressure. But surgeons now know, that the sure means of preventing *redundance* or *exuberance* of the *callus*, is to effect and maintain an accurate adaptation of the fractured bones; since it is plain that the more nearly they are in opposition, the less occasion will nature have for new osseous matter in re-establishing their connection. They therefore study the connections of the bones with each other, and their moving powers, confirm their reasoning by lessons afforded



from practice, and are thus enabled to determine on the best means of ensuring adjustment, which can always be effected by retaining the injured limb in a proper position, without any local pressure over the fracture. I say retaining the limb ; for as to trusting mere position without splints and bandages, I must express my most unqualified disapprobation. During the first week or ten days after the accident, there is a constant disposition in the limb to starting ; moreover, the patient is unaccustomed to retain it in one attitude, and its position at all times, and especially during sleep, is apt to be changed. In addition to these circumstances, so strongly tending to produce displacement, may also be mentioned the difficulty experienced in obviating the deranging effect exerted by the weight of the limb itself, and the impossibility of preventing, by any attention to position, the muscles from contracting when the fracture is oblique. It is true, that after a time the patient becomes habituated to the circumstances in which he is placed, and feels no difficulty in abstaining from motion. Even the starting of the muscles ceases ; but then all the mischief which can arise from want of rest must have already actually happened ; the great secret of treating fractures so as to prevent distortion, being to watch carefully over the primary or cartilaginous union. For it is the bed or mould of the future new bone ; it is formed immediately after the injury ; and must evidently, when completed, prevent any disjunction, except when great violence is employed. I am aware that bones will perform this process long after the period at which it should naturally happen has passed, as indeed we may learn from the facts recorded by those who imagine that the injuries of some organs are allowed to remain for days and even weeks without any effort being made by the system at reparation. I believe that a bone will unite though prevented from doing so for *twelve* days ; but I also believe that this time is sufficient for the completion of osseous union in many cases, especially those of young subjects. At all events, it is more time than requisite for the



primary or cartilaginous junction ; and I should also wish to suggest, that patient and long suffering as the bones would appear to be, they occasionally becoming hardened under misfortune and bad usage, form false joints, to the patient's irretrievable loss, and the practitioner's everlasting disgrace.

When the growth of bone is once established, by means of the irritation of a fracture, the new action is not confined to the surfaces occasioned by the fracture ; it extends to the internal cavity, which is speedily obliterated at its newly formed orifices, by osseous matter effused from the inner surface of its cylindrical wall. The purpose intended by Nature in thus shutting up the unnatural openings which lead to the medullary cavity, is probably not so much to defend the fluid there contained, as to render the bone stronger at a part otherwise so much weakened. When the process of re-union is completed, and when the bone joining the extremities, as well as that filling up their central cavity, is perfected, then, indeed, the shaft is more than usually strong at the place where it was fractured. But it does not long continue to be so ; for the absorbents soon remove the redundant matter, restore the canal, and render the bone altogether similar to its original condition.

An action of the same sort is probably concerned in the formation of exostosis, which may in general be traced to some local injury or cause of irritation, and in that of the irregular, knobbed projections, so frequently noticed on the surface of bones in whose neighbourhood there happens to be any organ unusually active.

Reproduction of bone, also, I have no doubt, is effected, in the majority of cases, after the same manner, and, indeed, it might naturally have been suspected that the means employed for the restoration would resemble those of their original formation. I simply state this opinion at present, leaving its particular consideration and defence to a more convenient opportunity.

When the action becomes more decidedly inflammatory,



that is, attended with pain and general fever, instead of having new bone effused on the surface of the old one, we find a general development or opening out of the osseous tissue, whose structure thus becomes much more apparent, and tells us plainly that it is not laminated, but reticulated.

This change, however, can be discovered only after careful and complete maceration. While the bone is still fresh, its newly formed cells are filled up with gelatinous matter, which conceals them from sight, and is indeed, after a time, probably converted into bone, so as to give that remarkable weight and density so familiar to those who are conversant with osteological pathology.

SCARPA, RICHERAND, LEVEILLE', and others, believe that this morbid change is very common, and that it occurs in all cases of external exfoliation, where they imagine that the sound external portion swells out, so as to form a case sufficiently strong for preserving due strength in the limb.

From all that I have seen and read, however, I am inclined to doubt this, and rather to suppose that the more mild and less unnatural process already described is most frequently employed.

When the vessels of bone are roused to an action still more intense, they not only secrete osseous matter, but also pour out that peculiar fluid named pus.

I shall not here make any attempt to decide whether this matter is simply effused by the extreme vessels, or is really a secretion from glands extemporaneously formed; neither shall I endeavour to explain its use, or why it is always present along with granulations. It is sufficient for my present purpose to know that bone, like every other organised tissue of the body, is capable of repairing its injuries by the process of granulation, although it seems very unwilling to employ it except when unavoidable. We not unfrequently see it effect the union of fractures complicated with external suppurating wounds, as, in these cases, the presence of pus between the broken ends prevents their union from being es-



established in the simple manner formerly described, while the high excitation of the soft parts calls forth their sympathy, and disposes them to enter into a corresponding action.

It is of great consequence that surgeons should be aware of this; for it is possible that an idea astonishingly common may otherwise lead them to the commission of serious errors. It happens sometimes that the objectionable precepts of great authorities are more adhered to than their really judicious advices. Thus, while Mr JOHN BELL's excellent principles are forgotten and unheeded, his unfortunate persuasion that manual compression was insufficient to restrain hæmorrhage during amputation, is pleaded as excuse for still persisting in the use of that inconvenient, and frequently prejudicial, instrument the tourniquet; and while the same gentleman's well-merited ridicule, and reprobation, of poultices in the treatment of compound fractures, are studiously kept out of sight, his distrust in the necessity of splints is as carefully published. So also PERCIVAL POTT's unlucky assertion, that no attempt at union is commenced in compound fractures until they cease to be so, that is, until the soft parts are healed, is more keenly maintained than any of the correct and useful opinions contained in his excellent observations.

That POTT should have inculcated this idea is to be regretted; but that it should still be retained is truly wonderful, considering the demonstration of its falsity afforded by every museum.

Indeed, what can be more clearly decisive as to the possibility of bones uniting by granulation, than the occurrence of ankylosis in joints from which there is a constant flow of matter? When the knee or hip-joint of a young person is opened, the surfaces of the bone are not unfrequently in much the same state as that of a common abscess, and occasionally unite together.

Now, it is very odd that this granulating state of a bone has been, and is very often confounded with caries. We often hear surgeons talking of the natural cure of this truly in-



curable disease, and appealing, in support of this opinion, to the ankylosis which sometimes results from white swelling, but more frequently from the morbus coxarius. It is difficult to conceive actions more distinct than the two here confounded. In the one case, the bone has the appearance formerly described as characterizing caries, in the other it displays neither a healthy nor unhealthy cellular structure, being porous and warty instead of cancellated on the surface.

When the nutritious vessels have their actions perverted, they occasion many very serious, very dissimilar, and, too frequently, very unmanageable diseases in the bones. Brittleness, rickets, and mollities ossium all result from irregular and imperfect deposition of the gelatinous and earthy constituents of osseous matter, while osteo-sarcoma \*, fungous growths, and porcelaineous looking alterations of the articulating surfaces proceed from the secretion of substances foreign to healthy bone, and characteristic of disease.

Whenever the vessels are either excited to more violent action than what is consistent with their strength, or worn out by long continued exertion, they cease to perform the function which belongs to them, and then of course the bone dies. The same circumstance happens not unfrequently from fractures, especially those caused by fire-arms, as every military surgeon must very well know. But since, without bones, the limbs of animals would be altogether useless, Nature very carefully provides for the reparation of their injuries; and it is the process which she employs for their reproduction that I shall now explain, according to the ideas that appear to me most correct.

Analogy leads us to believe that the bones are subject to

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\* It is certainly strange enough that such dissimilar affections, as mollities ossium and osteo-sarcoma should ever be confounded. "Mollities ossium, or osteo-sarcoma, called also mala costeon, is that disease wherein the bones become so soft that they may be twisted or bent in any direction, and in which, being deprived of their earthy property, as if they had been macerated in diluted muriatic acid, their animal constituent only remains."—LIZARS' *Pathological Observations*.



inflammation ; but the same sort of reasoning strictly forbids our supposing that the result of this action will be in general destructive of their entire substance. Sphacelus of the soft parts is an exceedingly common effect of their excited action, but almost never extends to every part of the organ in which it occurs. We ought therefore to expect (what experience has discovered,) that complete bones seldom if ever die at once in consequence of inflammation.

When the shell does not die through its whole thickness, the sequestrum is named an exfoliation, and may, of course, be either external or internal. When it is internal, SCARPA, RICHERAND, LEVEILLE', and others, imagine that the remaining healthy bone swells out so as to compensate for the diminution in its thickness \*. The observations concerning human Necrosis which I have read, as related by others, and the information which I have obtained from experiments on the lower animals, induce me to doubt the truth of this opinion, and to believe rather that the old bone is not opened out and expanded, but that new osseous matter is poured out upon its surface in the first instance, so as to give the increase of thickness, and that it is only after the absorbents have regained a healthy action that the new and old portions of bone become so similar as to appear homogeneous.

That this is the process of reproduction in cases of internal exfoliation in birds, I have satisfactorily ascertained by experiment, and still preserve specimens to shew, that in these animals the old bone is not thickened, nor indeed in any way altered, if we except the exfoliation from one surface, and effusion of osseous matter from the other. I can easily understand how the deception here has arisen ; for as the process of decay and reproduction are performed with great rapidity in the lower animals, unless the changes be examined early they will not be understood. Thus, though

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\* " The external layers of the old bone become vascular, spongy, and full of holes, for the transmission of vessels ; new osseous matter is deposited in the interstices ; the whole bone becomes much thickened, and occasionally heavy and compact."—Dr KNOX, *Edinburgh Medical and Surgical Journal*.



the healthy remnant of an old shaft be easily distinguishable from the new effusion while being formed, it ceases to be so very soon after it is completed. Indeed, the whole osseous substance becomes so similar throughout, as to be very readily mistaken for the old shaft, opened out by inflammation, and rendered dense by the infiltration, so to speak, of bony matter.

Internal exfoliations being thus repaired by osseous effusions from the external surface, it might naturally enough be conjectured, that the same circumstances should attend external exfoliation; and we accordingly find, that in this very common disease the internal cavities of bones are obliterated or diminished by an effusion from the inner surface of their walls. TROJA describes this change very distinctly. It is quite similar to that which occurs in fractures, and is probably employed by Nature, here also, for the immediate restoration of strength; and the new bone is just as much of a temporary nature, as the internal callus which is formed after fractures, and like it will of course be removed as soon as the cylinder has regained due strength, by having the cavity answering to the sequestrum filled up.

Here then is a beautiful example of the simplicity so invariably observed by Nature in all her operations. Exfoliations are always attended with effusion of new bone from the surface opposite to that in which they occur; and here we perceive another advantage in the cylindrical structure, besides affording greater strength with the same materials, since it very much facilitates the reparation of injuries to which the bones that possess it are, of course, more exposed than any others of the skeleton from being placed in the extremities, where they must frequently suffer in consequence of accidents and other external causes.

Such then being the manner after which superficial internal and external exfoliations are repaired, let us next consider what happens when the whole thickness of the cylinder is concerned.



In this case, RICHERAND and others talk of the remaining portion becoming so much developed, as to fill up the vacant space. LEVEILLE' describes a soft and gelatinous state of its surface when undergoing this change; and Dr KNOX again speaks of new osseous matter shooting from the sound edges. But these explanations, which are all essentially the same in principle, appear to me decidedly imperfect, inasmuch as they do not account for the facts, that sequestra, evidently consisting of the whole circumference and thickness of a bone, are often discharged without any diminution of length being suffered by the limb to which they belong, and that, previous to their separation, new osseous shells are formed around them. For my part, I cannot be persuaded that the function of a living bone is ever performed by a dead one, or that Nature would ever shut up a dead bone that must ultimately be removed from the body in a complete osseous case.

On some occasions, we know that the entire cylinder dies at once, and before a substitute is found to supply its place, in consequence, for example, of violence or very intense inflammation. But is the course of things here the same as in the common cases we meet with, where sequestra representing an entire portion of the cylinder are expelled? Does the limb retain its due length and strength? Is there hardness and swelling throughout its whole extent? And does the patient ultimately regain its use? On the contrary, are not the external symptoms of reproduction in these cases, confined to the living extremities of the bone? Does not a separation soon occur between the sequestrum and its sound attachments, just as happens in common cases, but with the melancholy difference, that fracture and distortion necessarily happen from a want of due strength in the limb, which is not provided with a substitute bone?

It would appear, indeed, that the process of reproduction in cases where the bone dies suddenly, differs very much from that employed in common cases of Necrosis.



In those cases where destruction happens quickly in consequence of violence, or very intense inflammation, I believe that reproduction, if effected at all, must proceed from osseous matter thrown out by the healthy remnant of bone, and in this way we know that fractures are repaired. But whenever the bone dies a lingering death, owing to continued irritation or moderate inflammation, I have no doubt that the substitute is formed, at least commenced, by the old shaft before it ceases to live.

We know that the invariable effect of inflammation occurring in any soft organized part is a swelling of its substance in consequence of a lymphatic effusion taking place into its cellular interstices. But in dense bones such an occurrence cannot attend the commencement of inflammation, owing to the unyielding nature of their tissue preventing any sudden alteration of structure. Are we then to suppose, that, contrary to what happens in all other organs, inflammation of bone is not attended with any effusion? Certainly not; for although, as I have just said, lymph cannot be deposited in the cellular interstice of dense bone, it may nevertheless be thrown out upon their surface; and is always to be found there in recently inflamed bones.

If inflammation does not run high, and soon subsides, the effusion which attends it in no long time disappears, and the parts concerned, whether osseous or of any other kind, speedily return to their original condition. But if the excited action be increased, or long kept up, then the effused substance becomes organized, and is converted into a tissue bearing the characters of that from which it proceeded. The necessary result of this circumstance occurring in a soft part will be a general and permanent enlargement; while the same thing happening in a bone, will provide it with an osseous case.

The inflammation often goes on after the effusion has become organized, and does not terminate until it has destroy-



ed a portion of the organ affected : this is termed slough or sequestrum, according to the kind of tissue which suffers.

Now, here we see the providence of Nature ; for by the time the sequestrum separates, there is always a sufficient substitute ready to supply its place ; external, if the exfoliation be internal, and *vice versa*. If the whole thickness of the cylinder is concerned, there will still be in general an adequate support for the limb ; but not so certainly as when a scale merely is thrown off : hence we frequently observe great distortion in consequence of the extensive Necrosis to which children are subjected.

In these subjects, I believe the whole shafts of bones are often renovated, although but a few dead fragments are discharged ; and in saying this, I am aware that the charge of inconsistency may be imputed to me, (p. 25.) ; but there is a wide difference between the death of a bone at once and entire, from its destruction piece by piece and at different times by repeated attacks of inflammation. It is easy to understand how, in such cases, a great portion of the old shaft may be removed by the absorbents before it ceases to live, and therefore still within their reach. Other parts, again, dying constitute sequestra, which, when once formed, must either be discharged through vacancies in the new shell (named cloacæ) be extracted by the surgeon, or remain keeping up irritation, and promoting further mischief.

The treatment of Necrosis may be comprehended in the four following rules :

1. When pain and general fever indicate the existence of inflammation in the bone, antiphlogistic remedies must be employed.
2. When the active stage is passed, and pus is profusely discharged, the patient's strength must be kept up by nourishing diet and country air.
3. When the sequestra are ascertained to be loose, they ought to be extracted.



4. When distortion is threatened, owing to the great and sudden destruction of bone, splints and other means of prevention must be assiduously employed.

FINIS.



