AN APPEAL

TO

PHYSIOLOGISTS AND THE PRESS:

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BY

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FELLOW OF THE KING AND QUEEN'S COLLEGE OF PHYSICIANS IN IRELAND; PHYSICIAN TO DR. STEEVENS' HOSPITAL, DUBLIN; LECTURER ON THE PRACTICE OF PHYSIC AND ON CLINICAL MEDICINE IN STEEVENS' HOSPITAL SCHOOL OF MEDICINE, ETC.

DUBLIN:
FANNIN AND COMPANY, 41, GRAFTON STREET.

1862.

R. D. WEBB AND SON,] [PRINTERS, DUBLIN.

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PREFACE.

It being my anxious desire to avoid, as far as possible, inconveniencing physiologists in the perusal of the following statements, I have studied to obviate, to the utmost of my power, the necessity of perplexing them with long and numerous quotations.

I have submitted merely a short epitome or outline of my views upon one or two of the questions I had contemplated, and that from the motive which I state; but being far from desirous of, in this way, obtruding my own views upon physiologists, I have not only confined myself to those topics, but in relation to them have introduced only such few and short quotations from my former publications as I felt to be indispensable as a verification of what I advance.

If any Scientific Society would do me the honour to investigate these statements, I should feel much honoured in being permitted to submit a copy of each publication referred to.

AN APPEAL,

&c.

As a writer on physiology, who feels himself to be a deeply injured man, I earnestly solicit from physiologists a hearing.

I appeal to their sense of justice but to grant I may be heard, while I detail an oppressive injustice which—alone and unaided—I am utterly powerless to resist.

As an author I am solitary and alone in the world, being unacquainted with a single physiologist.

Retiring in habits and known to but few, I have no one, connected however remotely with the press, to whom I could look for assistance.

An Irishman unknown to an individual editor, I could not ask a British journal to befriend me; and residing in a country where physiological science is comparatively little cultivated—and consequently comparatively little valued—I have no means but that now adopted of appealing to physiologists to decide whether or not I have been treated with injustice.

I ask but to be heard; and I confide in that sense of justice which is a national characteristic of Englishmen that—though an Irishman and friendless—a *hearing* will not be refused me while calmly and in the absence of irritation or excitement I submit a simple ungarnished statement of facts.

In the year 1848 I published a volume on organization in which I ventured to advocate original views upon several topics of leading physiological interest. Those views were pronounced at the time by *The British and Foreign Review* to be, for the most part, little more than—simply an expression in abstract terms of what others had worked out in the concrete form. The result—as was to be anticipated—was, that no one opened my volume; and neither then nor since has the slightest notice been *anywhere* taken of the propositions of leading importance I then ventured to advocate.

Views in many important particulars identical with several of those submitted by me in 1848 have recently (1861) been advocated by distinguished physiologists, while I have *everywhere* been excluded from the *slightest* participation therein.

My object in this appeal to physiologists is twofold, namely, first to ascertain if possible *the cause* of this exclusion; and, secondly, to submit that cause—if ascertained—to the judgment of physiologists and of the press, and to solicit their decision as to whether or not it be *just*.

As an example of the topics referred to, I shall in the first instance submit the following:

One of the motives which chiefly induced me to publish in 1848, was to submit to the consideration of physiologists views *altogether my own* as to the nature of the phenomena which are *essentially* associated with the manifestation of life. The result I had reached on this question may be briefly expressed in these words, namely—That for the *organization* or formation of *one* organic entity there is necessity for a simultaneous *disorganization* or decay of *another;* so that during the manifestation of vegetative life *both* these processes—namely, that of formation and of decay—are of necessity *simultaneously* in operation.

But it is necessary that I should submit the precise words I employed in 1848.

I had been contemplating the manifestation of life by what I termed "the simplest conceivable vegetation," while that vegetation, in conjunction with *inorganic* elements, was giving manifestation to vital phenomena. The two entities consequently under consideration at the time were—the simplest vegetation (viz. an *organic* structure) and *inorganic* elements. As to the *effects*—during the manifestation of life—upon the constituents of the former of these two entities (viz. an *organic* structure) I observed, "the final result must be that these constituents will eventually be reduced to the condition of inanimate chemical compounds," (p. 42) while the effects—during the manifestation of life—upon the latter entity (viz. *inorganic* elements), I observed, "must obviously be to convert them into an uniformly organized mass," (p. 43) and I summed up the subject with the following general statement, viz.: "thus are two essentially distinct and opposite processes of necessity concerned in producing the phenomena of active life; are of necessity in operation for the production of what we imply when we say of a thing, 'it *lives;*' and thus, too, becomes apparent how death is essentially a part of life."—*Freke on Organization* (1848), p. 45.

As this may be said to have been among the questions of leading importance which I ventured to investigate in 1848, I shall confine my observations for the present chiefly to it. My volume of 1848 was unread; as, from the cause I have stated, I had fully anticipated. The entire impression—with the exception of some dozen copies or so—was returned to me by my publisher.

In 1851–52 I somewhat expanded my views, and republished them in *The Dublin Medical Press.* Among other topics I introduced the suggestion referred to, and reiterated it some dozen or more times, illustrating my meaning at great length by its application to the formation of muscular fibre, &c. But it was useless; the impression had gone abroad that I was a pretender to "novelty," and but an expresser in abstract terms of what others had worked out in the concrete form. Again no one looked at my writings; not the slightest notice then or since was *anywhere* taken either of the suggestion referred to, or any other proposition I had ventured to advocate.

Disheartened and depressed, I resolved to abandon physiological pursuits, and determined never to publish on the subject again.

The mere accident of a coincidence between Mr. Darwin and myself upon a question in Natural History induced me, after the lapse of so many years, once more (in 1861) to republish my views, in the hope that while the mind of physiologists was alive to that subject, attention might possibly be directed towards my writings.

This hope was ill founded. I soon found in operation against the possibility of such being the case, an opposition so actively carried out, and so widely and variously directed—extending even to the distance of Madras —that it was utterly beyond my power to resist it.

I know nothing of the source from whence that opposition has emanated, but I know of its *existence* and *somewhat* of its extent and ramifications. My object however not being the crimination of others, but—after a silence of years—the protection of my own rights, I should prefer—so far as consists with what is due to myself—being silent on the subject. But lest the reader should be doubtful of the correctness of what I state, it is essential that I should submit at least one instance—out of many—as evidence that such opposition exists, and I do so with the less reluctance, inasmuch as the introduction of the instance referred to has by circumstances been rendered unavoidable.

Three months or so after the publication of my volume on "The Origin of Species," a volume in which the suggestion I have referred to was perhaps a dozen times repeated—was illustrated by its application to the formation of muscular fibre—was three times illustrated in a diagram facing the title page of my volume, so that it would have been scarcely possible to have opened that volume to ascertain its title without the attention being attracted to that very proposition—three months, I say, after the publication of that volume—in a Royal Institution which had declined to do me the honour of acknowledging its receipt—a gentleman of distinguished physiological attainments enunciates that very proposition, and does so manifestly under the impression that it was *then* enunciated *for the first time.* I say manifestly under that impression, inasmuch as that gentleman premised its enunciation with the words, "I would venture" to say—*venture* to say! in 1861 what I had ventured to say in 1848, and was then told by the leading physiological journal in England, that I had done little more than "simply expressed in abstract terms what others have worked out in the concrete form." I address myself to Englishmen, proverbial for justice, and ask them if this be not evidence of opposition from *some* source; and be that source *what it may*, to me is it not an oppressive injustice?—and yet it is not a tithe of what is in my possession to detail.

I shall presently introduce *quotations* from that gentleman's published writings and my own to verify what I here state, but I am desirous in the first instance to submit to the reader a general statement of facts, in order that he may be enabled with the greater facility to form an impartial decision upon the whole.

My motive, as I have observed in publishing in 1848, was to submit to physiologists views upon questions of fundamental importance, which had originated *exclusively and alone* with myself, and which—whether correct or erroneous—were (as I believe) *entirely* "novel" at that time; and it is this question as to the *novelty* or *otherwise* of those views in 1848 I now earnestly solicit physiologists to decide. I shall in the first place confine myself to the proposition referred to, and in the sequel introduce several others. I shall repeat that proposition, which is as follows, namely—

That for the *organization* or formation of *one* organic entity there is necessity for a simultaneous *disorganization* or decay of another; so that during the manifestation of vegetative life *both* these processes—namely, that of formation and of decay—are of necessity *simultaneously* in operation.

I repeat that proposition for the purpose of observing that it is not upon its *truth* or its *error* I now ask a decision; it may or may not be accepted. It is not to pronounce upon its *soundness* or otherwise I now venture to address physiologists; it is simply to solicit their voice as to its "*novelty*" or otherwise in the year 1848. It is simply to appeal to physiologists if they are aware of there being upon record—antecedently to November, 1848—any "hints or more formally expressed doctrines" which they consider could justly be regarded as suggestive of that proposition; or if they be of opinion that that proposition—whether correct or erroneous—was but the expression in abstract terms of what others had worked out in the concrete form antecedently to November, 1848.

Before directing attention to any of the other propositions contained in my volume of 1848, it will be necessary here to say a word upon its fate.

Aware that in my publication of 1848 I was venturing to give expression—upon topics of leading importance—to views which in many points did not coincide with those entertained by the distinguished conductor (at that time) of the *British and Foreign Review*, I put forward those views *with the utmost possible diffidence*. No man could feel a higher respect than I did for *any* opinion upon a physiological question entertained by that eminent physiologist; and I repeat it, when I ventured to express opinions which did not coincide with that gentleman's, I did so with the utmost possible diffidence, expressing that diffidence in my preface in the very strongest terms that suggested themselves to my mind at the time.

It can be conceived then with what feelings of anxiety and nervous excitement I looked forward to the reception my publication might meet with from the *British and Foreign Review*.

Secluded in habits, with none to consult, without the advice or even knowledge of any one, I had ventured to submit a suggestion *upon the most important question in physiology*, which was at variance with the views entertained on the subject by the leading physiological journal in England. What would be the result? Would that journal review me? How would it dispose of my suggestion as to the nature of the phenomena *essential* to the manifestation of life—the topic of leading importance in my volume? Would it annihilate my pretensions upon this and other questions? I confess I had my apprehensions it would. But I confess—whether I was justified in so feeling or not—that I certainly did anticipate *some allusion* to that proposition, and probably some critical investigation of its merits; and although I was not without apprehensions that the result would be the overthrow of my views, I felt assured that, should such be the case, it would be done in a kindly and generous spirit. In fact I had allowed myself—perhaps unjustly—to anticipate an analysis of my views; and, whether vanquished or otherwise, a friendly reception.

Such were my expectations at the time. I have since felt that perhaps I had no right to anticipate so much; but it matters not now.

That journal did review me, and did annihilate my pretensions, *but not in the manner I had apprehended.*

It would be difficult to express the *astonishment* with which I first read that review. It commenced with a complimentary expression, for which I felt at first much indebted; but as it proceeded, it rendered its compliment valueless, for although without assuming the appearance of being unkindly disposed, *it robbed me of everything to which I attached the slightest value.*

It was "*novelty*" I had laid claim to, and of "*novelty*" it deprived me, and in depriving me of *this* it deprived me of everything I cared for—of everything for which I had come before the public. If not "novel," my publication was *contemptible*, and its author not only contemptible but *dishonest.* The review made no allusion whatever to the proposition of leading importance in my volume; it made no allusion to *any* proposition but one which it represented as "hypothetic," and that without stating in what way. This surprised me not a little, and disappointed me greatly. But I am neither an idiot nor a madman, and, whatever may have been my expectations or hopes, I am neither so presumptuous or insane as to suppose that an author is at liberty to dictate to a journal to what portions of his writings it shall or shall not direct attention, or how it shall dispose of what it selects; and had the reviewer proceeded no further with his criticism, I should have felt myself *indebted* for his notice. Nay, more, had he taken exception to every proposition I advanced, without cruelly misrepresenting the *general character* of my publication in that for which alone it was published, and in *the very particular* upon which it *specially relied for support*, I should have felt complimented at being noticed in that journal. But it did not stop here. It proceeded to inflict on me the deepest wound it is possible for an author to receive who has ventured to put forward original views. I *knew* that my volume—be its errors and defects what they may —was as *original* as any publication that had ever emanated from the press; and I felt indignant as well as astonished that an anonymous writer should acquaint the readers of such a journal as the *British and Foreign Review*, that "the apparent novelty of his views in most instances results from his having simply expressed in abstract terms what others have worked out in the concrete form."

Having informed his readers that "in most instances" I had "simply expressed in abstract terms what others have worked out in the concrete form," without affording the slightest clue as to the *nature* either of my abstract or of the concrete of others, the reviewer proceeds to insinuate in very clear terms that the author had declined "to acknowledge his obligations to the various physiological writers by whose hints or more formally expressed doctrines he has obviously profited largely," affording not the slightest insight, except in one instance which I trust to convince the reader is *unjust*, as to the nature either of obligations or hints.

I ask the reader, *supposing* my publication to have been "novel," as I state, could anything have been a greater injustice than this? That the contents of my volume *were* "novel" in 1848, I trust to *prove* from the writings of eminent physiologists in 1861. I would further ask, could any complimentary remarks of a general nature—however flattering they might be—compensate for statements such as these, *if* my volume were in reality *original?* The complimentary remarks contained in that review—for which under other circumstances I should have felt myself deeply indebted—*thus associated*, I submit, could to the author be nothing but valueless.

The following is a copy of the review:—

"We now turn to Dr. Freke's 'Suggestions for the Construction of an Organic Atomic Theory,' in which also we find much that is deserving of attention, though less of novelty, perhaps, than the author imagines. The book consists of three chapters: the first on *inanimate creation;* the second on *animate creation;* the third on *the blood.* Upon the first chapter we need make no other remark, than that it embodies those views of the actions of inorganic matter which are made by the author to serve as the foundation of his subsequent speculations. We shall perhaps best exhibit his ideas of vital action, by quoting his explanation of the term *organizing atom*, which conveys the fundamental idea of his system :—

"'What we design to convey is, that as evidence supports the opinion that at the creation of the inanimate universe an omnipotent fiat called mineral matter into being, in the form of minute indivisible particles called mineral atoms; which mineral atoms, though limited in the number of their species, constitute, by the operation of the attributes impressed on them at their creation, the countless multitude of distinct forms of mineral matter found in the inanimate world, as also the harmonious arrangement now recognisable throughout inorganic creation—so it will be our effort to adduce evidence in support of the opinion, that at the creation of the world of organization the same Divine power called organized matter into being, in the form of minute indivisible (that is, physiologically indivisible) particles, which we would call organizing atoms—which organizing atoms, though in all probability limited in the number of their species, have constituted, by the operation of the attributes impressed on them at their creation, that vast aggregate of distinct forms of organized matter which decorate with living loveliness an animated world, as also the harmony of arrangement visible throughout all organic creation. Each mineral atom is simple, that is, composed of but a single species of mineral matter. Each organizing atom is, we conceive, compound, that is, composed of a (it may be an inconceivably minute) combination of several distinct species of mineral matter, to which combination, at the atom's creation, was imparted the organizing agency to constitute it an organizing atom. This agency, by antagonising with the specific attributes of the atom's component constituents, retains them in a relative position distinct from that which would result from the operation of their chemical laws; while the function of each atom is, we conceive, to impart (on the destruction of the antagonistic equilibrium) the organizing agency to other components. We say, the organizing atom is composed of several distinct species of matter; but though anatomically (for it is not chemically) compound, it is, notwithstanding, physiologically atomic or simple. Each mineral atom we have seen to be under the control of but two classes of attributes, namely, those we have called its laws, general and specific; but each organizing atom is, we conceive, influenced by four distinct classes of attributes, namely, the attributes of its components, or inert matter's laws; and those attributes peculiar to it as an organizing atom, or, in other words, its organic laws, general and specific. And as on the inanimate laws, general and specific, which control the operation of mineral atoms, are constructed our sciences of physics and chemistry; so, in the organic laws, general and specific, which regulate the action of organizing atoms, we recognise the objects of physiological research.'—(pp. 25–7.)

"Now if our author, instead of the hypothetical 'organizing atom,' had used the term 'cell-germ,' we apprehend that he would have been much nearer the truth. Indeed, the apparent novelty of his views in most instances results from his having simply expressed in abstract terms what other have worked out in the concrete form; and we think that he would have done well to acknowledge his obligations to the various physiological writers, by whose hints or more formally-expressed doctrines he has obviously profited largely. In the idea that the lower classes of animated being elaborate nutrient materials for the higher, and that in its entrance into the more complex organisms, the food is subjected to a series of elaborating processes by which it is gradually prepared for entering into the composition of the solid textures of the fabric,—ideas which constitute essential parts of his system,—our readers will not find anything very novel. Nevertheless, we repeat that the treatise will be read with pleasure by those who feel interested in speculations of this nature; new relations being developed among ideas that were previously familiar, and various suggestions being thrown out which may be profitably pursued."—*British and Foreign Review*, January, 1849, vol. iii., p. 197.

I ask the reader, is there a physiologist living would collect from that review, that in the volume thus criticised were submitted views, identical in many important particulars with original views which *thirteen years subsequently*, in the hands of eminent physiologists, would work such a revolution in physiology as is at this moment placing that science upon an altered foundation? And yet such is the fact. I ask him is there a physiologist living would collect from that review that in the volume thus criticised was submitted a proposition, upon *the most fundamental* question in physiology, which *thirteen years subsequently* a distinguished physiologist would enunciate in a royal institution with all the emphasis of the expression—"I would venture" to say. And yet such is the fact.

Had the journal been one of less importance, ability, and weight, I should have attached but little moment to the matter, but it was obvious to me that two effects must necessarily result from *such* a review appearing in such a journal—namely, 1st. That no physiologist living would open my volume; and, 2nd. That so far as my obscure name was likely to go before the world as an author, it must be in the character of something *very little better* than a plagiarist and a pretender to "novelty."

The question then with me was, what *ought* I to do? Ought I or ought I not leave those statements uncontradicted? A little reflection led me to decide I ought *not*. I knew I had put forward *new* views upon topics of importance, and I also knew full well there was not a physiologist living would dream of searching for those views in a volume of which it could with justice be stated, that "indeed, the apparent novelty of his views in most instances results from his having simply expressed in abstract terms what others have worked out in the concrete form."

I have already observed that I am without interest with any gentleman connected with the press. One of the London periodicals had generously reviewed my volume in highly flattering terms, and it occurred to me that possibly that journal might admit my reply. I learned, however, in a courteous letter from the editor, that such was contrary to his rule. Recognising at once the unreasonableness of attempting to involve others in my personal disputes, I felt the injustice of applying to the *professional* journals, and published the following letter in one of the Dublin advertising journals. That letter may, perhaps, have exhibited too much irritability, as it was written under the influence of *intense irritation and disappointment*, but, be that as it may, it is necessary I should here reproduce it.

"*To the Editor of* "*The British and Foreign Medico-Chirurgical Review.*"

"Dublin, 28, Holles-street, 31st January, 1849.

"SIR,—On the 13th of this month I addressed the subjoined letter to the editor of one of the medical periodicals. On the 24th instant I received a polite letter from that editor, acquainting me that it was 'a rule which he adopted not to admit discussions relating to other journals, especially in the review department.' By return of post I requested that my letter might be inserted in his journal as an advertisement, stating that it was of importance to me, for many reasons, that it should appear in print before the 1st proximo, and requesting that if it were inadmissible in that form into his forthcoming number, he would as a favor return to me the manuscript. I have since received no reply to my requests. Having no reason to expect that a greater amount of courtesy could be extended to me by any other of the professional journals, the natural channel through which to submit to the judgment of my professional brethren the justice of your reviewer's imputation appears to be closed; I am thus reduced to the alternative of either submitting in silence to that reviewer's imputation, or adopting my present line of conduct. I have preferred the latter, and that from the conviction that so long as I tacitly allow myself to be regarded by the profession as a pretender to 'novelty,' any further opinions which I may venture to publish cannot be received by that profession with an unprejudiced judgment. Satisfied that you are yourself personally unacquainted with the contents of my work, and, consequently, with the justice or otherwise of its review,

"I have the honor to be, sir,

"Your very obedient servant,

"HENRY FREKE."

"*To the Editor of* ——

"Dublin, 28, Holles-street, 13th January, 1849.

"SIR,—Might I, if it be not trespassing too far on your space, request that you would favor me by the insertion, in your periodical, of the following observations:—

"In this month's number of '*The British and Foreign Medico-Chirurgical Review,*" (to which my attention has just been directed), there is contained a notice of a work recently published by me, suggestive of an Organic Atomic Theory. In that notice the reviewer has felt himself at liberty to give expression, in connexion with my name, to imputations of an act, from the voluntary commission of which I should instinctively shrink, having ever regarded it as an instance of great literary baseness—I allude to the accusation of an attempt upon my part to appropriate to myself the opinions or arguments of others. Had the eminent physiologist who edits that journal cast his eye over my pages, I feel satisfied that that review would never have found an entrance into his periodical. Nothing could be more opposed to my inclinations or repugnant to my feelings, than in a manner like the present to be forced to obtrude upon public notice either myself or my publication; but when a charge has been gravely brought against me by a leading metropolitan journal, the tendency of which is to cast a reflection upon my character alike as a gentleman and a scholar, I feel that the profession will be too generous to impute to me an unworthiness of motive in thus attempting to shield myself from such an imputation, and declining to resign to others that which I regard as being legitimately my own.

"I shall now endeavour to present to that reviewer, '*in abstract terms*,' his review as it it has been '*worked out in the concrete form*.' Having quoted a passage from my work, expressive of my views as to the nature of organizing atoms, the reviewer thus proceeds with his criticism:— 'Now, if our author, instead of the hypothetical 'organizing atom,' had used the term 'cell-germ,' we apprehend he would have been much nearer the truth.' I shall, in the first place, take the liberty of informing that reviewer, that had he been more familiar with the principles of philosophic inquiry, he would have been more competent to pronounce correctly upon what constitutes hypothesis. I should be glad to be informed, being at a loss to understand in what respect the expression 'organizing atom' is more 'hypothetical' than is the term 'cell-germ.' The *existence* of the entity under consideration admits of *no* question, and consequently its *existence* can be no matter of hypothesis. Now, that this entity is an organizing something can only be questioned by such as question that the cell germinated has received organization therefrom; and that the term 'atom' is appropriately applied to it can only be denied by such as deny that this entity is physiologically indivisible. Does the reviewer question either of these facts, both of which I have maintained? If so, I should be glad to be informed of his grounds. If not, I call upon him to explain in what respect the expression 'organizing atom' is either 'hypothetical' or inappropriate. I shall repeat for that reviewer's information, that organizing atoms are *not* (like inanimate atoms), the objects of conjecture; their existence is as much matter of demonstration as is the existence of muscle or of man. The *properties* or *attributes* of such atoms is a question altogether distinct, and are arrived at by another and distinct order of inquiry. These attributes may or they may not be hypothetic, and if the former and if rational, they may constitute a legitimate foundation upon which to construct a rational hypothesis or theory. It was their *specific attributes*, and not their *existence*, which at the time of publication I regarded as being *then* hypothetical in organizing atoms, and it may be that ere long I may be enabled to present our reviewer with certain evidence of a somewhat more demonstrative kind, even upon this branch of our inquiries; nor do I entertain a shadow of doubt, that, in the event of my being enabled to establish any such specific properties, *they too will prove to be no more than an abstract of the reviewer's concrete*. Here then, as indeed throughout that review, is to be recognised either an inability or a disinclination (and the latter 'we apprehend is much nearer the truth'), on the part of the reviewer, to discriminate between inquiries in their nature distinct, while he feels himself at liberty to pronounce decisively upon my want of originality. But he proceeds, 'had he used the term 'cell-germ,' we apprehend he would have been much nearer the truth.'

"If by the words 'much nearer the truth' (for I confess myself unable to see clearly their force) the reviewer mean to convey that he apprehends

I should have employed a more appropriate expression, I beg leave to inform him that I apprehend the contrary. Here there is a difference of opinion, although doubtless propounded somewhat more *oracularly* than is habitual in the expression of *opinions*. The reviewer has not favoured us with the grounds upon which his has been formed. I shall be more generous, and shall afford him an opportunity of pronouncing upon the soundness of mine. Simply, then, I apprehend that in using the term 'cell-germ,' I should *not* 'have been much nearer the truth,' inasmuch as I conceive that the employment of such term so applied would be a direct violation of the principles both of logic and philosophy. If the views which I have advocated in my publication be correct, it is manifest that the formation of a cell is but a *stage* in a process which has as yet been *uncompleted*, and I assert, notwithstanding the reviewer's apprehensions, that it is an outrage upon the principles alike of philosophy and logic to designate any entity (it matters not what the nature of that entity be) by *a term expressive of a stage in its as yet unfinished function.* Vastly more appropriate would it be to call the acorn a shrub-germ, because the oak must have once been a shrub, than to designate that structure, whose function is to form muscular fibre, a 'cell-germ,' because, forsooth, a cell must have been constructed before that fibre was formed. Here, then, in the reviewer's own statement is manifested one of two things—namely, either that he could not ('we apprehend it would be much nearer the truth' to say, *would* not) understand the opinions I had attempted to support, and consequently could not be justified in pronouncing upon their want of originality or otherwise; or else that those opinions were distinct from such as he himself entertains; and if the latter, how did he feel himself at liberty to assert they were wanting in novelty? The reviewer thus proceeds:—

"'Indeed the apparent novelty of his views in most instances results from his having simply expressed in abstract terms what others have worked out in the concrete form; and we think he would have done well to acknowledge his obligations to the various physiological writers by whose hints, or more formally expressed doctrines, he has obviously profited largely.'

"In these statements there appears to me to be a manifestation of an absence as well of generosity as of justice. They are ungenerous, inasmuch as from the general and unspecific nature of the charge, the reviewer has withheld from me the possibility of a specific refutation; and they are unjust, because if my views had been understood by the reviewer, he must have been aware that his reproof was undeserved, and if unintelligible, it was injustice to pronounce judgment upon what he did not understand. How then am I to dispose of his 'concrete' of my 'abstract?' Its general analysis *here* would in me be unbecoming, as it would involve an unbecoming obtrusion of my own views. It remains, then, for me but to apply it to fundamental inquiries. The fundamental inquiries in physiology ever must be into the nature of life, and its relation to death. I have endeavoured to point out what I regard as the nature of life, and have defined living to be, 'the act of being elevated in the scale of organization.' To what physiological writers am I indebted for this? This is my 'abstract;' I demand the reviewer's 'concrete' of this? I have attempted to shew what I conceive to be the nature of death, and its relation to life, pointing to the essential dependency of the latter upon the former. Of *what* 'formally expressed doctrines have I so obviously profited largely' for that? That is my abstraction; I call for the reviewer's concretion of that. But I may not thus proceed. To what physiological writers *specially*, I demand of that reviewer, do I so obviously owe obligations? It is but justice that he should point to the writers, and at the same time should point to the debts. Does the reviewer mean to convey in his statements an insinuation that I have had the effrontery to feign an ignorance of the researches which, while physiology is science, will be ever held in remembrance, in connection with the imperishable names Schleiden and Schwann? If so, I indignantly repel the insinuation, and appeal to the profession. Could such be collected by an impartial reviewer from my own words, 'far-famed 'nucleated cell' ?'

"By *whose* 'hints,' then, 'or more formally expressed doctrines' have I more 'obviously profited largely,' than must *every* physiological writer at *every* period have profited by the writings of those by whom he has been preceded? I conscientiously assert that from the commencement of my volume to its close, there is not to be found a solitary opinion which has not originated exclusively and alone with myself, except it be on topics with which the physiologist is as familiar as is the astronomer with gravity; and had I been writing upon the subject of astronomy, I should not have apprehended the imputation of plagiarism, even though I had not informed my readers that Newton was the discoverer of gravity. Nay, further, I conscientiously assert that into that volume I have not introduced a solitary argument which has not been altogether and only my own. So much then for the *moral*, and now for the *philosophic* justice of this charge of obligation to physiological concretors. It would be difficult for language (in the same number of terms as are comprised in these statements of the reviewer), to evince a more complete and total unacquaintance with the nature of philosophic research. For how stands the enthymeme which the learned reviewer propounds? It is thus—the author's views are but the abstract of truths which were already known in the concrete, *and therefore his views are not novel !!!* Alas, sage reviewer, 'we much apprehend' your suppressed premise will be found wanting in logical as well as in ethical truth. What, I would ask that reviewer, does *he* conceive would constitute a *discovery*, or, if he prefer it, a '*novelty*' in natural science? If he know not, I shall take the liberty of informing him. It is no more than the reaching, in the abstract, truths that may already have been known in the concrete. What was the discovery of gravity but the recognition, in the abstract, of the then concrete truths of astronomy? And were the laws of gravity the less a discovery upon that account—or were they for that reason the less 'novel?' To what physical writers was Newton indebted for the concrete of this abstract?

"What '*novelty*,' I should be glad to learn, is likely *ever* to be reached in *any* branch of inquiry which will not also be likely to prove to be but an abstract of *some* truth known, perhaps even now, in a more concrete form? Away, then, with this ungenerous attempt at mystification.

"The reviewer thus proceeds :—'In the idea that the lower class of animated being elaborate nutrient materials for the higher, and that in its entrance into the more complex organisms, the food is subject to a series of elaborating processes by which it is gradually prepared to enter into the composition of the solid textures—ideas which constitute an essential part of his system—our readers will not find any thing very novel.' Upon the correctness of this statement, *in its relation to that part of my work to which it refers*, it is not for me to pronounce. But, although it is with extreme reluctance I could allow myself to be led to attribute unworthy motives to a reviewer, to whom personally I must be altogether unknown, I cannot help feeling that in the skilful ingenuity with which he has excluded, from *his* version of my views, a position of the greatest physiological importance which I there had been attempting to establish, he has given clear evidence that his judgment upon my work has been the result the rather of a disinclination than a disability upon his part to recognise that which *is* 'novel.'

"And now I have but to add, that I have been just or I have been unjust. If it be the latter, the profession is my judge; but if the former, might I not (were I so disposed) in justice retort his own charge upon that reviewer, and say :—You, with the aid, the irresistible, the crushing aid, of an influential and widely diffused journal—you, in the retreat of your own personal irresponsibility, have ungenerously and unjustly, and I regret to add *apparently* designedly and wantonly, availed yourself of the most efficient agency means could supply, of robbing of his rights a young author, who is unknown, and who consequently, you well knew, was likely to be *unheard.*

"I have done; and, in taking leave of that reviewer for ever, I have to assure him that I have not been actuated in these observations by any unkindliness of feeling. If I have been unjust, I repeat it, the profession is my judge. Should the reviewer feel that my remarks were not uncalled for, they may not be unproductive of good. Him, in his retirement, they never can harm—on me to have made them I have felt it was incumbent. Considerations widely apart from the mere desire of influencing such impression as that review may at present create, have told me that now, if ever, it became me as due to myself for the first, and I trust the last time, to do that which my nature dislikes; for they told me that if now, at the outset of a long and laborious train of research, I silently and undissentiently resign my earlier products (it matters not how unimportant their nature may be), my ulterior results (should I reach such) must, as a necessary consequence, with ease and inevitably become the property of others.

"Your obedient servant,

"HENRY FREKE."

I repeat it, that letter may perhaps have manifested an undue degree of irritability, as it was written at a moment when no man *should* write—namely, while, smarting from the effects of a recent disappointment, he is under the influence of irritation and excitement. At a moment too when my acquaintances, who knew nothing of the merits of the question, were taunting me as "*a pretender to novelty.*" But, admitting that fact, I reproduce that letter now simply for the purpose of submitting it to physiologists and the press, and of appealing to their sense of justice while I ask —if *this* be the cause—is it in their opinion *an adequate cause* to justify my being ignored as a physiological writer—to justify my being forced to abandon the pursuits of my taste? Is it, in their opinion, an adequate cause to justify any man who so fancies in quietly taking possession of my writings, and ignoring their author's existence?

That letter may have been irritable, but its irritability was directed towards one whom, as anonymous, it was harmless to hurt; towards one who had wounded me deeply in the most sensitive point it is possible to wound a man, who, aspiring to reputation in science, has ventured to give expression to original thoughts. It may have been irritable, but its irritability was directed towards one who had just robbed me of the results of prolonged and anxious mental labour—of that which at the time I valued most.

Thirteen years having elapsed since that letter was written, I now—after an interval of such length—can calmly look back on the whole.

In doing so I solemnly declare that so true are the statements contained in that letter, that were I now to republish my volume of 1848, for the express purpose of pointing to authors from whom I might possibly have derived some suggestion, I repeat it, I solemnly declare I could not point my finger to a single individual from whom I could conscientiously say I had derived a solitary thought, which I was in *the slightest degree* desirous of putting forward as my own, or to which I made the *slightest pretensions* as being original.

I doubtless owed obligations to Schleiden and Schwann, and no one reading my volume could for a moment suppose I ignored them. I might perhaps add that in a general way I may possibly have owed something to Liebig, although it would be out of my power to say *what.*

But I would ask is there a man living would dare publish original views if they are to be subject to this class of scrutiny? Who is there could express a thought upon any subject whatever, that must not necessarily have "profited largely" for that thought by others?

Farewell, then, to "novelty" and originality of thought, for if this be true criticism, they are but myths without real existence. But enough of this.

There are two topics referred to in that letter upon the justice or otherwise of my remarks, in relation to which it would be impossible for the reader, in the absence of my volume, to pronounce. I would, therefore, solicit permission—with as little trespass on his patience as is in my power—to submit to his judgment my views upon those topics. I am the more anxious to do so, inasmuch as they both refer to matters to which I had attached much importance, and both have relation to views which *I had distinctly put forward as "novel."*

If then the reader will bear with me for a moment, I shall introduce *two other* propositions—in addition to that already referred to—submitted in my volume of 1848; and in my remarks upon these I shall endeavour to afford as general an insight into the nature of my publication of 1848 as is admissible without trespassing too far.

The two topics I refer to are these—namely, 1st. As to the propriety of the expression "organizing atom," which I had ventured to propose; and, 2nd. In relation to that passage in my letter where I state that the reviewer—in the only example he gives as a proof of the want of novelty in my views—excludes from his version *the very proposition I had been attempting to establish.* [The word "position" in the paragraph of my letter referred to, should have been *proposition*, but it would be inadmissible to correct it now.]

First, then, as to why I proposed the terms "organizing atom," and the meaning I attach to those terms.

I would solicit the reader's attention for a moment, while I say a word on the *physiological* division of *organic* beings, as a thing altogether distinct from the mere physical or chemical division of matter.

I would submit to physiologists that there is such a thing as purely *physiological* division, and what I understand by that term is this—namely, such separation of two or more physiological integrals as does not destroy their physiological integrity. A muscle and nerve, for instance, which are physiologically combined may be *physiologically* separated, each after such separation retaining its physiological integrity unimpaired.

All compound organisms—say man, for example—are *physiological compounds;* namely, are composed of a number of distinct physiological integrals or independent organic structures (such as muscle, nerve, gland, &c.) physiologically united or combined. Each of these structures is a distinct independent integral, having an organization and function peculiar to itself, and each may be separated from the rest and still retain its organization and functional integrity.

The separation of a compound organism—say man, for example—into its several component physiological integrals, or independent organic structures (namely, nerve, muscle, gland, &c.) may, I would submit with propriety, be termed the *physiological division* of such organism.

I shall illustrate what I mean by this statement. Man, for instance, is *physiologically* divisible into muscles, nerves, glands, &c. each of which is an independent physiological integral possessed of a structure and function peculiarly its own. Some of these structures may admit of further *physiological* division: that is, may be *further* divided, and still retain their physiological integrity.

A muscle, for instance, may be *physiologically* divided into its several fasciculi; a fasciculus may be further *physiologically* divided into individual muscular fibres; and a fibre may be still further *physiologically* divided into elementary muscular molecules. But *here*—or at least *somewhere*—any further division can only be physical or chemical, but no longer *physiological.* Here—or at least *somewhere*—any further division of muscle must put an end to it as a physiological integral.

The supposition that muscular fibre could by possibility admit of *infinite* physiological division, would to me appear to involve in it a logical absurdity, while at the same time it would destroy all distinction between *organic* and *mineral* matter.

The same is true of organic structures *universally.*

Hence I would submit that in every organism *there must be* a point—whether we can reach it or not—at which *physiological* division must stop, and that at this point the entities there placed are correctly termed *physiologically atomic.*

When, then, we contemplate an animal or plant in relation to the question of its physiological divisibility, we find that each admits of physiological division within certain limits, but *only within certain limits.*

What are those limits?

What is the *ultimate* physiological division of organic creation universally?

This was one of the questions contemplated in my publication of 1848—namely, what is the point in organic creation at which physiological division must stop?

The results I arrived at were these—and I repeat it—I submitted those results to physiologists with diffidence, to solicit simply their decision on their merits. What I ventured to submit was as follows, namely—that organic creation universally—including all animals and plants—is reducible—as its *ultimate* physiological division—to two parallel chains of organic integrals. That the successive links in each chain are organic integrals, which *gradually ascend in the scale of organization,* from inorganic elements in the mineral world to the cerebral matter of man. That each link in the one chain was a *simple* (namely, physiologically indivisible) embryonic germ—that is a germ capable of developing *but a single* species of organic tissue, say *either* lignin, *or* albumen, *or* muscle, *or* nerve, &c.; but *not* a plurality of such structures as lignin, *and* muscle, *and* nerve, &c.

That each link in the other chain was a *simple* (viz. : physiologically indivisible) organized product (say *either* lignin, *or* albumen, *or* muscle, *or* nerve, &c.) developed by the *simple* embryonic germs that constituted the links of the former parallel chain.

The function of the one chain being to confer or impart organization, I termed the links in that chain organizing agents, and these organizing agents being *ultimate*—namely, physiologically indivisible—I designated them "organizing atoms."

The function of the other chain of ultimate integrals being distinct from that of imparting organization, I termed the links in that chain "organized residual products."

The following are the terms in which I summed up the foregoing views in my publication of 1848.

"Thus, then, as we have been led to believe in the necessary existence of a chain of progressively advancing organizing agents or atoms whose function is *to confer* organization; so, too, we believe in an equal necessity, before that function can be fulfilled, for the existence of a corresponding chain of progressively advancing organized structures, whose function is *to receive* organization, and which were designed and adapted for calling into operation the function of the organizing atoms."—*Freke on Organization,* (1848), pp. 40, 41.

Thus, then, by the expression "organizing atom," I designed to convey simply an *ultimate* organic molecule whose function is to organize, or impart organization.

I adopted the term "atom" because it appeared to me to fulfil the requisites for definition, being both "adequate and clear," and at a glance conveying its meaning to the least educated mind.

I used the term "atom" in exactly the same signification as in chemistry that term may be applied to an equivalent of any salt—say of carbonate of ammonia for example. An equivalent of carbonate of ammonia, though comprising in it four distinct elements, is notwithstanding atomic *as carbonate of ammonia,* inasmuch as any division of such equivalent must destroy its integrity and put an end to its existence *as carbonate of ammonia.* Consequently, *as carbonate of ammonia,* that compound entity is atomic.

So, too, I would submit of *ultimate* organizing molecules. An ultimate organizing molecule—although of necessity a compound body—is as an organizing molecule indivisible, inasmuch as any division of that entity must destroy its integrity and put an end to its existence *as an organ-*

izing molecule. Hence I would submit that *as an organizing molecule* it is atomic.

I had an additional motive in adopting the term "atom," upon which I dwelt at some length. It was this—

Recognizing, as I conceived, a striking analogy between the manner in which *simple* organizing molecules unite to form a *compound* organizing molecule in the organic world; and the manner in which *simple* mineral atoms—such as carbon, oxygen, hydrogen, and nitrogen—unite to form a *compound* mineral atom, such as an atom of carbonate of ammonia, in the inorganic world. I had hopes that the expression "organizing atom" might possibly attract attention to this analogy.

By the term *simple* organizing molecule, or *simple* embryonic germ, I mean an embryonic germ capable of developing *but one* species of organic structure—say *either* nerve *or* muscle, *or* cerebral matter, &c. And by a *compound* embryonic germ (such as the embryonic germ of man) I mean an embryonic germ capable of developing not only one *but several different* species of organic structure—say muscle, *and* nerve, *and* cerebral matter, &c. And it appeared to me that the latter or *compound* germ was formed by a union of several of the former or *simple* germs, which union appeared to me to be brought about through the agency of some *organic affinity*, analogous to the chemical affinity which effects a union between mineral atoms. Thus, for instance, the several *simple* germs which develope the several organic tissues (muscle, nerve, &c.) in man, combined in the Graaffian vescicle to form the *compound* human embryonic germ, which *compound* germ is competent to develope the *various* organized tissues which eventually constitute man.

In support of the above view of the constitution of *compound* embryonic germs, I would venture to submit the following remark.

An oak and a man are both developed from embryonic germs; but *the same* embryonic germ that developes the oak could not develope man, consequently *different species* of embryonic germ must exist to develope the different species of living beings in existence—such is an acknowledged fact. But an oak and a man do not differ from each other more absolutely and essentially in organic constitution and physiological properties than muscular fibre differs from nervous tissue in the same respects. Muscular fibre and nervous tissue are both developed from *germs*, but I would submit that it would be as irrational to suppose that *the same* germ which developes muscular fibre could also develope nervous tissue, as it would be to suppose that an acorn could give development to man.

Hence I would submit that as simple indivisible organic structures—differing from each other essentially both in organic constitution and physiological function—are actually found to exist in organic nature, there must be different species of *simple indivisible germs* to develope those different species of structure.

That the microscope should be incompetent to reveal any specific distinctions in those germs is little more, I would submit, than we are prepared to anticipate, and consequently, if such distinctions exist, I would submit they should be sought for through some other channel.

Such were my motives for proposing the term "*organizing atom.*"

I shall trespass no further on the reader with this topic, but shall pass to the consideration of the second parallel chain of ultimate organic structures; namely, to what I termed "organized residual products."

I would ask the reader to grant me his attention to this subject, as it was in relation to *it* I felt more hurt by the reviewer's remarks than perhaps by any other statement contained in that review. It was in relation to it that the reviewer observed, "our readers will not find anything very novel."

I have observed that the second *ultimate* division of organic matter might also be resembled to a chain composed of links, which links—like those of the first chain—gradually ascend in the scale of organization from mineral matter to the cerebral matter of man.

The relation I ventured to propose as subsisting between these two parallel chains is as follows:—

The material constituents of organic creation are derived from the inorganic world through the agency of the first or organizing chain. The function of that first chain is twofold, namely, first, to convert *inorganic* into *organic* matter; and, secondly, to push this organic matter gradually up in the scale of organization, till—as *one* of the final results—it may eventually become the cerebral matter of man.

The second chain is the *product or result* of the function of the first, and comprises in it lignin, albumen, &c., &c. in the vegetable world; and muscular fibre, nervous tissue, cerebral matter, &c. in the animal kingdom.

The proposed functions of the first or organiz*ing* chain are as follows:—*Inorganic* matter is the natural "nutriment"—or as I ventured to term it, "reciprocal or specific stimulus"—of the *first* link in the chain of organizing atoms. This first link I *subsequently* (viz.: in my volume on Species) termed "Geörgat," as being the atom which organizes earth.

Inorganic or mineral matter, I say, is the natural nutriment of the first link in the organizing chain. This first link—namely, georgat—in the discharge of its function *elevates mineral matter to the organic condition*, and eventually disposes of it thus—namely, it developes therefrom two distinct products, viz.: it reproduces georgat, competent at any future period to discharge the same function; and, secondly, it leaves an organized product which constitutes the first link in the chain of organized residual products.

The first link in the chain of products stands in the same physiological relation to the second link in the chain of atoms that mineral matter stood to georgat—namely, the relation of natural nutriment or specific stimulus.

The second link in the chain of atoms elevates the first link of products *one step higher* in the scale of organization, and eventually disposes of it thus—namely, it developes therefrom two distinct products—viz.: it reproduces its own type—namely, an atom of the second link—and, secondly, leaves an organized product which constitutes the *second* link in the chain of organized residual products.

The second link in the chain of products stands in the same physiological relation to the third link of atoms that mineral matter stood to georgat, and that the first residual product stood to the second link of atoms—namely, the relation of natural nutriment or specific stimulus. And so on of the successive links in the chain of organizing atoms. Each successive link remodels its type, and leaves an organized residual product elevated one step higher in the scale of organization.

In a word, the first link of atoms remodels its type, and forms the first link of products. The second link of atoms remodels its type, and forms the second link of products. The third link of atoms remodels its type, and forms the third link of products, and so on to the end of the chain, or, in other words, till the final results of nutrition are formed, such as lignin, &c. in the vegetable world, and muscular fibre, nervous tissue, cerebral matter, &c. in the animal kingdom.

Thus it can be perceived how the two parallel chains of ultimate organic structures *constantly keep parallel*, as the development of organic beings is proceeding; and also how mineral matter is gradually and by successive steps *elevated in the scale of organization*, till eventually—as the final results of the nutritive process—it becomes lignin, &c. in the vegetable world; and muscular fibre, nervous tissue, cerebral matter, &c. in the animal kingdom.

The foregoing might perhaps be simplified by being thrown into a tabular form, thus—

ORGANIC MATTER

1st ultimate division, viz.: first parallel chain, or chain of organiz*ing* atoms.		2nd ultimate division, viz.: second parallel chain, or chain of organiz*ed* residual products
1st Atom (Georgat)......	elevates *inorganic* matter to the organic condition; remodels its own type and forms	..............**1st Product.**
**2nd Atom................. **	*elevates* 1st product one step higher in the scale of organization; remodels its own type and forms	..............**2nd Product.**
3rd Atom.................	*elevates* 2nd product one step higher in the scale of organization; remodels its own type and forms	..............**3rd Product.**
&c.	**&c.**	**&c.**
Last Atom, viz. Cerebrat?	*elevates* ——— ? one step higher in the scale of organization; remodels its own type and forms	..... **Last Product, viz.: Cerebral Matter?**

The several *atoms* are *simple* germs: and the several *products* formed are lignin, albumen, &c., &c. in the vegetable world; and fibrin, muscular fibre, nervous tissue, glandular structure, cerebral matter, &c. in the animal kingdom.

I shall trespass no further with this subject, as I am undesirous of obtruding my own views further than is essential to justify the passage in my letter to which they refer; but I would appeal to physiologists if it be their opinion that in the foregoing views—whether correct or erroneous—the readers of the *British and Foreign Review*, in November, 1848, would have been unable to "find anything very novel."

As evidence of the "novelty" of the foregoing views in 1848, I would submit the recent able and original researches of Dr. Lionel Beale on the same subject in 1861.

Dr. Beale, in his researches thirteen years subsequently to mine, arrived at results *some of which* coincide very nearly with some of the foregoing; and, inasmuch as Dr. Beale's researches were universally recognised by the press as "novel" in 1861, I would submit that so far as there is a coincidence between Dr. Beale's views and mine, *thus far at least* my views were "novel" in 1848.

I introduce Dr. Beale's name *solely* to adduce that gentleman as evidence of the "novelty" of the foregoing views in 1848; for although in many important particulars Dr. Beale does *not* coincide with those views, I confidently appeal to that gentleman, and ask him if he—as a physiologist who has devoted much thought to the subject—be of opinion—even where we do not coincide—that in 1848 physiologists could have recognised nothing "novel" in those views? or if he be of opinion that I was then indebted for them to any "hints or more formally expressed doctrines" of others?

As points of at least partial coincidence between Dr. Beale's views and the foregoing, I would submit the following :—

I had described elementary organic matter as being of two kinds—namely, what I termed "organizing agents" and "organized residual products."

Dr. Beale describes elementary parts as consisting of matter in two states, which he terms "germinal matter" and "formed material."

The function I had attributed to organizing agents was that of "conferring or imparting organization."

I thus expressed myself on this function in my volume on Species :—

"The function of every such embryonic organism or organiz*ing* agent being, as I have observed, to confer or impart organization, in discharging this function it gives origin to, or *generates*, two distinct classes of organic structure, viz. : a plurality of embryonic organisms or organiz*ing* agents identical with itself, and at the same time some organiz*ed* residual product, such, for instance, as woody fibre, muscular fibre, nervous tissue, cerebral matter."—page 89.

Of "organiz*ed* residual products" I spoke in the same publication as "organiz*ed* structures, which like the woody fibre are possessed of some other physiological property distinct from that of conferring organization."—page 4.

I entered at great length upon these respective functions in my communications to the *Dublin Medical Press* in 1851, '52, '53.

Dr. Beale describes germinal matter as "active, and being capable of multiplying itself," and "formed material as passive, and incapable of multiplying itself."—*British Medical Journal, March*, 9, 1861.

I could point to other points of coincidence between Dr. Beale and myself, but I have no desire so to do. I repeat it, I have no motive but the one mentioned in introducing that gentleman's name; and I have no hesitation in adding that, notwithstanding the partial coincidence between Dr. Beale and myself, there is to be recognised in that gentleman's writings the freshness of originality which brings conviction to the mind—at least to mine—that he is indebted *alone to himself* for his views.

Thus far my effort has been to convince physiologists of the truth of the following three statements, namely—

1st. That my publication in 1848 *did* contain views on important questions in physiology for which I was in no way indebted to any hints or more formally expressed doctrines.

2nd. That the views I refer to could not be justly stated to be simply the expression in abstract terms of what others have worked out in the concrete form ; and,

3rd. That some of those views were such as to justify my being recognized as a physiological writer.

The evidence I have submitted in support of these three statements is this, namely—the recognised originality and acknowledged merit of coincident views—or views in *some points* coincident—advocated by eminent physiologists thirteen years subsequently.

I would now ask permission to say a word upon the origin and fate of my publication of 1861.

Finding that my communications to the *Dublin Medical Press* in the years 1851 and following years were, so far as I could ascertain, like my former publication, unread; and observing that no notice was taken of any proposition I advanced by any of the Retrospects of the day, I became discouraged, and thought it most prudent to retire from such pursuits. I accordingly abruptly brought my communications to a close long before I had completed what I had contemplated. I then withdrew my thoughts altogether from physiological pursuits, intending at the time that it should be for ever.

Towards the close of the year 1860, an eminent friend of mine, who was intimately acquainted with the circumstances connected with my publications, asked me my opinion on Mr. Darwin's work on Species. I replied, that having long since withdrawn myself from pursuits of that kind, I had not read Mr. Darwin's work. "I understand," my friend observed, "he has reduced all species to a single primordal germ." "Why I did that nine years ago," I replied, "and no one would read me." My friend replied, "If that be the case, you have now an opportunity that may never occur again of attracting attention to your physiological views."

I had never specially contemplated the question of the origin of Species. It was a subject I had never placed directly before my mind. I had inductively and unexpectedly arrived at the same conclusion as Mr. Darwin, while engaged in inquiries of a different kind; and I was myself as much astonished when that result forced itself upon my mind as any man could be who contemplated it for the first time. I introduced incidentally in a note the conclusion I had thus unexpectedly reached, attaching but little moment to it at the time, as I was occupied with other inquiries; and I banished it entirely from my mind.

Under these circumstances, when it was suggested to me to write, I felt that, should I do so, I ought to confine myself exclusively to the one point of coincidence between Mr. Darwin and myself—the only topic in fact in relation to the question of Species I had ever contemplated.

Accordingly, in October, 1860, I did publish a short circular embodying simply the note I had published on that one topic in 1851.

I found, however, that without the context this was scarcely intelligible, the more particularly so, as the terms I had employed required that context for their explanation.

It thus became necessary—if I wished to be understood—to publish a volume embodying my general views. I did so, and such was the origin of my volume on Species.

It can then readily be understood that, being suddenly and unexpectedly called on to prepare, in the course of a few days, my views on an abstruse question in science that I never had specially contemplated, I could not presume in so doing to aspire to *literary* claims.

I did not, I *could* not so aspire; the only thing it was possible for me, under the circumstances in which I wrote, to aspire to, was *scientific* and not literary merit. And I had some hopes that the want of the latter might possibly be overlooked in the former—should the former be recognised in my writings.

But its literary defects disappointed me even more than I had anticipated.

My volume consisted almost exclusively of quotations from former publications, but rudely and indifferently connected, and passed through the press in the course of a few days, at a time I was myself unable to attend to it as I should. The result was that passages were introduced that should have been omitted, and omitted that should have been introduced. Words were omitted, introduced, misplaced, and mis-spelt, and in fact my volume—in addition to the imperfections incidental to the circumstances under which it was published—went before the world with every defect that is likely to be found in a work that is not revised by its author.

I am aware that the fault was entirely my own, but I mention these facts for the purpose of observing that I not only understand but fully feel the justice of the criticisms of the purely *literary* journals that found fault with my publication for its style.

One objection that was extensively raised to my volume—and I acknowledge its justice—was, that I perplexed the reader with my incessant quotation marks. I confess they were objectionable, and ought perhaps to have been omitted. As I was not *then*, as now, laying claim to priority for my views—there being no other writer in the field with coincident views till three months subsequently—I ought perhaps to have dispensed with quotation marks.

I should have done so but for one reason, and it was this. My great difficulty was *time*. I was already far, far too late in the field upon Species, and were I to allow the moment of public excitement on the question to pass, I saw no likelihood of my volume being read.

Many years had elapsed since my views were first published, and much of *the phraseology* then employed in expressing those views is perhaps not such as I should *now* wish to use were I rewriting my views.

Thus, for instance, the ideas I desired to convey by such expressions as "the process of living" and "the process of dying" have more recently been expressed by the terms "the process of formation" and "the process of decay." These and such like expressions I should perhaps, in the present advanced state of physiological science, have felt it desirable to change. But I had no time to rewrite my views in order to express them in modern nomenclature, and unless I used the old terms, I should have been obliged to give up the idea of publishing.

I had some hopes that the distance of the time they were first used might possibly be admitted as some apology for the quaintness of several of my expressions.

Such was my motive for using quotation marks.

I had sacrificed *everything* to bring my views—as I had hoped—*at once* before the eye of physiologists, and I was destined a third time to be disappointed.

My defects were too patent to escape observation; my merits—such as they were—were unrecognized.

To *The Spectator* I cannot refrain from expressing *my deep sense of gratitude* for the able, impartial, and comprehensive analysis it generously gave of my volume.

I say I was destined a third time to be disappointed in attracting the notice of physiologists to my writings, and resolved again to give up the contest as hopeless.

Such was my determination when, in August last, I received the year book for 1861, of "The New Sydenham Society."

On opening that volume, I found that no allusion was made to my writings.

The views of three gentlemen—all of whom had published some three months subsequently to myself—were introduced.

Between the writings of each of those gentlemen—namely, Dr. Hughes Bennett, Professor Beale, and Mr. Savory—and my own, there were *some points* of coincidence upon questions of importance.

Even if the contents of my volume had been published *for the first time* in January, 1861, I was at least three months in advance of each of those gentlemen in those points in which we coincided; and yet no allusion whatever was made to that coincidence.

Here was a National Society, of which I am myself one of the members, ignoring me as a physiological writer.

Why was this so?

I could only conclude that the gentleman who selects for that Society's volume had been led upon high authority to believe that in my volume I had done little more than "simply expressed in abstract terms what others have worked out in the concrete form," and consequently very naturally declined being at the trouble of perusing my writings.

It then occurred to me to make this appeal to physiologists.

Such, then, was the origin of my volume on Species. I shall now say a word on its fate, and in doing so shall point out two of the coincidences I refer to.

My volume on the Origin of Species was published on the 1st of January, 1861.

On the 2nd January, 1861, I did myself the honour of presenting (by post) a copy of that volume to the "Royal Institution of Great Britain," in Albemarle Street.*

The Royal Institution did not do me the honour of acknowledging the receipt of that volume.

A considerable portion of that publication had been devoted to the consideration of the question as to "the relation which subsists between the three kingdoms of Nature with regard to organization," and my views on that subject were concisely summed up in a tabular form which as a diagram faced the title page of my volume.

In the spring of 1861, Mr. Savory delivered a lecture in the Royal Institution (subsequently published in *The Lancet* for 8th and 15th June, 1861) "On the Relation of the Vegetable and Animal to the Inorganic Kingdom"—a question, as may be perceived by a reference to the diagram facing the title of this pamphlet, to which I had devoted a considerable portion of my volume.

* By the same post I had the honour of presenting copies of my volume to the Royal Society, the Geological Society, the Athenæum Club, the Medical and Chirurgical Society, the Linnean Society, and the Hunterian Society. All these distinguished institutions did me the honour to acknowledge the receipt of my volume. The Royal Institution of Great Britain did not do me that honour.

I direct attention to that lecture for the purpose of pointing to a close coincidence, upon two important questions, between Mr. Savory in 1861 and myself in 1848; and of submitting that since Mr. Savory's views upon those questions were original with that gentleman in 1861, it is obvious that here is another eminent physiologist who knew nothing in 1861 of those "hints or more formally expressed doctines" by which I had profited so largely in 1848, when venturing to submit the same views upon two of the most important inquiries in physiology.

Two questions, I say, of leading importance were introduced by Mr. Savory into that lecture—namely, 1st. as to the manner in which muscular fibre is formed; and, 2nd. as to the distinguishing features of life.

Two more important questions could scarcely engage the attention of physiologists.

On both these questions I had ventured, in the volume referred to, to express myself at considerable length, and on both these questions there was a close coincidence between Mr. Savory's views and mine.

I shall submit our respective views for the reader's consideration; and first as to the manner in which muscular fibre is formed.

In my publication of 1848, I had ventured—possibly erroneously—to express it as my opinion that what is termed "a nucleated cell" is in reality nothing more than a cytoblast enveloped in blastema.

In *this* opinion—which may doubtless be an error on my part—Mr. Savory does *not* coincide; in other respects there appears to me to be a close coincidence between Mr. Savory's description of the manner in which muscular fibre is formed and my own.

In summing up my observations (in 1848) upon the manner in which "the simplest conceivable vegetation," or, as I also there termed it, "the first link in the organizing chain," so acts upon inorganic elements as to convert them into an organized vegetable, I expressed myself in the following words:—"Do we not here, as a result of the operative action of the first link in our organizing chain,—do we not here, as a product of the functional operation of the simplest conceivable of vegetation, recognise the physiologist's far famed "*nucleated cell?*" [On Organization, (1848) p. 43,] pointing out how—as I conceived—the same was true universally of cytoblast, or, as I there termed them, "organizing atoms" and expressing it as my opinion, that such was the manner generally of the formation of our various organic tissues.

In my communication to the *Dublin Medical Press* in 1852, I entered *most minutely* into this question, contemplating it *specially* in relation to the very subject before us—namely, as to the manner in which muscular fibre is formed. My views on this subject were quoted at length in my volume on Species.

It will be borne in mind that—for reasons already explained—I used the terms "*muscular atom*" to express the cytoblast by which muscular fibre is developed; and the terms "specific stimulus or nutriment" to express what is understood by the term blastema. The following are the terms in which I expressed myself in 1852, upon the manner in which muscular fibre is formed—quoted in my volume on Species. "The muscular atom now presented, with its specific stimulus or nutriment, radiates thereon its own influence centrifugally and in every direction, thereby surrounding itself with an envelope," &c. * * * "and, 2nd. It generates an organized residual product named *muscular fibre.—Origin of Species*, p. 112.*

The following are Mr. Savory's views on the same subject in 1861. Having quoted the original opinion of Schwann on the subject, Mr. Savory expresses himself thus:—"The description which I will venture to give you of this process, as the result of investigation, is completely at variance with this one.

"In an early embryo, if a portion of the substance in which muscular fibre is formed be examined, free nuclei or cytoblasts scattered through a clear and structureless blastema in great abundance will be seen. The first stage in the development of striated muscular fibre consists in the aggregation and adhesion of these cytoblasts, and their investment by blastema so as to form elongated masses. In these clusters the nuclei are not at first generally arranged in a single series, but two or three, or even more, occasionally lie side by side in apparent disorder. Almost if not quite as soon as the cytoblasts are thus aggregated into these long masses, they become invested by the blastema, and this substance at the same time appears to be considerably condensed, so that the outlines of the nuclei become almost or completely obscured. The fibre thus appears to be irregularly cylindrical or somewhat flattened, with a rough and uneven surface. In some cases, before the nuclei come into contact, a layer of apparently condensed blastema may be already discerned forming around them, and this external investment, if not very carefully examined, will occasionally give them the appearance of nucleated cells."

And, again, further on, he observes:—"Muscular fibre is formed by the aggregation of cytoblasts, and their investment by surrounding blastema. No nucleated cells are concerned in any way in the process." Further on, adding:—"It appears to me that a nucleus thus simply invested by blastema has been sometimes described as a nucleated cell."—*Lancet, June* 8, 1861, p. 555.

Thus, although the terms employed to express our respective views are altogether different, I would submit that the views themselves pretty nearly coincide.

But the foregoing is by no means the more important topic upon which there is a still closer coincidence between Mr. Savory in 1861, and myself in 1848.

The other inquiry—upon which there appears to be a close coincidence of views—is perhaps the most important that can engage the attention of physiologists. I allude to the question as to the distinguishing features of Life.

I have already ventured to submit the views I advanced on that question in 1848, which I stated might be briefly expressed in the following terms, namely—That for the *organization* or formation of *one* organic entity there is necessity for a simultaneous *disorganization* or decay of another; so that during the manifestation of vegetative life *both* these processes—namely, that of formation and of decay—are of necessity *simultaneously* in operation.

I also submitted the *precise words* I employed in 1848, (see page 4) observing that I had then summed up the subject with the following general statement, viz.: "Thus are two essentially distinct and opposite

* Dublin Medical Press, 21st January, 1852.

processes of necessity concerned in producing the phenomena of active life; are of necessity in operation for the production of what we imply when we say of a thing "*it lives*," and thus, too, becomes apparent how death is essentially a part of life."—*On Organization*, (1848) p. 45.

In my publication of 1852 I expressed myself thus on this question—and observe the following passages were quoted in the volume which the Royal Institution declined doing me the honour to acknowledge.

"Organic or living creation has so been arranged or constructed by nature, that *the act* of giving development or manifestation to what are termed "*vital phenomena*" in general, has been inseparably associated with and made indispensably dependent upon *the depression or degradation of matter in the scale of organization*."*

Again, in directing attention to "the simplest conceivable vegetation," while conjointly with mineral elements it was manifesting life, I observed: "Upon a closer analysis of what was taking place during the reciprocal action of those two entities upon each other, we shall find that what *one* was *obtaining* the *other* was *losing*; we shall find that, at the same time that the process of the elevation of dead or inanimate matter to the organized condition was in operation, another and directly opposite process was also in progress; namely, the simple vegetation which was conferring that organization was itself undergoing the process of disorganization or degeneration, was itself *descending* in the scale of organization. When its function was finished, that vegetation was reduced to the condition of inanimate matter. In a word, when its function was finished, that vegetation was *dead*."†

I repeat it, both these passages were quoted in the volume which the Royal Institution declined to do me the honour to acknowledge.

Mr. Savory thus expresses himself on the same subject in 1861, in the the Institution referred to.—*Lancet*, *June* 15, 1861, *p.* 580.

"I would venture, then, to speak of life as being essentially a state of dynamical equilibrium; as consisting fundamentally and universally in a definite relation between destruction and renewal—in a regulated adjustment between waste and repair, whereby the condition is maintained notwithstanding constant change. * * * * * Waste or destruction is a necessary, an inevitable condition of the manifestation of life. It is involved in every vital act. And the power of compensating for this waste or change, the repair or reproduction necessary to the continuance of life, involves that of assimilation—that is, the power of converting foreign matters into the structure of the organism. In other language, the power of appropriating food. * * * * * But in life there is the constant and concurrent operation of these two processes. Both actions are involved in the idea of life, whereby it is distinguished from mere change on the one hand, and from repair on the other. Thus while inorganic and dead organic matter tend to a state of statical equilibrium, during life the equilibrium is the result of opposite forces."

Mr. Savory goes on (be it observed, in 1861) to add: "Nor does it appear to me that we can at present safely venture further than this."

Cannot safely venture further than *this* in 1861! Why *this* is *the identical extent* to which I had ventured in 1848, when I was told by the lead-

* Dublin Medical Press, 21st January, 1852. Origin of Species, p. 121.
† Dublin Medical Press, 21st January, 1852. Origin of Species, p. 125.

ing physiological journal in England, that I had done little more than "simply expressed in abstract terms what others have worked out in the concrete form."

That suggestion had been repeated, I suppose, a dozen or so times in the volume that the Royal Institution had declined to do me the honour to acknowledge. It had been extensively illustrated by its application to the very question that had just engaged Mr. Savory's attention—namely, the formation of muscular fibre—and it had been three times distinctly illustrated in the diagram facing the title page of that volume—a diagram devoted exclusively to the identical subject upon which Mr. Savory lectured.

I would submit, that when an author preludes an announcement with the words, "I would venture to speak," it is manifest he is giving expression to something which has originated with himself, and which, at the time he advances it, *he* at least believes to be "novel." Otherwise, there can be no meaning in the terms. There can be no great *venture* in a man's speaking what another man has spoken before, provided the latter be a recognized fact.

Therefore, I submit that Mr. Savory has proved that, at least in *his* opinion, my views in 1848 *were* "novel."

I, an obscure Irishman, unknown to physiologists, might easily be supposed capable of *anything* [should it happen to suit the convenience of *any one* so to represent]. But here is a gentleman, doubtless well known to physiologists to be incapable of a *shadow* of what's wrong.

Here is a gentleman residing in the very focus of science—intimate, it may be, with the leading physiologists of the age—who has doubtless frequently during the last thirteen years reflected on this question, and is no doubt familiar with all such hints or more formally expressed doctrines as exist upon the subject.

Here is a gentleman who doubtless would be appalled at a shade of imputation on his honour, and would no doubt shrink from the appropriation of the shadow of a hint or of a more formally expressed doctrine of another.

Here is a gentleman who I presume would pale at the bare thought of simply expressing in abstract terms what others have worked out in the concrete form, and submitting *such* to the public as "*novel.*"

How then, I would ask, does it happen that when Mr. Savory enunciates *his* views in 1861, he makes no allusion to those "hints or more formally expressed doctrines" by which *I* had so "obviously profited largely" in 1848, when giving expression to similar views.

I ask physiologists and the gentlemen of the press if *this* be not oppressive injustice?

I would ask permission to add a word on the definition of "*living*" I ventured to submit in 1848.

In speaking of "*living,*" I speak exclusively of that life which is common alike to the animal and the plant—the universal life of all organic creation—namely, what is termed organic or vegetative life. The phenomena of *animal* life pertain to another and distinct branch of inquiry.

The reader will please to bear in mind, that—if my views be correct—so long as the phenomena of "living" are being manifested, the two processes already referred to—namely, that of formation and of decay—are incessantly simultaneous in operation.

It will then be observed that these two distinct and opposite processes—namely, that of formation and of decay—which are inseparably associated and simultaneously in operation during the manifestation of vegetative life—it will, I say, be observed that these two distinct and opposite processes give rise to two distinct and opposite results—namely, to the *elevation* of matter on the one hand, and to its *depression or degradation* on the other, in the scale of organization.

The formative process is *elevating* one portion of matter in the scale of organization at the same moment that the process of decay is depressing or degrading another; and—during the combined operation of both—manifestation is given to vegetative life. During the manifestation, then, of vegetative life, *matter is in the act of being elevated in the scale of organization;* and during that manifestation, matter is also in the act of being degraded or depressed in the scale of organization.

Such, then, are the two results of the operation of these *combined processes* of formation and of decay—namely, the *elevation* and the *depression* of matter in the scale of organization.

I would venture to submit that the former of these two results—namely, the *elevation* of matter in the scale of organization—was *the* result of the two for the accomplishment of which nature—if I might so say—had combined these two opposite processes. Nature's design—if I might so express myself—in combining the process of formation and of decay—was *the elevation of matter in the scale of organization;* and the *depression* of matter in the scale, which simultaneously takes place, is but *contingent* on that design.

The design was the *elevation* of matter—the contingent to that design its *depression.* The end to be obtained was *living;* the means to accomplish that end was "dying."

The *END* was *formation*, the *MEANS* was *decay.*

Induced by these considerations, I ventured in 1848 to submit, as a definition of "*living,*" "the act of being elevated in the scale of organization."

I submitted that as a definition of *living* in contradistinction to dying—namely, the *contingent* process of depression or decay which is essentially simultaneously in operation; and I ventured to observe—although smiled at, at the time—that it thus became apparent how "death is essentially a part of life."—*On Organization*, (1848) p. 45.

Mr. Savory, as I have pointed out, has done me the honour to coincide with me in many important points; but that gentleman is careful to inform his readers that there *are* points in which he does *not* coincide, and one of those points is this question of *definition.*

Having so modestly expounded his lucid exposition of life, Mr. Savory is careful to acquaint his readers that *he* would not presume to exhibit his own ignorance in any attempt at a *definition* of life—no, that *presumption* may doubtless be left with an Irishman.

Mr. Savory upon this subject observes, "there is no need to exhibit one's ignorance in any attempt to define life;" adding, further on, "it will be observed that this is no pretence towards a definition of life. "It is only an attempt to distinguish life *by its essential features when reduced to its simplest condition.*"

I have marked the foregoing words in italics, for the purpose of observing that what Mr. Savory's views may be as to what are the requisites to constitute *definition* I know not; but I do know what were Aristotle's—namely, "proximate genus and essential difference." If then, "life reduced to its simplest condition" plus "its essential features" be not *Aristotelic* definition of life, I confess myself at a loss to comprehend the meaning of terms.

At the moment that I was exhibiting my own ignorance in attempting a definition of "*living*," I had been contemplating life in its "simplest conceivable" condition, and submitted, as its essential features in that condition, "the act of being elevated in the scale of organization."

Fourteen years have since then rolled by, and yet—notwithstanding the exhibition I thus make of my ignorance—I would again respectfully venture to submit to physiologists, as a definition of "*living*"—the act of being elevated in the scale of organization. I would respectfully submit that wherever that act is in operation, *there* and there *only* is *living*—that is, organic or vegetative living.

I shall not intrude further on the reader with this subject, but it is one upon which I entered *at great length* in my volume on Species, and illustrated in a great many ways.*

I shall in conclusion briefly recapitulate the foregoing remarks, and earnestly submit them to the decision of physiologists and the press.

Fourteen years have elapsed since, with diffidence, I came forward as a physiological writer, to submit original views upon questions of interest.

No notice was taken of the more important of those views.

Two years subsequently I republished them, but with a similar result; no notice was taken of any proposition I advanced. Propositions of importance, which I had proposed in 1848, I subsequently saw from time to time advocated by others; but, conscious of the insurmountable obstacles I should have to contend with, should I lay claim to them, I remained silent.

Among other suggestions, twice submitted to the public and both times unnoticed, was one on the most fundamental question in physiology—namely, the question as to the nature of the phenomena *essentially present* during vital manifestation.

Several years subsequently I have seen that suggestion of 1848 gradually become widely and extensively recognised. I have seen it adopted by several leading physiologists both in Europe and in America. I have seen it pronounced, by an able physiological writer, "the foundation and key-stone of physiology regarded as a science." And I have seen myself *everywhere* excluded from the slightest participation in it. I consequently abandoned physiology.

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* As it is quite manifest Mr. Savory never did me the honour to open my volume, it is obvious that gentleman could not do me the honour to allude to so obscure an individual *by name*. But inasmuch as Mr. Savory dwells with some emphasis upon this exhibition of ignorance, and "pretence towards a definition of life" it is at least within the range of possibility that that gentleman may have collected *from some source or other*, that some presumptuous Irishman *had* made such an exhibition of his own ignorance; and *did* make that pretence towards a definition of life, which Mr. Savory with such modesty declines.

An accident induced me once more, after many years' silence, to republish. I again reiterated and illustrated that suggestion at great length.

Again, it is *everywhere* unnoticed.

A Royal Institution, one of whose objects is "to further scientific research," declines to acknowledge the volume in which that suggestion is reiterated. In the same Institution, three months subsequently to its publication, a gentleman of distinguished physiological attainments adds to his high reputation by enunciating that very suggestion submitted by me thirteen years before. Nay, more, at the very moment that eminent physiologists are acquiring increased reputation by enunciating propositions contained in my volume, I see that volume itself—even at the distance of Madras—held up to derision and mockery!

I appeal to physiologists and the press, and ask if this be not oppressive injustice?

And why is this so, for there must be *some* cause?

What is that cause?

Is it because I am friendless, with no British journal to defend me?

Is it because, an Irishman, I dared to express original views, and then dared to maintain their originality?

Is it because I have been pronounced by the *British and Foreign Review* an abstractor of others' concrete?

Is it because I have had the boldness to express a manly indignation that an anonymous writer should not only so misrepresent the *general character* of my publication as that no physiologist would read it, but should at the same time insinuate a dishonesty of purpose?

Is it because I have declined to acknowledge imaginary obligations which I have proved to have no real existence?

Or is it because I presumed to express opinions of my own which at the time did not coincide with those of others?

I appeal to physiologists, and ask if in their opinion any or all of these causes be an *adequate cause* to justify a retiring unòbtrusive man in being scared for ever from scientific pursuits. For, be the cause what it may, the result has been that.

Although no longer the young man of ambition and hope that in 1848 looked sanguinely forward to a generous reception from the *British and Foreign Review*, still again, though the youth now no longer exists, I once more—after the forced retirement of so many years—feel the same ardent attachment to physiology as ever.

But the ambition and hope are no more—they have been dispelled by the facts just detailed.

For the third time—forced to abandon the pursuits of my taste—I feel compelled once more to withdraw into retirement. But I could not *now* do so—with becoming respect for myself—without submitting the foregoing to physiologists. The opposition against which I appeal has now become *too widely* extended to admit of my longer being silent. I could not now retire without placing upon record my protest, and stating that I do so not from choice but by compulsion.

My views are uncompleted, and I *dare* not now pursue them to completion, I dare not even lay claim to what is already my own. I abandon physiology because I feel that an Irishman and friendless, with no one to look to, I am powerless to resist the tide of opposition that awaits me should I ever again dare to publish. I do so because I see that everything *of merit* I put forward at once becomes the property of others—my name being altogether ignored; while my defects, which are numerous—and, it may be, my errors—are diligently sought out to be held up *with my name* to derision.*

Is not this an oppressive injustice?

H. FREKE.

Dublin, 28, Holles-street,
October, 1862.

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* It again becomes indispensable, as a verification of what I state, to submit at least one instance, out of many, as evidence of the injustice above referred to.

In the number of the *Madras Quarterly Journal* for October, 1861, there is contained a notice of my volume on species.

That notice, the editor acquaints his readers, was "favoured by a distinguished contributor."

In order to show the animus of that contributor towards my volume it will be necessary to say a few words in explanation.

I have already observed that one of the expressed objects of my publication was to point out a "striking analogy which I conceive may be recognized between certain of the general leading principles upon which the two great divisions of nature appear to have been originally constructed—that is, the analogy (as regards their *constitution* and *the manner of their formation*) which, I conceive, may be recognized between the objects, respectively, of the inorganic and the organized world, or of inert and of living creation."—*Origin of Species*, page 65.

One of the points in this supposed analogy upon which I dwelt at considerable length was the following. Speaking of the two great divisions of nature, I observed:—

"The former of these divisions, or the unorganized world, is composed, or as it were built up, of a limited number of what we term *distinct species* of mineral matter; to which species respectively (counting about sixty in all) have been given the names, carbon, oxygen, hydrogen, nitrogen, &c. Each of these distinct species of mineral matter has so been originally created as that it shall comprise under it—or, in other words, as that the entire species collectively shall consist of—an incalculably vast number of inconceivably minute particles, which particles, from their supposed physical indivisibility, have been designated *mineral atoms*. By the union of such atoms in various forms of combination (in obedience to what we term their chemical and physical laws) has the inorganic world, as it now exists, been constructed. Thus, then, the vast, the almost countless forms or varieties of mineral matter which are found throughout inorganic creation *have been formed by the union, in various forms of combination, of atoms of a comparatively limited number of distinct species of mineral matter.*

"Such, my readers are aware, are the opinions now generally entertained as regards the constitution of the inanimate or unorganized world. Such, it is my opinion, is also the case as regards the constitution of the *animate* world, or world of organization. The same, in a word, I have endeavoured to show, has also been nature's arrangement in the construction of *living* or *organizing* creation. It, too, (as has been my effort to make apparent), is composed, or as it were built up, of a limited number of distinct species of *organizing* matter; and each of those species has, as I conceive and have endeavoured to prove, been originally so constructed as at *present* to comprise under it an incalculably vast number of microscopically minute granules, to which granules, in consequence of their physiological indivisibility, I have ventured to give the name *organizing atoms.* Thus, then, the vast, the almost countless forms or varieties of *organizing* matter (that is, in other words, of *living beings*) found throughout organic or living creation, have, as it appears to me, been formed, or developed, as the result of the functional action of *a union (in various forms of combination) of organizing atoms of a very limited number of distinct species of organizing matter.*"

To establish this supposed analogy—whether successfully or otherwise is not now the question—was one anxious object of my publication, and, as I have already observed, was one of the chief motives which induced me to adopt the expression, "organizing atom."

With this explanation the reader can at once perceive the animus with which the contribution now referred to was written.

The distinguished contributor to the *Madras Journal* commences his contribution with the statement that he had "read the book honestly, from cover to cover;" consequently he must have read the passages I have just quoted, and must therefore have been perfectly aware of the analogy I was desirous of establishing.

Having previously turned my publication into the greatest derision, the distinguished contributor goes on to observe:—"In his book, high-sounding forms of words and terms grandiloquent are met with, yet in almost every page impenetrable obscurity prevails. Let us take for example the following."

The distinguished contributor then withdraws a passage from all connexion with its context, giving it isolated and alone, without making the slightest allusion to the analogy to which it related, and without a knowledge of which the passage was by itself altogether unintelligible.

The passage quoted is as follows:—

"If, having made ourselves familiar with inanimate creation, with its atoms, its compounds, its worlds and their systems; if, having learned of its attributes and laws, it were announced to us that some researcher in science, having recognized a new species of creation, having seen matter under aspects hitherto unobserved, had attained to the discovery of a new class of compounds—of compounds possessed of symmetrical form—should we not, making analogy the guide of our reason, be led to attribute this new class of compounds to the operation of their attributes, general and specific, in an hitherto unrecognized or NEW CLASS OF ATOMS."

Here the quotation stops short, leaving my meaning of course unintelligible. The two paragraphs which *directly succeed* the passage quoted are as follows:—

"Such experimental researcher is mankind at large; experience presents all with this new class of *compounds* in the varied departments of an organized world.

"As we proceed, I shall endeavour to make it appear that such new class of *atoms* does in reality exist in the form of *physiologically* indivisible organiz*ing* agents; I shall further endeavour to show that every *embryonic germ* in existence is composed of one or more such atomic, or physiologically indivisible organiz*ing* agent; and that all such embryonic germs (or organiz*ing* atoms) on having completed the discharge of their physiological function, have, *as that function*, develop*ed* the various new classes of *compounds* referred to, namely, in other words, the various vegetables and animals to be met with throughout organic creation."

I shall merely observe on the foregoing, that here was a review of my publication furnished by some gentleman of distinction. He "had read the book honestly from cover to cover," and consequently must have met those passages which coincided with the views which, at the very moment he was honestly reading me from cover to cover, were adding to the high reputation of eminent physiologists. Being "*distinguished*," he could be at no loss to recognize their importance; but he prefers to pass them by unnoticed, and seeks for some passage with which, by isolating it from its context, he may succeed in making the author appear ridiculous.

I would ask the reader what *possible* motive—other than the degradation of an author in public estimation—could any man of distinction have in sending such a review to Madras?

I ask the reader if this be not conclusive evidence of the existence of the opposition I have referred to? And it is but a solitary instance selected from many of the same kind.

Thus, at the same moment that distinguished physiologists are acquiring increased reputation by enunciating propositions contained in my book, other men of distinction are destroying my reputation by making that book appear absurd.

Does not this amount to persecution?