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REVIEW

OF

DR. CALDWELL'S PAMPHLET,

ENTITLED

PHYSIOLOGY VINDICATED,

IN A CRITIQUE ON LIEBIG'S ANIMAL CHEMISTRY.

BY ROBERT PETER, M. D.

Professor of Chemistry and Pharmacy in Transylvania University.

CINCINNATI:

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1843.

REVIEW.

Remarks on a Pamphlet, entitled "PHYSIOLOGY VINDICATED *in a Critique on Liebig's Animal Chemistry.* By CHARLES CALDWELL, M, D, Jeffersonville, Ia, 1843." 8vo. pp. 95. By ROBERT PETER, M. D, Professor of Chemistry and Pharmacy in Transylvania University.

THE avowed design of the author of the pamphlet in hand, as given in his preface, is "*conservative* rather than *promotive*—to prevent the science of physiology, in whose behalf it was conceived and resolved on, from being *injured and degraded*, rather than actually to IMPROVE and ELEVATE it;" and the belief that its publication will *not* tend to improve and elevate knowledge in general, or any branch of science in particular, but that it will rather injure and degrade; with the conviction that it is my duty to oppose, to the extent of my abilities, all such tendencies, from whatever quarter, is the strong reason which urges me to offer the following remarks to the medical public.

In undertaking, in the present instance, the disagreeable task of exposing error, many motives are presented to induce me to prefer the ease of silence. The venerable age and acknowledged standing of the author; the untiring ability with which he wields his pen, and the most ready use of argument to sustain his positions, so as often to make the "worse *appear* the *better* reason;" the consideration that the work which he attacks in the present pamphlet, cannot be put down by a mere clash of logical arms, by the most ingenious mis-statement of its propositions, nor the strongest array of perverted or misquoted facts; the belief that any man of sense, or of clear unbiassed judgment, who had studied the productions of Liebig, would at once, without my assistance, perceive the injustice done to truth, logic, and that author, in the pamphlet of Professor Caldwell; and lastly, but not least, the fact, that, in the performance of the task I have assumed, I shall be obliged to convict the Professor, not only of practical adherence to the old mode of philosophizing, namely, that of the school of Aristotle, but also of wilful or ignorant misconstruction of facts and

arguments; and, what is more disagreeable, I shall be forced to expose, in his production, an amount of ignorance of science in general, and even of physiology, whose cause he undertakes to *vindicate*, and of which he has been professedly a teacher for so great a number of years, as would disgrace a tyro, and must appear incredible to the common observer.

To this array of motives and arguments, tending to withhold me from the publication of these remarks, the answer is clear and satisfactory: Truth is more venerable, exalted and powerful than the Professor, and error must be continually unmasked, to prevent her from imposing herself upon those who have not the time, inclination or ability to look beneath the surface of things. The respect which I, in common with many others, entertain for the venerable Dr. Caldwell, cannot excuse me for the desertion of her standard; and the apparent want of courtesy, which may be supposed to exist in my animadversions, must be excused in the necessity of the case. Truth must be promulgated and sustained; and, if those who oppose it are offended, the fault is only with themselves.

Courtesy, indeed, is not challenged by my opponent, as the following quotations from his pamphlet will prove, which are as uncourteous in manner, towards both the author and the subject which he opposes, as dogmatical in character.

"So unlimited in its ambition and rapaciousness, has been the spirit; under which the aggressions referred to have been committed, as to indicate a determination to usurp dominion over the whole philosophy of living organized matter, and consign it to the keeping and control of chemistry, and, of too many, who *pretended* to be physiologists, this determination received the approval. Such pretenders, therefore, were prepared to submit to chemical domination, and to remain content in passive obedience and all its concomitant degradation."—*Preface, p.* 5. * * *

"During this state of expectancy and inaction on my part, a new foe to the *philosophy of life* appeared in resolute and vigorous operation, in the person of Professor Liebig, much more formidable than any of his predecessors. Without any inordinate extravagance of hyperbole, well might he, in his relation to the science of living organized matter, be likened to Attila, of whom, under the soubriquet of the "*Curse of God*," it was figuratively declared by him, that, beneath the hoofs of his war-horse, the very verdure of the earth where he trod was blighted. So certain, sudden, and numerous were his triumphs, that the "*Veni, vidi, vici*" of Cæsar seemed

fitly enough to characterize the sweep of his achievements. He had but to speak, and all were enchanted—but to proclaim his dogmas, and they were swallowed as pearl-drops from the fountain of truth."

"By those who are strangers to the circumstances of the case, it will be very naturally supposed, that this immense flood-tide of popularity and influence, accompanied by such unprecedented success in the work of proselytism, must have been owing to the uncommon merit, scientific or literary, or both, of Professor Liebig's volume on Animal Chemistry. No such thing! His success was attributable almost entirely to two other very different causes—the high reputation, whether solid or not, which he had already acquired by his work on Agriculture, and to the bold and confident tone and manner in which he made known his statements, calculation, and opinions."—*Preface, p.* 7.

Commenting on some statements of Professor Liebig, he indulges in the following tirade, viz.:

"As relates to this point, we do not pronounce the Professor an *ignoramus*, because the term would be unbecoming in us. But, were we to do so, it would puzzle his ablest and most ingenious friends to falsify the charge.* And, though we do not say that a blunder on a single point, however disreputable, proves him to be a blunderer in every point, yet we do say, that the multitude of groundless assertions, which he has wantonly and dogmatically made, has, with us, greatly impaired his authority—not to say his *credibility*—as a writer."—*p.* 71.

All who are conversant with the writings, or who have listened to the prelections of Dr. Caldwell, know that one of his prominent peculiarities —and he has several—is his strong and unceasing opposition to what he terms the encroachments of chemistry into the dominions of physiology. To prevent these, he has wielded his most pointed wit, his scorching sarcasm, and the sharpest weapons of his logic. This fear has been the phantom which has haunted him for almost half a century, until his imagination has given to it a reality which terrifies him from his propriety.

While chemistry was yet young, and its disciples had accumulated but comparatively few facts to sustain it, the logic and ridicule of the Professor was doubtless successful, if not in checking its progress, at least in depriving it, in the opinion of many, of the honor which belongs to it.

*This reminds me of the Irishman's speech: "I love a liar—but you please me too well—not saying that you are one."

The phantom, held between the horns of most logical dilemmas, and pierced by the keenest ridicule, often apparently yielded to the prowess of its adversary: in other words, the "crest-fallen *chemicalist*" adopted "the expediency of retiring from the place of his own defeat, the downfall of his doctrine, and the withering of the bay-wreath his fancy had woven."†

It has been his fate, however, to meet still other Richmonds in the field—his task to slay again the slain, like that of the immortal Falstaff in his combat with the Percy. The weapons, once so keen and powerful in his hands, which, wielded by his skill and dexterity, had so often made him victor in the lists, no longer avail him against the horrid spectre of his fancy.

Some of these weapons will be recognized in the present pamphlet, by all who know him, in the account of "*chemical man-making*," the "*goose-egg* proposition," &c., &c.—stale jokes, at best—based on a monstrous misapprehension of the real end and aim of organic chemistry, which can no more check the progress of discovery, than the wavings of a fan can stay the onward rushings of a tornado!

The design of the late publications of Professor Liebig, as far at least as I am able to make it out from the words and tenor of the works themselves, is, simply to recommend the application of the new and improved processes of chemistry to the ascertainment of the *chemical relations* of vital phenomena; and, by applying the balance, *i. e*, quantitative analysis, to physiological research, to point out a new and comparatively untrodden path to the discovery of the true nature of the vital laws and processes. The formulæ, equations, and theoretical views, which form the body of his "Animal Chemistry," are to be taken only as exemplification of the method he proposes, and of some of the facts which have already been established; and are not—as the author states in his preface—"to be viewed as ascertained truths, and furnishing a complete, or the only explanation of the vital processes treated of,"—but as "yet true in this sense; that, being deduced from facts by logical induction, they must stand as long as no new facts shall be opposed to them." The critic, therefore, evidently misconstrues, or misunderstands the tenor of the publications of Professor Liebig, when he accuses him of dogmatically asserting the supremacy of chemistry over what he is pleased to term physiology, or in the slightest degree of aiming at *chemical man-making*.

†Preface, 15.

The suggestions and illustrations of Liebig have been received with almost universal approbation by all who are prepared to comprehend the subject; and by more than one distinguished philosopher it has been declared, that they are destined to "mark the commencement of a new era in Physiology." This declaration is met by Professor Caldwell in his present pamphlet, by a most unqualified negation; and by a most acute and *logical* deduction from *his own peculiar definitions and propositions*, is, to his own satisfaction, proven to be utterly unfounded. The Doctor's logic and temper, on this and other points, strongly remind us of some of the ancient worthies, whose memories are preserved to eternal fame in the history of the progress of discovery. We are told by his talented biographer,* that when Galileo published his discovery of the satellites of Jupiter, its importance was at once felt and acknowledged by both his friends and enemies; yet it really was denied by more than one whose old prejudices and envious feelings destroyed the evidence of their senses and their reason. "A protege' of Kepler's, of the name of Horky, wrote a volume against Galileo's discovery, after having declared, "*that he would never concede his four new planets to that Italian from Padua, even if he should die for it.*" This resolute Aristotelian was at no loss for arguments. He asserted that he had examined the heavens *through Galileo's own glass*, and that no such thing as a satellite existed round Jupiter." Kepler, however, forced him to see them. More obstinate was the principal professor of philosophy at Padua, who "resisted Galileo's repeated and urgent entreaties to look at the moon and planets through his telescope; and even labored to convince the grand duke that the satellites of Jupiter could not exist." But the glory of the most *logical* refutation of this discovery was reserved for Sizzi, an astronomer of Florence, who maintained that as there were only *seven* apertures in the head—*two* eyes, *two* ears, *two* nostrils, and *one* mouth—and as there were only *seven* metals, and *seven* days in the week, so there could be only *seven* planets." Forced, however, to admit the visibility of the four satellites through the telescope, he argued that, "as they were invisible to the naked eye, they can exercise no influence on the earth; and being useless, they did not exist!"

That the reader may not be forced to rely on my assertion, in regard to the character of the logic of Professor Caldwell—in his disbelief that the work of Professor Liebig can possibly "mark the commencement of a new era in physiology"—I will endeavor to give his syllogisms, as clearly

* Dr. Brewster in his Martyrs of Science.

as possible, divested however, of some of the verbosity in which they are shrouded in the pamphlet.

These, then, are his logical positions:—1. Physiology treats of the state, action, and products of living organized matter; 2. Chemistry treats exclusively of unorganized matter; 3. Therefore, Liebigs Animal Chemistry can never mark the commencement of a new era in physiology. Again: 1. To improve in physiology, new facts must be discovered, or new applications of facts, or both, in strict conformity to *vital* laws, (*i. e.* new animal functions, or a new exposition of these functions). Professor Caldwell *says* that he *will prove* that Liebig has done neither. 3. Therefore, his Animal Chemistry cannot mark the commencement of a new era in physiology.

In further "evidence of the soundness of his views," the Professor gives this additional syllogism:—1. Improvements in physiology cannot be made in the laboratory of the chemist; 2. All Professor Liebig's improvements are chemical; 3. Therefore, etc, etc, as before.

That these propositions are correctly given from the author of "Physiology Vindicated" may be seen by reference to pp. 3, 4, 5, 6, and 7 of that work.

It must be evident to every one, however, that these syllogisms require the clear definition of what constitutes a *vital law*—in order that it may be made evident when and where the daring and presumptuous chemist, shall invade the territory of physiology; or whether or not he may not stumble on a discovery in physiology. Professor Caldwell is obliged to acknowledge his utter inability to supply the test required:—

"Were any one to ask us what we mean by the phrase *vital laws* or a *vital force?* we should be unable definitely to answer the question; because of vitality—its effects alone excepted—nothing is known. We *might* reply, however, that by the expression, we intend to designate a source or power of action, differing in its principles, manner, and products, from every other power. We might also pronounce it of a higher order than any other earthly power inherent in matter."—*Phys. Vind. p.* 6.

This unfortunate deficiency destroys the whole force and power of the argument of the critic, and until it is supplied, Professor Liebig and other chemical analysts and observers may materially improve physiology—even the physiology of Dr. Caldwell—by proceeding on a principle of exclusion in their discoveries. Every chemical or physical law, force, action, or product, which they have demonstrated to exist in the organic world, will be

a difficulty removed which was in the way of a clear comprehension of *vital laws* and *forces*; and when they shall have discovered and separated *all* chemical and physical laws and action from living organized matter, all that may remain will be PURE VITALITY; and thus a most important service will have been rendered to the "*vital physiology*" of our critic.

In opposition to this view of the case, it will not do to join Professor Caldwell in his denial that *any physical* or *chemical* law or force exists, or is operative in the animal or vegetable economy—indeed the logic by which he attempts to sustain this position is as flimsy as can possibly be imagined:

* * * "It is to be remembered that both chemistry and vitality work with matter in its smallest divisions—we mean in its *atomic condition*. If therefore, they be both at work in the *entire mass* of the same organ at the same time, they must be also, at the same instant, both engaged in acting *on* and *with* the same individual particles—each in conformity to its own laws; and if these laws are *different* and even *contrary*, (as they most assuredly are), then must the strife engendered be necessarily fatal to the operations of both."—*Phys. Vind. p.* 53.

This supposed conflict, and mutual destruction of dissimilar forces, or, as he terms them, *laws*, can no more be admitted than that the vital forces of a living body necessarily destroys the attraction between the matter of that body and the mass of the earth. To say that it is mastered and controlled by vitality, is also to admit that it not only exists but is in a state of subordinate activity. Such an admission is clearly made by the Professor in the following paragraph:—

"Here it is obvious that no two causes or principles of action, different from each other, are employed at the same time, to produce any one specific effect. On the contrary, not only each cause, but each individual organ is used, as an instrument, under permanent laws, to produce the special effect for which it is designed and fitted."—*p.* 54.

One of the greatest difficulties, in the mind of our critic, in the correct-application of certain facts, is his fixed and pertinacious adherence to certain figments of Aristotelian logic, which he reveres as undoubted axioms, and, by which he persists to test them—such obstacles continually retard the progress of the Baconian philosophy.—The principal of these, which he denominates a fixed law of causation, is the fact, that two different causes are never productive of the same effect.

Now we will not pretend to deny or to affirm the truth of this as an

abstract metaphysical proposition, but do affirm, that it would be as difficult to *apply it to natural phenomena*, as it would be to demonstrate to the senses a mathematical point. This law of causation may be an axiom with respect to *simple effects*, but who ever saw a simple effect, or is able to decide whether an effect witnessed, either in the living body, or in dead matter, may not be a complex one, and thus have been produced by *several causes*? Indeed, notwithstanding the Professor's challenge to his opponents to produce a single effect in nature which was the result of more than one cause, the latter half of the paragraph, last quoted from his pamphlet, (from p. 54) renders the trouble unnecessary; for in that he clearly admits the existence and action of *subordinate* causes, in what he has denominate a *special* or simple effect.

These examples of the critic's mode of argumentation in the support of his dogmata, cannot fail to excite the astonishment of all who read them—coming, as they do, from the production of a veteran and talented teacher of the institutes of medicine, in the middle of the nineteenth century. They are, however, stated with rigid justice and are but a mere specimen of the work before us; the spirit and matter of which, while they will bring no new honor to the author, will convey impressions abroad, in relation to the condition of science in the great Valley of the Mississippi, which are both humiliating and erroneous.

Not the least prominent trait, of the "critique" before us, is a wilful or ignorant misconstruction of the sense of the author criticised.

Professor Liebig, in the commencement of his Animal Chemistry, p. 1, recognises the existence of a remarkable force in a state of *rest*, in the animal ovum as well as in the vegetable seed—which force is called the *vital force*, *vis vitæ*, or *vitality*—and he attempts, subsequently, to give some of its *conditions* when in the *active state;* as, for example, its absolute dependence, in this relation, on nourishment and the constant absorption of the oxygen from the atmosphere.

Vital *activity*, as is well known, cannot exist without these two adjuvants. In the precise paragraph of Liebig—which cannot, however, be fairly estimated separate and apart from its context—"All vital activity arises from the mutual action of the oxygen of the atmosphere and the elements of the food."—*Animal Chemistry, p.* 9.

The proper construction of this passage is this;—when the vital force is brought into the state of *activity*, in animals, food and oxygen are the [illegible] *causes* of that activity. Professor Caldwell, how-

ever, determined to force the German chemist into the assertion that *chemical force* is the *cause of vitality*, changes the sentence, in the following unjustifiable manner by the use of italics.—*Phys. Vind.*—"All *vital* activity arises from the *mutual* action of the *oxygen* of the atmosphere and the *elements* of the food."

Had any member of this sentence been italicised by its author, it would undoubtedly have been the word *activity*, which is clearly the emphatical one of the isolated passage. But Dr. Caldwell is not even satisfied with this perversion, he proceeds to torture, still farther, the mangled sentence, and in order to extract from it an *inquisitorial confession*, he subjects it to yet greater strain and dislocation in the following comment—*Phys. Vind, p.* 8: "All vital activity, (*i. e.* the power to act)," etc, etc.

Most astute and logical commentator! Could you but force the words *vital activity* in this sentence, with its context, to mean *power to act* in the most complete sense of the word, and thus exclude the vital force entirely from the action, then would you succeed in convicting the truculent chemist of high treason against *vital physiology*. But this cannot be done, for it is evident, that taken with the propositions which precede it in the work of Liebig, the word *activity* in this sentence means the *demonstrations* of power, and not the power itself—*actions* and not the *power to act*.

In the same desperate attempt to *distort*, in order to gain an occasion to combat, other sentences have been improperly italicised by the critic, with as little credit to himself as advantage to science; as, for example, the following, in which the italics are entirely the addition of Dr. Caldwell—*Phys. Vind, p.* 9:—"In the processes of nutrition and reproduction, the *ultimate causes* of the different conditions of the *vital force* are *chemical forces*."

The legitimate sense of this proposition—which, according to the previous declaration of its author, is not to be viewed in the light of an established fact, but merely as the representation of our knowledge on the subject—is, that "the *ultimate* causes of these different *conditions* of the vital force are chemical forces. In other words—chemical forces are the *last causes*, [illegible] in the chain of causation, of the different *conditions* of the vital force. The existence of the *first* cause of vital actions, namely, the *vital force*, having already been clearly stated, in the first proposition and elsewhere, in the Animal Chemistry, nothing but a want of capacity to comprehend the connection of a series of propositions, or unpardonable inattention, can clear Professor Caldwell from the charge of having wilfully perverted the sense of the author, and, as far as he

could, changed the character of isolated sentences, in order to vilify where he is unable to refute.

Not a little ingenuity, as well as such a want of acquaintance with well ascertained physical and physiological truth as is unpardonable in a person of his elevated station, is displayed by the critic in his opposition to the views supported by Professor Liebig on the subject of the proximate causes of Animal heat. The nature of these may be understood in the following passages from the Animal Chemistry, p. 17.

"All living creatures, whose existence depends on the absorption of oxygen, possess within themselves a source of heat independent of surrounding objects.

"This truth applies to all animals, and extends, besides, to the germination of seeds—to the flowering of plants—and to the maturation of fruits."

"It is only in those parts of the body to which arterial blood, and with it the oxygen absorbed in respiration, is conveyed, that heat is produced. Hair, wool, or feathers do not possess an elevated temperature.

"The high temperature of the animal body, or, as it may be called, disengagement of heat, is uniformly and under all circumstances the result of the combination of a combustible substance with oxygen.

"In whatever way carbon may combine with oxygen, the act of combination cannot take place without the disengagement of heat. It is a matter of indifference whether the combination takes place rapidly or slowly—at a high or at a low temperature; the amount of heat liberated is a constant quantity."

In the following passages—quoted by Professor Caldwell, from the Animal Chemistry, p. 20—a mere *comparison* is made for the sake of illustration; which is altogether proper: for no one will contend that different objects, which happen to possess some common points of resemblance, are *identical* in every respect; nor can any system of correct reasoning bring us to the conclusion, drawn by the critic, from these and similar passages, that it is denied "that *vitality* has any shadow of agency in the matter." This is the quotation in question:—

"It is evident, that the supply of the heat lost by cooling is effected by the mutual action of the elements of the food and the inspired oxygen, which combine together. To make use of a familiar, but not on that account a less just illustration, the animal body acts, in this respect as a furnace, which we supply with fuel. It signifies nothing what intermediate forms food may assume—what changes it may undergo in the body, the last change is uniformly the conversion of its carbon into carbonic acid, and of

its hydrogen into water; the unassimilated nitrogen of the food, along with the unburned or unoxidized carbon, is expelled in the urine or in the solid excrements. In order to keep up in the furnace a constant temperature, we must vary the supply of fuel according to the external temperature; that is, according to the supply of oxygen.

"In the animal body the food is the fuel; with a proper supply of oxygen we obtain the heat given out during its oxydation or combustion," etc.

The theory, that animal heat is evolved by the union of the oxygen of the atmosphere with the carbon and hydrogen of the body, under the influence of vitality, and by the assistance of the organic structure, may now be called an old one; and although it cannot be considered established—nor does Liebig so assert it—yet, the difficulties to its complete demonstration and reception have been in gradual progress of removal for a number of years.

When it was first proposed, a great difficulty existed, in the unfounded belief that the oxygen of the air combined with the carbon and hydrogen of the venous blood exclusively in the lungs: according to which those organs must necessarily be much hotter than any other part of the body—a condition which was demonstrated not to exist—the theory therefore temporarily gave way, notwithstanding the ingenious attempt to overcome the objection by means of the change of *capacity for heat* which the blood was supposed to undergo in the lungs. When it was ascertained that this conbination does not take place in the lungs exclusively, but throughout the whole system—the oxygen being carried by the arterial blood from the lungs, and the carbonic acid brought back by the venous blood to the same organs to be discharged—this difficulty was in some measure removed. Objections yet arose, based on the imperfection of our real knowledge. The known influence of the nerves on the process was supposed to prove that the nervous system was the independent source of heat; but this supposition is not so rational, as that they are merely instruments for the conduction of force, without which there could not take place, in the animal body, those metamorphoses which constantly supply, in the form of separated, effete matter, the carbon and hydrogen, necessary to the production of heat by union with the oxygen conveyed into the tissues in the arterial blood.

Another difficulty is founded on the experiments of MM. Dulong and Despretz, which seem to prove that an animal produced more heat in a given time, than was equivalent to the carbonic acid given out in the same

time. A certain residuary amount of heat was therefore supposed to exist, the production of which was to be attributed to some other source.

These experiments—in which the animals were placed in close vessels, surrounded with *cold* water, and the amount of the heat they communicated to the water accurately compared with the oxygen they consumed—are now known to have been imperfect, and are consequently inconclusive. They prove nothing but that when an animal is confined under circumstances calculated to terrify it, and surrounded by a medium of a temperature much lower than its own, will for a time give out more heat than corresponds with its respiration. The body of the animal being, to some extent, a *reservoir* as well as a *source* of heat, probably lost some of its actual temperature in the process; and had MM. Dulong and Despretz applied the thermometer to the animals, before and after they were placed in the cold water box, they would have found a source of error, in the actual cooling of their bodies, which has vitiated all their results. Moreover, such experiments, to be conclusive, must be continued throughout a considerable length of time; with animals placed as nearly under normal influences as possible; and then, comparing the results of a great number of observations, a mean might be obtained which would have some bearing on the question at issue.

It will be seen that the experiments of the French philosophers were based on the practical assumption, that the temperature of the living animal body is fixed and invariable; which destroys in a great measure the utility of their labors. This assumption is as unfortunate as it is remarkable; more particularly in the case of Despretz, who at the end of the article, in his Triate' de Physique (p. 906), details the fact that Legallois had proven that the *temperature* of the animal body *lowered* when it was caused to respire rarified air, or placed in a situation which would impede respiration.

That the living animal body can be temporarily heated and cooled, within certain limits, by external agency, is now fully established, and must be known to every one who has kept pace with physiological science. Yet on the assumption of the contrary does Dr. Caldwell rely, in some of his most ingenious arguments against the theory of animal heat which is advocated by Professor Liebig.

In his Physiology Vindicated we find the following:—"But, perhaps, the most herculean objection to Professer Liebig's hypothesis of vital temperature remains to be mentioned. It consists in the power which the

human body possesses to maintain its temperature, in defiance of the influence of an extremely hot, as well as of an extremely cold atmosphere. Nor do we know that this power has ever yet been fairly analyzed, commented on, and applied to the purpose of expounding the production of animal heat.

"As far as reports of experiments inform us, the temperature of the body of man, in a healthy condition, has never been raised above from 100° to 101° or 102° of Fahrenheit. Yet have men at sundry times exposed themselves to an atmosphere whose heat ranged from 200° to 500°." p. 44.

Judging from the known industry and accumen of the critic, as well as from the confidence of his style, the reader would be led to believe that the Professor had studied and weighed the results of all the experiments which had ever been published on the subject in question; but it will be seen, upon examination, that the only experiments on which he relies to sustain his positions, are those which were made, in the years 1775 and 1777 by Sir Joseph Banks, Dr. Fordyce, Dr. Blagden, Dr. Dodson of Liverpool, and the celebrated John Hunter; whilst all that have been performed for the last sixty-six years are either unknown to him or wilfully disregarded. Nor does he take into estimation the several disturbing circumstances and causes of error, connected with the experiments as performed by Dr. Fordyce and his cotemporaries; which misled them in their deductions; but which more modern experimenters, with their improved knowledge of nature, have properly appreciated. We mean, for example, the imperfection of their thermometers, and the neglect to ascertain the precise temperature of their bodies before and after the experiment; the dryness and moisture of the hot air to which they exposed themselves, and, consequently, the relative amount of evaporation from their surfaces; the extreme slowness with which heat is communicated from hot air; and the influence of clothing.

Yet, even in the quotation of these ambiguous experiments Dr. Caldwell has not confined himself strictly to the letter of the text, but has occasionally *amended* it to suit his own peculiar views.—He says, for instance, that men have exposed themselves to an atmosphere whose heat ranged "from 200° to near 500° Fahr;" but on reference to the Philosophical Transactions of the Royal Society, for 1775, in which the account of these experiments is published, I find the fact to be, that it ranged from 110° to a little above 260°; which was the highest temperature to which any of them were exposed.

It may be said that further authority is given in the following passage of the critic:—"Before the time of the performance of these experiments in London, MM. Duhamel and Tillet, two distinguished and enterprising French physicians, exposed themselves, (or rather two young women), to an atmospheric temperature of 325°. And, not many years ago, it was confidently asserted that M. Chaubert, (usually called the *Fire-king*,") exposed himself to a heat of about 500°. We do not vouch for the heat to which the "fire-king" was exposed, having been as high as 500°; but it is not questioned that it was very intense."—*Phys. Vind, p.* 45. But on reference to the record, I find the greatest heat sustained by the young women was 264°; and it is well known that the wonderful exploit of the "fire king" was eked out by putting the thermometer in the hottest part of the oven while he remained in a comparatively cool position; so that nothing certain can be predicated of them.

Again, Dr. Caldwell says.—"Into a room *about twenty feet square,* heated to 210°, Dr. Blagden entered alone, and in a few minutes, the thermometer, suspended several yards from him fell to 198°. The heat was once more raised to 211°, and Sir Joseph Banks entering alone, it soon fell again to 198°. And when the three entered together, it fell of course with much more rapidity, and therefore to a greater extent in a given time. Thus, we repeat, did the human body not only retain its own standard heat, in the midst of a temperature much more than 100° above it; it speedily reduced that temperature, in the entire atmosphere of a large room in one instance 12°, and in another 13°. The precise amount of its fall, when the three experimenters entered at once we do not remember." *Phys. Vind, p.* 44.

But the Philosophical Transactions state, (p. 607), that the room was "14 *feet by* 12 *in length and width and* 11 *in height* heated by a round stove or cockle of cast iron, which stood in the middle, with a tube for the smoke carried from it through one of the sides." They further say, that after the three gentlemen had entered the room, and the thermometer "sunk very fast," it was agreed that for the future, only one person should go in at a time, and orders were given to raise the fire as much as possible. Soon afterwards, Dr. Solander entered the room alone, and saw the thermometer at 210°, but during the three minutes he staid there, it sunk to 196°. Another time he found it almost five minutes before the heat was lessened from 210° to 196°. Mr. Banks closed the whole by going in

when the thermometer stood at 211°, he remained seven minutes, in which time the quicksilver had sunk to 198°, *but cold air had been let into the room by a person who went in and came out again during Mr. Banks stay.*" This latter passage, suppressed by the critic, gives us a key.

These experiments were performed in cold weather—on the 23rd of of January—and the admission of cold air, on the opening of the door, is sufficient to account for the greater part of the depression of temperature observed; which was indicated by their thermometers only after some little lapse of time. Another quotation from the Transactions may elucidate this:—"The slowness with which air communicates its heat is further shown, in a remarkable manner, by the thermometers they brought with them into the room, none of which at the end of twenty minutes, in the first experiment, had acquired the real heat of the air by several degrees." (p. 608). In the subsequent experiments, made in April of the same year, when the weather was warmer, and when pains were taken by lighting the fire the preceding day, and keeping it up all night, to make every thing in the room, and the walls, warm; it is not stated that the mercury fell on the entrance of the experimenters, but that it rose while they were in the room. So that the *immense* and mysterious cooling power of the animal body, which Dr. Caldwell attributes to the independent action of *vitality*, is not proved by the experiments which he triumphantly mis-quotes.

Equally erroneous impressions are conveyed by the following statements of the professor, and his remarks and arguments based upon them.

"Messrs. Banks, Blagden and Fordyce affirm, that when in the heated rooms, their persons instead of being *exhaling* bodies, were powerfully condensing ones."—*Phys. Vind, p.* 45.

"When either of the gentlemen, when in the heated room, held in his hand a Florence flask filled with water; its heat rose to 120° Fahrenheit; and, even at that temperature *it* was a condenser of the surrounding vapor."—*Phys. Vind, p.* 46.

The *truth* is, that *Dr. Fordyce alone* performed the experiment alluded to; using for the purpose a suite of three rooms, the hottest of which was heated to a temperature of from 110° to 130°, by flues in the floor; and its air loaded with steam *by pouring on boiling water.* The observations of all the other gentlemen named were exclusively made in *another room,* of which we have already given the description, containing hot *dry air*; which, it is expressly stated, "had served for many of his (Dr. Fordyce's) experiments with dry air," and in which evaporation from their

bodies must not only have been unchecked but very much accelerated. The Florence flask was used *but in one experiment*, by Dr. Fordyce alone, in his second experiment; the water in it was at the temperature of 100°; when handed to him, in the hot vapor chamber heated to 130°, and it condensed moisture on its sides until the water within had risen to 122°. The heat of his body was brought up to 100°, but he omits to mention what was its previous and natural temperature.

Caloric was undoubtedly communicated to his body, and in larger amount than would have been conveyed in *dry air* equally heated; as is proved by the following passages in the account of these experiments:—"Dr. Fordyce has since had occasion, in making other experiments, to go frequently into a much greater heat, where the air was dry, and to stay there a much longer time, without being nearly so much affected."—*Phil. Trans*, 1775, *p.* 606. But the exact amount of that heat cannot be rigidly ascertained from the history given, and it is by no means certain, that the moisture which appeared on his body, and which he supposed was the effect of *condensation*, may not have been mainly the result of *transudation from it*; nor that vapor, of a high tension, may not have been continually passing from the exterior surface of that moisture, into the atmosphere of the room. The matter is therefore not so simple as Professor Caldwell would lead us to believe; nor do the facts sustain him in the old and exploded hypothesis stated in the following paragraph:—

"They are adduced for the purpose of proving, and we contend they do prove with clearness, the existence, in the body of man, of an anti-calorific power essentially different in its nature and action from any chemical power. Why? Because it does what chemistry can neither do, nor explain. It is, moreover, of a higher order than any thing chemical. It is a power that can *warm* without *combustion*, and *cool* without *exhalation*. For in the cases just referred to it evidently did both."—*Phys. Vind, p.* 47.

If, upon accurate examination and calculation, it should be found that the *actual amount of heat communicated* to the body, in such experiments, *added to that generated in it*, for the time being, is greater than the quantity lost by cutaneous and pulmonary transpiration and evaporation, added to what is necessary to keep up or raise the temperature of the body;—then, and not till then, may we look for a power of annihilating heat in the animal organism. But this cannot be proven by any of the experiments which have hitherto been performed; and therefore the results of

these offer no objection to the theory of animal heat which is supported by Liebig.

All experiments of this kind, in *heated air or vapor*, are exceedingly fallacious; and principally from the *extreme slowness* with which they part with caloric to bodies immersed in them; thus communicating a much smaller amount, in a given time, than would be conveyed by better conductors of heat at a much lower temperature. For example, whilst persons can live in air at 200° or 300°, for some little time; their bodies being but very slowly heated by it, especially when they touch no good conductors, and are, in some measure, protected by clothing; contact with heated metalic substances, at a much lower temperature, would almost instantly destroy life.

This fact was observed by the experimenters above alluded to, as will be seen in the following extract from the "Philosophic Transactions." "The same person who felt no inconvenience from air heated to 211°, could not bear quicksilver at 120°, and could just bear rectified spirits at 130°." Quicksilver, we are told in a note, was insupportable above 117°, water above 123°, and oil above 129°. The truth conveyed by these facts is clear, that the better the conducting power of the medium, the lower the temperature which can be supported by the animal body exposed to it; because caloric is more rapidly communicated by the good conductors than by the bad ones, and the heat of the body is more rapidly carried up by them, to the limit of intolerance.

Some of the experiments of Dr. Dodson of Liverpool, performed also in 1775, and detailed in the same volume with those of Dr. Fordyce and others, exhibit in a most striking manner, the extreme slowness with which heat passes from hot air into bodies which are immersed in it, and which are not in contact with any good conductor.

"*Experiment* 7.—Part of the shell of an egg was peeled away, leaving only the film which surrounded the white; and part of the white being drawn out, the film sunk so as to form a little cup. This cup was filled with the albumen ovi, which was consequently detached as much as possible from every thing but the contact of the air and of the film which formed the cup. The lower part of the egg stood on some light tow in the bottom of a gallipot, and was placed on the wooden seat in the stove. The quicksilver in the thermometer still continued at 224°. After remaining in the stove for an hour, the lower part of the egg, which was covered by the shell, was firmly coagulated; but that which was in the little cup

was fluid and transparent. At the end of another hour it was still fluid, except on the edges where it was thinnest; and here it was still transparent; a sufficient proof that it was dried, not coagulated." As the pure white of egg would have coagulated at 140°, we have here the proof, that in the course of two hours, that temperature had not been communicated to it, although the heat of the surrounding air was at 224°. Had the albumen been poured on the better conducting medium, an iron plate, heated only to 150°, it would have attained the temperature of coagulation almost in an instant.

When the animal body is exposed to hot air, as in the experiments above quoted, heat is very slowly communicated. Yet it does pass into it, and for the time, a portion of that heat is consumed in raising the *actual temperature* of the body. The powers of the system oppose this action by the means of the unusual transpiration and evaporation of moisture—every particle of water evaporated, carrying off with it, in the latent state, nearly 1000 degrees of caloric. These processes increase as the temperature rises; and, if they are not sufficient to prevent the gradual rise of the heat of the body, beyond a certain, not very distant limit, the powers of life succumb and the animal dies.

In former times, the rise of water in the stem of a pump, was supposed to be explained by the assumption, that nature had a *horror or dread of a vacuum;* but, when it was found that water would not thus rise much above thirty feet in height, the Aristotelian philosophers were obliged to admit that the *horror* was a limited one. In like manner, Professor Caldwell prefers to supercede the facts above stated, by the assumption of a *constitutional instinct*, as follows:

"The system of man, then, we say, obviously possesses a hidden and unknown power, which enables it to control caloric, and render it subservient to its own ends. And that power is a *vital* one. In the case we are considering, the welfare of the system required the production of *latency*, and the latency was produced. In another case, where the same welfare requires caloric to be *evolved*—the *evolution* is effected. In each instance, a power that might well perhaps be denominated a *constitutional instinct*, fitted to subserve the *good* of the system, acts in accordance with the *exigency* of the system."—*Phys. Vind. p.* 50.

We shall proceed to show, that this mysterious *instinct* is also, like the ancient *horror vacui*, limited in its existence.

In no experiment has an animal sustained, even in the imperfect conducting medium, atmospheric air, the high temperature referred to above, for more than two hours.

The most accurate and complete experiments of this kind were performed by MM. Delaroche and Berger, in 1806, an abstract of which may be found in the admirable work of W. F. Edwards, "*On the Influence of the Physical Agents on Life*," which we commend, especially, to the more attentive study of our critic, as a work that may remove many of his difficulties, and probably may increase the amount of his physiological knowledge: Some facts from which, however, we will take the liberty to quote.

First, then, in relation to *dry* hot air, as compared with that containing vapor, and the still better conducting medium, hot water.

M. Berger sustained, the temperature of 229° Fah., in hot dry air, for seven minutes, and Blagden that of from 240° to 260° Fah., for eight minutes; but on the other hand—

"M. Delaroche could not support, above ten minutes and a half, a vapor bath, which, at first, at 99° Fah., rose in eight minutes to 124° Fah., and afterwards fell one degree.

"M. Berger was obliged, in twelve minutes and a half, to come out of a vapor bath, of which the temperature had risen from 106° Fah., to about 129° Fah. He was weak, and tottered on his legs, and was affected with vertigo. The weakness and thirst lasted the remainder of the day.

"Lemonnier, being at Bareges, plunged into the hottest spring, which was at 113° Fah. He could not remain in it above eight minutes. Violent agitation and giddiness forced him out."

In this connection, M. Edwards also states: "I have never seen batrachians which could live above two minutes in water, at 104° Fah., although I have taken the precaution of holding a part of the head out of the water, to allow the pulmonary respiration to continue; whilst individuals of the same species, (frogs,) have supported the heat of air, charged with vapor, at the same temperature, above five hours."

Some exceptional cases can doubtless be quoted as objections to these facts; but we believe that, if those which *really exist*, are properly examined, they will give their own explanation in some *peculiar* provision, adapted to the *unusual phenomena* they present.

In relation to the fact, that the animal temperature *may be changed* by external heat, notwithstanding the resources and agents employed by the

vital powers, and that, when carried beyond certain limits, death is caused, we will quote still farther from Edwards.

MM. Delaroche and Berger, in several experiments, found their own temperature to rise to the extent of from 3° to 9° Fah., when they exposed their persons, for the space of from eight to sixteen minutes, to the influence of hot air or vapor, heated to from 106° to 186° Fah.

"Experiments upon man cannot of course be carried far enough to ascertain what is the highest degree which his temperature can attain under the influence of excessive atmospheric heat." But in various species of mammalia and birds, exposed by MM. Delaroche and Berger, "to different degrees of hot air, the lowest 122° Fah., and the highest 200° 75 Fah.," in which they were left until they died. "Notwithstanding the diversity of species and of classes, and of the degrees of heat to which they were exposed, they all acquired nearly the same increase of temperature, the limits of the variation being from 11° 25 Fah. to 12° 92 Fah. The bodily temperature having been ascertained by a thermometer introduced far into the rectum," is free from objections.

"When we consider the uniformity of the above results, we may infer generally, that man and warm-blooded animals, under the influence of excessive heat and a dry air, could not, during life, experience a higher elevation of bodily temperature than 12° 6 Fah., or 14° Fah."*

This heating of the body, it is evident, must be taken into consideration in all calculations relative to the heat-sustaining or *heat-destroying* power of animals. It must be equally clear, that, although it is usual to state in *general terms*, (and we find the statement in Liebig's animal chemistry,) that human temperature remains unchangeable in all external vicissitudes; and, although Capt. Parry relates that the animal heat was not sensibly modified in the extreme cold to which he and his crews were exposed, yet, such a statement is not to be received or applied in its most definite sense.

In addition to the facts above detailed, we will state, that the observations of Dr. Jno. Davy proved, that the human temperature increased from the poles to the equator; and the recent experiments, made during a late French voyage of discovery, in the ship *Bonite*, are to the same effect.

*Edwards on Phys. Agts. London, 1832. pp. 196—7.

These observations, made by MM. Eydoux and Souleyet, on ten of the crew of the Bonite, of different ages and temperaments, all submitted to nearly the same regimen, and engaged in the same employments, were taken at the same hour, 3 P. M., of every day, from the time of their arrival at at Rio Janeiro until they returned to France. They amounted to more than four thousand in number, and the results led to the conclusion, that the *human temperature rose and fell at the same time with the exterior temperature,* although in a smaller ratio. According to the meaning of their observations, a difference of 40 degrees, (centigrade,) in the external atmosphere, produced only a change of 1 degree (centigrade) in the living body. The temperature of birds, observed in the different latitudes, presented a difference of from 90 to 106° Fah. These experiments were made simply with the ordinary thermometer, introduced into the mucous cavities, and particularly into the rectum.

To exemplify still farther the facts, arguments, and manner of Dr. Caldwell, I will transcribe another portion of his "critique":

"To render the substance of our views of the *cooling* power of the human body the more familiar, and their *truth* the more easily tested, we propose the following simple experiment, which may be made by any one.

"Take a large tubful of water, heated to the temperature of 120° of Fahrenheit. Immerse your feet and legs in it, and the sense of burning produced by it will be painful to you. Allow your limbs to be still for a few minutes, and the burning will cease. Remove them to another place in the water, several inches distant, and the burning will be reproduced. Hold them again motionless, and again you will be freed from pain. And thus may the burning be alternately renewed and abated, until the temperature of the water shall be so reduced, as to be near the temperature of your own system—say that of 100°, or two or three degrees higher; for the temperature of your extremities suffers but a very slight change.

"Of these alternations of pain and relief, the reason is plain. Your limbs, without becoming heated themselves, as if they were dead matter, *cool* the water in immediate contact with them, until it ceases to burn. Nor is this all. They also cool it for some distance around them. In proof of this, make another experiment.

"When you first introduce your limbs into the water, at the temperature of 120°, introduce also a thermometer, and bring the bulb of it in contact with your skin. In that case, six-eighths of the bulb will be still in immediate contact with the water.

"Notwithstanding this, if, as soon as the water shall cease to burn you, the thermometer be examined, it will be found to stand but a few degrees above blood heat. Why? Because, we repeat, the leg has cooled the water to some distance around it. To be still further convinced of this, place the thermometer in the water, at the distance of six or eight inches from your limb, and the mercury will again rise.

Once more:—"Let another interesting experiment be tried, which will be found to eventuate to the same effect.

"Immerse in the same tubful of water, heated to 120°, two lower extremities of the same size—one of them alive and sound, and the other dead—let them be fifteen or eighteen inches distance from each other. In twenty minutes after immersion, apply your thermometer, under the water, alternately to each; and you will find the difference in their temperatures to amount to eight or ten degrees or probably more. The dead limb will be near the water in temperature, and the living one many degrees below it. Of this result the cause is the same—the control of living matter on caloric, even to the reduction of it to a state of latency.

"To the enlightened and scientific reader we need hardly observe, that all the facts we have just laid before him, are in plain and positive contradiction of Professor Liebig's hypothesis, and of every other *chemical* hypothesis, respecting the production of *vital* temperature."—*Phys. Vind, pp.* 51, 52.

Need we tell the reader, these experiments are, in sum and substance, transcribed from the works of John Hunter, with the substitution merely of one member of the body for another, and the addition of some rich verbiage? If he will refer to the edition of Hunter's Surgical Lectures, published in Philadelphia in 1839, by Haswell, Barrington and Haswell, on pages 81, 82, he will find them; and if he will examine the notes, which are appended to the text, he will see explained the source of the error of the conclusions drawn from them by their author, and enlarged and improved by Professor Caldwell.

In the day of John Hunter the error was excusable, because of the more imperfect state of physical science; but in the present condition of knowledge, a candidate for medical honors would not be worthy of his diploma, if he could not see the impropriety of these conclusions. In relation to our critic, therefore, the only question to be decided is, whether he is culpable of ignorance or wilful perversion.

In answer to the Professor's argument, I will propose to *him* some experiments, which are not however to be found in Hunter's writings.

Take your tubful of hot water. Immerse in it a large thermometer; marking first the height of the mercury; allow it to remain immersed, *but at rest*, for one minute; — observe, then, how much the column has risen. Now move it about forcibly in the water; immersed to the same depth, for the space of another minute; and if the mercury does not expand greatly more, in equal times, while the instrument is in motion than while at rest, I will not only believe in nature's horror of a vacuum, but in all the dogmas that have ever been propounded.

The simple truth is, that hot fluids part with their heat more rapidly when they impinge, in a current on the body to be heated, than they do when at rest; — and the true reason why the water felt hotter, to the limbs immersed in it, when it was agitated than when it was in a state of rest, was, that it *gave them more heat in a given time.*

But the Doctor says "the leg has *cooled the water to some distance around it!!*" Shade of Rumford! Can it be believed that a learned professor, in the middle of the nineteenth century, should know so little of the currents which exist in heating and cooling fluids, or hydrostatics, as to suppose, that a hollow column of cold water, extending to the distance of some inches, could forget its specific gravity so far, as to stand up, and maintain its place around the leg, although surrounded on all sides by hotter, and specifically lighter water!

If this is true, the wonderful *vital* principle — the mysterious *constitutional instinct* — not only masters and destroys chemical and physical forces within the body; but its power is *even extended to some inches beyond it!!* We leave the doctor to decide whether this proves too much, or nothing at all, or both.

The living member *does* cool the water, however, and much more rapidly than is in proportion to its rise of temperature. But the particles of water, which lose their heat by contact with the leg, becoming more contracted and therefore heavier, by cooling, immediately fall in a current to the bottom of the fluid, giving place to other and warmer particles. A process which continues as long as the cooling takes place, and without which it would not be easy either to heat or cool water.

In order that he may understand how the members may cool the water, without being themselves heated in a proportionate degree; or how a dead limb is heated, under these circumstances, more than a living one, I will propose to the professor other interesting experiments; — which, if he is desirous of finding truth, he will perform: —

Take a tin tube, closed at one end—say a foot long and a couple of inches in diameter—fill it with water at 98 deg, and place a thermometer in it. Take also another similar tube and thermometer, but let the open end of the tube be passed, with a tight joint, into the bottom of a tin vessel of the capacity of a gallon, so that they will form one continuous vessel, filled entirely with water at 98 deg. Now immerse both tubes in the tub full of hot water, to an equal depth, say eight inches; and observe the thermometer and you will witness the wonderful fact, that the temperature of the water in the unattached tube will rise rapidly, while but a comparatively slight effect will be produced in the tube whose contents are in communication with the gallon of water above it.

What does this prove? That vessels of *a peculiar form exert a control over caloric*, so as to destroy it, "*even to reduce it to a state of latency?*" Or, that the heat given to the water in the attached tube is equally diffused, throughout the whole gallon of water with which it is joined, *by the circulation* which is set up in the fluid? Dr. Caldwell may take which solution he pleases; but the *fact* is, that it proves as much as the experiment of John Hunter, with the dead and living member; viz, that the *circulation* conveys away and distributes the caloric.

The *circulation*, which is operative in the tubulated vessel, is the same that has been adverted to above, as the cause of the rapid diffusion of heat in liquids; whilst the circulation present in the living member and absent in the dead one, is that of the blood; which constantly passing into and out of the living limb, distributes the caloric, which it may receive, from the hot water, throughout the whole extent of the body.

It has been one of the labors of the life of Dr. Caldwell to oppose the application of chemical or physical facts and knowledge to the elucidation of the vital phenomena. His *logic*, in treating of this subject, as exhibited throughout the present pamphlet, may be exemplified in few words. It is as follows—1st. The *vital* force is *superior* to all other forces; 2nd. It consequently *destroys* all other forces in the *living* body; 3rd. Therefore, it is high treason to "*vital physiology*" to attribute to those forces *any agency* in the production of *vital* phenomena! Hereafter, however, he ought not only to apply himself diligently to the acquisition of chemical and physical knowledge, but also to recommend it to all who would understand or improve physiology. The foregoing experiments and facts must convince him, that although there is a hidden and powerful force called *vitality*, yet other forces are likely to complicate its actions; and we must thoroughly

understand these other forces before we can give to it its just extent of power and action. That this peculiar force (the *vital force*) is fully appreciated by Professor Liebig, is shown by the passages already quoted and may be more fully seen on reference to his Animal Chemistry, pp. 198, 199, etc, to which we commend the reader.

In the labored attempt of Professor Caldwell to disprove the doctrine of animal heat, sustained by the Chemist of Giessen, as well as by the majority of those whom he terms the "*would be physiologists*" of the present day, he resorts to various expedients, and inferences, which indicate his zeal in his cause rather than his correct knowledge or clear perception of truth. Of these I shall proceed to give a few more examples.

"During the hottest period of hot climates, the heat wasted by the human body, through atmospherical influence, does not amount on an average, to more than 5 deg, perhaps not so much. That quantity, therefore, and no more, must be supplied by the calorific process.

"But during the winters of the frozen north, the heat abstracted from the body of man, by atmospheric agency, amounts, not unfrequently, to from 140 deg. to 150 deg. This is from twenty-eight to thirty times as much as is abstracted under the influence of tropical heat. In such a case, therefore, the calorific process must supply that amount, else the temperature of the body will sink.—*Phys. Vind, p.* 12."

Here the learned critic finds it *convenient* to forget the compensating influence of clothing; and, for the time, to disregard entirely the abundant perspiration and great evaporation of moisture from the human body, in hot climates, which carries off from it in the latent state, a much larger proportion of the heat which is generated in it than in cold climates. Every particle of water, when it evaporates, renders as much heat latent as would have raised nearly to the red heat. He shortly afterward, however, takes occasion to introduce this "*great cooling process of the chemico-medical physiologists*," in order to accuse Liebig of having neglected it in *his* calculations. But in this he is clearly mistaken; as will be seen by the following extract from the author: —

"If we assume, that the quantity of water vaporized through the skin and lungs in 24 hours amounts to 48oz, (3lbs), then there will remain, after deducting the necessary amount of heat, 144137.7 degrees of heat, which are dissipated by radiation, by heating the expired air, and the excrementitious matter."—*Animal Chemistry, p.* 33.

It cannot fail to be observed, that many of the objections, which are

urged by Dr. Caldwell against the publication of Professor Liebig, are based on his own misapprehension of the design, plan, and character of the work he criticises. Written in a most concise style; the facts being arranged in a series of propositions, briefly stated, each having a relation to its antecedent, and nothing being repeated which has once been sufficiently explained; in order to comprehend the sense of the author in any one paragraph, it is neccssary to understand all that has been previously stated and is taken for granted. It may be further stated, that the learned chemist presupposes, in the minds of his readers, a certain amount of scientific facts and training, which, unfortunately, is not always to be found, even among veteran Professors of Physiology.

In the maintenance of the healthy, uniform temperature of the body, in different climates, many compensatory means are employed in the animal economy. In addition to those already adverted to, we find the diminution of the amount of oxygen inspired in hot climates, as a consequence of the rarified and expanded condition of the air breathed, and the relatively greater amount of food consumed in cold climates, affirmed and exemplified by Liebig, and controverted by his logical opposer, who takes up and examines each separately and apart, as though it had been contended that each was the SOLE AND ONLY CAUSE of all the effects produced, and not, as is the fact, that it was *only one of a number of means, all tending to the same end.* We quote his own words:

"Suppose a *cubic foot of air*, at the temperature of 96 deg., which is high tropical heat, contains *three cubic inches of oxygen*, raise a cubic foot of polar air from zero, or even below it, to the same temperature, and we doubt exceedingly whether its oxygen will be so far expanded as to occupy the space of *six cubic inches.* To employ terms of weight, which will represent the matter *with greater accuracy*—admit the oxygen contained in a cubic foot of tropical air to amount to *ten grains*—that contained in an equal volume of polar air, will not, we suspect, amount to *twenty grains.* These terms of weight and measure, we have used, not to express, in *fact*, the precise amount of oxygen in a given quantity of atmospheric air, at different temperatures and in different latitudes, but merely in illustration of the principle we wish to establish. *As we never, moreover, either made the experiments, or positively know that they have been made by others, we offer them in the character of probabilities—but as probabilities strong in their claim on attention and belief.*

"Even admitting, then, that our author's combustion hypothesis of ani-

mal heat is sound in principle, we are confident, we repeat, that the small differences in the amount of oxygen contained in equal volumes of polar, middle latitude, and tropical air, are far from being sufficient to sustain it in *fact*."—*Phys. Vind.*, p. 16, 17.

Here let the reader remark, in the passages we have taken the liberty to italicise, another striking evidence of the critic's positive and unpardonable want of *scientific information*, or of something worse. Does he not know, that the proportion of oxygen in the atmosphere has been accurately ascertained, both by weight and measure, and that the rate of expansion and contraction of airs and gases, in different temperatures, has been long since established; or does he purposely attempt to mystify, that he may be enabled to mislead?

If he is disposed to make the calculation, so as to ascertain with *accuracy, how much influence* these conditions of the atmosphere *may exert*, in the phenomena of calorification, I will inform him, and in all modern text-books of chemistry, he will find the fact stated, that air and gases of all kinds expand and contract equably and regularly, at all temperatures, to the amount of about 1-480 of their volume at 32 deg. for every degree of Fahrenheit. If he enters into the examination, he will not find the amount of the changes of volume of air to exceed, under the circumstances proposed, one fourth of the volume at 32 deg.; yet he cannot fail to see, even in this proportion, an important *auxiliary* to the combustion theory.

The human body, in adapting itself to different temperatures, is also somewhat dependant on the nature and quantity of its food. This is one of the points on which Professor Liebig dwells; and although its enunciation is not original with him, he has illustrated and applied it more forcibly, perhaps, than any previous writer. To this Dr. Caldwell strongly objects, in the following passage.

"It is not true, that men *generate vital heat*, and *sustain wintry cold*, in proportion to the amount of oxygen they breathe, and the quantity of carbon and hydrogen they swallow. They do *both* much more in proportion to *usage* and *habit*. And, in the production and maintenance of them, no chemical process has, or will be alledged to have, a shadow of agency. They are essentially and exclusively *vital* attributes. To refer them to chemistry, therefore, would be a rank misappliance, not to say prostitution of science."—*Phys. Vind.* p. 19.

He makes many statements, which he supposes are entirely at variance with the fact that the nature and quantity of the food may affect the heat-

producing powers of the body, and lengthened arguments to prove its fallacy. But it seems, that, after he had committed them to paper, to the discomfiture, as he supposed, of Liebig and all his followers, fearing that he had been too severe, or wishing to console his defeated opponents, he adds, in the preface and in a postscript, the following acknowledgement, that all which he had previously said must be understood *only in a "Pickwickian sense."*

"I do not positively *assert* that oxygen, carbon, nitrogen, and hydrogen, have no concern in the production of vital temperature, or in the formation of bile or urine, because I do not positively *know* that they have none; and my assertion never transcends my knowledge. But I do assert that these gases do not act on each other in the living system of man, and other forms of vital organized matter, precisely as they do in the laboratory of the chemist."—*Phys. Vind.*, p. 11.

* * * "Through the respiratory organ the system receives a large amount of oxygen, which is accounted, by the party, a prime agent in the production of vital heat. Nor do we deny that agency—or rather, the *instrumentality* of oxygen in that process. We only deny its *chemical* instrumentality. We deny that it acts in warming our bodies precisely as it does in warming our stoves. It operates under vital control—not under mere chemical affinity.

"In the production of animal and *vegetable* temperature, the vital power must employ means; and we are willing to admit that these means, or at least some of them, *may be*, and probably *are*, oxygen, carbon, and hydrogen. But they are employed, we say, as *vital, not chemical*, instruments or means of action.* The vital force has the control of them as entirely, as chemical attraction has of sulphur and oxygen in the formation of sulphuric acid, or of oxygen and hydrogen in the formation of water. Nor has genuine chemistry any more agency in the functions and economy of living organized beings, of any description, than vitality has in the processes of making gunpowder and calomel."—*Phys. Vind.* p. 91.

Really, we did not expect this from the Doctor!—we thought that he was in earnest; and we have often heard him quote

——"What 's in a name!
A rose by any other name would smell as sweet!"

But the statements and arguments in the body of the pamphlet, will

* Liebig contends for no more!—R. P.

justify me in giving a few additional facts, to those mentioned by Liebig, illustrative of the influence of the food on the heat-producing power of the animal body.

In the Narrative of the Second Voyage of Capt. Sir John Ross, in search of a north-west passage, (Am. ed. 1835. pp. 115) we find the following statement as the result of his extensive experience on the point in question:

* * * "But this at least seems certain, that men of the largest appetites and most perfect digestion produce the most heat; as feeble stomachs, whether dyspeptic, as it is termed, or merely unable to receive much food, are subject to suffer the most from cold; never generating heat enough to resist its impressions.

"Physicians must determine whether the strong digestive power, and the heat-generating one are but parts of one original constitution, or whether the large use of food is not a cause of the production of heat; but what follows is at least practically true, as the reason seems abundantly plain. He who is well fed, resists cold better than the man who is stinted, while the starvation from cold follows but too soon a starvation in food. This, doubtless, explains in a great measure, the resisting powers of the natives of these foreign climates: their consumption of food, it is familiar, being enormous, and often incredible. But it is also a valuable remark for those who may hereafter be situated like ourselves; since if these views are correct, as I believe them, both from experience and reasoning to be, it shows that no effort should be spared to ensure an ample supply of the best food."

"Our system, whether in the navy or merchant's service, and in whatever parts of the world, be it the icy seas or the tropical ocean, has been as fixed as it is uniform; and perhaps I ought not to blame those who have made regulations, when they did not know, and could not therefore take into consideration the grounds on which their orders ought to have been regulated. If the allowance of the food for seamen, under all differences of climate, or labor of service, technically speaking, has been fixed and uniform, implying circumstances and involving consequences respecting which I dare not here take room to speak, so in the case immediately before me, have we been accustomed to fix the allowance of food, to restrict it, I may fairly say, through an experience founded on far other circumstances, or under a system calculated from very different data."

"The conclusion therefore in which I wish to rest, willingly as I

would have extended these remarks, and perhaps then extending them so as to produce the greater conviction, is this; namely, that in every expedition or voyage to a polar region, at least if a winter residence is contemplated, the quantity of food should be increased, be that as convenient as it may. It would be very desirable indeed if the men could acquire the taste for Greenland food; since *all experience has shown that the large use of oil and fat meats is the true secret of life in these foreign countries, and that the natives cannot subsist without it; becoming diseased, and dying under a more meagre diet.*"

Dr. Caldwell says that this is all false; but who are we to believe—a theoretical physiologist who has an hypothesis to sustain, or those who have been to the polar regions?

These facts are in accordance with the experience of every one. Every farmer has observed that it requires more food to keep, in what is termed a *good condition*, a cow or other animal which is exposed to the full severity of the winter's cold, than one that is sheltered from the storms;—and every one knows that the appetite for solid food is much less in warm weather than it is in cold. Yet all this, and the general fact that the inhabitants of cold climates require more food than those of hot; as well as the statement that the animals of prey of the polar regions are more voracious than those of the torrid zone; are denied by our critic in the most unqualified manner.

The animals of high latitudes are endowed with special protectives, in the form of coverings of fur, blubber, &c., which, in *some measure*, preserve the heat of their bodies; yet they are undoubtedly voracious to an unusual degree, in consequence of the increased necessity for food to sustain their temperature. The beasts of prey of the torrid zone, as is objected by Dr. Caldwell, gorge themselves with food when they can obtain it, it is true;—but another element must be taken in the comparative estimation; which is, *the length of time which elapses between their several meals*;—when this is considered, we suspect that none of them will be found to equal in voracity the *Glutton*, which is comparatively a small animal, inhabiting the arctic regions. According to Mr. Klein, quoted by Smellie, one of these animals brought from Siberia to Dresden, eat *every day* thirty pounds of flesh without being satisfied! We can safely conclude that this *old story* is, like others of an equal date which find favor with our critic, a little exaggerated, yet all authors agree in the general fact of the great voracity of this animal.

To the same neglect of that important element of the calculation, *the time during which a single meal will supply the wants of an animal*—do we owe the objection first urged by Virey, of Paris, and now reproduced, without acknowledgment by Professor Caldwell; namely, that the Anaconda, and some other of the serpent tribe, &c., are of "prodigious voracity," when they can obtain food, yet their respiration is *comparatively limited*, and their temperature *proportionately low*. But the Professor himself gives the answer in his next paragraph.

* * * "They are capable of living *months, we know,* (and we are assured on authority we know not how to discredit, or even question, that the term may be extended to *years,*) in a state of entire abstinence from food, and of still maintaining their *ordinary temperature*. Nor are they materially reduced in weight by the privation."—*Phys. Vind. p.* 35.

He makes this passage the subject of a taunt, but it proves nothing but that, in his own language, when the snake takes his *fuel* into his *corporeal stove*, he lays in enough to keep up his small fire for a considerable length of time.

We cannot follow him in all that he says in relation to the agency of the food; it would not be profitable so to do; for it is evident to every one, that he relies on very uncertain data, and has not comprehended the sense of the propositions of the author he criticises.

All physiologists admit that birds maintain a higher temperature than any other animals; and they also see in them a proportionately greater extent of the respiratory function. To these, our critic refuses to give credence; and his peculiar logical astuteness is exhibited in his argument.

"To the lungs of birds are attached numerous tubes, which convey the air they inspire into various parts of their bodies. In some birds this diffusion of air is very extensive, being pushed, not merely into the muscles and other soft parts, but into the bones and some of the feathers, especially the large wing feathers."—*Phys. Vind. p.* 36.

Physiologists say that the air thus conveyed and renewed, throughout the body of birds, serves in some measure, to aerate the blood.

Dr. Caldwell affirms, on the contrary, that "these tubes form no part of the *true respiratory apparatus*" of these animals.

But, add the *would-be-physiologists,* the air thus conveyed, parts with its oxygen and receives carbonic acid in exchange, just as it does in the lungs proper.

Whether that is true or false, replies Professor Caldwell, which I will neither affirm nor deny, for I do not know,—*I say* that it is not *genuine respiration*:—and, *therefore*, as the lungs proper of birds are proportionately smaller than those of quadrupeds, they afford an example of a discrepancy between the temperature and the extent of the respiratory process!

We leave the reader to judge, both of the cogency of the argument and of the accuracy of the physiological knowledge of the critic. But there is something yet more glaring to be exhibited.

Dr. Caldwell objects to the theory of animal heat in question, that "the vegetable kingdom abounds in facts in direct opposition to it."—Relying on some experiments of John Hunter and others, which prove that growing trees are not, in the heat of summer and in the cold of winter, of the same temperature, internally, as the surrounding atmosphere; he reiterates the old hypothesis long since exploded, of an independent heat-producing power in all vegetables. He forgets that wood is an imperfect conductor of heat, and that, therefore, caloric requires some time to penetrate it—that the constant evaporation from the leaves of the living tree would tend to keep down its temperature during the summer—and that its fluids are derived from the soil some distance below the surface, and are consequently, always at something like a *mean temperature, i. e.* colder in summer and warmer in winter than the air. He forgets, neglects, or is ignorant of all these facts—and asserting that vegetables maintain a peculiar temperature by an independent heat-producing power of vitality, adduces this assertion as at once an evidence of the fallacy of Leibig's theory, and a proof of his inconsistency!

On this subject I would commend to him the study of modern works of Vegetable Physiology, and more particularly the admirable "*Physiologie Vegetale*" of the late De Candolle. Lest, however, the work may not exist in his library, I will transcribe a few passages on this point.

After noticing the experiments of Hunter and others, De Candolle adds:—"Pictet and Maurice* repeated these observations at Geneva during several years, and obtained the same results; they added to them an important observation, because it leads to the explanation of the fact—they placed several thermometers, some in the trunk of a large Chesnut tree, others at divers depths in the earth, and they saw that the variations of the thermometer which indicated the temperature of the interior of the trunk, corresponded sensibly with those of a thermometer placed at

* Biblioth. Britann. premiere annee.

four feet in the earth, that is to say, at the medium depth of the roots of the tree. M. Schubler and Neuffer have more recently obtained analogous results,*" &c. &c.—*pp.* 879–80.

De Candolle proceeds to detail the facts already stated in relation to the imperfect conducting power of wood, and the mean temperature of the fluids taken up by the roots of trees, and adds:

"It ought then naturally to result from this double effect, that the temperature of the interior of the trunks ought to be analogous to that of the soil in which their roots are plunged, that is to say, warmer than the air in winter, and colder in summer—and that in order to explain these facts, it is *not necessary to admit in vegetables a calorific faculty analogous to that of warm-blooded animals.*"—*p.* 882.

Equal unfairness, or want of knowledge, must be observed in another portion of the pamphlet of Dr. Caldwell—where he attempts to show that the supposed heat-producing power is weaker in young, than in mature trees.

* * * "All young animals are defective in their *calorific power.*—Of young vegetables the same may be affirmed. It is the young and tender plant, leaf, twig and fruit, that are most readily and certainly destroyed by an untimely frost. There exists in Louisville at the present time, a striking example to this effect.

"There stands in the court-yard of a gentleman, two saplings of a tree of the South, commonly called the "Pride of China," (its Botanical name not recollected.) They were brought from their native climate and planted here when very small and young. For several years after their transportation, it was necessary to protect them from the severity of our winters, by a covering of straw, or some other heat-retaining article. But now, having attained the sapling size, and being somewhat accustomed to our climate, they pass through the winter unhurt, *without a covering.*"—*Phys. Vind. p.* 29.

Here we will again employ the language of De Candolle, to expose the error of the Doctor, because it gives concisely the facts in relation to the case in hand.

"Young trees are more easily attacked by frost or drought, because, that among other causes, their roots are less deep in the soil, and the number of their layers of bark being less, defend them more feebly against external heat and cold."—*Physiologie Vegetale, p.* 883–4.

*Bull. Sc. Nat. 20, p. 261.

In this connection, we will propose an enigma to the venerable critic Why is it that the new branches of an old and established grape-vine, of a foreign variety, will be killed by the frost, if not protected by some artificial covering, during the *first* and sometimes the *second* winter after their growth, but are hardy forever afterwards? These branches are parts of an *old vegetable*, and therefore ought, according to your theory, to have attained their full *calorific power*. Gardeners, and vegetable physiologists see in the annual increase of the layers of bark on the surface of the branches, a sufficient solution of this phenomena. But how will you account for it?

In continuation of his objections, drawn from *his own peculiar* vegetable physiology, we find the following passages.

"In the Isle of Bourbon, when the temperature of the atmosphere was but 80 deg. of Fahrenheit, Hubert found the temperature of the flowers of the *Arum Cordefolium*, to be 134 deg. But when sanctioned by the authority of Hubert, we can neither disbelieve not even question the statement. And it is well known to Botanists, that the temperature of the blossoms of sundry plants rises to 119 or 120 deg.—the temperature of the atmosphere at the time being that of summer in temperate climates. In such cases the blossoms generally grow in clusters."

"How will our author reconcile these phenomenas with his hypothesis of vital temperature? Does the combustion of carbon or hydrogen or both take place in these flowers? If so, where, and what are the evidences of the fact? Do the flowers referred to *absorb* oxygen, that by its chemical union with carbon and hydrogen, it may awaken combustion, form carbonic acid and water, and produce heat? Or do they (as our author asserts that all vegetables *necessarily* do,) continue to *discharge* oxygen? For both processes at once, we think, they cannot perform. In a special manner how can these facts just cited be made to harmonize with the following extract from "Animal Chemistry?" And how can the two paragraphs about to be extracted, be made to harmonize with each other? (See p. 2.)

"The observations of vegetable physiologists, and the researches of chemists have mutually contributed to establish the fact, that the growth and developement of vegetables depend on the *elimination* of oxygen, which is *separated from the other component parts of their nourishment.*

"In contradistinction to vegetable life, the life of animals exhibits itself in the constant *absorption* of the oxygen of the air, and its combination with certain component parts of the animal body."

"If we understand these paragraphs correctly, they are, as will presently be demonstrated, entirely out of concord with each other, not to employ a stronger term, we pronounce them mutually contradictory."—*Phys. Vind. pp.* 38-9.

A sufficient escape from this dilemma of Dr. Caldwell is reserved by Liebig, and it is strange "not to employ a stronger term," that it should have escaped the attention of our critic. On page 17 of "Animal Chemistry," we find it stated, (as we have already quoted)

"All living creatures *whose existence depends on the absorption of oxygen*, possess within them a source of heat, &c.

"This truth applies to all animals, *and extends, besides*, to the germination of seeds, to *the flowering of plants*, and to the maturation of fruits."

Liebig never countenanced the exploded hypothesis that vegetables in general possessed an independent calorific power, and it is evident that he was familiar with the exceptional fact, as well as the cause, of the temporary production of heat during their flowering.

The phenomena attending the production of heat during the flowering of plants, the maturation of their fruits, and the germination of their seeds, are now universally known to be the reverse of those which are present during their *general growth and development*. Their *general* action is to decompose carbonic acid and water, and to *eliminate* oxygen, although they only do so when exposed to light; but in these particular processes, in which heat is produced, oxygen is *absorbed* from the atmosphere, and it combines with carbon and hydrogen to produce carbonic acid and water, and evolve heat just as in animal respiration. Let us again refer to De Candolle, whose work is considered one of the best text-books of vegetable physiology.

"The petals, in common with all the parts of the flowers which are not green, are endowed with the faculty of deteriorating the atmospheric air, these parts yield a portion of their own carbon, which, uniting with the oxygen of the air, forms a volume of carbonic acid nearly equal to that of the oxygen.

* * * "M. Theod. de Saussure,* was the first to ascertain this fact with exactitude,—he placed the flowers in a recipient of atmospheric air, closed by mercury, of which they did not occupy more than one 200th part, and measured the quantity of acid produced, comparing it with the volume of the flower employed taken as unity."

* Traite de la Vegetation, 1 p. 178.

The result was that they absorbed from three and a half to eighteen and a half times their volume of oxygen. He continues:

"But of all the plants, that in which the destruction of oxygen by the floral parts is the most prominent, is the *Arum Vulgare*. Its spathe destroyed four times its volume of oxygen,—its spadix thirty times its volume, and in the part which bears the sexual organs, as much as thirty-two times its volume.

"This effect is evidently allied with another fact presented by the same plant,—namely, the heat which its spadix emits at the epoch, which corresponds with that of the destruction of oxygen," &c.—*Physiologie Vegetale. pp.* 549-50-1.

Other objections to Liebig's views of the sources of Animal heat, by which he affirms they are "irretrievably overthrown," are derived by Dr. Caldwell from the following facts:—namely, that in a paralized limb, or when the nerve is cut, the circulation proceeds as usual, and yet the temperature sinks; and that in the bodies of consumptive patients, whose lungs are nearly destroyed, and which do not contain an ounce of fat to furnish carbon, "when the amouut of their food and drink is exceedingly small and none of it oily," the temperature kept up to high fever heat.

These, like almost the whole of his objections, are susceptible of most easy removal when the subject is understood. They indeed furnish corroborative proof of the doctrine which is promulgated in the work of Professor Liebig, which they are, in every respect, accordant.

The production of heat in the animal body is dependant on nutrition; in this respect—that unless certain particles are given to be deposited in the tissues, they cannot be separated from them in order to combine with the oxygen of the arterial blood to produce heat. It is also clearly dependant on the subsequent separation of these particles from the tissues, which takes place during vital actions,—*for it is not contended that the oxygen can combine with them while they are under the complete control of the vital force.*

In the paralized limb, therefore, nutrition is lessened, and the seperation of particles, or the metamorphosis of the tissues, is also lessened—the whole sum of the vital actions is diminished, and *consequently*, although oxygen may be present in the arterial blood sent to the member, the amount of the effete or separated matter which combines with it, is not so great as in the normal state, and, therefore, cannot maintain the usual temperature.

On the other hand, it may be supposed that such a general diminution of the vital force exists in the body of the consumptive patient, that but little resistance is offered to the action of oxygen on the tissues, and the wasting, as is well known, is thefore very rapid. The abundance of Uric acid in the urine voided after a febrile exacerbation strengthens this view; for had there been a superabundance of oxygen, in relation to the combustible matters which combine with it in, the body, this would have been replaced by urea, which is a more highly oxydized product.

Less oxygen is indeed taken into the lungs, than in the normal state, but more is yet inhaled to them and furnished to the skin, than is sufficient to account for the phenomena. For in the healthy state of the respiratory organs, *a great deal more air is habitually taken in than is consumed;* and the relative proportion of the oxygen which is exchanged for carbonic acid, in every act of respiration, is *exceedingly small.* By accurate experiments it has been ascertained that only one-third of the oxygen is consumed, in air which has been once respired, in the healthy state of the organs.—Doubtless that proportion would be found much greater in the expired air of the consumptive patient. Moreover, it may be stated for the information of our critic, that the healthy lungs retain in their cells, after ordinary breathing, eight times as much air as is renewed in each inspiration.

Another fact which bears on this question is, that it is not invariably true that in all consumptive patients whose lungs are very much disorganized, the animal temperature is kept above the healthy point. Andral in his Medical Clinic (Diseases of the chest) makes the following remarks.

"It appeared to me a matter of curiosity, to ascertain whether in phthisical patients also the temperature was less raised than in other persons. I accordingly found that in a considerable number of these patients, Reuumur's thermometer placed under the axilla, did not rise above 29 deg. (equal to 97 deg. Fahrenheit,) 'in some it did not go beyond 28 deg.' (95 deg. Fahrenheit.) 'This temperature, lower than that of the natural state, was moreover observed only in persons whose lungs contained a great many caverns, and were indurated in a great part of their extent,' (Amer. Ed. p. 255.) He adds, however, that in other similiar cases, he has observed the thermometer to rise to 'between 31 and 32 deg. as in the healthy state.'

Here we cannot avoid adverting to some of the *facts*, we might perhaps more properly say *discoveries*, of Dr. Caldwell—by means of which he attempts to prove the chemists in error, and to annihilate the doctrines

of Liebig. One of these is the probable discovery of a new constituent of the atmosphere, and the formation of a new theory of respiration—announced by him in the following paragraph.

"Respiration is the inlet of the vital principle into the bodies of animated beings. It is therefore the only function that is *truly and primarily vital.* The circulation of the blood, and the functions of the brain and nerves, are vital only relatively, and in a secondary degree. Admitting it to be true, then, that animals possess a temperature, proportioned in height to the extent of their respiration, the fact is to be attributed, not to the superior amount of oxygen, but to that of the *vital principle* received by them in the process. This position, though not susceptible of positive proof, might be rendered highly probable, could we dwell on the subject." —*Phys. Vind., p.* 92.

Could any one possibly read such a statement with a serious face? But we find another *new discovery* of the Doctor on page 80:

"There exist two small insects, the *terebellum saxosum* and the *terebellum marmoreum,* the former of which, as entomologists of the highest order assure us, subsists on *argillaceous stones,* and those formed of other sorts of primitive earth, and the latter on *marble.* Certain it is, that the parent insect penetrates into the stones referred to, and there deposits her eggs. And it is *held equally certain,* that, when she comes out again, she leaves behind her, for her young, no aliment, either vegetable or animal. She supplies them with a small amount of *stone* and *marble dust,* which she formed by cutting her way inward, with her gimlet-like proboscis. On *that, therefore,* when hatched, they are *compelled to subsist,* or perish.

"What authority, then, has Professor Liebig to include, as he does, these insects in his sweeping declaration, that the food of *all* animals, in *all* circumstances, consists of parts of organisms?"—*Phys. Vind.*

This is not all. Dr. Caldwell seems also to have discovered that the larva of the "*septemdecennial locust,*" lives on nothing but pure *mineral* substances during the whole of its *seventeen years sojourn* in the soil! The Doctor has been praised for his extensive knowledge of natural history, displayed in this pamphlet. He taunts Liebig with ignorance of zoology; and these insectile *discoveries* of his, throw even professed entomologists into the shade!!

I have examined the works of Linnæus, Cuvier and Latreille, Kirby and Spence, and others, and find no *insect,* bearing the name of *terebellum.* If these curious insects, with the singular taste for hard rocks, are *new dis-*

coveries, as we suspect they *must be*, we would recommend to the Professor to give them another generic name—that of *terebellnm*, having been already appropriated to a genus of *univalve shell-fish*, the species of which are as innocent of *boring* as the Doctor is of chemical physiology.

In searching through the several entomological works, in the large library of the medical department of Transylvania University, for light on this interesting subject, I found that a very old German author, named Lesser, had, in a work, which had been translated into the French, in 1745, by M. P. Lyonnet, entitled "Theologie des Insects," (the Theology of Insects,) spoken of some which had fed on rock and earths, or rather, which he *supposed* fed on them, *because they pierced them*. His translator, however, thought there was no ground for the belief; and, on reference to the "Introduction to Entomology," by the celebrated English entomologists, Kirby and Spence, I found the following passage, which places the *discovery* of the learned critic in its true light.

"I have said that insects, like other animals, draw their subsistence from the vegetable or animal kingdoms. But I ought not to omit noticing that some authors have conceived that several species feed upon mineral substances. Not to dwell upon Barchewitz's idle tale of East Indian ants, which eat iron, or on the stone-eating caterpillars, recorded in the memoirs of the French Academy,* which are now known to erode the walls on which they are found, for the purpose of forming their cocoon; Reaumur and Swammerdam have both stated the food of the larvæ of the *Ephemeræ* to be earth, that being the only substance ever found in their stomach and intestines, which are filled with it. This supposition, which, if correct, renders invalid the definition by which Mirbel and my friend Dr. Alderson, of Hull, long before him, proposed to distinguish the animal and vegetable kingdom, is certainly not inadmissible; for, though we might not be inclined to give much weight to Father Paulian's history of a flint-eater, who digested flints and stones, the testimony of Humboldt seems to prove that the human race is capable of drawing nutriment from earth, which, if the odorous Ottomaques can digest and assimilate, may afford support to the larvæ of the Ephemeræ. Yet, after all, it is perhaps *more probable* that these insects feed on the *decaying vegetable matter*, intermixed with the earth in which they reside, from which, after being swallowed, it is extracted by the action of the stomach—like the sand, that, from being

* x. 458.—Can these be the Doctor's *terebelli*?

found in a similar situation, Boralli erroneously supposed to be the food of many *Testaceæ*, though in fact an extraneous substance."—*pp.* 389—90.

Another *discovery* of Professor Caldwell, we find in his pamphlet on page 41:

"The living seeds of vegetables—say of wheat, rye, barley, and flax—have the power of maintaining, each its own specific temperature. Take, for example, two equal quantities, (let the measure be a bushel,) of the same sort of wheat, both of them fresh and living. Boil one of them so as to destroy its vitality, and do no injury to the other. The weather being cold, put the two parcels into two casks precisely alike in size, shape, and material; expose them to the same temperature, and introduce into them two thermometers of the same sensibility. Of this experiment, if correctly performed, the result will be, that the thermometer, surrounded by the dead wheat, will indicate the temperature of the atmosphere at the time, whilst that in the midst of the living wheat, will express a higher one."—*Phys. Vind.* [The Doctor does not say in what length of *time*.]

That *dry* seeds, which were in a complete *state of rest*, and not in a *growing*, or *germinating* condition, have a *temperature-preserving power*, is undoubtedly *new*. But it is to be questioned whether, in the above experiment, the *superior heat-conducting* power of the wet wheat may not fully account for the difference supposed to be observed. When seeds are *germinating*, oxygen is elaborated, carbonic acid evolved, and heat is generated, as in the malting process; but, that perfectly *dry* grain has any temperature-preserving power, independent of its imperfect conducting power, must be proven by more accurate experiments than those above detailed.

The *living* egg is supposed to be more difficult to congeal by cold than the *dead* one; but this only proves a superior resistance to *solidification*, and by no means indicates that *reduction of temperature* is also resisted.

While dwelling on the *novelties* and *curiosities* of the pamphlet before us, I will transcribe another specimen of his improved *logic:*

"Again: if, according to our assurance from the same authority, it be a fact, that 'the more warmly we are clothed, the less urgent becomes our appetite for food,' it follows, of course, that, in case our *clothing be sufficiently warm*, our appetite for food will be *entirely extinguished*. This is no high-drawn carricature. Nor is it intended to be so. It is, we say, a fair exposition of our author's wildness and extravagance in error."—*Phys. Vind., p.* 20.

It must be evident that Liebig's assertion, that clothing is an *equivalent* for a *certain amount of food*, no more justifies the inference that it might be made to *supersede all food*, than the recent improvements in the economy of stoves, for heating our houses, should lead us to the belief that one may be made which will give heat without consuming any wood.

In the same category must we notice some other *peculiar* facts and arguments, urged against the admission of chemical action within the sphere of the vital phenomena. These may be found on page 55 of Physiology Vindicated; and are, in substance, that chemical action cannot go on, if any other force is present and operative, and that the "chemical forces are nullified or deranged by a constant supply of fresh materials!"

If the brewer, he adds in proof, agitates a vat ful of malt, (wort) when he sets it to ferment, "*the principles of mechanical motion mingle* and interfere with those of chemistry, and prevent them from producing their legitimate results." But the chemist knows that it would be the *oxygen of the air which would mingle*, in consequence of the agitation, and that the chemical action, so far from being lessened, would be so far increased and accelerated, as to carry the liquid into the acetous fermentation. Mechanical agitation, in fact, aids chemical action in many cases; and the chemist accelerates his process by stirring. In testing, precipitates are thus caused to appear in a few seconds, which, at complete rest, would have required hours.

The process of crystalization, says the Doctor, is *prevented*, or rendered imperfect and irregular, by agitation. But the chemist believes, that, although rendered irregular and imperfect in respect to the *size* of the crystals, their deposition is actually accelerated by mechanical means.

To satisfy himself that the constant supply of fresh materials will *not* nullify or derange the chemical forces, something more is required than the *complicated one*, of the fermentation of beer or wine, which he proposes. We invite him to devote a day to a *very simple* and *strictly chemical* experiment, which will set this point at rest—provided his philosophy is not that of "very great curiosity and very bad eyes."

Let him take equivalent quantities of muriatic acid and solution of carbonate of soda, in separate and convenient vessels, and then let him pour them with care, in a constant and gentle stream, with any degree of agitation he pleases, into a third vessel; and if, at the end of the experiment, he finds any thing else than the definite chemical compound, com-

mon salt, he will have proved to the world that he knows more of chemistry than Davy or Berzelius.

An equal amount of chemical information is shown by the Professor, when he defies the chemist to exhibit a case of the combustion of carbon and the formation of carbonic acid, at a temperature of 98° to 100° Fah., *out of the animal body.* He has forgotten his frequent boast, that he was the first, in this country, to cause the ignition of charcoal by mixing it with nitric acid, to the extreme delight of the late Professor Woodhouse; or else he does not appreciate the fact, that this combustion is nothing but the combination of oxygen with carbon, and that the product is only carbonic acid; and that, although the heat of the combination goes far above 100° Fah., it *commences* below that temperature—the temperature being carried up by the caloric evolved by the first combination. A few facts more may be added. Powdered charcoal, left in a heap in the air, often becomes spontaneously heated, and sometimes ignited. From what cause? The combination of oxygen and the production of carbonic acid, at a temperature below 100° Fah. The same cause operates, and the same effects are produced, in the fermenting manure-pile, the heated mass of moist tan-bark, the drying of paint-oils, the spontaneous combustion of oily cotton, or the malting of grain—in all of which, the combination commences below 98°, and in many is not carried far beyond it.

It is no objection to say that hydrogen is also present in most of these cases, and may aid the process; for the same element is present in the animal body to produce the same effect.

It will be seen, therefore, that the propositions of Professor Liebig are not invalidated by the objections of our critic; and, although those propositions are not stated as *ascertained facts*, and doubtless contain many errors which future discovery must remove, as well as a few, although *very few*, statements that do not accord with our present knowledge; the cogency of the arguments is not diminished by the few flaws which have been really detected in the "Animal Chemistry," and the beneficial influence of the work cannot be prevented.

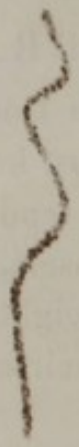
With these remarks I will conclude my animadversions on the singular pamphlet of Professor Caldwell; having perhaps already extended them too far. There are many parts of it equally illogical and unscientific with those which have been exposed; but the good sense of the reader, who has perused the work of Liebig *understandingly*, will find nothing which presents a serious objection.

If I have been apparently severe in the exposition of the sophistry and misapprehensions of the venerable critic, I humbly plead, in exculpation, that when spurious facts, and *ingenious*, although *inconsequent* inductions, tending to retard improvement, are boldly announced by high authority, in the most *confident manner* as well as *polished style*, nothing but the *stern exposition of the truth* can avert the mischief, which, in the minds of many, may be done to the cause.

Physiology is yet, to some extent, under the dominion of an imperfect philosophy: too many *a priori* propositions are received as axioms by her followers, which ought to be submitted to the test of observation and experiment. This task is, to some extent, proposed in the late works of Liebig, who himself leads the way in one important branch of the investigation. When it shall have been fully performed, in the course of time, on correct Baconian principles—according to which no ascertained *truth* can be considered impious or immoral in its tendency—all theory must be submitted to facts, and all reasoning be strictly logical induction or comparison—the science of physiology purified, simplified, and regenerated, will shed redoubled lustre on the healing art.

ERRATA.

On page 8—sixteenth line from the bottom, the last word in the line (the second *not*) must be left out in reading. On page 10—last line—for *primative* read *ultimate*. On page 14—twenty-first line from bottom, insert, in reading, the word *by* before *comparing*. On page 23—eighth and ninth lines from top, instead of "the *meaning* of their experiments" read "the *mean* of their experiments."

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