wish to convey no personalities, and have therefore wrote as generally as the nature of the case would admit. We are aware that our generalizing may be taken amiss by certain members of the faculty, who feeling equally with ourselves the injurious tendency of the system of quizzing, give to it their most decided disapprobation. It is an unfortunate, although a necessary evil, that when there is any variation from correct thinking or acting in one or more members of a body corporate, the blame is thrown not on the individual, but on the corporation. We sincerely hope, however, that these our observations will be taken in the spirit in which they are intended. When we observe the great strength which rival institutions on each side of us are acquiring, we tremble for our alma mater. We would wish to purge her of every thing which can derogate from the respectability of her diplomas, and to open the eyes of all her professors to the fact, that it will require from them great exertions to retain the standing which this school now holds. Let them not, we beseech of them, continue to deceive themselves by applying this flattering unction to their minds, that, as the school of Pennsylvania was once the great medical establishment of the United States, it must for ever remain so. It was their Woodhouse, their Barton, their Rush, and their Wistar, that gave to it this high and elevated standing, and although their great and deserved reputation may have thrown a halo around it, which it will require some time to dissipate, still the hour is rapidly approaching when the university of Philadelphia, must be measured by that talent which she possesses, not by that which she could formerly number.

To the Editor of the American Medical Recorder of Original Papers and Intelligence.

Sir,—In the number of the work above mentioned for July 1819, you employ the following language, in speaking of the calorimotor, a galvanic instrument of which I had published an account a short time before: "This instrument appears to us,

nothing more than an extension of the elementary battery of Wollaston."

Subsequently, in conversation, I pointed out the injustice of this treatment. You admitted the propriety of my animadversions, and promised to correct your error, alleging your own incapacity to judge of the subject, and the necessity you had been under of adopting the impressions of others whom you had considered as judges. It seems however that you have concluded not only to reiterate in the Recorder for April, the opinion above mentioned, but to support it by statements which I shall prove could never have been made sincerely, had you understood the authors to whom you refer.

In the first place I must observe, that the calorimotor as represented in the plate annexed to my memoir, consists of *four galvanic surfaces*, or *two pairs*,* with *one recipient*. I assert, that you cannot adduce any *analogous contrivance*, *small* or *great*, of *Dr. Wollaston* or *any other person*. Such a combination of *uninsulated* surfaces, would not probably have occurred to any one deeming galvanism to be electricity under a slight modification; as that ingenious philosopher has supposed and endeavoured to prove. I should have hoped that before criticising my pamphlet, you would at least have examined the engravings which accompany it. A much more vivacious and intense ignition may be produced, by two large galvanic pairs situated as I have arranged them for one recipient, than could be excited by double the extent of surface in one pair formed like Wollaston's. After my experiments with one large pair were known, Mr. Wetherill, expecting to produce more striking or powerful results, made a pair of double the size. By this, a wire of platina or iron, whether large or small, could not be heated much more than red hot; though similar wires by my smaller apparatus of two uninsulated pairs, were rapidly fused or burned off. Mr. R. Peale having also made an apparatus of one pair more extensive than mine, was disappointed as Mr. Wetherill had been.

I advised him to separate the two surfaces into four surfaces alternating. When thus altered, platina or iron duly exposed to

* See plate and description.

their action, were rapidly thrown into a state of fusion or combustion.

I will next observe, that the action of a pair of galvanic surfaces was known not only before the time of Wollaston, but before that of Galvani himself. The taste excited by putting two plates of different kinds of metals one above, and the other beneath the tongue, and bringing into contact, their projecting extremities, was just as much the effect of a galvanic discharge as the ignition produced by the elementary battery. Hence it must be evident that it is only in size, arrangement, and application that this last mentioned apparatus can possess originality. But in all these points I trust that the characteristics of the calorimotor are at least equally peculiar.

Yet as Wollaston constructed a minute galvanic pair *for producing* ignition, and I constructed a pair enormously large by which ignition *may be produced;* you seem to infer that the one must be an extension of the other. It did not occur to you, that both, might be opposite varieties of a contrivance previously existing. Had the novelty of Wollaston's battery been dependent on the isolated fact of its producing ignition, your remarks had been more justifiable. But it was nothing new to produce galvanic ignition, and from his account of the elementary battery which I shall presently quote, it does not appear that he considered the result as meriting attention independently of the *minuteness of the means* by which it had been accomplished. It is not to the fact simply of his *igniting wire* that he attaches importance, but to the circumstance of his having *ignited it by an apparatus almost inconceivably small.* To use his own words, the purpose of his experiments was "to ascertain the most compendious form of apparatus by which visible ignition might be produced."

I am aware that with plates of a moderate size he made an instrument for producing instantaneous light, but having neither seen any account of that contrivance in the scientific journals, nor heard of any, I am doubtful whether it was considered as new, unless in the arrangement or application. Even this enlargement of his battery destroyed the most striking, and what he seems to have deemed the most important distinction, between it

and an ordinary pair: and admitting that he were the first to employ such a pair for the purpose of ignition, there had on that account been no more reason for representing him to be the inventor of this most simple and most ancient galvanic combination, than for attributing the invention of the Voltaic pile, to Carlile and Nicholson, because of their priority in applying it to the decomposition of water. Granting therefore that the ignition which I produced, was nothing but an extension of that which he had before effected, it does not follow that my apparatus was nothing but an extension of his; nor that there may not be as much originality in the enlargement of the original galvanic pair, under one form, for the purpose of investigation, as in the diminution of it under another form, for the same object.

But unfortunately for the correctness of your strictures, you seem to test my pretensions to experimental invention by one result of my contrivance, totally overlooking that, by which charcoal was shown to be impermeable to the galvanic fluid, as extricated by the calorimotor. The experiment by which this was proved, is thus mentioned in my memoir:

"The thinnest piece of charcoal intercepts the calorific agent, whatever it may be. In order to ascertain this, the inside of a hollow brass cylinder, having the internal diameter two inches, and the outside of another smaller cylinder of the same substance, were made conical and correspondent, so that the greater would contain the less, and leave an interstice of about one-sixteenth of an inch between them. This interstice was filled with wood, by plugging the larger cylinder with this material, and excavating the plug till it would permit the smaller brass cylinder to be driven in. The excavation and the fitting of the cylinders was performed accurately by means of a turning lathe. The wood in the interstice was then charred by exposing the whole covered by sand in a crucible to a red heat. The charcoal, notwithstanding the shrinkage consequent to the fire, was brought into complete contact with the inclosing metallic surfaces by pressing the interior cylinder further into the exterior one.

"Thus prepared, the interior cylinder being made to touch one of the galvanic surfaces, a wire brought from the other galvanic surface into contact with the outside cylinder, was not affected in

the least, though the slightest touch of the interior one caused ignition. The contact of the charcoal, with the containing metals probably took place throughout a superficies of four square inches, and the wire was not much more than the hundreth part of an inch thick, so that unless it were to conduct electricity about forty thousand times better than the charcoal, it ought to have been heated; if the calorific influence of this apparatus result from electrical excitement."

To meet you, however, on the very ground which you have chosen, I must first quote Dr. Bostock's account of the experiment, of which, it would seem, you consider mine to be an extension; and the passage in my memoir, in which the latter is described.

"Dr. Wollaston constructed a very curious apparatus, which he calls an elementary galvanic battery, the *object* of which is to exhibit the *most minute* arrangement of *electrical* substances, by which *visible* ignition can be produced. The *smallest* that he has constructed, consists of a thimble without its top, flattened, until its sides were about one-fifth of an inch asunder; a small plate of zinc was then contrived to be fixed within the thimble, but without touching it, and a proper appendage of platina wire was added. The zinc plate was less than three-fourths of an inch square, and even when water was employed that contained one-fiftieth part of sulphuric acid, a platina wire of one three-thousandth part of an inch in diameter, and from one-thirtieth to one-fiftieth of an inch in length, was readily fused."

The passage in my memoir above alluded to, is as follows: "I had observed that as the number of pairs in Volta's pile had been extended, and their size and the energy of interposed agents lessened, the ratio of electrical effects to those of heat had increased till in De Luc's column they had become completely predominant; and on the other hand, when the pairs were made larger and fewer, (as in Children's apparatus) the energy of interposed agents not being lessened, the calorific influence had gained the ascendency. I was led to go further in this way, and to examine whether one pair of plates of enormous size, or what might be equivalent thereto, would not exhibit heat more purely, and demonstrate it equally with the electric fluid a primary product of galvanic combinations.

The elementary battery of Wollaston, though productive of an evanescent ignition; was too minute to allow him to make the observations which I had in view."

How can an experiment designed for the examination of the heat moving powers of the *largest* galvanic pair ever made, be considered as an extension of another, which has for its "object, the most minute arrangement of electrical substances, by which visible ignition can be produced?" Besides, an extension of the generating surfaces alters the character of the ignition. The phenomena of the minute battery of Wollaston, appear much more liable to be mistaken for those of electricity, than the sluggish movements of the calorific principle, as exhibited in a galvanic pair of the largest size. By such an apparatus, though inadequate to fuse a very small platina wire, I have seen a strip of tin plate, nearly four inches broad, kept at a gentle heat for a considerable time.

Every galvanic observation or modification, subsequent to the use of the original elementary pair, may be said to have grown out of that contrivance. By multiplying such combinations, the agency of electricity in galvanism, was displayed. By lessening the original pair, Wollaston ascertained the curious result, that even when *very minute*, ignition may be effected by it. By enlarging it, I sought to show the phenomena inconsistent, with the idea that the ignition resulted merely from an electrical discharge. Commencing both from the same original point, we proceeded in opposite directions, and with different designs. The fact, that ignition might be produced by galvanism, having been long known, it was, in the examination of the characteristics of this ignition, with a view to the nature of its cause, that further elucidation was expected by me. Assuming the cause to be electricity, he neither suited his apparatus, nor directed his thoughts to such an investigation; while my apparatus, expressly designed for this purpose, by magnifying results, made them more susceptible of scrutiny, and using charcoal as a test, I availed myself of the opportunity to demonstrate the existence of a decisive difference between the calorific agent in galvanism, and the fluid with which it had been identified by him. It were then very unfair to convey the idea, that my experimental enquiry was

nothing more than an extension of his. I have already shown, that this notion, could not, with propriety, be conveyed, with respect to my apparatus.

Moreover it will be admitted that experiments derive much of their value from the hypothetical considerations, by which they are induced, or with which they are associated. You have described the theory which I have advanced as "new, plausible, and ingenious."

In the following passage in my memoir, it is briefly stated:

"According to my view, caloric and electricity may be distinguished by the following characteristics. The former permeates all matter more or less, though with very different degrees of facility. It radiates through air with immeasurable celerity, and distributing itself through the interior of bodies, communicates a reciprocally repellent power, to atoms but not to masses. Electricity does not radiate in or through any matter, and while it pervades some bodies as metals with almost infinite velocity; by others it is so far from being conducted that it can only pass through them by a fracture or perforation. Distributing itself over surfaces only, it causes reaction between masses, but not between the particles of the same mass. The disposition of the last mentioned principle to get off by neighbouring conductors, and of the other to combine with the adjoining matter or to escape by radiation, would prevent them from being collected at the positive pole, if not in combination with each other. Were it not for a modification of their properties consequent to some such union, they could not, in piles of thousands of pairs, be carried forwards through the open air and moisture, the one so well calculated to conduct away electricity, the other so favourable to the radiation of caloric.

"Pure electricity does not expand the slips of gold leaf between which it causes repulsion, nor does caloric cause any repulsion between the ignited masses which it expands. But the compound fluid extricated by galvanic action which I shall call electro-caloric distributes itself through the interior of bodies, and is evidently productive of corpuscular repulsion, it is in this respect more allied to caloric than to electricity.

"It is true that when common electricity causes the deflagration of metals, as by the discharge of a Leyden jar, it must be supposed to insinuate itself within them, and to produce a reaction between their particles. But in this case, agreeably to my hypothesis the electric fluid combines with the latent caloric previously existing there, and adding to its repulsive agency, causes it to overpower cohesion."

Agreeably to this doctrine, the mysterious difference between galvanism and electricity is explained, by supposing the former, the result of the joint action of caloric and the electric fluid, their ratio to each other in quantity varying according to the apparatus used to produce them. In De Luc's column, the electric fluid is presumed to be at a maximum, the caloric of course at a minimum; the phenomena being apparently those of mechanical electricity, though the means of production are galvanic.

In the pile of Volta, or the troughs of Cruickshank and others, both fluids are conceived to be active, the ratio of their quantities to each other according to the law, stated in the passage already quoted.

To complete the illustration, a third form of apparatus was requisite, in which caloric should be at a maximum, electricity at a minimum.

Your animadversions in the Recorder for April, are obviously intended to promulgate the impression that Children's Battery and Wollaston's had left nothing further to be desired. But as the characteristics of De Luc's column are, that the size of the galvanic surfaces shall be very diminutive, their number very great; to attain the opposite extreme, it was necessary to seek the smallest number, and the largest size that could be made to avail. Children's apparatus wanted the former characteristic; Wollaston's the latter; and though I was the first to supply this deficiency, although my theory of galvanism which you call "plausible and ingenious," is as far as *you know* new, and it must be evident that the calorimotor grew up with, and had become a necessary prop to that hypothesis; you allege "If Professor H. therefore, wished to produce a very powerful evolution of heat without a concomitant production of electrical phenomena, his course of

proceeding with a knowledge of the above facts was at once simple and obvious."

As even the discovery of the new world appeared simple and obvious to some persons after it had been accomplished by the great Columbus, an author must be monstrously vain and madly sanguine, who expects that his performances may not be viewed in the same way. The language you have here adopted might be employed to disparage discoveries which have gained the highest applause. You might have said when Nicholson and Carlile had effected the decomposition of water by the Voltaic pile, if Sir H. Davy wished to decompose substances in which the elements were more forcibly combined, "his course was at once simple and obvious," he had only to use an apparatus as much more extensive, as the decomposition was more difficult. If Dr. Black wished to ascertain whether any, and what principles were extricated during the calcination of lime-stone, "his course was at once simple and obvious," he had only to heat it in a retort, and catch the products. After a peculiar gas had thus been discovered in lime-stone, Dr. Priestley being desirous to learn whether there were not other kinds of gas in nature, "his course was at once simple and obvious;" he had only to subject other substances to the same process. When Watt had perfected the steam engine, and boats had been moved by paddles, if Fulton wished to propel them by steam, "his course was at once simple and obvious."

The galvanic fluid as generated by Children's apparatus, permeates and ignites charcoal. It is therefore a galvanic instrument of the middle class, and not analogous to the calorimotor. The vivacity of the ignition in Wollaston's battery may awaken some doubt whether the disproportion between the component principles of the galvanic fluid, as excited by it, is at a maximum as in the case of a very large pair like mine. Hence the contrivances of these eminent chemists are not only in form unsuitable to be classed in the extreme, the opposite of that, in which we find De Luc's column; but the phenomena do not warrant such a classification.

I adverted in my memoir to Wollaston's Elementary Battery, and the ignition produced by it; when stating the fact of my having

made, for the purpose of investigation, a larger pair than any used before. Independently of the peculiarities of such a pair, already mentioned, there is in the distribution of the surfaces, a novelty which may be better estimated by turning to the latter part of the paragraph already quoted from Bostock. He says, speaking of Wollaston: "In the formation of his minute apparatus this sagacious experimentalist made an observation of great importance in enabling us to produce as much effect as possible from the smallest quantity of materials. He remarks that the zinc plate should have a counterpart of copper opposed to each side of it; for in the usual arrangement, although both sides of the zinc are oxidated, that side only is efficient which has a copper surface opposed to it." Agreeably to this arrangement *so very highly commended*, there must be *twice as much copper as zinc*. In my apparatus, *every zinc surface has its counterpart of copper excepting the last*; though *the quantity of the metals is the same*. I have reasons in addition to these, for believing I have adopted a more efficient mode of using the same extent of surface, than had been devised before.

In support of the following passage you adduce *no authority*, I aver it to be a statement not only *groundless*, but contradictory to *Wollaston's own words*, to *facts* that are *well known*, and to your own admission that my theory is as far as you know new. You say, "in conformity with those principles, Dr. Wollaston constructed an apparatus of two plates only, and found that by thus lessening the number of the plates, although very small in size, heat was powerfully evolved without a proportional production of electrical effect." So far is Dr. Wollaston from distinguishing the heat produced by his apparatus from the electrical effect, that he considers it an electrical effect. He is the author of an experiment to show that the galvanic, and electric fluids are the same, the latter requiring to be emitted from an impalpable point. Had he made the distinction you allege, he had anticipated my hypothesis; the fundamental basis of which is, that the ignition of wire, by the calorimotor results from a current of caloric, not of electricity. The diffidence which qualifies your commendations with the words, "as far as I know," would never have

operated with better advantage to yourself than when writing the above mentioned account of Wollaston's hypothetical inferences. Let Wollaston speak for himself. Annals of Philosophy, vol. 6. page 209. He thus expresses himself in a letter to the editor:

"DEAR SIR—I now send you a description of a *small battery* which I shewed you some time since, and shall feel obliged by the insertion of it in your Annals. Since the ignition of metallic wires is rather instructive with respect to the vast *quantity of electricity* evolved during the solution of metal, I made three years since a series of experiments for the purpose of ascertaining the most compendious form of apparatus by which visible ignition might be shewn.

"The result of these trials was, that a single plate of zinc one inch square, when rightly mounted, is more than sufficient to ignite a wire of platina even when the acid employed is very dilute."

He says again in the last paragraph of the same letter, "Although in this description I have mentioned a wire $\frac{1}{30}$ of an inch in diameter, I am doubtful whether this thickness is the best, I am however pursuaded that nothing is gained by using a finer wire; for though the quantity of matter to be heated, be thus lessened, the surface by which it is cooled does not diminish in the same ratio, so that when the cooling power of the surrounding atmosphere is the principal obstacle to ignition, a thicker wire which *conveys more electricity* in proportion to its cooling surface, will be more heated than a thin one, a fact which I not only ascertained by trials on these minute wires, but afterwards took occasion to *confirm* on the largest scale by means of the *magnificent battery* of *Mr. Children* in the summer of 1813."

Let me next cite the impressions of an editor, who does not write, on what he does not understand. "Dr Wollaston's elementary galvanic battery described in a late number of the *Annals of Philosophy*, constitutes a discovery of considerable importance. It demonstrates the quantity of electricity which is disengaged during the chemical action of acids on metals, and thereby serves to throw much additional light upon the still obscure

theory of galvanism."* It is unnecessary to state the only deduction which can arise from a comparison of these quotations, with that above made from your critique.

The following statement is equally unfounded: "Thomsdorf, Thenard, Fourcroy, Vauquelin, and particularly Children, have demonstrated that the *electrical* action is not increased by the increase of the size of the plates, but by the increase of the numbers of pairs, and that on the contrary the evolution of heat is increased with the size of the plates."

Since the simple galvanic combination of Wollaston was supposed to afford instruction by the quantity of *electricity* given out, more complex combinations which by your own premises are more highly electrical, could not but be viewed in the same way.

The reason given by Dr. Wollaston in the passage above quoted, why a large wire will be more easily heated than a small one is, that it "conveys more electricity," and he has been confirmed in this explanation "by means of the magnificient battery of Mr. Children."

Is it not then evident that he considered electricity as the common cause of the heat in both his battery, and that of Children. If evidence be required of Children's opinion, let us turn to Dr. Thomson's account of this battery. In *Annals of Philosophy*, vol. 7. p. 11. he tells us, "The order in which metallic wires connecting the two poles of this battery became red hot, was as follows:

Platinum	Copper	Zinc
Iron	Gold	Silver

"*Mr. Children conceives, that the metals conveying electricity, became red hot inversely as their conducting power.*" Sir Humphrey Davy, in mentioning a large battery of Mr. Children, says "This battery, when in full action, had no more effect on water or on the human body, than one containing an equal number of small plates; but when the circuit was made through metallic wires, the phenomena were of the most brilliant kind."† The rationale of this difference is, according to the same author, that

* Thomson's Annals. † Davy's Elements, p. 84.

"the quantity in the small plates is as much, or more, than such imperfect conductors as water, and the human body can carry off by a small surface, whilst better conductors, can transmit the whole quantity afforded by the large plates, even when used in very thin lamina or wires."*

From these passages it must be evident that Sir H. Davy considers both the ignition produced by a few large pairs, and the shock or decomposition resulting from many small ones, as the effect of the same electric fluid, in quantity greater in the first case, in the last more intense. It will be found on consulting Thenard, that he has no other conception, than that all the effects of the pile result from the electricity which it generates or puts in motion.

It is then undeniable, that the very authors whom you erroneously cite, as having demonstrated that heat might be produced by galvanic apparatus without a proportional production of electrical effect, really consider the heat thus produced, as an electrical effect, and of course, had not discovered it to be otherwise. Hence, correcting one of your quotations, it would read thus: "Heat and electrical effect, was evolved without a proportional production of electrical effect!!!"

Experimental novelties have a two-fold value; first, as they add to that captivating splendour to which science owes so much of its power, in affording innocent and rational amusement; and secondly, as they tend to create new distinctions, either between facts or principles, previously confounded. Those who have witnessed the combustion of wire by the calorimotor in its improved state of two uninsulated pairs, with the attendant phenomena of the inflamed hydrogen, allege that it affords one of the most splendid spectacles in the chemical course, especially when compared with the simplicity of the process, and the facility of repetition. I submit it to my pupils to say how far any experiment with a *thimble*, and an almost inconceivably small wire, can ever, in this respect, come in competition with those which I showed them by means of the instrument in question. According to the second view of the subject, I deem it of more importance to have ascertained, that a

* Traite de Chimie.

large galvanic pair, or even two uninsulated pairs, *will not ignite charcoal*, than that a small pair *will ignite wire*. The first mentioned experiment distinguishes the fluid put in motion by one or two uninsulated pairs, from that evolved by the Voltaic pile, which the latter does not.*

My theory of galvanism has been republished in some of the most respectable journals in Europe, as well as in this country, without any idea being excited, that I have not, as you would insinuate, "given unto Cæsar, the things that are Cæsar's;" an injunction originally intended to sanction the payment of a tribute actually required by Cæsar, and in a coin carefully ascertained by competent judges to bear his stamp. But it was not designed to authorize the officious and unskilful to render unto Cæsar, things, which Cæsar had not demanded of them, which he would despise as coming from them, and which might, for aught they could judge, be the property of another.

Wollaston is too well fortified by his splendid genius and extensive acquirements; too well surrounded by his scientific compeers, to require the aid of your eloquence or erudition, to guard him from the consequences of any wrong which I have the power or disposition to commit. And I doubt, if your readers will feel themselves indebted to you, for deviating from the obvious duty of a recorder of original papers and intelligence, in order to communicate the opinions or misconceptions of yourself or your advisers, on the subject of my calorimotor.

* For the information of readers who may not have witnessed this spectacle, I beg leave to state, that by the time the calorimotor is completely immerged in the acid solution, the wire in the forceps is rendered white hot, and takes fire, emitting the most brilliant sparks. In the interim, an explosion usually gives notice of the extrication of hydrogen in a quantity adequate to reach the burning wire. Immediately after the explosion, the hydrogen is reproduced with less intermixture of air, and rekindles, corruscating from among the twenty interstices, and passing from one side of the machine to the other, in opposite directions and at various times, so that the combinations are innumerable. The flame assumes various hues, from the solution of more or less of the metals, and a froth apparently on fire, rolls over the sides of the recipient. When the calorimotor is withdrawn from the acid solution, the surface of this fluid for many seconds, presents a sheet of fiery foam.

Since writing the above, I find from the following observations of the celebrated Dr. Thompson, that *he* considers my apparatus as a "new modification of the galvanic battery."

"*Galvanism.*—Dr. Hare, Professor of Chemistry in the Medical Department of the University of Pennsylvania, has published (in a paper to be found in the *Annals of Philosophy*, xiv. 176,) a theory of galvanism differing considerably from all those hitherto started. According to him, the galvanic fluid is a compound of caloric and electricity. The electricity is increased by the number of pairs of plates, and when this number is very great, as in De Luc's column, the calorific effects become evanescent. The caloric is evolved by the increase of the surface, and he has shown that it may be very intense, when only a single pair of plates, or what is equivalent to it, is used. He has given us a drawing of a galvanic battery constructed on this principle, which produces intense ignition without any electrical phenomena.

"As I have not yet had an opportunity to study the phenomena exhibited by this new modification of the galvanic battery, I should consider it as improper to make any remarks on Dr. Hare's ingenious hypothesis at present. What I have to say on the subject, therefore, I shall reserve for another opportunity."—*Thomson's Annals of Philosophy.*

EXPLANATION OF THE PLATE.

A *a*, Fig. 1st, two cubical vessels, 20 inches square, inside. *b b b b* a frame of wood containing 20 sheets of copper, and 20 sheets of zinc, alternating with each other, and about half an inch apart. T T *t t* masses of tin cast over the protruding edges of the sheets which are to communicate with each other. Fig. 2, represents the mode in which the junction between the various sheets and tin masses is effected. Between the letters *z z*, the zinc only is in contact with the tin masses. Between *c c* the copper alone touches. It may be observed, that, at the back of the frame, ten sheets of copper between *c c*, and ten sheets of zinc between *z z*, are made to communicate, by a common mass of tin extending the whole length of the frame, between T T: but in front, as in Fig. 1, there is an interstice between the mass of tin connecting the ten copper sheets, and that connecting the ten zinc sheets. The screw forceps, appertaining to each of the tin masses, may be seen on either side of the interstice: and likewise a wire for ignition held between them. The application of the rope, pulley, and weights, is obvious. The swivel at S permits the frame to be swung round and lowered into water in the vessel *a*, to wash off the acid, which, after immersion in the other vessel,

might continue to act on the sheets, encrusting them with oxide. Between *p p* there is a wooden partition which is not necessary, though it may be beneficial.

Fig. 3, represents an apparatus alluded to, page 419.* It consists of a couronne des tasses, reduced to a form no less compact than that of the trough. Hollow parallelopipeds of glass are substituted for tumblers or cells. The plates are suspended to bars counterpoised like window-sashes.

The advantages are as follows. The material is one of the best non-conductors, is easily cleansed, and is the most impervious to solvents. The fracture of one of the cups is easily remedied by a supernumerary. They may be procured (as in the United States) where porcelain cannot be had. The shock from 300 pairs is such as few will take a second time. Some of the effects have already been stated.†

At Fig. 4, one of the hollow glass parallelopipeds on an enlarged scale is represented.

Remarks by Dr. Eberle.

I HAVE not erased a single word from the above reply, that the offended professor, may not imagine that I deprecate the severity of his animadversions, any more than I dread the force and acuteness of his reasoning.

The statement given by the professor, relative to the conversation that passed between us, is, I explicitly aver, erroneous, as to its import. I would not, however, be thought to believe, or wish to insinuate, that the misrepresentation arose from design. I am certain, that this cannot have been the case. Soon after the publication of my remarks, on his calorimotor, he accosted me, in nearly the following words: "If the person, who wrote the remarks on my calorimotor, published in the last number of the Recorder, had wished to do me justice, he would have quoted this passage," (at the same time pointing out the passage alluded to,) "instead of the one, which he did quote." I replied, that I wrote the article myself; and, in order to let him know, that the opinion, I expressed in that article, is entertained also by others, as competent at least, to give an opinion upon subjects of this kind as himself, I stated, that not wishing to rely wholly on my

* Of Vol. II. Hen. Chemistry.

† The glasses may be had by applying to Edw. A. Pearson, No. 71 Cornhill, Boston.

CALORIMOTOR.

Fig. 1.

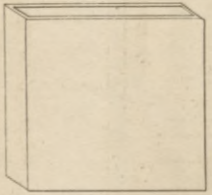

Fig. 1.

Fig. 2.

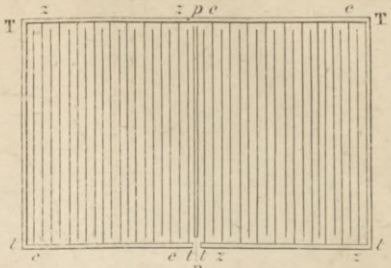

Fig. 2.

Fig. 3.

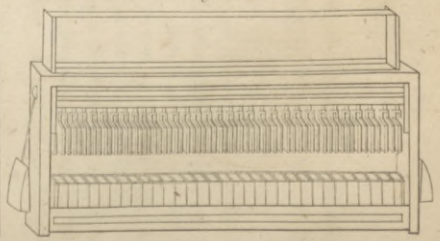

Fig. 3.

Fig. 1.

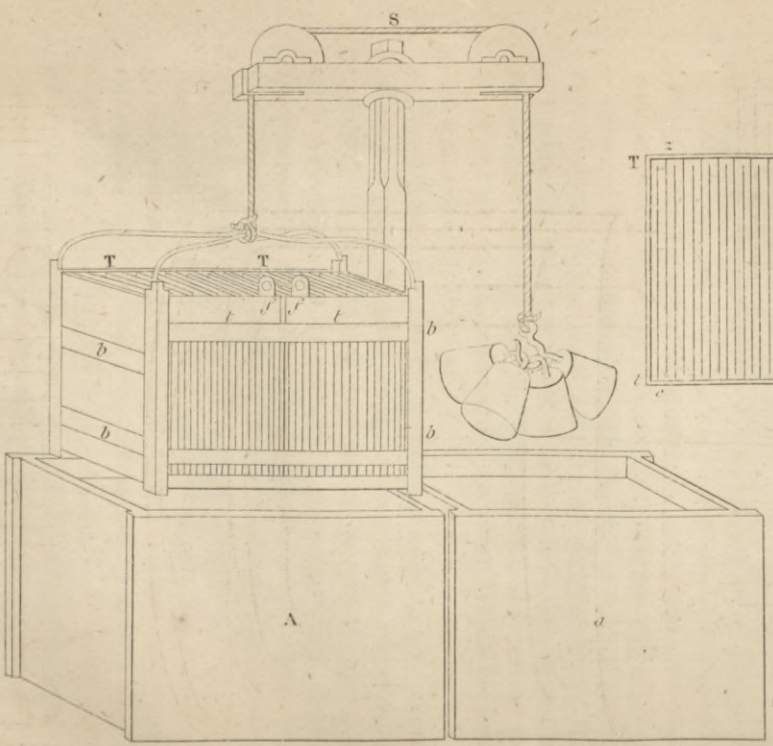

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Fig. 1.

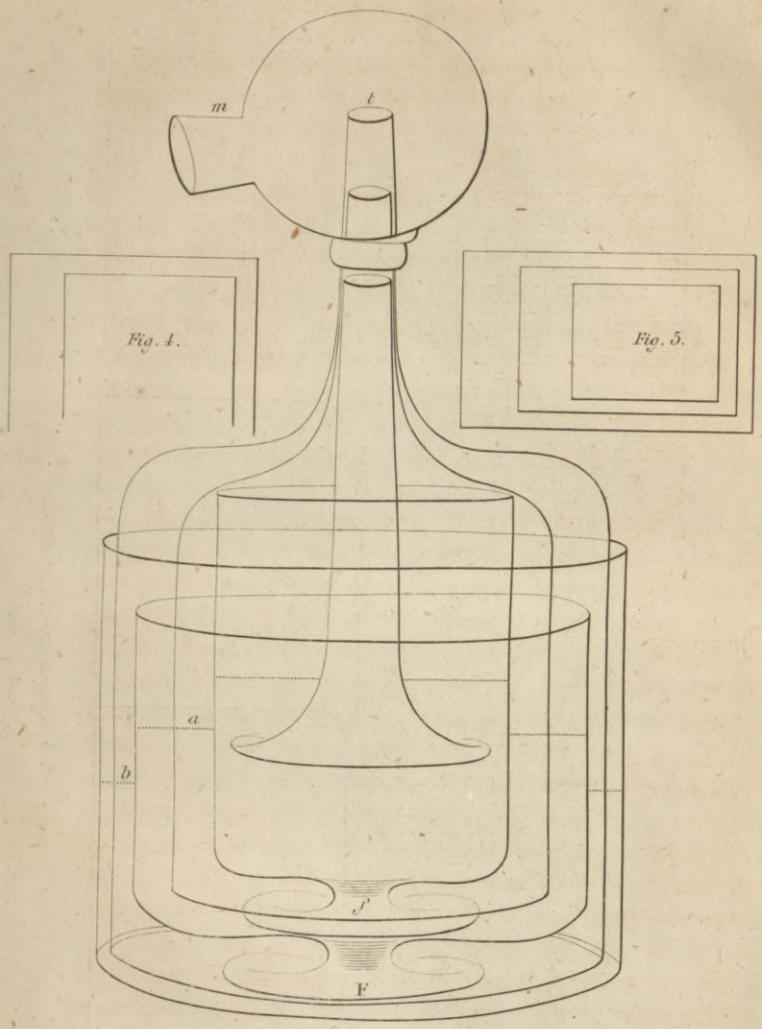

Fig. 4.

Fig. 5.

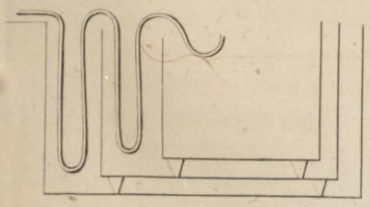

Fig. 3.

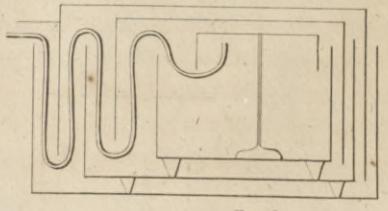

Fig. 2.

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own judgment in the present case, I had consulted several competent gentlemen on the point in question, and that they concurred with me in opinion. I further stated, that I would again examine into the subject, and if I should find myself in error, I would, in a succeeding number of the Recorder, cheerfully correct my mistake.

Having, however, my opinion confirmed, rather than shaken by a second examination of the professor's pamphlet, I again, in reply to some observations of Dr. Hall on this subject, stated my conviction, that his apparatus is in fact and principle, nothing more, than an extension of Wollaston's elementary battery. This opinion, I supported with arguments, which the professor labours to invalidate, in the above reply. Let us see how he has succeeded.

In the first place, then, he alleges, that his apparatus consists of *two pair* of plates in *one recipient*, whilst Wollaston's consists only of *one pair*, in one recipient, and that therefore, his apparatus cannot be considered as an extension of the latter. Let us try this argument. Wollaston constructed an apparatus, in which there are only *two minute* galvanic surfaces; this apparatus evolves an intense heat, without a concomitant production of electrical effect. Dr. Hare at first constructed one consisting of *two large* galvanic surfaces; and, as might have been expected from the experiments of Children, and from the effects of the elementary battery of Wollaston, very intense heat was evolved by it. So far, then, the professor's calorimotor was truly and obviously nothing more, than an extension of Wollaston's battery. They were strictly analogous, both in effect and in principle of construction; the one was an apparatus of *two minute*, and the other, one of *two large* plates. But, he did not stop here; he made a *duplication* of his first apparatus;—that is, he combined *two simple* calorimotors, each having *two* galvanic surfaces, in such a manner, as to produce one, of *four* galvanic surfaces, and capable of being immersed, in the same recipient. By this contrivance, then, he obtained the *combined effects* of *two simple* apparatus, constructed on the principle of Wollaston's battery. It must not be forgotten however that a mere duplication of a *simple* apparatus, is insuffi-

cient to alter the principle of its action, or the analogy of its structure. If the professor were asked, to prove, that a *double barrelled gun*, is not in fact and in principle, an extension of *a pistol*, I am at a loss to know, by what mode of reasoning, he would attempt this, unless it should be, by arguments similar to those which he here employs, in order to prove that his calorimotor is not in fact, and in principle, an extension of Wollaston's battery.

Again he says, "the action of a pair of galvanic surfaces was known, not only before the time of Wollaston, but anterior to that of Galvani himself. Hence, it must be evident, that it is only in *size*, *arrangement*, and *application*, that Wollaston's apparatus can possess originality. But, in all these points, I trust, the characteristics of the calorimotor, are at least equally peculiar." What does this prove? certainly nothing more than that the professor's calorimotor differs in *size*, *arrangement*, and *application*, from Wollaston's. As to its unlikeness to all others in *size* and *arrangement*, I am not at all disposed to dispute. I never did intimate any doubts on this score. I stated explicitly in my remarks, that as a *mechanical* contrivance, I thought it entitled to merit; and I am certain, that the professor will do me the justice to acknowledge, that I have never asserted it to be similar in *size*, to that apparatus, of which I consider it an extension. The fact is, the principal reason why Wollaston's battery was inapplicable to the purpose which Dr. Hare had in view, in the construction of his apparatus, according to his own confession, is, "*that it was too minute to allow him to make the observations he had in view.*" He was therefore obliged to construct an apparatus of a larger size, on the same principle, in order that he might apply it to his particular purposes. It consequently differed in *size*, *arrangement*, and *application* from Wollaston's battery, and cannot therefore, according to Dr. H. be considered an extension of it. Let us try this logic. A. makes a pistol, B. wishes to shoot wild geese; he finds the pistol "*too minute*" for his purpose. He therefore "examines" what effect "*one enormous*" barrel will have—whether it will not make more noise, and carry a bullet further than the pistol. He finds it to do so; but he is still not satisfied; he

seeks to increase the efficacy of his large barrelled gun; he adds another barrel,—he constructs a gun of two "*enormous*" barrels, and it answers his purposes. Now, as this gun undoubtedly differs from the pistol in "*size*, *arrangment*, and *application*," it cannot be considered, according to the dialecticks of the professor, an extension of it. *Quid vetat ridens discere verum?*

As to its *application*, I think I am warranted in asserting, that the *calorimotor* differs in no respect as a *calorimotor* or *heat-mover*, from Wollaston's battery. Both these apparatus are employed to evolve great degrees of heat; and both do so, by virtue of the same law of galvanic surfaces;—that law, by which the electrical action of a battery is not increased by the increase of the size of the plates, but by the increase of the number of their pairs, whilst the *heat-moving power* is increased with the size of the plates, and the *decrease* of the number of pairs. It is upon the identity of effect, as *heat-moving powers*, depending on identity of principle of construction, and by consequence, on identity of galvanic law, that I predicate the opinion I entertain of the perfect similarity in the essential characteristics of the two instruments, and that I consider the calorimotor of Dr. Hare as differing in nothing from the elementary battery of Wollaston but in external conformation, and that it is manifestly and truly, only an extension of it. He seems to lay much stress on the *object* which Wollaston had in view in the construction of his battery, and the theoretical notions he entertained respecting galvanism. He thinks that, as he himself had a different object in view in the construction of his apparatus, it cannot be considered as arising out of Wollaston's. Can the purposes, for which an apparatus is designed, or the particular theory which it is intended to confirm, alter the nature of its construction, or the laws of its action? If Wollaston's theoretical notions relative to heat and electricity, differ from those of Dr. Hare, does it follow, that the elementary battery of the former, differs in the laws of its action and the principle of its construction, from the calorimotor of the latter? Certainly I may leave him to enjoy the triumph of such logic.

It appears, by his own account, that in consequence of the expe-

riments of Children and De Luc, he was led to examine, whether by extending the principle which was demonstrated to exist by these experimentalists, he might not exhibit heat more purely by using "*one pair* of *plates* of enormous size, or *what might be equivalent thereto. The elementary battery*, he says, "*of Wollaston was too minute to allow him to make the observations I had in view.*" Is this not, in fact, acknowledging that Wollaston's battery wanted but *extension*, or *increase* of size, to render it a fit apparatus for the purposes Dr. Hare had in view? Wollaston's apparatus consists of *one pair* of plates, but they are *too small* to produce sufficient effect for the purposes Dr. Hare had in view. He therefore, very ingeniously, and with vast originality, "was led *to go farther*, and to examine what an apparatus of *one pair* of *enormous* plates" would produce. Having come to this original determination, of using "*two enormous plates*, or what might be equivalent thereto," instead of *two very small ones*, which were "too minute for the observations he had in view," he constructed an apparatus, which he thus describes. "Twenty copper and twenty zinc plates, about nineteen inches square, were supported vertically in a frame, the different metals alternating at one half distances from each other. All the plates of the same kind of metal, were soldered to a common slip, *so that each set of homogeneous plates formed one continuous metallic superficies.*" This then was the *first* step in the professor's brilliant discovery. By a subsequent one, he extended this apparatus, so that by bringing two of this first and simple form together, in such a manner as to enable them to be immersed at once, in the same recipient, he obtained the combined effects of two simple elementary batteries. In this, I willingly confess, there is displayed considerable mechanical ingenuity. As a mechanical contrivance, it has no small degree of merit. But I maintain, that, as a philosophical contrivance, it is a direct and obvious derivation from Wollaston's battery. All the essential characteristics of their action and construction are virtually identical. If Dr. Hare's calorimotor produces an intense heat, without a proportionate degree of concomitant electrical effect, it is from the same principle precisely, that Wollaston's battery produces a similar effect. If

the effects of Dr. Hare's apparatus are greater than those of Wollaston's, it is because the former is an extension of the latter.

He asks, "how can an experiment designed for the examination of the heat-moving powers of the largest galvanic pair ever made, be considered as an extension of another, which has for its objects, the minutest arrangement of electrical substances, by which visible ignition can be produced?" How can it not? Surely if *two* "enormous plates" are employed to evolve a powerful degree of heat, there is no contradiction in calling it an extension of another experiment in which heat is evolved by *two* plates of the *smallest* size.

In the following quotation, the professor again unwittingly admits the justice of my remarks relative to his calorimotor. "By *lessening* the original pairs, Wollaston ascertained the curious result, that even when *very minute*, ignition might be effected by it. By *enlarging it*, I sought to shew the phenomena, inconsistent with the idea that the ignition resulted merely from electrical discharges."

Upon the statement I made, in my remarks on the calorimotor, that Wollaston found that heat was powerfully evolved by his elementary battery, without a concomitant production of electrical effect, Dr. Hare remarks: "So far from distinguishing the heat produced by his apparatus, from the electrical effect, Dr. Wollaston considers it as itself an electrical effect." This may be so; but the fact is, that heat was evolved without those effects which are usually denominated electrical or galvanic; and it is only with facts and not with the speculative opinions of Dr. W. or Dr. H. that we have to do on the present occasion.

The professor then goes on and says, the following statement is equally unfounded. "Thornsdorf, Thenard, Fourcroy, Vauquelin, and particularly Children, have demonstrated that the *electrical* action is not increased by the increase of the size of the plates, but by the increase of the nnmber of pairs, and that on the contrary the evolution of heat is encreased with the size of the plates." This statement, I am able to substantiate by the authority of an Editor, "who does not write on what he does not understand,"—one whom I am glad to find Dr. H. respects.

In the 70th, number of the Annals of Philosophy, page 137, we find the following paragraph: "Thornsdorf's discovery of the efficacy of large metallic plates in producing combustion, was the next step in the improvement of the galvanic apparatus. This discovery was verified by Fourcroy, Thenard, and Vauquelin. These gentlemen found that the *electrical* action of the battery was not increased by the increase of the size of the plates, but by the increase of the number of pairs; but the chemical action, as far as *combustion is concerned*, and as far as the decomposition of those bodies which are very difficult of decomposition is increased, with the size of the plates." This is the language of Dr. Thompson, a chemist, "who does not write about what he does not know," and yet Dr. Hare, who I presume writes about nothing but what he does know, says it is unfounded! I will leave him to settle this with Dr. Thomson.

Dr. Hare seems to think, that as an editor of a Recorder of original communications and intelligence in medicine, I have deviated from my duty in offering my opinion on his calorimotor. He moreover insinuates, that I am incompetent to give my opinion upon the subject which I have ventured to discuss. If the professor will look into the prospectus of our Journal, as well as into its contents, he will find that it is, as it has always been, a part of our plan to review books on medicine, and the collateral branches, freely and independently. As to my qualifications to give an opinion on this or any other subject, Dr. Hare must know more of me than he does, before I can allow him to decide.

Rejoinder to a Reply to the Review of Dr. Spalding's Pamphlet on the Scutellaria.

It has ever been a principle with us, to open our pages to free discussion. It was under the influence of this disposition, that we gave a place to Dr. Spalding's reply to the review of his pamphlet; and not, that we consider d his letter as a satisfactory