

Bull - (M)



A
DEFENCE

OF THE

EXPERIMENTS

TO

DETERMINE THE COMPARATIVE VALUE

OF

THE PRINCIPAL VARIETIES

OF

FUEL

USED IN THE UNITED STATES, AND ALSO IN EUROPE.

CONTAINING

A Correspondence with a Committee of the American Academy of Arts and Sciences;

Their Report, and Remarks thereon;

And Animadversions on the Manner in which the Trust confided to the Academy by Count Rumford has been managed.

BY **MARCUS BULL,**

MEMBER OF THE AMERICAN PHILOSOPHICAL SOCIETY, &C.

34

Philadelphia:

JUDAH DOBSON, CHESNUT STREET;
G. & C. CARVILL, BROAD WAY, NEW YORK;
HILLIARD, GRAY, LITTLE & WILKINS, BOSTON.

London:

JOHN MILLER, 40, PALL MALL
1828.

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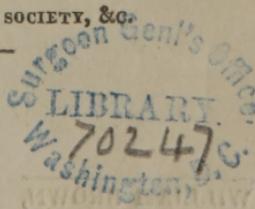
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THEIR WEIGHT AND NUMBER

BY MARSH'S BILL

WILLIAM BROWN,
Printer.

INTRODUCTORY REMARKS.

WHEN I determined to commence a course of experiments to discover the relative value of different kinds of fuel, and during the long period in which I was occupied in its prosecution, I know of no one thing which would so completely have abated my ardour, and paralyzed my efforts, as a conviction that they would eventually engage me in a controversy, similar to that which I now deem necessary to my reputation. I was stimulated to the performance of the task, which I voluntarily imposed upon myself, by the hope of doing good, and of acquiring some reputation for having done so; other incentives I neither felt, or needed, nor was I aware that any other was within my reach. The time and manner of my being informed of the existence of the Rumford premium will be seen on a perusal of the subjoined pages.

It is unnecessary for me to detain the reader with the history of the circumstances connected with my application to the American Academy, as these are fully made known in the correspondence with the committee, their report, and the remarks thereon, together with the animadversions on the manner in which the trust confided to the Academy by Count Rumford has been managed; but it appears to me proper to assign my reasons for appealing to the press, notwithstanding an unfeigned aversion to authorship.

The account of my experiments on fuel, which gave rise to the correspondence, was read before the American Philosophical Society, on the 7th of April, 1826, and was immediately published by them in their Transactions, and also in the Franklin Journal, and was extensively circulated both in this country and in Europe: it has since been copied in whole or in part in some of the English, and has also been translated and published

in several of the French Journals : and the reviews and notices relating to it, have, it is believed, been uniformly commendatory. Gentlemen of the first eminence in the physical sciences in this city, and in other parts of the Union, have concurred in testifying to the accuracy and importance of my investigations, and the value and novelty of the results obtained. Thus circumstanced, I may justly feel that the arduous labours in which I have been engaged, have acquired for me a reputation which I ought to defend.

Had I not been thus sustained, the opinions expressed by the committee of the American Academy, would have formed the closing scene of my appearance before the public, and I should have hoped that the curtain which had thus fallen, would for ever hide my labours from public inspection : but whilst my own conviction that a partial and incorrect view has been taken of my experiments, is confirmed by the opinions of those who are well qualified to form a judgment, and who have no interest to serve upon the subject, but that of science ; I shall not be accused of temerity, for attempting to prove that the charge of inaccuracy, brought against my experiments, is altogether unfounded ; even though this charge may have emanated from a body so highly respectable as the American Academy.

MARCUS BULL.

Philadelphia, March 10th, 1828.

CORRESPONDENCE.

No. I.

Dr. Bigelow to Mr. Bull.

BOSTON, June 3, 1826.

DEAR SIR—Your letter of May 8th was duly received; likewise, nine pamphlets by mail, containing the account of your experiments. I lost no time in placing one of your pamphlets on the Academy's table, and another on that of the Athenæum; the remainder were distributed among members of the Academy and scientific institutions.

The Academy met on the 30th. Several claims for the Rumford premium were submitted on behalf of candidates, yours being the first proposed. A committee was appointed to examine these claims, and report at the next quarterly meeting. This committee, after going through the examination of the communications referred to them, are of opinion, that no one of them contains any "discovery or improvement on heat or on light" sufficiently important to entitle its author to the premium.

Your experiments are considered by the committee as deserving great credit, for the ingenuity and perseverance exhibited in their performance. But the following circumstances, among others, are considered at variance with the correctness of the results.

1. The double room employed in your experiments, although it might *retard*, could not *prevent* the escape of heat from the inner room to the atmosphere. Hence the same experiment would not give the same result in a warm day as in a cold one, or in a windy day as in a calm one.

2. A more or less perfect combustion of the fuel, must affect the degree of heat produced in the experiments. This is influenced by circumstances difficult to regulate, such as the shape, position, and subdivision of the fuel; the rapidity of the current of air passing through it, and the amount of combustible surface in contact with fresh air. The smoke, or volatile combustible matter, would in one instance be burnt, augmenting the heat; in another it would not be burnt, but deposit soot.

I will not trouble you with any further objections from the committee, while these are unremoved. In the mean time, they will not report to the Academy until August next, previously to which time their objections will be open to your remarks, should you think proper to address any to them or to me.

I am, dear sir, your obedient servant,
JACOB BIGELOW.

MARCUS BULL, Esq.

No. II.

Dr. Bigelow to Mr. Bull.

BOSTON, June 26, 1826.

DEAR SIR—On my return yesterday from a journey in the state of New York, I found your two letters of the 9th and 12th inst.* I regret that my absence should have occasioned any delay in the answers you request.

In these letters you request to be furnished with the remaining objections against your experiments, and also with an explanation of the first objection contained in my letter of the 3d.

As the last inquiry is perhaps the most important, it may properly be first considered.

It is, *what effect we suppose would be produced upon the result of an experiment, whether the day was warm or cold, windy or calm.* The following statement will perhaps make the meaning of the committee intelligible.

Suppose that at the time of beginning your experiment the temperature of your inner room is at 100° of the thermometer, the outer room at 90° , and the atmosphere 80° . (These numbers are hypothetical, others will do as well.) Suppose that, during your experiment, the atmosphere falls to 50° . It must follow, that the outer room will give off to the atmosphere a greater quantity of heat, than it would have done in the same time, had the atmosphere remained stationary at 80° . Now, to supply this loss of heat, the outer room will immediately draw heat from all warmer bodies in its neighbourhood. These are two, viz. 1st, its own stove; and, 2d, the inner room. It follows, then, that the inner room will *lose* a certain number of degrees or *measures* of heat, which it would *not have lost* had the atmosphere remained at 80° .

This may explain why the result of an experiment will be likely to differ in a cold day, from that of the same experiment in a warm one. In like manner, in a windy day, the number of atmospheric particles coming in contact with the building being greater, they will conduct off more heat from the rooms than in a calm day.

* The purport of these letters being recited by Dr. Bigelow in his reply, their insertion is not deemed necessary.

The foregoing statement is made, to present the subject in a more intelligible form. You will probably be able to infer from it, that the sustaining of a *relative* temperature in two apartments, does not afford a correct indication of the *positive* amount of heat produced in one of them.

In regard to the other objections of the committee, they are principally these. That the subject does not admit of the philosophic accuracy in its conclusions, which you appear to attach to it, and that therefore no two experimenters would be likely to produce the same results. The same species of wood differs according to the season in which it is cut, the dry or wet soil in which it has grown. Heart wood differs from sap wood; young, from old; and wood in which decomposition is begun, from that in which it is not, &c.

The committee appointed at the meeting of the Academy in May, to report on applications for the Rumford premium, are Mr. Daniel Treadwell, Dr. John Ware, and myself. You will excuse the brevity of this letter, as it is written under the pressure of urgent engagements.

With respect, your obedient servant,

J. BIGELOW.

MARCUS BULL, Esq.

No. III.

Mr. Bull to Dr. Bigelow.

PHILADELPHIA, July 10, 1826.

DEAR SIR—Your letter of the 26th ult. which explains, in a very clear manner, the meaning of the committee as to the first objection made against the accuracy of my experiments, together with their remaining objections, has been received.

To place the subject in the most eligible point of view, I shall transcribe the several objections, and annex my answers.

1st objection. “The double room employed in your experiments, although it might *retard*, could not *prevent* the escape of heat from the inner room to the atmosphere. Hence the same experiment would not give the same result in a warm day as in a cold one, or in a windy day as in a calm one.” “Suppose that at the time of beginning your experiment the temperature of your inner room is at 100° of the thermometer, the outer room at 90°, and the atmosphere 80°. (These numbers are hypothetical, others will do as well.) Suppose that, during your experiment, the atmosphere falls to 50°. It must follow, that the outer room will give off to the atmosphere a greater quantity of heat, than it would have done in the same time, had the atmosphere remained stationary at 80°. Now, to supply this loss of heat, the outer room will immediately draw heat from all warmer bodies in its neighbourhood. These are two, viz. 1st, its own stove; and, 2d, the inner room. It follows, then, that the inner room will

lose a certain number of degrees or *measures* of heat, which it would *not have lost* had the atmosphere remained at 80° . This may explain why the result of an experiment will be likely to differ in a cold day, from that of the same experiment in a warm one. In like manner, in a windy day, the number of atmospheric particles coming in contact with the building being greater, they will conduct off more heat from the rooms than in a calm day."

In reply to this objection I have to remark, that the conclusions to which the committee have arrived, are drawn from false premises; as they overlook the fact that the temperature of the outer room is maintained by heat which may be said to be *entirely independent of that generated in the inner room*, as in no other case could the outer room be of any service whatever. In detailing my experiments, I have stated that I took advantage of the heat transmitted from the interior to the exterior room, and made it subservient, as far as possible, for the purpose of regulating the temperature of the latter room; but as this heat had been measured in its proper place, (within the interior room,) whether it was then permitted to escape, or was made use of for the purpose described, would be entirely immaterial to the result of the experiment, and may be considered as *entirely independent*, as the same amount of heat generated from fuel in the stove of the exterior room.

We will now examine the case supposed by the committee, in which the temperature of the atmosphere, at the commencement of an experiment, should be 80° , and subsequently be depressed to 50° . The effect would undoubtedly be to depress the temperature of the exterior room, but this could only take place by supposing the operator to neglect his duty, and this would immediately be indicated on the scale of the differential thermometer in the interior room, and also by the thermometer of the exterior room, which we will suppose to have sunk to 89° .

Now, the only method which could possibly be taken to restore the relative difference of 10° between the two rooms, and maintain the interior at 100° , would be to increase the fire in the stove of the *exterior*, as any attempt to raise the temperature of the latter room by increasing the fire in the stove of the interior, must obviously only increase the difficulty, in proportion to the difference in the content of the conducting surface of the two rooms, or nearly as two to one. As every experiment must be considered a failure, unless completed at the same temperature at which it was commenced; we must now suppose that I have applied additional fuel in the stove of the exterior room, and again elevated its temperature to 90° , as this stove, although its office appears to have been overlooked by the committee, is fully competent to counteract the effects of a change even greater than they have supposed. The temperature of the exterior room being restored to 90° , I would respectfully inquire of the committee, whether they can suppose, that the transmission of heat from the interior room would not also be restored to its former rate, supposing the atmosphere to remain at 50° ? If the interior room, at 100° , is constantly surrounded by a thick interstice of air at 90° , this must possess the same power of con-

ducting heat, whether the atmosphere is 50° or 80° , and the only difference in performing the experiment would be, that if at 50° , I should be obliged to use more fuel in *the stove of the exterior room*, an important circumstance, which appears not to have been sufficiently noticed by the committee, as they appear to suppose, that part at least of the extra loss of heat, must *necessarily* be drawn from the inner room, as they say—"Now, to supply this loss of heat, the outer room will immediately draw heat from all warmer bodies in its neighbourhood. These are two, viz. 1st, its own stove; and, 2d, the inner room. It follows, then, that the inner room will *lose* a certain number of degrees or *measures* of heat, which it would *not have lost* had the atmosphere remained at 80° ." The tendency which bodies of unequal temperatures evince to equalize their heat, when *brought in contact*, is admitted; but I am not aware that cold bodies possess any influence to "draw heat," or increase its radiation through a stratum of *intervening warmer air*. The amount of radiated heat from the same surface, and at the same temperature, I have supposed to be always equal, and that the heat transmitted by *contact* through an air medium, would be always proportional to the *difference* in the temperature of the *bodies in contact*, other things being equal. Now, if the exterior room, by the *aid of its own stove*, is maintained at 90° , and no other supposition can be made, whence comes the *necessity* or even *possibility* of the inner room losing a greater portion of heat by radiation or contact, than would have been the case had the atmosphere remained at 80° ?

During the great length of time consumed by my experiments, it will be obvious that they must have been subject to all the changes of our variable climate; and as experiments were made upon the same kinds of fuel at *every* season of the year, and with the *same results*, I know not how to offer for the consideration of the committee, more satisfactory practical evidence, as to the correctness of the means made use of to ensure uniform results.

2d objection. "A more or less perfect combustion of the fuel, must affect the degree of heat produced in the experiments. This is influenced by circumstances difficult to regulate, such as the shape, position, and subdivision of the fuel; the rapidity of the current of air passing through it, and the amount of combustible surface in contact with fresh air. The smoke, or volatile combustible matter, would in one instance be burnt, augmenting the heat; in another it would not be burnt, but deposit soot."

In reply to the 2d objection, it will be apparent, that perfect similarity in "the shape, position, and subdivision of the fuel," among articles so dissimilar, would not only be "difficult to regulate," but entirely impossible and unnecessary, and indeed injurious, could it have been done. The object of my experiments being entirely practical utility, to attain that object it was necessary that the different kinds of fuel should be consumed as near as possible in the manner in which this takes place in the ordinary processes to which fuel is applied. "The shape, position, and subdivision of the fuel," so far as it related to *each kind*, was as similar as possible; and as to "the

amount of combustible surface in contact with fresh air, and the rapidity of the current of air passing through it," these would entirely depend upon the kind of fuel experimenting upon; for instance, the quantity of anthracite coal would be larger than would be required of any other article, and the quantity of air admitted was proportional to the heat required to be produced, and its "rapidity," it is presumed, was proportional to the heat, both of which are supposed to have been equal in every experiment. That part of the objection which relates to the different degrees of heat which would be produced by consuming the smoke in one instance, and not in another, will now be answered. As none of the chimney fire-places, grates, or stoves, made use of in this country, do, to my knowledge, possess the necessary requisites for consuming their own smoke, and as the anthracite coals do not present any smoke to consume, I had supposed the only method of making a *fair* comparison would be, to produce as perfect combustion as is ever done in the large way; but to have consumed the smoke from the woods and bituminous coals, would certainly have exposed my results to an objection, when compared with anthracite coal, an article of fuel, the value of which, it will be admitted, is of great importance to have accurately ascertained.

3d objection. "That the subject does not admit of the philosophic accuracy in its conclusions, which you appear to attach to it; and that therefore no two experimenters would be likely to produce the same results. The same species of wood differs according to the season in which it is cut, the dry or wet soil in which it has grown. Heart wood differs from sap wood; young, from old; and wood in which decomposition is begun, from that in which it is not."

In reply to this objection, I am quite prepared to agree with the committee, that experiments upon this, as well as upon every other philosophic inquiry, must be supposed to bear the common impress of *fallibility*; but I must protest against the inference to be drawn from this objection, that, because we cannot arrive at *perfection*, we should do *nothing*.

I am not prepared to agree with the committee, that the same *weight* of any particular *dry* wood, would not be likely to produce the same results, by different and equally judicious experimenters; even supposing it to have been cut at different seasons, and to have grown in different soils; as I presume the component parts of the wood would be nearly the same, although I admit that it would probably possess different specific gravity. My experiments on the woods were made with great care, so as to include a proper proportion of *bark*, *sap*, and *heart*, and as some difference might exist between wood of different ages, I selected a medium between the two, ("old and young,") as usually sold.

The largest portion of wood sent to market being *sound*, I did not suppose it necessary to experiment upon wood which had undergone the various degrees of "decomposition" of which it is susceptible; nor am I in possession of any method, if these experiments had been made, by which I could have pointed out to the public the manner of determining the "season" in which wood has been cut, the "soil"

upon which it has grown, its "age," or degree of "decomposition" which it may have undergone.

I remain, very respectfully, your obedient servant,

MARCUS BULL.

To JACOB BIGELOW, M. D. &c. &c. &c.

No. IV.

Dr. Bigelow to Mr. Bull.

BOSTON, August 4, 1826.

DEAR SIR—Your letter of July 10th was received three days since, and has been submitted to the committee. They remain of opinion that their former conclusions, although pronounced by you to be "drawn from false premises," are nevertheless correct and true.

As unnecessary prolixity is, in scientific matters, a great evil, I beg leave to call your attention at once to a remark in your last letter, in which you state, that you are "not aware that cold bodies possess any influence to 'draw heat,' or increase its radiation, through a stratum of intervening warmer air." Now this is the precise point in which the committee and yourself disagree. They have always understood that cold bodies *do possess* an influence to draw heat, or increase its radiation, through a stratum of air, whether warm or cold. They have yet to learn, that radiation can be prevented by the intervention of an atmospheric medium of *any temperature whatever*. And until they are instructed how this may be accomplished, they will retain their former belief, that the results of your experiments must *vary* with changes in the temperature of the atmosphere.

In regard to their second objection, the committee will explain further. Suppose that hereafter two persons should repeat either of your experiments, and that one of them should divide his fuel into ten parts, and place them disadvantageously; while the other should divide the same fuel into twenty parts, and place them more advantageously for the circulation of air. In one case more smoke would be burnt than in the other, more heat produced, and a different result afforded by the experiment. In your letter you state, that "the object of" your "experiments being entirely practical utility, to attain that object it was necessary that the different kinds of fuel should be consumed as near as possible in the manner in which this takes place in the ordinary processes to which fuel is applied." Now, in ordinary practice, there is no very uniform mode of burning fuel, since every man builds his fire differently from his neighbour. If there is any thing in which most of the practical world agree, it is in never using fuel in the state in which you use it, viz. that of absolute dryness.

In replying to the third objection, you protest against the inference "that because we cannot arrive at perfection, we should do nothing." On this subject the committee think, that where we cannot arrive at

perfection, we should take care to do nothing which will lead others into error. In science, a state of ignorance is better than a state of error. No lover of truth can willingly adopt as laws in philosophy, conclusions founded on results which, from their nature, are likely to be overturned by the first succeeding experimenter.

The committee have no further wish in this business, except that you and your scientific friends, in common with themselves, may arrive at a joint understanding of the truth. They are in no haste to make up their report, but will patiently wait for any further remarks you may choose to offer on the subject.

Very respectfully, your obedient servant,

JACOB BIGELOW.

MARCUS BULL, Esq.

No. V.

Mr. Bull to Dr. Bigelow.

PHILADELPHIA, August 11, 1826.

DEAR SIR—I received your letter of the 4th on the 8th inst. The committee will, I hope, excuse the “prolixity” of my last letter. Replications can seldom be as concise as objections.

I must plead great carelessness, as my only excuse, for having stated in my last letter that ‘I am not aware that cold bodies possess any influence to “draw heat,” or increase its radiation through a stratum of intervening warmer air,’ and I feel no reluctance in agreeing with the committee, that radiation cannot be prevented by the mere temperature of an intervening atmospheric medium.

I should have said, after the word *through*, ‘a *surface* uniformly heated by a stratum of intervening air, warmer than the cold bodies.’

The committee will probably agree with me, that the quantity of radiated heat, lost by a heated body in a given time, will be uniform, provided the temperature of its *surface*, and the *surface* of the recipient or colder body in opposition, remain the same; or, in other words, that radiated heat has only to do with the temperature of the *surface* of solid bodies.

Now supposing the internal *surface* of my exterior room to be maintained at a uniform temperature, I contend that a cold body, or the colder parts of the wall *on the outside*, cannot operate to “draw heat,” or increase its radiation from the inner room.

The process of transmitting the heat I conceive to be changed from the radiating to the conducting state, at the moment of absorption at the *surface*; and whether this new process then becomes more or less rapid, is not material, provided we possess at an intermediate point, viz. at the interior surface, a recipient of uniform powers. This I suppose to be effected by the body of warm air in contact with the walls, which is constantly exerting itself, not only to preserve the surface at the same temperature, but to extend its influence even through

the walls, so that every change in the atmosphere is met near the *outside*, and is constantly lessening in effect as it approaches the internal surface.

From the bad-conducting materials of which the room is composed, I am led to believe that no appreciable difference would be found to exist in the temperature of its internal surface, under the ordinary changes of our climate. In this opinion Professors Hare and Silliman both agree.

In confirmation of this opinion, I beg leave to repeat the fact, that the *same results* have been obtained from the *same kind of fuel*, at every season of the year.

It appears to me, that the only point of difference between the committee and myself, is, upon the question whether the surface of the walls of a room similar to that used by me, and as uniformly heated, will not be of the same temperature as the air of the room, or of a uniform difference of temperature, under the ordinary changes of our climate. Any person may readily satisfy himself on this point by a simple experiment.

In reply to the further explanation of the second objection, I remark, that it would be very difficult to place in my stove the small quantity of fuel used at one time, in a disadvantageous manner for combustion. If we suppose a second experimenter to divide his fuel in every instance as uniformly, but more minutely than I have done, and to obtain more heat; yet still our *comparative* results must evidently *be the same*.

The quotation from my letter, 'the object of my experiments, &c.' was intended to apply simply to the states of *aggregation* in which the fuel was used. This, it appeared to me, the committee supposed should be the same, whether the article was *wood or coal*.

The committee will not, I think, upon reconsideration, object to my having used the fuel absolutely dry, this being the only state to obtain it uniform, and the precise state recommended by Count Rumford, in whose place the committee may be supposed to stand, so far as relates to their official capacity in this business.

Although "in science a state of ignorance is (may be?) better than a state of error," supposing the latter to be material, yet I presume the committee will agree with me, that perfection is rarely if ever attained, and that near approximations are in most cases *all that utility* requires.

As I am not a chemist by profession, I took the liberty of sending the objections of the committee to Professor Silliman, requesting his opinion upon them, which I received a few days since, and take the liberty of transcribing.

I remain, very respectfully, your obedient servant,

MARCUS BULL.

To JACOB BIGELOW, M. D. &c. &c. &c.

Professor Silliman to Mr. Bull.

YALE COLLEGE, July 17, 1826.

DEAR SIR—I have twice perused with attention your communication of the 6th instant, covering the report of the Committee of the American Academy of Boston, upon the subject of your experiments upon the heat evolved in combustion, &c.

In reply to your request, that I would give you my opinion of the objections made by the committee, and of your reply to them,* I proceed to remark :

1st. I conceive that the exterior room, being sustained at a given temperature by a source independent both of the inner room and of the external air, is as good a non-conductor as can be provided, and that the inner room is as effectually guarded as possible from any influence from the external air, and that it is sufficiently guarded to prevent any appreciable inaccuracy from that source.

2d. There being no *visible* smoke from the anthracite coals, and scarcely any volatile combustible matter, that is not immediately consumed by the fire, there is, in the case of this fuel, no room for the combustion of the smoke ; and as the object of the experiments was to show the comparative quantity of heat evolved in the *usual* modes of burning fuel, in domestic economy and in the common arts, and not the whole possible amount, it did not come within your plan to compass this object, nor does it appear to be necessary for the purpose in view.

3d. The spirit of these remarks is applicable to the third objection: your selection of fuel appears to have been sufficiently precise to furnish the *average* result of the good fuel in market, and this was all that the case required.

For my general opinions of the value of your paper, I beg leave to refer you to the American Journal, vol. xi. page 98, just published, where, under the date of May 11th, you will find my impressions concisely but fully expressed.

Entertaining the greatest respect for the Committee of the American Academy, and having myself the honour to be a member of that body, I trust they will receive with candour the opinions which I have expressed, and which would have been communicated with equal frankness had I been so fortunate as to coincide with them.

I remain, dear sir, yours very respectfully,

B. SILLIMAN.

MR. BULL.

* Letter No. III.

No. VI.

Dr. Bigelow to Mr. Bull.

BOSTON, August 20, 1826.

DEAR SIR—The correction contained in your last letter does not appear to throw any additional light on your subject, since it is founded on a fresh mistake, that of supposing surfaces to partake the temperature of the contiguous atmosphere, more than that of the solids to which they belong. The two surfaces of your outer room cannot be of the same temperature, for the same reason that the surface of a stove containing a fire, is not of the same temperature as the surface of the apartment in which it is placed.

The committee do not find in your letter any further reasons, from any source, requiring answers other than those already put into your possession.

In regard to the opinions which you adduce of Professors Silliman and Hare, and also the statement of the mode in which Count Rumford employed fuel; the committee consider these as entitled to no further weight, than that of meriting a respectful consideration. The business of the committee is with the merits of the question, and not with the authority of names.

The committee having now given what they consider a patient and ample hearing to your claims, feel themselves called on to report; that your experiments do not contain any discovery or improvement sufficiently important to entitle them to the Rumford Premium.

Very respectfully, your obedient servant,

JACOB BIGELOW,
For the Committee.

MARCUS BULL, Esq.

No. VII.

Mr. Bull to Dr. Bigelow.

PHILADELPHIA, August 24, 1826.

DEAR SIR—Your letter of the 20th inst. was received this morning.

The "fresh mistake" with which the committee think proper to charge me, appears to rest with themselves, as I apprehend it would be difficult for them to prove that two surfaces of solid bodies, (bad conductors, such as my walls, exposed to different temperatures,) *do not* "partake the temperature of the contiguous atmosphere, more than the solids to which they belong." The admission of such a state of things by the committee, must prove fatal to their first objection.

If the cold air on the exterior surface of the wall, did not possess more influence to lower its temperature, than the interior strata possess by their reaction to maintain it at *their* temperature, I would ask

the committee in this case, how the wall when once heated, could ever become cool? and when once cooled, could ever be heated by the action of the contiguous air?

The committee will permit me to observe, that I was not prepared to expect at this stage of the business, when the principal objection urged by them is reduced to a point capable of being proved or disproved by actual experiment, which I had determined to institute,—that they should think proper to foreclose any further hearing from me on the subject, and particularly as you state to me in your letter of the 5th inst. that “The committee have no further wish in this business, except that you and your scientific friends, in common with themselves, may arrive at a joint understanding of the *truth*. They are in *no haste* to make up their report, but will patiently wait for any further remarks you may choose to offer on the subject.”

I have respectfully to request, that the committee will delay their report upon my application for the Rumford Premium, until an opportunity shall be given me to institute the experiments necessary to determine the point in question between us.

I remain, sir, very respectfully, your obedient servant,

MARCUS BULL.

To JACOB BIGELOW, M. D. &c. &c. &c.

No. VIII.

Dr. Bigelow to Mr. Bull.

BOSTON, September 2, 1826.

DEAR SIR—The passage in my last letter reads thus, in the copy which I preserved of it: “a fresh mistake, that of supposing surfaces to partake the temperature of the contiguous atmosphere, more than *that of* the solids to which they belong.” In your quotation of this passage, I find the words “*that of*” are omitted, and thus a foreign meaning given to the sentence. Without these words, the sentence is irrelevant to the question; with them, it agrees in connexion with what follows.

If the argument in your last letter is founded on this mistake, it is only necessary to refer you to the *true* meaning of the committee; if otherwise, it is difficult to comprehend its application to the case.

The committee remain of their former opinion, that the opposite surfaces of your exterior room cannot be of the same temperature. If they were of the same temperature, they could reciprocally neither give nor receive heat, and the question would remain for yourself to answer, how the inner room “when once heated, could ever become cool?” &c.

The time of the August meeting having passed, the committee can-

not present their report to the Academy till the next meeting, which is in November.

Respectfully, your obedient servant,

JACOB BIGELOW.

MARCUS BULL, ESQ.

No. IX.

Mr. Bull to Dr. Bigelow.

PHILADELPHIA, September 7, 1826.

DEAR SIR—I have the pleasure to acknowledge the receipt of your letter of the 2d inst. and am glad to find that the committee have deferred their report until November.

The omission to which you allude, in the passage quoted in my last letter, from yours of the 20th ult. was entirely accidental, and a departure, as I find, from the first sketch of my letter.

The words “*that of,*” which were omitted by me, appear clearly to refer as *relatives* to the antecedent word *temperature*; and if so, I do not perceive that their *omission* could occasion any different, or “foreign meaning” to be applied to the plain and obvious import of the sentence in your letter; which I presume would accurately convey the sense of the committee, if it were stated thus, viz. That I had committed “a fresh mistake, that of supposing surfaces to partake the temperature of the contiguous atmosphere, more than” (the temperature of) “the solids to which they belong.”

Is it not plain from grammatical construction, that the reference to the word *temperature*, fully supplies the omission of the words “*that of,*” without in any manner altering the “true meaning of the committee?” In other words, is it not plain, that the words “*that of,*” or “*more than,*” have a direct and exclusive reference to the word *temperature* in the passage of your letter, and can refer to no other word or words in it?

If the language made use of in your letter of the 20th ult. does not convey the “true meaning of the committee,” it is my wish that it may be corrected; as it has ever been my desire, to give your letters the most fair and liberal construction which their language will admit.

You say, “The committee remain of their former opinion, that the opposite surfaces of your exterior room cannot be of the same temperature.”

I am not aware that I have *ever contended* that the opposite surfaces of my exterior room *were* of the “same temperature;” but on the contrary, have always considered that they must of necessity be of *different* temperatures.

My experiments were based upon a constant *loss* of heat, which

could only take place in consequence of the existence of different temperatures, and this is clearly stated by me as I conceive, at page 17 of my paper, to which I beg leave to refer the committee.

I remain, sir, very respectfully, your obedient servant,

MARCUS BULL.

To JACOB BIGELOW, M. D. &c. &c. &c.

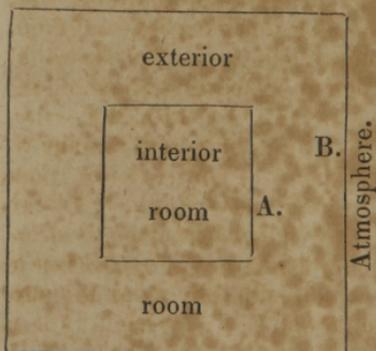
No. X.

Dr. Bigelow to Mr. Bull.

BOSTON, September 11, 1826.

DEAR SIR—It has been my aim to express the meaning of the committee intelligibly, but as it seems I have not been fortunate in this respect, I will endeavour to state it in a form which cannot be mistaken.

By the word *room* the committee mean *cavity*, and by *opposite surfaces of the room* they mean the *surfaces of the cavity* which are in *opposition to each other*. Or, to make the matter more plain, they mean the surfaces marked A and B, in the annexed diagram.



Now, the committee assert, that during your experiment the surface B is colder than the surface A, and that this difference of temperature will be in some measure proportionate to the difference between the atmosphere and interior room. And therefore, if, during your experiment, the atmosphere should fall in temperature; then, more heat will be radiated from A to B, and of course more heat will escape from the interior room to the atmosphere,

than if no reduction of the atmospheric temperature had taken place. And an experiment performed under such circumstances would give a different result, from one performed when the atmosphere was stationary from the beginning.

Very respectfully yours,

JACOB BIGELOW.

P. S. I am reluctant to add any thing more, which may lead you to digress from the main point; but since you say, "I am not aware that I have ever contended that the opposite surfaces of my exterior room were of the same temperature," let me cite some different passages from your letter of August 11th. You there say, "I should have said after the word *through*, a surface *uniformly heated* by a stratum of intervening air, warmer than the cold bodies." Again: "The

committee will probably agree with me, that the quantity of radiated heat lost by a heated body in a given time, will be uniform, provided the temperature of its *surface* and the *surface* of the recipient or colder body *in opposition*, remain the same," &c. Again: "Now supposing the *internal surface* of my *exterior room* to be maintained at a *uniform* temperature, I contend," &c. Again: "From the bad-conducting materials of which the room is composed, I am led to believe that no appreciable difference will be found to exist in the temperature of its *internal surface*," &c.

No. XI.

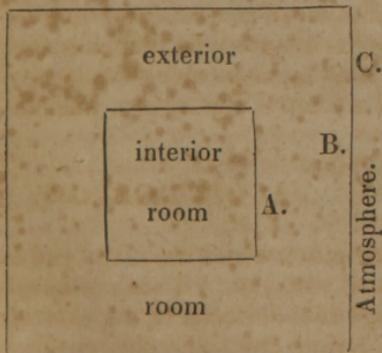
Mr. Bull to Dr. Bigelow.

PHILADELPHIA, September 16, 1826.

DEAR SIR—I received your favour of the 11th inst. yesterday, and will adopt with pleasure what you now state to be the meaning of the committee, however much it may be at variance with the obvious import of the language made use of in your letters of the 20th ult. and 2d inst.

I regret that I cannot perceive on the part of the committee, that spirit of candour towards me, which I have a right to expect from them in their present official capacity of *judges*; as its perception would prevent the necessity of commenting upon any part of your letter, except the explanation contained therein.

In your letter of the 2d inst. you say, "The committee remain of their former opinion, that the opposite surfaces of your exterior room cannot be of the same temperature." In your last you say, "By the word *room* the committee mean *cavity*, and by *opposite surfaces* of the *room* they mean the *surfaces* of the *cavity* which are in *opposition* to each other. Or, to make the matter more plain, they mean the surfaces marked A and B in the annexed diagram."



Without the aid of a diagram, your meaning would have been perfectly intelligible had you said, "The committee remain of their former opinion that the *exterior surface* of your *interior room*, and the *interior surface* of the *exterior room*, cannot be of the same temperature."

It is well known by the committee, that these rooms have always been considered, not only by me, but by themselves as shown in your diagram, as *entirely distinct*.

By the "opposite surfaces of the room," (for example, the exterior room,) is clearly to be understood, the two surfaces of *its own walls B and C*, and cannot by the aid of any diagram be confounded, or made to mean or include as *one* of the surfaces, the *exterior* surface of *another*, viz. the interior room. We should not say that the *surface* of a large *box*, standing in a room, is one of the "surfaces of the room;" and such a box may, for explanation, be considered to represent my interior room.

In reply to what I consider your very *unfortunate* P. S., I must repeat, that 'I am not aware that I have ever contended that the opposite surfaces of my exterior room were of the same temperature,' and I do not perceive that any proof can be drawn from the passages cited by you from my letter of August 11th to disprove it.

The words in the first passage cited, "a surface *uniformly* heated," cannot mean *two* or "opposite surfaces."

In the second passage I was speaking of the temperature of the surfaces of a *hot* and *cold* body, and the words "remain the same," can only refer to the same *difference* of temperature, it being impossible for me to conceive, how the surfaces of two bodies, the one *hot* and the other *cold*, should still be of the *same temperature*.

The two last passages refer to maintaining the "internal surface of my exterior room" at a *uniform temperature*, but do not include its *external surface* also, which would be necessary to sustain your charge.

Having noticed those parts of your letter which have compelled me "to *digress* from the main point," I will now notice the latter.

The objection of the committee may be stated intelligibly in very few words, viz. *That the surface B cannot be maintained at a uniform temperature, if, during an experiment, the temperature of the atmosphere should fall.*

The fact whether "the surface B is colder than the surface A," during an experiment, is *entirely immaterial*, provided they remain at the "same *difference* of temperature."

I beg leave to refer the committee to those parts of my former letters which are intended to prove that the surface B *may be maintained at a uniform temperature*, and I have particularly to request that they will give an *attentive perusal* to that of August 11th, in doing which I am persuaded they will withdraw their charge of its being "founded on a fresh mistake."

If the committee are still of opinion that the surfaces of my walls B and C, *do not* "partake the temperature of the contiguous atmosphere more than that of the solids to which they belong," the external surface, which I have marked C on the diagram, will not be affected by the *changes* they have supposed in the temperature of the atmosphere in contact with C; consequently the temperature of B cannot be affected thereby, which would be fatal to their objection.

If the committee take the opposite ground, and say, that the surfaces of my walls B and C; *do* "partake the temperature of the contiguous atmosphere more than that of the solids to which they belong," this position appears to be *equally fatal* to their objection; as

in admitting that the surface C partakes the temperature of the cold atmosphere in contact with C, they must also admit that the surface B must also partake the temperature of the warm air in contact with B, and as the warm air in contact with the latter is of a *uniform* temperature, consequently the temperature of B must remain *stationary*, so that taking the matter *either way*, the committee must perceive that their objection cannot be maintained.

I remain, sir, very respectfully, your obedient servant,

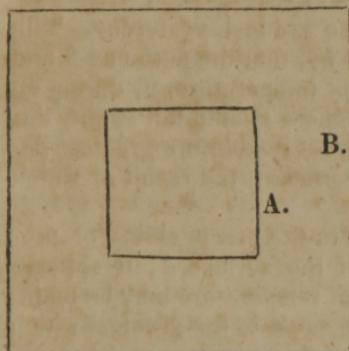
MARCUS BULL.

To JACOB BIGELOW, M. D. &c. &c. &c.

No. XII.

Dr. Bigelow to Mr. Bull.

BOSTON, September 22, 1826.



DEAR SIR—In your letter of the 16th, you say, “The fact whether the surface B is colder than the surface A during an experiment, is entirely immaterial, provided they remain at the same difference of temperature.”

As this statement bears more upon the question than any other in your letter, I beg leave to state, that during changes of the atmosphere, in the opinion of the committee, these surfaces will *not* remain even of the same *difference* of temperature. Suppose that, in an experiment, the difference of temperature between A and B is two degrees. Then let the atmosphere fall twenty degrees, and the difference between A and B will become *more than* two degrees, and will so continue until the interior room and the atmosphere shall arrive at their former relative temperature. But during all this time (as we formerly stated) more heat will be radiated from A to B, and of course more heat will escape from the interior room to the atmosphere, than if no reduction of the atmospheric temperature had taken place. Therefore, the result of the experiment will differ from what it would have been, had the atmosphere remained stationary.

The committee also “are still of opinion” that your surfaces “do *not* partake the temperature of the contiguous atmosphere more than that of the solids to which they belong.” For example, the surface C will never be reduced to the temperature of the atmosphere, so long

as the wall, to which it belongs, is warmer than the atmosphere. But it will nevertheless "be affected by changes in the temperature of the atmosphere," so as to expend or give off more heat when the atmosphere is cold, than when it is warm. And consequently more heat will escape from the interior room to the atmosphere, than if (in the case already supposed) no reduction of the atmospheric temperature had taken place.

Your obedient servant,

J. BIGELOW.

MARCUS BULL, Esq.

No. XIII.

Mr. Bull to Dr. Bigelow.

PHILADELPHIA, September 27, 1826.

DEAR SIR—I received your favour of the 22d inst. yesterday. The objection of the committee appears now to be, that the surfaces A and B will not remain of the same difference of temperature, if, during an experiment, the temperature of the atmosphere should fall twenty degrees; this, however, I will remark, is about double the greatest depression ever experienced during any experiment, the result of which is given in my Table.

As no objection will probably be made to the practicability of producing a uniformity in the temperature of the surface A, it appears only necessary to show, that a uniformity of temperature may be maintained upon the surface B, during a change equal to the greatest which ever occurred during my experiments.

You say, "Suppose that, in an experiment, the difference of temperature between A and B is two degrees. Then let the atmosphere fall twenty degrees, and the difference between A and B will become more than two degrees."

The changes in the temperature of C are admitted, but these cannot influence A except by first lowering the temperature of B, which is the barrier against these changes, and the inference which you have drawn, can only be shown to be true, by proving the impossibility of maintaining the surface B at any required temperature, through the agency of heated air. If the temperature of B is 80° , I conceive that the same heat would be radiated from A to B in a given time, whether the temperature of C is 70° or 40° .

The committee do not I presume intend to suppose an *instantaneous* change of twenty degrees, but even admitting this for the sake of argument; do they suppose that this change operating upon the surface C would also produce a similar instantaneous change upon the surface B, or even such as to prevent the possibility of counteracting it by increasing the fire in the stove of the exterior room?

We do not find it impossible to maintain the temperature of our dwellings at 60° or 70° , when the temperature of the atmosphere is even at zero; and it is well known, that a considerable period of time is required, for a change of twenty degrees to produce any material effect upon the temperature of rooms in brick buildings, even where no fires are kept up to counteract it.

To give an instance of the practicability of maintaining two surfaces of a solid body at very different temperatures, I have only to state, that my Air Furnace (which is built in a cellar) has walls ten inches thick, and although the interior surface is sometimes near a *white heat* for a number of hours together, I do not recollect an instance in which I could not keep my hand without inconvenience upon the exterior surface of the wall, opposite to the furnace.

The temperature of the atmosphere has, with us, for a few days past, been unusually cold for the season, and was last evening at 55° . During the night it changed, and this morning, at 9 h. 30 m. it was 76° : a change so remarkable, induced me to make the following simple experiment, to ascertain whether the surface B would possess the same temperature as the air of the room, during the process of warming it from the atmosphere.

To perform this experiment, I made use of three mercurial thermometers, accurately corresponding with each other, one of which was suspended in its case from the interior wall of the exterior room, another was removed from its case, and the bulb placed against the surface B, the back side or half of this bulb being exposed to the influence of the wall, whilst the former was screened from its direct influence by the case, which did not touch the wall. These two thermometers were placed near each other, and at the same height on the wall. The third was suspended from the exterior wall, in the atmosphere, in the shade.

The following results were obtained.

Time.	Air.	Surface B.	Atmosphere.	Surface C.
9 h. 30 m.	69°	69°	76°	
1 30	71°	71°	78°	
3 30	72°	72°	80°	
4 30	$72^{\circ}.5$	$72^{\circ}.5$	78°	78°

It did not occur to me to try the surface C until the last period of time noted, although the sun had been obscured nearly the whole time.

The walls on the two sides of the room exposed to the atmosphere are ten inches thick. At 4 h. 30 m. the centre, or mean temperature of the wall, may be supposed to have been $75^{\circ}.25$, as the surface B was $72^{\circ}.5$, and C 78° . It is worthy of remark, that it required six hours to elevate the temperature of the room 3° , the atmosphere having been not less than 7° warmer during the whole time.

You will not probably have an opportunity to repeat the experiment under the same circumstances, but you will undoubtedly very soon have the atmosphere so cold as to be able to make equally satisfactory

experiments, in which case, the committee will be able to decide, whether their opinion contained in the last paragraph of your letter is correct.

If the committee are of opinion that the surface B cannot be maintained at a uniform temperature, by the same aids made use of in performing my experiments, I have to request, that they will suggest what they would consider as satisfactory experiments to determine this point, which, if practicable, I will perform in the presence of any gentlemen they may name.

Dr. Hare suggested to me, that the American Philosophical Society would, at your request, appoint a committee for this purpose.

Yours, very respectfully,

MARCUS BULL.

TO JACOB BIGELOW, M. D. &c. &c. &c.

No. XIV.

Dr. Bigelow to Mr. Bull.

BOSTON, October 5, 1826.

DEAR SIR—You will please to observe, that the objections of the committee are not limited by the extent of an example. Examples are illustrations of *general principles*. What is said in my last of twenty degrees, is true of any supposable number of degrees.

The committee have too much respect for the discernment of yourself, and of your friend Professor Hare, to suppose, that after *maturely* considering what has been said in my late letters, you can really believe that the surfaces A and B can be kept, during atmospheric changes, “at the same temperature,” or at “a uniform difference of temperature.” The reasoning in your last letter is fallacious. Do you not perceive, that in order to sustain the surface B at, or near, a given temperature, while the atmosphere *falls*, you must *raise* the heat of the air in your exterior room, and that in so doing you will raise the heat of A, and thus produce the same relative difficulty, which you are seeking to avoid?

The Committee have no experiments to suggest, as they really know of none which would remove your difficulties.

Very respectfully yours,

J. BIGELOW.

MARCUS BULL, Esq.

No. XV.

Mr. Bull to Dr. Bigelow.

PHILADELPHIA, October 10, 1826.

DEAR SIR—To exculpate Professor Hare from any suspicion of asserting opinions without mature consideration, or those which he does not “really believe,” you will permit me to remark, that he has not seen any of your “late letters,” or my replies thereto; having been seriously ill for some weeks. The suggestion of his, contained in my last, was made to me some time since.

My answer to your query, “Do you not perceive,” &c. is in the negative: as in the case supposed, no necessity exists that I should “raise the heat of the air in the exterior room:” all that is required is, that it should not be permitted to *fall below* the temperature at which the experiment was commenced. If, for example, an experiment was commenced with the interior room at 80° and the exterior at 70°, they were maintained at *these temperatures* throughout, or the experiment was considered a failure.

But even admitting for argument that your assertion is true, that “you must *raise* the heat of the air in your exterior room, and that in so doing you will raise the heat of A”—you must perceive, that as the *same air* acts also upon B, their “*relative*” difference of temperature would not be affected thereby.

There is evidently a misconception on the part of the committee relating to my experiments, which I fear I shall not be able to remove, except by a personal interview.

Yours, very respectfully,

MARCUS BULL.

TO JACOB BIGELOW, M. D. &c. &c. &c.

No. XVI.

Dr. Bigelow to Mr. Bull.

BOSTON, October 14, 1826.

DEAR SIR—In reply to the statements in your letter of the 10th, beginning “If for example,” &c. I will state a plain case, which may serve for a general answer.

Suppose the air of your exterior room at 70°, and the atmosphere 60°, and the surface B at a given temperature. It is required to preserve B at the same (i. e. a uniform) temperature. It is obvious, that B is held in equilibrium, between the *warming* influence of the heat generated within, and the *cooling* influence of the atmosphere without; for although the wall is a partial non-conductor, it can only retard, not prevent, the establishment of this equilibrium. Now, so long as the

warming influence may continue at 70° , and the cooling influence at 60° , B will continue at a uniform temperature. But let the cooling influence change from 60° to 50° , then the temperature of B will *certainly fall*, unless the warming influence is *raised* proportionally above 70° , so as to reproduce the former equilibrium.

Respectfully yours,

J. BIGELOW.

MARCUS BULL, Esq.

No. XVII.

Mr. Bull to Dr. Bigelow.

PHILADELPHIA, October 17, 1826.

DEAR SIR—Your letter of the 14th inst. is received. It gives me pleasure to find that you have at length stated the objection of the committee in an intelligible form, and in perfect agreement with what I had conjectured it to be, as stated in a number of my former letters, viz. That the surface B cannot be maintained at a uniform temperature, during the ordinary changes in the temperature of the atmosphere.

In addition to what I have already stated against the validity of this objection, I remark, that as the bulb of the thermometer suspended in the exterior room, was exposed to the direct influence of B, it would indicate the joint temperature of that surface, and the air of the room; so that A could never be materially affected by the changes of B, without a corresponding and *visible* effect being indicated by this thermometer. Now, as evidence that B did not experience the changes you have supposed, I state, that the bulb of the *differential* thermometer in the exterior room was *screened* from the direct influence of B, and was only affected by the air of the room, but I found it always to agree with the mercurial thermometer of that room, at fixed points on their scales, which would not have been the case, had not B been permanent in its temperature.

The objection of the committee is of a *practical* nature, and capable of being proved or disproved by experiment. To this *test* I have requested, in my letter of the 27th ult. that it may be submitted, in any practicable manner they may suggest, and which I would perform in the presence of any gentlemen they may name. This they have declined.—If the objection possesses any *practical* weight, it can be discovered; but if merely *theoretically* true, it will not affect the practical accuracy, or *utility* of my results.

I remain, very respectfully, your obedient servant,

MARCUS BULL.

To JACOB BIGELOW, M. D., &c. &c. &c.

No. XVIII.

Dr. Bigelow to Mr. Bull.

BOSTON, October 21, 1826.

DEAR SIR—The committee will thank you to state DEFINITELY whether, in your opinion, their objections are, or are not, “*theoretically true.*”

In other words, whether you believe that, theoretically speaking, the surfaces A and B can be maintained, during an experiment, at the same temperature, or at a uniform difference of temperature; if, in the mean time, atmospheric changes of temperature take place.

A DIRECT answer will oblige your obedient servant,

JACOB BIGELOW.

MARCUS BULL, Esq.

No. XIX.

Mr. Bull to Dr. Bigelow.

PHILADELPHIA, October 30, 1826.

DEAR SIR—Your favour of the 21st inst. was duly received. The committee ask me “to state DEFINITELY whether, in my opinion, their objections are, or are not, ‘*theoretically true.*’”

I would reply, that let my answer be either in the *affirmative* or *negative*, I am unable to perceive, what possible bearing it could have, of a profitable or useful character, in relation to the subject matter of our investigation, this being *exclusively practical*; and I have in no case thought it either proper or necessary, to travel into any inquiries relating to the *abstract* or *theoretical* nature of the subject.

Suppose, for example, that I were to *admit* that the objections of the committee *were theoretically true*, but at the same time demonstrate to their satisfaction, or that of any competent judges, that this admission did not in any assignable way affect the *practical* results. The committee must in this case, as candid judges, wave their objections.

Suppose, on the other hand, I were to *deny* the *theoretical truth* of their objections: these they would endeavour to maintain, and we will suppose with success: but what would be the result? why truly, that there was some minute defect in the *theory* on which my experiments were founded, but which did not in any assignable or tangible degree, affect or invalidate the substantial practical results.

What then would be gained by the committee, but a *dilemma* in either case.

In relation to this subject, the committee will not perhaps disagree with me in opinion, that it is in the nature of *all theories* to fall short of a *perfect* application in *practice*.

No two thermometers perhaps have ever been made to agree exactly through all the degrees of their scales; nor was there probably a *LINE* ever drawn (in practice I mean) that was absolutely or *theoretically straight*.

What then would become of all the arts, if an absolute conformity to *theory* was required as indispensable to them.

I remain, sir, very respectfully, your obedient servant,
MARCUS BULL.

To JACOB BIGELOW, M. D., &c. &c. &c.

No. XX.

Dr. Bigelow to Mr. Bull.

BOSTON, November 2, 1826.

DEAR SIR—Your letter of October 30th, is this day received. After a delay of nearly six months, the committee, finding no reason to change the opinions expressed to you in June last, will present the report which was then contemplated, at the meeting of the Academy on Wednesday next.

Respectfully, your obedient servant,

MARCUS BULL, Esq.

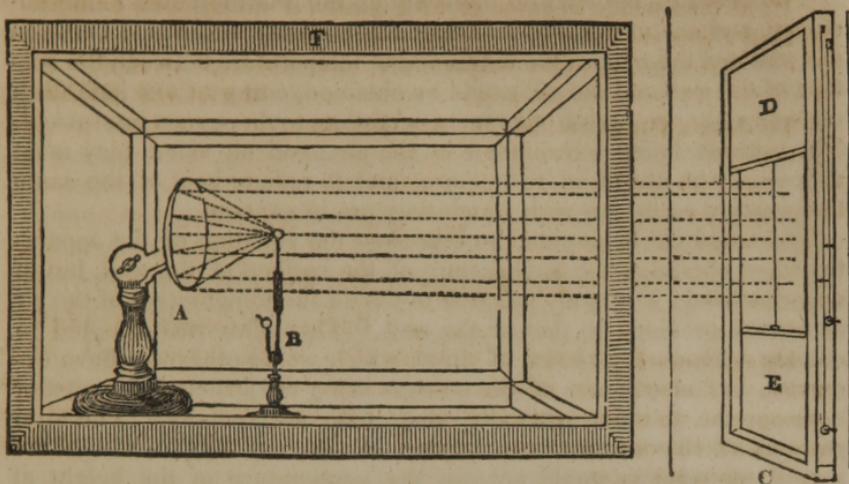
JACOB BIGELOW,
For the Committee.

In the commencement of the foregoing correspondence, (Letter No. II. page 6,) the committee state, "That the subject does not admit of the philosophic accuracy in its conclusions, which you appear to attach to it," and yet strange as it may appear, they condemn me in the sequel for not having effected what they themselves admit to be an impossibility.

The want of candour which the committee appeared to me to evince throughout the whole of the correspondence, was not well calculated to inspire me with much confidence in the soundness of the objections which they urged, as tending to invalidate the correctness of the deductions which I had drawn from my experiments, especially as these objections were in direct opposition to the opinions expressed by some of the best chemists of our country.

My experiments being entirely of a practical character, the validity of the only objection worthy of notice urged against their accuracy, was capable of proof by actual experiment; and had the committee been governed by the motives they professed, viz. a sincere desire to "arrive at a joint understanding of the truth,"* they would not have rejected the proposition contained in my letter No. XIII. to submit their objection to the test of experiment. To this test, however, I determined to submit it, and accordingly during the succeeding winter (1827) arrangements were made, with the kind assistance of Professor Hare, of the University of Pennsylvania, for the performance of the following experiments; the utmost care being used to construct the apparatus in such a manner as to give to the objection of the committee its greatest weight.

* Letter No. IV. page 11.



The following is a concise description of the apparatus made use of, together with the results of a course of experiments instituted to determine the question,—*Whether the interior surface of the walls of my exterior room, can be maintained at the same temperature as the air in contact with it, or at a uniform difference of temperature, during the ordinary changes in the temperature of the atmosphere?*

The wood cut represents a perspective view of the apparatus, which consists of a parabolic mirror of brass, A, 12.5 inches diameter, a differential thermometer, B, together with a frame, C, placed against the wall, in which frame two screens, D and E, slide in grooves at different distances from the wall, and are moved by cords attached to them passing over pullies in the top of the frame, and thence down the side of the latter. One of the screens, D, is of wood, the front surface of which is one inch from the wall; the other, E, consists of a plate of looking-glass, the silvered side being turned towards the wall, to prevent any immediate influence from the radiation therefrom, in consequence of its proximity, being placed within one-eighth of an inch of it.

To produce uniformity in the radiating power of the surfaces of the wall, and screens, the whole are covered with fine white drawing paper. White paper was selected, in consequence of its superior radiating power, being, as determined by Mr. Leslie, as 98 to 100, when compared with a surface of lamp-black.

The parabolic mirror is placed two feet from the wall, and parallel with it. In the focus of the mirror is placed one bulb of the differential thermometer, the other bulb being at a considerable distance below the influence of the mirror. To the stem of the former bulb the scale is attached.

The mirror and thermometer are covered by a glass case, F, to prevent as much as possible any influence from currents of air, and from the face and hand of the operator in examining the scale of the thermometer, and moving the screens. The glass case is open at one end, opposite the portion of wall experimented upon.

The effect of the wall on the scale of the thermometer being observed, and one of the screens being subsequently interposed between the wall and the mirror, the difference of temperature between the surface of the *wall* and the *air* would be obtained, either at one inch from the wall, or at one-eighth of an inch, according to the screen interposed. The screens being surrounded by the air upon all sides, they must necessarily be supposed to acquire, and to radiate heat of the same temperature as the air with which they are in contact.

The wood cut is intended to represent the situation of the apparatus when obtaining the temperature of the surface of the wall, but in experimenting, it was my practice to obtain the temperature of the air or screen, previous to that of the wall. This plan was adopted to obviate a temporary source of error, which would otherwise have occurred, in consequence of the screens being constructed, for greater convenience, to move vertically rather than horizontally. The temperature of the strata of air varying at different heights within the room, the screens would acquire the temperature of the height at which they should be suspended, and if at the top of the frame they would be warmer, or at the bottom colder, than the stratum of air opposite to the portion of the wall experimented upon, so that if presented to the mirror from either of the former situations, they would not, immediately, indicate the proper temperature.

The experiments have been performed at various times during the past winter, when the temperature of the air in the exterior room had been elevated 10° , 20° , and 30° , above that of the external atmosphere, and always with similar results at these different temperatures.

Using the glass screen, when the temperature of the air in the room was 30° above that of the atmosphere, (this being about double the difference experienced during any of my experiments on fuel,) the *greatest* difference found to be produced on the most delicate alcoholic *flat* bored differential thermometer which I could procure, the sensibility of which, when compared with Fahrenheit's, being as 120 to 1, was found to be 6° , from which deduct 2° for the oscillations of the instrument, (found by observation,) leaves an effect of 4° , or $\frac{1}{30}$ th of a degree of Fahrenheit's scale, the air being, at this distance, that much warmer than the surface of the wall. The greatest effect produced by the wood screen (one inch from the wall) was 16° , under the same circumstances; so that, as the strata of air *increase* in temperature as we recede, with the screen, from the wall, if it were possible to interpose a screen possessing merely *surface*, within an infinitely small distance of the wall, no perceptible difference would probably be found to exist between the temperature of the surface of the wall and that of the air in contact with it. When it is recollected that this maximum effect (equal only to $\frac{1}{30}$ th of a degree Fahrenheit) was produced by a surface of 122.71 inches, (the content of the mirror, 12.5 inches diameter,) concentrated upon a bulb of $\frac{3}{4}$ inches in diameter, no difficulty will be found in believing what I state as a fact, that *no effect whatever* could be observed on the Fahrenheit's thermometers used in my course of experiments on Fuel, when placed in the focus of the mirror.

From these experiments it may be seen, that the objection of the committee cannot be sustained by any effect observable on the ordinary instruments made use of to measure degrees of heat, although the difference of changes in the temperature of the atmosphere should be *double* those ordinarily experienced, and the effect of any given extent of surface be magnified more than *a hundred fold*; and unless the committee can show, that the ordinary mercurial thermometers are not sufficiently accurate for the purposes to which they are daily applied, their objection must fail of being substantiated, and may be safely rejected as futile, and of no possible practical importance.

These experiments were performed in the presence of a number of scientific gentlemen, who will if necessary corroborate the statement of the results obtained.

The preceding experiments were performed in the winter of 1826-7, and with a view to solicit from the Academy a reconsideration of my claim for the Rumford premium, for the purpose of giving me an opportunity to exculpate my former labours from the charge of inaccuracy, made by their committee; I visited Boston in May, 1827, prepared to exhibit a drawing of the apparatus made use of, together with a written description of these experiments, and their results.

At my interviews with the gentlemen composing this committee, it was stated to me that no objections would be made by them at the approaching meeting of the Academy, to granting the reconsideration about to be requested; but on the contrary that they would advocate such a request, and if granted, they had no doubt that a *fresh* committee would be appointed, who would have an opportunity of reviewing their labours; this course appearing also to them to be the only proper one to give me a *fair* hearing.

The following is a copy of my Memorial presented to the Academy.

BOSTON, MAY 28th, 1827.

To the Hon. James Savage, Recording Secretary of the American Academy of Arts and Sciences.

SIR—I have to request that you will do me the favour to lay before the Academy the following statement.

At the meeting of the Academy in May, 1826, an application was made on my behalf, for the Rumford premium, in consequence of a long course of experiments having been made by me, to determine the comparative quantities of heat evolved in the combustion of the principal varieties of wood and coal, used in the United States for fuel; and also to determine the comparative quantities of heat lost by the ordinary apparatus made use of for their combustion.

The apparatus employed in my experiments on fuel, was of a new construction, and has been considered by many scientific gentlemen both in this country and in Europe, as capable of insuring the most satisfactory results hitherto obtained on this intricate and highly important subject.

In consequence of certain objections made by the committee of the Academy to whom my claim was referred, tending to question the ac-

curacy of the apparatus employed by me, a correspondence took place between the committee and myself in relation to the validity of their objections. Failing to convince the committee by argument, that their principal objection was not well founded, I proposed to them to submit this question to the test of experiment, which they thought proper to decline; and in November last, an unfavourable report was made on my claim.

Since the report of the committee was made, their principal objection has been subjected to the most rigid and delicate test of experiment; and I have respectfully to request, that the Academy will be pleased to reconsider my claim for the Rumford premium, that an opportunity may be given me, to lay before them the results of these experiments, in any manner they shall be pleased to direct.

I have the honour to be, sir,

With great respect,

Your obedient servant,

MARCUS BULL.

After the meeting of the Academy, I received a note from Dr. Bigelow, of which the following is a copy.

Tuesday 29th May, 1827.

DEAR SIR—Your Memorial to the Academy is referred to the former committee, who will be ready to meet you at my house this evening at 8 o'clock.

Yours,

J. BIGELOW.

MARCUS BULL, Esq.

It is impossible for me to describe in words what were my feelings on learning that my reasonable expectations as to the appointment of a *fresh* committee had been disappointed; but they may be compared to those of a historical painter, whose keen sensibility should be roused by being told that he must submit his work, and reputation as an artist, to the decision of those who had previously, and in his opinion unjustly, decided against him.

During my conference with the committee at the house of Dr. Bigelow, I was informed by them that they did not consider the experiments instituted by me for the purpose of determining the validity of their objection, as in *any way bearing on the question!* and one of the gentlemen stated to me, that although *no* effect whatever could be observed on the Fahrenheit's thermometers when placed in the focus of the mirror, as stated by me; and supposing the difference of temperature between the surfaces A and B to vary 1° , yet he believed this source of error to be so great, that 25 per cent. more fuel would be consumed at one time than another, in performing any given experiment!

In proof of this opinion being well founded, he stated, that a mercurial thermometer when exposed to the radiation of heat from a sheet iron stove, as ordinarily heated, and within three or four feet of it, would be affected from five to ten degrees.

Now, suppose we fix the temperature of the stove at the low heat of 300° , and the effect of radiation as 10° on the scale of the thermometer. If we suppose the temperature of the stove to be reduced 1° or to 299° , the effect of radiation by arithmetical calculation would then be 9.97° or $\frac{3}{100}$ of a degree less, a difference in effect which would not be observable on the scale of any mercurial thermometer, and the difference in the amount of fuel which would be required to maintain the stove at 300° or 299° , to supply the loss of heat by radiation only, and for a period of time equal to that occupied by any of my experiments, may be supposed to be equally trifling.

The foregoing has been for some time prepared for publication, but this has been delayed for the purpose of obtaining from the Academy a copy of the Report of their committee, which they have at my request just furnished, and it is with great pleasure that I am enabled to give them an opportunity of speaking for themselves.

To the American Academy of Arts and Sciences.

THE committee to whom was referred the application of Mr. Marcus Bull for the Rumford premium, beg leave to report.

That Mr. Bull has laid claim to the premium on the ground of certain experiments performed with an apparatus suggested to him by Dr. Hare "to determine the comparative quantities of heat evolved in the combustion of the principal varieties of wood and coal used in the United States for fuel; and also to determine the comparative quantities of heat lost by the ordinary apparatus made use of for their combustion." The apparatus consists of two rooms, one constructed within the other; a stove being placed in the inner room to contain the combustible, of which the heating power is intended to be measured; also a stove in the outer room, to aid in sustaining an artificial temperature during the experiments.

Mr. Bull objects to the experiments of his predecessors, on subjects of this nature; on the ground of their "inaccuracy;" and quotes the observations of count Rumford to shew, "that in so intricate a subject, the utmost care is requisite, lest, after much labour, the inquirer should be forced to content himself with *approximations*, instead of accurate results, and valuations strictly determined." From this we are to conclude, that the object of Mr. Bull's experiments is to furnish us, in lieu of approximations, with accurate results, and valuations strictly determined.

In performing these experiments, equal quantities by weight of each kind of fuel, previously made absolutely dry; are burnt in the stove of the inner room; and the time is observed, during which, the combustion of each article will maintain the temperature of the inner room ten degrees higher than that of the outer room; which time is *supposed* to give the true relative heating power of the article. It is *endeavoured* to counteract the disturbing influence of different atmospheric temperatures, by regulating the heat of the outer room, so as to keep it always ten degrees below that of the inner room. This object Mr. Bull

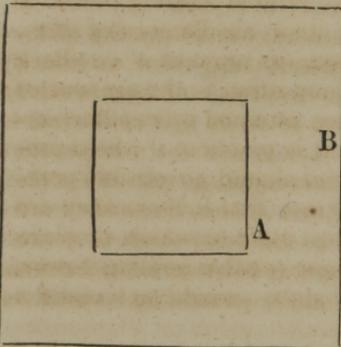
states that he effected, by opening a window in the outer room, when it was too warm; and kindling a fire in it, when it was too cold.

The committee have heretofore considered the experiments performed by Mr. Bull, with great attention; and were not able to perceive in them such "important discovery or useful improvement, on heat or on light" as should entitle Mr. Bull to the Rumford premium. The late communication of Mr. B. having relation to that decision of the committee; it may be proper to state some of the grounds on which it was formed, in detail.

Although the committee were of opinion that all due praise should be allowed to Mr. Bull, for the patient industry with which he has gone through a long and tedious course of experiments; yet at the same time they felt bound to state their conviction, that he had failed in the principal objects for which his experiments were intended. There are various grounds, on which these experiments are objectionable; of which the following will probably be deemed sufficient.—

1st. The principle, upon which these experiments are founded is radically defective; so that their results do not furnish any philosophical truths, on which reliance can be placed.

2d. They are of no practical value, and cannot be converted to any useful purpose, in the common concerns of life.—



In regard to the first of these objections, it will be seen, by referring to Mr. Bull's book and to the diagram which accompanies this report; that he has overlooked the effect of radiation which takes place from the inner to the outer wall, or from the surface A to the surface B in the diagram. This radiation will cause a continual escape of heat from the inner room to the atmosphere, whatever may be the intervening temperature of the air in the outer room; and this escape will

be more or less rapid *cæteris paribus*, in proportion as the weather is warmer or colder; so that any given experiment must afford a different result, according as the external atmospheric temperature should rise or fall during the time of its performance.

The above fact must be self evident to any person acquainted with the laws of the distribution of heat, and the committee believe Mr. Bull to be now convinced that it is philosophically true. He has however attempted to prove by subsequent experiments which were communicated to the Academy at their last meeting, and referred to this committee, that the surfaces A and B. are at nearly the same temperature with the air in contact with them, or at a uniform difference of temperature; during atmospheric changes. The committee are of opinion that if these new experiments prove any thing, they merely shew, that the different vertical strata of air in the outer room are of different temperatures; and that they increase in heat in proportion as they are nearer to A, and more distant from B. And if we admit this to be the fact, it does not alter the main and important truth, that A

and B will differ from each other, in proportion as the inner room differs from the atmosphere; whatever may be the temperatures existing between them. In regard to Mr. Bull's statement, that his experiments gave the same results during different temperatures of the atmosphere; this, if apparently true in some cases, would only show, that his outer wall is a slow conductor of heat. But the walls of houses, although slow, are nevertheless, sure conductors of heat; and it was expressly with a view to counteract their incompetency for philosophic purposes, that Mr. Bull's experiments were undertaken.—(See page 8th of Mr. B's book.)

The second objection of the committee belongs to the practical usefulness of Mr. Bull's experiments, supposing the principle on which they are founded to have been correct. Fuel as it is commonly burnt, contains more or less moisture; and is seldom or never used in the state employed by Mr. Bull, viz. that of absolute dryness. The quantity of water or aqueous matter retained in fuel by capillary attraction, amounts often to a third, and even to a half or more, of its weight; as we may learn from Mr. Bull's own book. Now this water in passing into vapour, renders latent a part of the heat produced by the combustion of the fuel. And since the more porous fuels, such as pine, afford much more capillary space, than the denser fuels, such as hickory; these articles will not have the same *relative* heating power in their common state, that they possess in their absolutely dry state. As the density of one fuel is found to be less, its amount of capillary space will be found to be greater; and in proportion to the amount of capillary space, will be the amount of water retained by capillary attraction; which water will go to render latent in practice, the heat produced by combustion. So that any man, who should govern his practice by Mr. Bull's tables; and should suppose that fuels as they are burnt, either in a green state, or in the driest state to which they are reduced by age and covering; have the same relative heating power, which they may possess in their totally dry state; would act under a perpetual error.—

The committee therefore do not recommend to the Academy, that any further measures be adopted in regard to this subject.

July 11th, 1827.

(Signed)

JACOB BIGELOW,
DANIEL TREADWELL, } COMMITTEE.
JOHN WARE,

At a meeting of the American Academy of Arts and Sciences holden at Cambridge, July 11th, A. D. 1827, Dr. Bigelow, from the committee to whom was referred at the preceding meeting a communication from Marcus Bull, Esquire, of Philadelphia, made a report, which was accepted, and of which the above is a true copy.

Attest

F. C. GRAY, *Recording Secretary.*

It is with reluctance that I make a single remark on the foregoing Report of the committee; but, as they have made some errone-

ous statements, and presented fresh matter requiring comment, a review of it becomes necessary.

The committee state that they "have heretofore considered the experiments performed by Mr. Bull, with great attention." For this purpose it would be supposed necessary that my account of them should at least have been *read* with "attention:" but that this has not been done, is apparent from the first paragraph in their Report, in which they say, "That Mr. Bull has laid claim to the premium on the ground of certain experiments performed with an apparatus suggested to him by Dr. Hare," and they even refer subsequently, for another purpose, to the very page (8, quarto, 11, octavo edition,) and passages of my paper, from which I take the following extract, to show that they cannot have *read* my paper with common attention, and also for the purpose of correcting their misstatement.

"Having spent nearly four months of application in perfecting my apparatus, and removing difficulties which presented themselves at the threshold of every stage of the investigation, and feeling desirous to avail myself of any improvements which might be suggested to me, either in the apparatus, or the intended plan of conducting the experiments, I invited several gentlemen to examine it for that purpose, and among them, Dr. Hare, professor of chemistry in the University of Pennsylvania.

"The method which had been adopted, as described, to comply with the last requisition, [that the surrounding refrigerating medium be permanent at any required temperature,] did not appear to Dr. Hare to possess that degree of accuracy which was necessary, nor did it equal that which every other part of the apparatus, together with the intended plan of conducting the experiments, as described to him, appeared to possess. Dr. Hare stated to me, that "he had long been under the impression, that no accurate comparison could be made by means of the same single room heated at different times, with different fuel, on account of the varying temperature of the weather; nor by different rooms at the same time, from the difficulty of finding two rooms sufficiently alike, in form, aspect, size, and materials. It seemed to him indispensable, to have one room within another, so that, in the interval, a uniformity of temperature might be artificially sustained." As the method suggested by Dr. Hare, would remove this difficulty with which I had unsuccessfully contended, no time was lost in making a practical application of his suggestion, and a room of smaller dimensions was in consequence constructed within that originally intended for my experiments, in the best manner which my architect could devise; by which a free circulation of air is produced on all the exterior surfaces of the interior room, and this air may be sustained of a uniform temperature."

Dr. Hare was not apprised of my experiments, until nearly four months after they were commenced, at which time he suggested the *addition* of the *interior room* to my "*apparatus*" as then constructed, and this is distinctly stated by me, as I had no wish to conceal it; but this alone constitutes only a *small part* of the *apparatus* employed by me; by which word, unless qualified, I have been led to suppose that an idea of the *whole of the instruments employed for any purpose*, is intended to be conveyed.

In the second paragraph the committee say, "Mr. Bull objects to the experiments of his predecessors, on subjects of this nature ; on the ground of their 'inaccuracy.'" This is very true, and I know of no other ground on which any man of sense would think it necessary to repeat laborious experiments upon *any* subject; but my experiments were almost exclusively made upon articles of fuel, not before experimented upon by any of my predecessors.

To the inference which the committee have drawn for me, from the observations of count Rumford, quoted by me, and by them, in the second paragraph, I must plead guilty ; as the avowal of any other "object" than that of furnishing "accurate results, and valuations strictly determined," for all practical purposes, must have impeached my judgment.

The assertion of the committee that, "The principle, upon which these experiments are founded is radically defective," has already been proved by experiment to be false in *practice*, and those only who are captiously disposed, and who wish to "split hairs with razors," will care whether in *theory* it be *true* or *false*.

The committee say that, "In regard to the first of these objections, it will be seen, by referring to Mr. Bull's book and to the diagram which accompanies this report ; that he has overlooked the effect of radiation which takes place from the inner to the outer wall, or from the surface A to the surface B in the diagram." If the committee mean to convey the idea that, at the time of constructing my apparatus, I was not aware of the fact, that heated bodies lose a portion of their heat by *radiation*; I will excuse such an idea for the sake of its modesty; but if they mean to be understood, that I have "overlooked the effect of" an *unequal* "radiation which takes place from the" "surface A to the surface B," in consequence of the latter surface presenting (as they aver) a refrigerating medium of *inconstant power*; I must admit that I was not only ignorant at *that* time of any appreciable inequality of "effect" from such a cause, but that I am *now* ignorant of it, as

"— optics sharp it needs, I ween,
To see what is not to be seen."

M'Fingal.

The committee proceed in their Report by giving the following opinion, that "This radiation will cause a continual escape of heat from the inner room to the atmosphere, whatever may be the intervening temperature of the air in the outer room ; and this escape will be more or less rapid *cæteris paribus*, in proportion as the weather is warmer or colder ; so that any given experiment must afford a different result, according as the external atmospheric temperature should rise or fall during the time of its performance."

The committee advance the foregoing *opinion*, as a matter of fact ; as they say, "The above fact must be self evident to any person acquainted with the laws of the distribution of heat, and the committee believe Mr. Bull to be *now* convinced that it is philosophically true." If the committee really *believed* their opinion to be self-evidently

true, and so apparent, I can scarcely suppose that they would have formed such an estimate of my understanding, or my candour, as to have supposed that I could not perceive, or would not acknowledge, that it was *theoretically* or "philosophically true:" but to show that *their* fact does not amount to a "self evident" proposition, I have merely to state that among the many gentlemen of science who have expressed their opinions on this objection, urged by the committee, and who are perhaps quite as well "acquainted with the laws of the distribution of heat," no one of them has ever stated to me his belief in its *possible influence in practice*, when confined to the question of my walls, and to the difference and variations of temperature to which they are subjected, and but very few have expressed their belief in its probable truth in *theory*.

The question respecting the *philosophical* truth of the objection which the committee have urged, I have never considered of sufficient importance to admit or deny;* but if I had done the former, I also admit, as probably do the members of the committee, that when a stone falls to the earth, the earth also approaches the stone, but must leave the practical amount to be estimated, by those who are fond of works of supererogation.

The committee state that I have "attempted to prove by subsequent experiments which were communicated to the Academy at their last meeting, and referred to this committee, that the surfaces A and B are at nearly the same temperature with the air in contact with them, or at a uniform difference of temperature; during atmospheric changes." I cannot refrain from expressing my surprise that such a statement should be made by gentlemen professing to have considered this subject "with great attention."

The experiments alluded to relate *entirely to the surface B*, as may be seen by the reader on referring to page 29, the practicability of maintaining this surface at a uniform temperature having been the exclusive object of these experiments, and the only important question of difference between the committee and myself. The wall of the surface A being constantly acted upon *from within* by an equal temperature, it was not exposed like the wall of the surface B *from without* to the variations in the temperature of the atmosphere.

I have never made any experiments to determine the difference of temperature between the surface A and the air in contact with it; but as the wall of this surface is of wood, and only one inch in thickness, and the wall of the surface B is of brick, and is ten inches thick, and both walls being usually exposed during an experiment to about the same difference of temperature on their reverse or opposite sides; I am inclined to believe, that a perceptible, but uniform difference would be found to exist between the temperature of the surface A, and the air in contact with it, and this in consequence of the different conducting power of the materials of which they are composed, and the great disparity in their thickness; this wall being but one inch, instead of ten inches, as is the case with the wall of the surface B, between which, and the air in contact with it, or rather at the distance of

* See Letters, No. XVII, XVIII, XIX.

one eighth of an inch, no practical difference was found. The committee will no doubt consider this, as a concession to their views; and if this straw will prevent their sinking they are at perfect liberty to catch at it, and their doing so would be quite in character, as is evident from many of their letters in the foregoing correspondence, particularly No. XVIII.

The committee do not appear to be satisfied with the results of these experiments, or even to admit that they bear upon the question at issue between us, and they still contend "that A and B will differ from each other, in proportion as the inner room differs from the atmosphere." Now, in confirmation of the accuracy of these experiments, and to show that they *bear directly* on the question at issue between the committee and myself, it will be proper to state in this place, that I have recently ascertained by placing against the surfaces A and B, the bulbs of two mercurial thermometers, accurately corresponding with each other, that when the temperature of the interior room is maintained 10° higher than that of the exterior, (as was done in my experiments on fuel,) the difference in the temperature of the surfaces A and B is 3° , and that no perceptible variation can be observed on the thermometers, during a change of 10° in the temperature of the atmosphere, which is as great as any which occurred during my experiments on fuel.

It will be observed by the reader, that the source of error urged by the committee, is entirely confined to that small portion of the heat given out by *radiation* from the surface A, to the surface B; but has no relation to that larger portion taken from A by the *air* of the exterior room, by the *conducting* process.

I have already stated that the difference in the temperature of the surfaces A and B, was found to be 3° during an experiment for that purpose, and I believe it to be very generally known by every "person acquainted with the laws of the distribution of heat," that the *whole amount* of heat radiated between bodies differing but 3° in temperature is *very small*, and as the committee appear to prefer opinions, to matters of fact; I give it as my opinion, that the *difference* in the amount of heat radiated between two bodies differing 3° in temperature at one time, and at another 4° , could not be discovered by any effect observable on the ordinary instruments used for measuring degrees of heat; and I presume, from the supposition already stated as having been made to me, by one of the committee, that they do not claim a greater *variation* than 1° in the temperature of the surfaces A and B; they also now admit that my "outer wall is a slow conductor of heat," this is all that I wish them to allow, and all that "any person acquainted with the laws of the distribution of heat," would require to know, to be convinced, that under such circumstances, no difficulty would be experienced in maintaining the surfaces A and B at a practically uniform difference of low temperature.

After the admission of the committee just quoted, they say, "But the walls of houses, although slow, are nevertheless, sure conductors of heat; and it was expressly with a view to counteract their incompetency for philosophic purposes, that Mr. Bull's experiments were undertaken. (See page 8th of Mr. B's book.)" If the walls of

houses were *not* "sure conductors of heat," and never lost it when once communicated to them, no urgent necessity would have existed that any experiments should have been made on this subject: but does the fact that they *are* "sure conductors of heat," render it impossible to make experiments sufficiently accurate for all practical purposes? What possible difference could there be as it respects utility, whether we ascertain the *relative*, or *positive*, amount of heat produced by the combustion of the different kinds of fuel, even if it were possible to ascertain the latter?

In noticing the latter part of the foregoing quotation, which appears to be rather obscure, I have merely to remark, that my experiments were not undertaken, as the committee say, "expressly with a view to *counteract* their [the walls of houses] *incompetency* for philosophic purposes," nor do I find in the matter of page 8, to which they refer, or indeed in any other part of my paper, any allusion to such a design. The intention of my experiments is clearly stated at page 21, (35 octavo) in the following words. "The object of my experiments being *practical utility*, rather than *scientific research*, I have estimated the comparative values of the different articles. These will be found in the last column of the table, and are equally applicable not only to every market, but for every change in the prices that can take place."

Whether this attempt to render the results of my experiments practically useful, has degraded them in the estimation of the committee I do not know; but it is very certain that a gentleman well acquainted with the manner in which scientific institutions are too frequently managed, observed, at the time I was computing the comparative values of specified quantities of the different articles of fuel, that this attempt to make them practically useful, would render them too *unphilosophical* in the eyes of the Academy.

"The second objection of the committee belongs to the practical usefulness of Mr. Bull's experiments," and in attempting to show that they are of no practical value, the committee state that, "Fuel as it is commonly burnt, contains more or less moisture; and is seldom or never used in the state employed by Mr. Bull, viz. that of absolute dryness." To the truth of this statement I most fully assent, (with the exception of the word "*never*,") but I am unable to perceive that it in any way disproves the "practical usefulness" or value of my experiments, and I have yet to learn, in what better state than "that of absolute dryness," it could have been employed by me.

As a preliminary and general answer to all their remarks about the quantity of moisture contained in the woods, and to those relating to the cause why green wood in some cases gives less heat than dry, and also to furnish my reason for having employed it in the latter state, I shall make some extracts from my paper, page 38, (62 octavo); for although the committee allude to some of these very passages, the whole of which are closely connected, yet it is evident, that they have not read them with much attention, as in that case, it is to be presumed that they would have devised some other objections more relevant to the point, or have omitted them entirely.

"The quantity of moisture absorbed by the woods individually, was

not found to diminish with their increase in density; whilst it was found that the green woods, in drying, uniformly lost less in weight in proportion to their greater density. Hickory wood taken green, and made absolutely dry, experienced a diminution in its weight of $37\frac{1}{2}$ per cent., white oak, 41 per cent. and soft maple, 48 per cent.; a cord of the latter will therefore weigh nearly twice as much when green as when dry.

“If we assume the mean quantity of moisture in the woods, when green, as 42 per cent., the great disadvantage of attempting to burn wood in this state must be obvious, as in every 100 pounds of this compound of wood and water, 42 pounds of aqueous matter must be expelled from the wood, and as the capacity of water for absorbing heat is nearly as 4 to 1, when compared with air, and probably greater during its conversion into vapour, which must be effected before it can escape, the loss of heat must consequently be very great.

“The necessity of speaking thus theoretically on this point, is regretted; but, it will be apparent, that this question of loss cannot be solved by my apparatus, as the vapour would be condensed in the pipe of the stove, and the heat would thereby be imparted to the room, which, under ordinary circumstances, escapes into the chimney.

“The average weight of moisture in different woods which have been weather seasoned from eight to twelve months, will not be found to vary materially from 25 per cent. of their weight; every economist, therefore, will see the propriety of keeping his wood under cover in all cases where this is practicable.”

Now, suppose that I had made experiments upon the woods at all the different degrees of humidity of which they are susceptible; for example, 49 different experiments upon soft maple, 1 of which in an absolutely dry state, and 48 others, differing from each other 1 per cent. in moisture. The first parcel experimented upon we will suppose to have been absolutely dry, and to have weighed 100 ounces, the second 101 ounces, or 100 of wood and 1 ounce of water, as we must suppose that each parcel contained 100 ounces of wood or ligneous matter, although the quantity of moisture should have varied from 1 to 48 ounces in the different parcels. Is it not apparent, that any heat rendered “latent” by converting the moisture into vapour in the body of my stove, would have again become *free* caloric, as the vapour condensed in the pipe of the stove, and would thereby have been imparted to the air of the room? and can there be any doubt that the whole quantity of moisture so converted into vapour, would have been condensed in passing through forty-two feet of extra thin *black* tin pipe, two inches in diameter, and principally formed into elbow joints; the pipe near the extremity for several feet being also *always at the temperature of the air of the room?*

This fact must make it appear self-evident, that the moisture resulting from the condensed vapour, must be reduced to the *precise temperature* that it possessed in the wood before the combustion of the latter; and there can be no doubt that this moisture would contain no other heat than its *specific heat* of that particular temperature; and that this specific heat would be neither more nor less than that of the moisture when combined with the wood, as the compound must ne-

cessarily be supposed to have acquired the temperature of the room, from being placed within it before, and remaining in it during the experiment.

That the results of these 49 different experiments would have been the *same*, no "person acquainted with the laws of the distribution of heat" will doubt: what then would have been gained, had the same process been gone through with all the woods experimented upon?

I ask the committee to state, what other, or more accurate standard can be taken in making comparative experiments upon *any* subject, than the *positive* weight or measure of the things subjected to experiment? If we wish to learn the quantity of heat given out in the combustion of the different woods, we must know the absolute quantity of *wood* or ligneous matter burned in each instance, and how are we to know this unless we deprive it of its extraneous moisture? and must not this standard be resorted to in any case, whether we burn it *green* or *dry*?

My experiments were intentionally made upon *wood* and *coal*; and not upon these articles compounded with water; and the question whether it be more economical to burn wood *green* or *dry*, was not the object of my inquiry, nor is it necessarily connected with the subject, as this must entirely depend upon the *manner* of burning it; for example, were it consumed in a stove, with a pipe similar to that used by me in my experiments, or in any other construction of apparatus for burning fuel, by which no portion of the heat generated is lost by escaping into the chimney, the same quantity and kind of wood or ligneous matter, would obviously be of the same value, in every possible state, from *green* to that of *absolute* dryness; but if consumed in a chimney fireplace of ordinary construction, by which 90 per cent. of heat is lost, the *dry* would undoubtedly be the most economical; this fact is stated in my paper, is urged by the committee at some length, and is also very generally known by every man who has paid any attention to, or cares about economy in his fuel; and it must be evident that I am in no way answerable for the losses sustained by those, who either ignorantly, or from choice, think proper to burn green wood, in place of dry, or who burn either, in an improper manner.

Experiments to determine the question of the comparative economy of burning wood absolutely dry, or with the various degrees of moisture of which it is susceptible; could only be performed by burning it in these several states, and *in every kind of apparatus made use of for its combustion*. This question, I leave to be solved *philosophically*, by those who may think it of sufficient importance, to justify the performance of such a course of experiments; but for all *practical* purposes, the results of my "*experiments to determine the loss of heat sustained by different constructions of apparatus ordinarily used for the combustion of fuel,*" will probably be found sufficiently accurate;* as, in proportion to the loss of heat sustained by the apparatus made use of, will be, in most cases, the comparative loss of burning wood in any

* The committee have not thought proper to raise any objections, or even to notice these experiments, although obviously of the first importance in the question of economy in the use of fuel; and respecting which, they appear to be so very solicitous on points of comparative insignificance.

other state than that of absolute dryness ; and this loss will be proportional to the quantity of moisture positively contained in the woods ; but not to their *capacity* of containing it, as stated by the committee, in their report.

The committee attempt to prove from the fact, that the woods possess different *capacities* for containing water, or aqueous matter, according to their density, that "these articles will not have the same *relative* heating power in their common state, that they possess in their absolutely dry state. As the density of one fuel is found to be less, its amount of capillary space will be found to be greater ; and in proportion to the amount of capillary space, will be the amount of water retained by capillary attraction ; which water will go to render latent in practice the heat produced by combustion : " and from these premises, they draw the following grave conclusion ; " that any man who should govern his practice by Mr. Bull's tables, " *would act under a perpetual error.*"

The "common state" of wood as to moisture is an indefinite term, and may be supposed to comprehend all states ; as it is well known that bakers, generally use their wood absolutely dry, whilst other persons use it as they please, with all the degrees of moisture of which it is susceptible ; but whatever may be the state intended to be referred to by the committee, the question as to its "*relative* heating power," has already been sufficiently noticed.

That the "capillary space," or the *capacity* of these spaces in the different woods for containing water, should be stated by the committee as an invariable standard for what they do, or may *happen to contain*, is certainly a doctrine entirely *unique*. The question with the economist would be, *ceteris paribus*, not what they are *capable* of containing, but what do they *actually contain*, and I am not aware that the solution of such a problem could be given (were its solution necessary) by any "tables," or indeed by any thing short of actual desiccation.

My "tables" are constructed to give the relative value of specified quantities of the different *woods* and *coals*, regard being had to their relative heating power in equal quantities by weight, and also to the *average weight* of the *combustible matter* contained in the quantities specified ; taken, for the woods, as is stated in my paper, page 26, (43 octavo.) "From a pile of swamp white oak of medium size, which had been cut the preceding winter, and weather-seasoned during the interval, this being the state in which the *largest portion* of wood is sold ;" and with the *coals*, there was no difficulty, their volume being the same whether *wet* or *dry*.

It is believed that my "tables," so far from leading those governed by them into "perpetual error," give as accurate information as utility requires, or the nature of the subject will admit ; it being evidently impossible for any man to construct a set of tables adapted to every change in volume produced by the different degrees of humidity of which the woods are susceptible ; but even were this practicable, and had it been done by me, I ask any man possessing common sense, whether such a set of tables would enable him to determine by bare *inspection*, the *real quantity of moisture contained in any parcel of wood that might be shown him* ? Such tables could pretend to no other

possible use, and their entire incompetency to such a purpose must be admitted. If it be really necessary that this fact should be determined, it must be done by the only possible process, that of desiccation: the question of utility then returns; whence the use of such tables, after the fact is determined? If the committee cannot answer this, they are less clear-sighted than most other individuals.

I have now concluded my review of the Report of the committee, and take the liberty of remarking, that as my experiments were not made with a *pen upon paper*, I do not fear any candid examination to which they can be subjected. The results stated in my tables, were obtained by *actual and accurate experiment*; and the apparatus employed by me, is at the service of any *philosophical inquirer*, who has the patience and the talent necessary to repeat them.

Let the experiments be made upon *any* article of fuel which may be selected, taken in equal quantities of the *combustible matter* by weight, and let these be burnt both in the *wet* and *dry* states, and at different periods, presenting the *greatest* and the *least* diversity in the changes of temperature in the atmosphere; and if the sources of error which are stated by the committee to exist, be matters of fact, they will be found, and their *exact value* or importance learned, as they will be measured, precisely, by the difference in the *time*, which equal quantities of the combustible, shall, by the heat given out in its combustion, maintain the temperature of the interior room, 10° higher than that of the exterior.

I have ever been aware of the great responsibility incurred by those who publish as accurate, experiments which have not been conducted with that rigour which investigations of this kind always demand, and more especially when the results given are intended to regulate any of the common concerns of life. Under a strong conviction of this truth, and having had no theories of my own to establish, or any interest in either coal or wood lands, my researches were prosecuted, and have been presented to the world. If the results which I have obtained are correct, mankind would be benefited by acting upon them, and if any man, or body of men, whose stations give currency to their opinions, should impugn them upon grounds which are untenable, they undertake a responsibility equally great, and to their retarding of useful knowledge, add individual injustice. If I am not mistaken, I have in the foregoing remarks proved that the committee have placed themselves in the latter predicament, and that they have taken a course calculated to lead the public into "perpetual error."

Account of the Donation made by Count Rumford to the American Academy of Arts and Sciences, for the establishment of a biennial Premium, extracted from the Boston Journal of Philosophy and the Arts, for April 1824.

In the year 1796, Count Rumford, then residing in London, presented to the American Academy of Arts and Sciences five thousand dollars, in three per cent. stock, for the purpose of establishing a biennial premium to be awarded to the author of the most important discovery or most useful improvement on heat or light which should be made in any part of America. The following letter addressed to the President of the Academy, accompanied the donation, and contains an account of the views of the liberal donor, and of the terms upon which the premium was to be awarded.

To the Hon. John Adams, President of the American Academy of Arts and Sciences.

SIR—Desirous of contributing efficaciously to the advancement of a branch of science, which has long employed my attention, and which appears to me to be of the highest importance to mankind; and wishing at the same time to leave a lasting testimony of my respect for the American Academy of Arts and Sciences, I take the liberty to request that the Academy would do me the honour to accept of five thousand dollars, three per cent. stock, in the funds of the United States of North America, which stock I have actually purchased, and which I beg leave to transfer to the Fellows of the Academy, to the end that the interest of the same may be by them, and by their successors, received from time to time, for ever, and the amount of the same applied and given, once every second year, as a premium to the author of the most important discovery, or useful improvement, which shall be made and published by printing, or in any way made known to the public, in any part of the continent of America, or in any of the American islands, during the preceding two years, on heat, or on light, the preference always being given to such discoveries as shall, in the opinion of the Academy, tend most to promote the good of mankind.

With regard to the formalities to be observed by the Academy in their decisions upon the comparative merits of those discoveries, which, in the opinion of the Academy, may entitle their authors to be considered as competitors for this biennial premium, the Academy will be pleased to adopt such regulations, as they in their wisdom may judge to be proper and necessary. But in regard to the form in which this premium is conferred, I take the liberty to request that it may always be given in two medals, struck in the same die, the one of gold, and the other of silver, and of such dimensions, that both of them together may be just equal in intrinsic value to the amount of the interest of the aforesaid five thousand dollars stock, during two years; that is to say, that they may together be of the value of three hundred dollars.

The Academy will be pleased to order such device or inscription to be engraved on the die they shall cause to be prepared for striking these medals, as they may judge proper.

If during any term of two years, reckoning from the last adjudication, or from the last period for the adjudication of this premium by the Academy, no new discovery or improvement should be made, in any part of America, relative to either of the subjects in question, (heat or light,) which, in the opinion of the Academy, shall be of sufficient importance to deserve this premium; in that case, it is my desire that the premium may not be given, but that the value of it may be reserved, and being laid out in the purchase of additional stock in the American funds, may be employed to augment the capital of this premium; and that the interest of the sums by which the capital may from time to time be so augmented, may regularly be given in money, with the two medals, and as an addition to the original premium, at each succeeding adjudication of it. And it is further my particular request, that those additions to the value of the premium arising from its occasional non-adjudications may be suffered to increase without limitation.

With the highest respect for the American Academy of Arts and Sciences, and the most earnest wishes for their success in their labours for the good of mankind,

I have the honour to be, with much esteem and regard,

Sir,

Your most obedient,
humble servant,

RUMFORD.

London, July 12th, 1796.

Upon the receipt of the donation, the thanks of the Society were presented to Count Rumford in the following terms:

Voted, That the thanks of the Academy be presented to Count Rumford, for this his very generous donation, and that they experience the highest satisfaction in receiving this additional and very liberal aid for the encouragement and extension of those interesting branches of science, which he has specified as the objects of his gratuity, and which *he* has so successfully cultivated: That they entertain a high sense of the sentiments and views, so becoming a philosopher, which have prompted him to this distinguished act of liberality; and in the execution of the grateful office, which they have undertaken, of awarding and distributing the premium which Count Rumford has thus appropriated, they will sacredly comply with the conditions of the donation; indulging the hope, that he will meet his reward, in learning that many in his native country are thereby excited to emulate his labours, and to promote the accomplishment of his beneficent wishes for the advancement of science, and the augmentation of human happiness.

At a meeting held in May 1801, the Society voted, that they would, at their meeting in May of the next year and afterwards, biennially at their May meeting, decide upon the discovery or improvement which appeared to deserve the Rumford premium. The subject we believe has frequently been brought before the Society, and they have been ready at the appointed time to confer the premium upon any in-

dividual whose claims were sufficient to authorize it. No discovery, however, or improvement has yet been made which has been deemed worthy this honour, and the fund has of course been accumulating, according to the terms of the donation, ever since it has been in the hands of the Society.

At the present time the fund amounts to \$7361, 19, in 6 per cent., and \$7050 in 7 per cent. stocks. The premium awarded would therefore be the interest of these sums for two years; three hundred dollars in the form of a silver and a gold medal, and the residue in money. This we believe is one of the largest premiums offered by any society or institution for discoveries in science or improvements in the arts, and is well worthy the attention of the scientific and ingenious men of our country.

The next meeting of the Society will be held on Tuesday the 25th of May next, at their room in the Boston Atheneum. At this meeting the Society will be ready to decide upon any claims which may be offered for the premium in question; it being the regular biennial period at which, by their vote of 1801, this subject comes before them.

As I do not now consider myself a candidate for the Rumford premium, it will not be deemed improper for me to review the conduct of the American Academy, both as it regards their general management of the trust committed to them, and their particular treatment of my late claim.

The donation of Count Rumford was made in July 1796, yet it appears that the Academy did not fix any period for the adjudication of the premium until May 1801, thereby suffering so much time to elapse as that the donation would appear to have become liable to forfeiture. They have been also remiss in their duty to the public, in neglecting to give sufficient publicity to the letter of Count Rumford accompanying the donation, by which it appears that the premium is intended to be a matter of fair competition to every inhabitant "in any part of the continent of America, or in any of the American islands." The ground for supposing the Academy to have been remiss in this particular, is inferred from a statement made to me by a member of that body, that with the exception of myself, the claimants for the premium have been residents of New England; and also from the knowledge of the fact, that very few of the present generation are aware of its existence.

The intention of the Count appears to be so obvious, that it is difficult to conceive of any construction which can be put upon the language made use of in his letter, to justify the Academy in never having awarded the premium, although it has been at their disposal for a period of nearly thirty years.

In the vote of thanks presented by the Academy to Count Rumford, they say, "that they experience the highest satisfaction in receiving this additional and very liberal aid for the encouragement and extension of those interesting branches of science, which he has specified as the objects of his gratuity, and which *he* has so successfully culti-

vated:" and further, that they "indulge the hope, that he will meet his reward in learning that many in his native country are thereby excited to emulate his labours." Now it is very well known that these branches of science were cultivated by him with a view almost exclusively to their utility and application in the ordinary concerns of life, and he may without impropriety be emphatically styled *the practical experimenter*. His experiments were not prosecuted with a view to discover *improved substitutes* for, or *new sources* of heat, or light, but to render those, then in possession, together with the sources of their production, more subservient to the good of mankind.

The discovery of new sources of heat or light, should these ever occur, will probably be the result of *accident*, rather than of any laborious research, and we cannot suppose that the Count would think it expedient to establish a premium, as a reward for fortuitous discoveries; nor can we suppose that he ever contemplated that successive discoveries of this nature would in any way be made every two years, and these sufficiently numerous to admit of "competitors for this biennial premium." That the objects which Count Rumford intended to encourage were of a very different nature, more extensive and diversified, and less difficult to accomplish than has evidently been supposed by the Academy, is plainly to be inferred from the following passage in his letter: "And it is further my particular request, that those additions to the value of the premium arising from its *occasional* non-adjudications, may be suffered to increase without limitation." And it is equally evident that he contemplated a state of *progressive improvement* in the several objects intended to be encouraged, and not *perfection*, in any instance, as, had the latter been his intention, whence the necessity of establishing a succession of premiums coextensive with the duration of time?

The large amount to which the premium has accumulated, in consequence of never having been awarded, is another and increasing difficulty both to the Academy and to the candidates, and as it respects the latter, it is most unjust, inasmuch as the objects which would be considered entitled to the *original* premium, are not considered entitled to it in its present state, so that as the premium increases in amount, the requirements on the part of the Academy also increase, and this evil cannot now be removed, as even supposing it to be awarded for the future biennially, the amount of the premium will never be lessened, except in the case of a reduction of the interest of the stocks in which the fund is invested.

By the conditions of the trust, no necessity exists that applications should be made for the premium; it is then clearly the duty of the Academy to seek for the proper objects of its bestowment, in case applications shall not be made for it: if so, why is it, that among the more prominent discoveries and improvements which have been made in this country on the subjects of heat and light since its foundation, that the premium has not been awarded to Professor Hare, for the discovery of the "Hydrostatic or *Compound Blow-pipe*," and for his "Galvanic Deflagrator?" and to Professor Olmsted, for the discovery of the applicability of cotton seed to the making of "*Illuminating Gas*?" If

it be said in reply, that the two first were discoveries of a character too *strictly philosophical*, and therefore not calculated "most to promote the good of mankind," the same objection cannot surely be urged against the latter, as it evidently possesses a *practical* character; but should it be said in reply to this, that it was not *sufficiently practical*, inasmuch as it could not be used by every body, and consequently could not "be converted to any useful purpose, in the common concerns of life," in this case I beg leave to refer the Academy to their transactions, where, as I am informed, they will find *many* applications for the premium for discoveries or improvements of *precisely the latter character*, and no insuperable difficulty can be supposed to have existed in determining which was the "*most important*" that occurred, during any of the many biennial periods which have elapsed. Should the *discretionary power* with which they are invested be resorted to as a general justification of their conduct, this covert appears to be too unsound to afford the desired protection.

With respect to their treatment of my claim, I shall be permitted to remark, that my experiments on Fuel, &c. were not commenced with any intention to make them the ground of an application for the Rumford premium, having been entirely ignorant of its existence, until after my arrangements for their performance had been principally matured, and a few of the preliminary experiments actually performed.

My course of experiments was commenced in November 1823, (and finished in April 1826,) and prosecuted almost literally night and day, until May 1824, at which time I had occasion to visit Boston, and when on my way thither, in passing through New York, a gentleman who had seen the apparatus constructed for my experiments, put into my hands the "Boston Journal of Philosophy and the Arts," for April 1824, containing the foregoing account of the donation made to the American Academy by Count Rumford. My arrival in Boston was only a day or two previous to the regular biennial period fixed by the Academy for deciding upon any claims for the Rumford premium, and I was not then prepared to become a competitor; yet as I expected to complete my experiments before the next meeting of the Academy in August, I was advised to present a memorial to that body, explaining the object of my experiments, and requesting them to suspend their decision on the Rumford premium. A particular description of my apparatus, and the intended plan of conducting the experiments, was also given to Dr. James Jackson and to Dr. Jacob Bigelow, who were on the committee subsequently appointed by the Academy, (on the 25th of May, 1824,) to report on applications for the Rumford premium, and to which committee, I presume, my memorial was referred.

Intending to leave Boston on the 27th, and being desirous of learning the fate of my memorial to the Academy, I called at the residence of Dr. Jackson, but had not the pleasure of finding him at home. Very soon after, however, I received a friendly note from him, in which he appears most fully to have anticipated the object of my call at his house; and from his known candour, and his uniform kindness to me, both at that period and subsequently, I feel assured that he will not be displeased with my making the following extract from his note,

which, although it is not considered by me to have been written in his official capacity of chairman of the committee, is nevertheless viewed as expressing their sentiments. "I take it that our committee will do nothing until they receive your communication. If you are not able to make that as early as the 10th of August, it will be best for you to address a note to me stating how the matter stands, and when you probably can make the communication. It is a matter of doubt whether the premium of the last two years can be awarded to you, if your communication should be satisfactory—as your discovery was not made known during those two years—that matter will be considered: but at least you will be a fair candidate for the next two years."

I have been thus particular in my description, and have made the preceding extract, for the purpose of showing what was the opinion of these gentlemen, at that time, respecting my claim as a fair candidate for the premium; and to show that every inducement which I had a right to expect from members of the Academy, was held out to me, to prosecute my labours to the greatest degree of perfection within my power. The course of "experiments to determine the comparative loss of heat sustained by using apparatus of different constructions, for the combustion of fuel," was not originally contemplated by me, but was suggested by them, and considered as a very desirable appendage to render the subject complete. My experiments were not instituted for the purpose of solving *philosophical* but *practical* questions, and such as were considered of great importance to all classes of society, and I claim for them the discovery of the relative value of the different kinds of wood and coal used for fuel, and also the discovery of the comparative economy of the ordinary apparatus made use of for their combustion; and these discoveries were made at a sacrifice of health, convenience, money, and nearly one and a half years of time, which would not have been compensated by the whole amount of the premium, large as it may appear. But entirely independent of these considerations, I have yet to learn, wherein consisted the justice of the Academy, in rejecting my claim to the Rumford premium, upon the untenable ground set forth by their committee, the futility of which may be considered as the strongest, among the many encomiums which have been paid to the accuracy of my experiments, both in this country and in Europe.

The following considerations in relation to the trust, and the duties of the trustees, appear to me to be so obviously just, as to require little additional remark.

It was the plain and evident intention of Count Rumford, that the premium should be awarded to the author of the *most important* discovery, or useful improvement, on the subjects of *heat* or *light*; and there can be no doubt, that those subjects are susceptible of discoveries and improvements, of such a nature as was contemplated by the donor. Yet the Academy has never awarded the premium, and seems to act under the impression that no discovery or improvement of the kind can be made. What then is the plain duty of the Academy? If its opinion be, that the trust cannot be executed, that opinion ought to be announced in the most public manner; that the fund which is

in the hands of the trustees, may go, where of right it belongs, to the representatives of Count Rumford. If no such opinion as this exists in the Academy, or if the opinion does exist, and is unsound, the Academy ought to execute the trust. To them is imparted by the donor the power of awarding the premium to the most important discovery or useful improvement; and consequently the power of judging; but this power is to be guided by a sound discretion, and to be exercised under a due sense and just observance of the trust reposed in them. These then are questions, which the Academy is bound to answer, and to answer satisfactorily—how is it, and why is it, that this premium has never been awarded? how is it, and why is it, that this trust is not executed? how is it, and why is it, that scientific men are invited to direct their efforts, prosecute their researches, and exercise their faculties, on these subjects, under a delusive hope and promise of distinction and reward, which can never be attained?

