

**Experiments and observations on animal heat, and the inflammation of combustible bodies : being an attempt to resolve these phaenomena into a general law of nature / by Adair Crawford, A.M.**

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Dobson, Thomas, 1751-1823  
National Library of Medicine (U.S.)

**Publication/Creation**

Philadelphia : Printed for Thomas Dobson, MDCCLXXXVII [1787]

**Persistent URL**

<https://wellcomecollection.org/works/xyhxbxk9>

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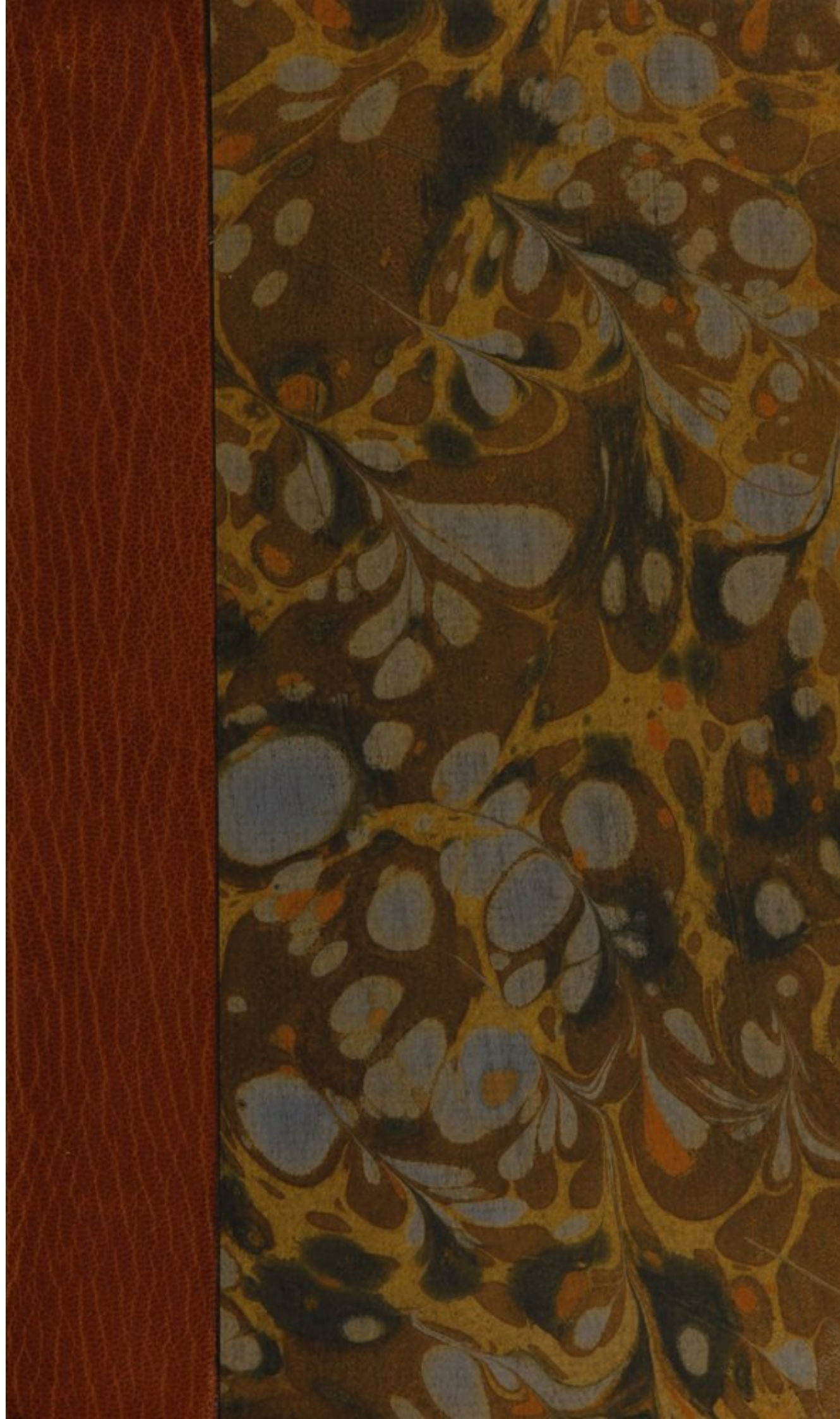
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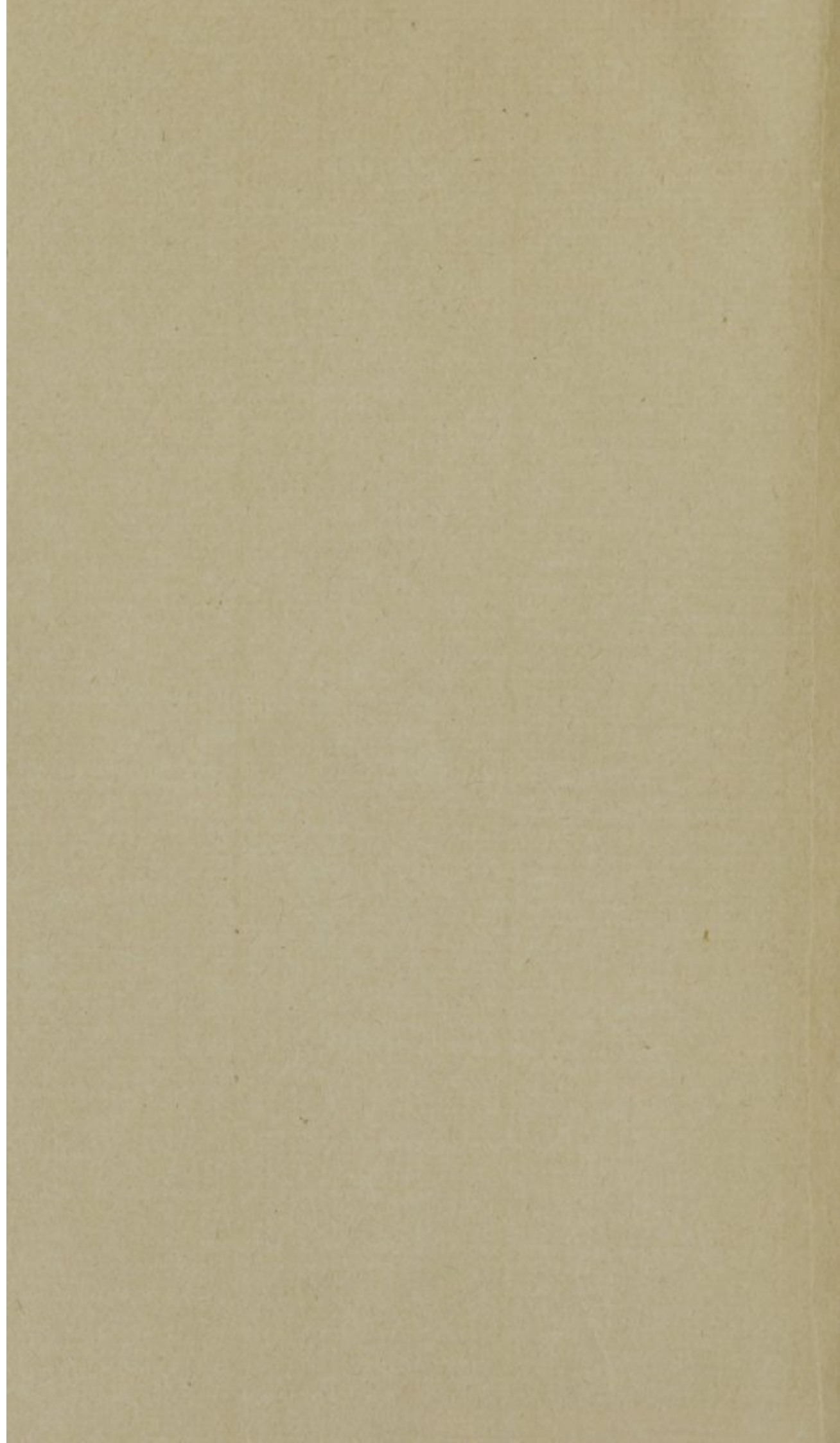
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EXPERIMENTS

AND

OBSERVATIONS

ON

ANIMAL HEAT,

AND THE

INFLAMMATION

OF

COMBUSTIBLE BODIES.

BEING AN ATTEMPT

TO RESOLVE THESE PHÆNOMENA

INTO A

GENERAL LAW OF NATURE.

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BY ADAIR CRAWFORD, A. M.

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PHILADELPHIA:

PRINTED FOR THOMAS DOBSON, IN SECOND-STREET, BETWEEN  
MARKET AND CHESNUT-STREET.

M,DCC,LXXXVII.



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BY ADAM L. BARNARD

PHILADELPHIA

PRINTED FOR THE AUTHOR, IN THE CITY OF PHILADELPHIA, BY J. B. LIPPINCOTT, 1844.

12-11147  
T O  
JOHN WATKINSON, M. D.

DISTINGUISHED BY  
HIS SKILL IN THE ART OF MEDICINE,  
BY HIS KNOWLEDGE IN  
PHILOSOPHY AND POLITE LITERATURE;

AND BY THOSE  
PRINCIPLES OF PROBITY AND HONOUR,  
WHICH SECURE THE ATTACHMENTS OF FRIENDSHIP,  
AND THE  
CONFIDENCE OF SOCIETY,

T H E S E E X P E R I M E N T S

A N D  
O B S E R V A T I O N S  
ARE INSCRIBED,

BY HIS MOST SINCERE FRIEND,  
AND OBLIGED HUMBLE SERVANT,

THE AUTHOR.



## A D V E R T I S E M E N T.

**I**T is proper to apprize the reader who would wish to repeat the experiments recited in the following pages, that such experiments are liable to be affected by a variety of adventitious circumstances, so minute as to require the most attentive observation. A change in the temperature of the air in the room, a variation in the time that is employed in mixing together the substances which are to have their comparative heats determined, a difference in the shape of the vessel, or in the degree of agitation that is given to the mixture, will often produce a considerable diversity in the result of the same experiment. It is possible however, by a series of repeated trials, with well constructed thermometers, in the same circumstances, to make a very near approximation to the truth, and, from a coincidence of facts, to obtain a degree of evidence, on which the mind may rest with entire confidence.

Much time and labour have been employed in endeavouring to render the following experiments accurate: But if, after all, some mistakes of less moment should be discovered, it is hoped that the candour of the public will excuse them, as the author is persuaded, that the facts from which the general conclusions have been drawn, are well founded.



# EXPERIMENTS

A N D

O B S E R V A T I O N S

U P O N

ANIMAL HEAT, &c.



THE words *heat* and *fire* are ambiguous. Heat in common language, has a double signification. It is used indiscriminately to express a *sensation* of the mind, and an unknown principle, whether we call it a *quality* or a *substance*, which is the exciting cause of that sensation. \*

By many ingenious philosophers, in modern times, the word Heat has been applied to the unknown principle, and has been taken in a much greater extent than in common language. For, in common language, it is used to express such a degree of the unknown external cause, as will produce a given effect upon the senses: but in the philosophical acceptance,

\* See Dr. Ried's Inquiry into the Human Mind.



tation, it expresses the external cause in the abstract, without regard to the effects which it may produce. For the sake of greater accuracy, in the following Dissertation, I shall, with the ingenious Dr. Irvine of Glasgow, call the latter *absolute* heat; and what is denominated heat in common language, I shall call *sensible* heat.

From this view of the matter, it appears, that *absolute* heat expresses that *power* or *element*, which, when it is present to a *certain* degree, excites in all animals the *sensation* of heat; and sensible heat expresses the same power, considered as *relative* to the *effects* which it produces.

Thus we say, that two bodies have equal quantities of sensible heat, when they produce equal effects upon the mercury in the thermometer; and that the same body has a greater or less degree of sensible heat, according as it produces a greater or a less effect upon this fluid.

But it will hereafter appear, that bodies of different kinds, have different capacities for containing heat; and, therefore, in such bodies, the absolute heat will be different, though the sensible heat be the same. If a pound of water, for example, and a pound of antimony, have the same temperature, we say that their sensible heats are equal. But we shall find, that the former contains a much greater quantity of absolute heat than the latter.

Fire, in the vulgar acceptation of the word, expresses a certain degree of heat, accompanied with light; and is particularly applied to that heat and light which are produced by the inflammation of combustible bodies. But as heat, when accumulated in a sufficient quantity, is constantly accompanied with light, or, in other words, as fire is always produced by the increase of heat, philosophers have generally considered these phenomena as proceeding from



from the same cause; \* and have, therefore, used the word *fire* to express that unknown principle, which, when it is present to a certain degree, excites the sensation of heat alone, but when accumulated to a greater degree, renders itself obvious both to the sight and touch, or produces heat, accompanied with light.

In this sense, the element of fire signifies the same thing with absolute heat.

These definitions and remarks being premised, I shall proceed to give the reader a concise view of the general facts upon which the experiments, recited in the following pages, are founded.

1. Heat is contained in great quantities in all bodies, when at the common temperature of the atmosphere.

In the deserts of Siberia, as we learn from Baron Demidoff, the mercury sometimes falls an hundred and fifty degrees below the freezing point. This is the most intense natural cold that has ever been known. But a much greater degree of cold has been produced by art. At Petersburg, in the year 1759, the heat was so much diminished by a mixture of snow and spirit of nitre, in the time of a severe frost, that the spirit of wine thermometer sunk to 148, and the mercurial thermometer to 352 degrees below the beginning of Fahrenheit's scale. As in this experiment the mercury was frozen, and as before its congelation, it contracted suddenly and irregularly, it has been concluded by Dr. Black and Dr. Irvine, that the cold which was then produced, was such as was indicated by the spirit of wine thermometer; and, therefore, 148 degrees below 0 is considered as the freezing point of mercury. This is the greatest cold that has ever been observed in nature;

\* See Boerhaave, Martin M'Quer, &c.



nature ; and yet we have no reason to believe that the bodies which were exposed to it, were deprived of their whole heat. From these facts, however, we may conclude with certainty, that heat is contained in great quantities in all bodies, when at the common temperature of the atmosphere.

2. Heat has a constant tendency to diffuse itself over all bodies, till they are brought to the same degree of sensible heat.

Thus, it is found by the thermometer, that if two bodies, of different temperatures, are mixed together, or placed contiguous, the heat passes from the one to the other, till they both come to the same temperature ; and that all inanimate bodies, when heated, and placed in a cold medium, continually lose heat, till, in process of time, they are brought to the state of the surrounding medium.

3. If the parts of the same homogeneous body have the same degree of sensible heat, the quantities of absolute heat will be proportionable to the bulk or quantity of matter. Thus the quantity of absolute heat contained in two pounds of water, must be conceived to be double of that which is contained in one pound, when at the same temperature. This I think is evident from the similarity in the particles of the same homogeneous solid and fluid substances. For the particles being similar, their powers will be equal ; their capacities for receiving heat will be the same ; and therefore the quantities of absolute heat which they contain, will be in proportion to the bulk or quantity of matter.

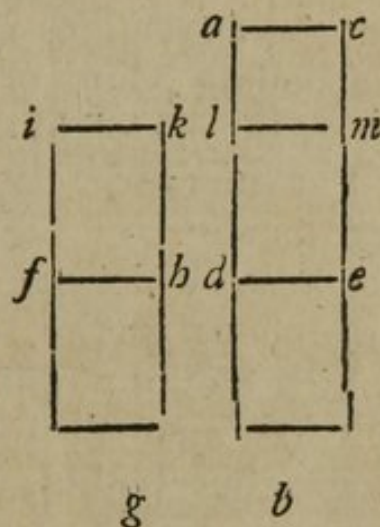
4. The mercurial thermometer is an accurate measure of the comparative quantities of absolute heat, which are communicated to the same homogeneous bodies, or separated from them, as long as such bodies continue in the same form.

To



To illustrate this, let us suppose two equal parts of mercury, or of any other fluid. And let the part A be raised to a higher temperature than the part B, A will therefore contain a greater quantity of absolute heat than B. Let them now be mixed; and according to the second general fact, they will arrive at the same common temperature. But they cannot come to the same temperature, unless A communicates to B, one half of the excess of its heat above the original heat of B.

For, let the whole heat contained in A, previous to the mixture, be represented by the figure,  $a b c$ . And let the heat contained in B be represented by  $f g h$ . When they are mixed together and brought to the same temperature, let the heat of A be diminished by the figure  $a l m c$ , and let that of B be increased by the figure



$i f h k$ . And since the heat which is taken from A is the very same with that which is added to B, it is manifest that  $a l m c$  must be equal to  $i f h k$ . Now it is affirmed, that  $a l m c$ , or  $i f h k$ , is equal to  $l d e m$ , or in other words, that A communicates to B one half of the excess of its heat above the original heat of B. For from the third general fact, it is evident, that when A and B are brought to the same temperature, they contain equal quantities of absolute heat. Therefore the figure  $i g k$ , is equal to the figure  $l m b$ . For the same reason the figure  $f g h$ , is equal to the figure  $d b e$ . And therefore the remainder  $i f h k$ , or  $a l m c$ , is equal to the remainder  $l d e m$ .

If, therefore, a mercurial thermometer, of the same temperature with a quantity of water, the absolute



bsolute heat of which is represented by  $d b e$ , be immersed in the same water, when its absolute heat is represented by  $l b m$ , and be again immersed in it, when its absolute heat is represented by  $a b c$ ; and if the expansions of the mercury be in these instances, in the ratio of one to two, it is evident that the expansions, and the increments of absolute heat, will be in exact proportion.

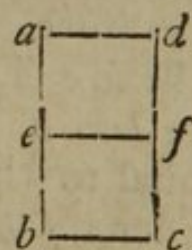
A variety of experiments were made upon this principle by *Monf. De Luc*, from which it appears, that the expansions of mercury between the freezing and boiling points of water, correspond precisely to the quantities of absolute heat applied.

The mercurial thermometer therefore is an accurate measure of the comparative quantities of absolute heat, which are communicated to the same homogeneous bodies, or separated from them, as long as such bodies continue in the same form.

It has been already shown, that in the same homogeneous bodies, if the quantities of matter be different, but the temperatures the same, the quantities of absolute heat will be, in proportion to the quantities of matter.

It now appears, that in the same homogeneous bodies, if the temperatures be different, but the quantities of matter the same, the quantities of absolute heat will be in proportion to the temperatures, or to the expansions in the mercurial thermometer.

For let the whole of the absolute heat contained in a pound of ice, at the temperature of 0, the beginning of Fahrenheit's scale, be represented by the figure,  $a b c d$ , and let this heat be diminished till it becomes equal to  $e b c f$ , which is the one half of  $a b c d$ . If a mercurial thermometer were deprived of its whole heat,  
and





and applied to the ice when its heat is represented by  $e b c f$ , and were again applied to the same ice when its heat is represented by  $a b c d$ ; the expansion produced by the heat  $e b c f$ , would be to that produced by  $a b c d$ , in the ratio of one to two\*. If  $a b c d$  were tripple of  $e b c f$ , the expansions would be in the ratio of one to three; and, universally, the temperatures being different, but the quantity of matter the same, the quantities of absolute heat will be in proportion to the temperatures, as measured by the mercurial thermometer.

This conclusion is an inference from De Luc's experiments, which prove, as was observed above, that the expansions of mercury are in proportion to the increase of the absolute heat, and its contractions to the diminution of this element, in all the intermediate degrees, between the boiling and freezing points of water: from whence it is inferred, that if the mercury were to retain its fluid form, its contractions would be proportionable to the decrements of the absolute heat, though the diminution were continued to the point of total privation.

COROLLARY. If therefore the sensible heat of a body, as measured by the mercurial thermometer, were to be diminished the one half, or the one third, or in any given proportion, the absolute heat would be diminished in the same proportion.

5. The comparative quantities of absolute heat which are communicated to different bodies, or separated from them, cannot be determined in a direct manner by the thermometer. Thus if the temperature of a pound of mercury be raised one degree, and that of a pound of water one degree, as indicated by the thermometer, it does not by any means

B 2

follow,

\* It is here supposed that the mercury continues fluid when it is deprived of its whole heat.



follow, that equal quantities of absolute heat have been communicated to the water and the mercury.

It has been thought by some philosophers, that the quantities of absolute heat in bodies, are in proportion to their densities. Boerhaave was of opinion, that heat is equally diffused thro' all bodies, the densest as well as the rarest, and therefore that the quantities of heat in bodies are in proportion to their bulk.

But it appears from experiment, that the law of the distribution of heat throughout the various classes of natural bodies, is not according to the ratio either of *bulk* or *density*.

The first attempt to determine by experiment the comparative quantities of absolute heat in bodies, was made by Farenheit, at the desire of Boerhaave.

The following is a short sketch of this attempt, nearly in the words of the author :

If you take equal quantities of the same fluid, and give them different degrees of heat, and mix them intimately together, the temperature of the mixture will be half the excess of the hotter above the colder,

If, for example, a pint of boiling water at 212, be mixed with a pint of the same fluid at 32, the temperature of the mixture will be 122: the warm water will be cooled 90 degrees, and the cold water heated 90.

But if this experiment be made with water and mercury, in the same circumstances, the effect will be very different.

For if you take equal bulks of mercury and water, and give the water a greater degree of heat than the mercury, the heat of the mixture will always be greater than half the excess of the heat of the water above that of the mercury.

On



On the other hand, if the mercury be hotter than the water, the temperature of the mixture will constantly be less than half the excess of the heat of the mercury above that of the water. The changes which are produced in the temperature of the water and mercury, in the first of these instances, are found to correspond to those which are produced, by mixing three parts of hot water with two of cold; and in the second instance, to those which take place, when three parts of cold water are mixed with two of hot. That is, the change produced in the heat of the mercury, is to that produced in the heat of the water, as three to two.

From the former of these experiments it was justly concluded by Boerhaave, that in the same body, the distribution of fire is in proportion to the bulk or quantity of matter.

But from the experiments with water and mercury, he concluded very unwarrantably, that heat is equally diffused thro' all bodies, the densest as well as the rarest; and therefore that the quantities of heat in different bodies, are in proportion to their bulks, or to the spaces which they occupy.

These experiments have been repeated and varied in the present age, and very different conclusions have been drawn from them.

I have already observed, that, if a pint of mercury at 100, be mixed with an equal bulk of water at 50, the change produced in the heat of the mercury, will be to that produced in the heat of the water, as three to two; from which it has been inferred, that the absolute heat of a pint of mercury is to that of an equal bulk of water, as two to three: or, in other words, that the comparative quantities of their *absolute* heats are reciprocally proportionable to the changes



ges which are produced in their *sensible* heats, when they are mixed together at different temperatures \*.

The truth of this conclusion may be illustrated in the following manner.

If ~~four~~<sup>three</sup> pounds of diaphoretick antimony at 20, be mixed with one pound of ice at 32, the temperature of the mixture will be very nearly 26. The ice will be cooled six degrees, and the antimony heated six. If we reverse the experiment, the effect will be the same. That is, if we take six degrees of heat from three pounds of antimony, and add it to a pound of ice, the latter will be heated six degrees. The same quantity of heat, therefore, which raises a pound of ice six degrees, will raise three pounds of antimony six degrees.

If this experiment be made at different temperatures, we shall have a similar result. If, for example, the antimony at 15, or at any given degree below the freezing point, be mixed with the ice at 32, the heat of the mixture will be half the excess of the hotter above the colder. From hence we infer, that the result would be the same, if the antimony were deprived of its whole heat, and were mixed with the ice at 32. But it is evident, that, in this case, the ice would communicate to the antimony the half of its absolute heat†. For, if 200 below frost, be conceived to be the point of total privation, the antimony will be wholly deprived of its heat, when its temperature is diminished 200 degrees below 32; and the heat contained in the ice, when  
at

\* This fact has, for several years, been taught publicly by Dr. Black and Dr. Irvine, in the Universities of Edinburgh and Glasgow: and it has been applied by Dr. Irvine, to the solution of a variety of curious and important phenomena.

It is hoped, that those learned and ingenious philosophers will soon favour the world with their respective discoveries in this branch of science.

† See the Corollary to the 4th general fact.



at 32, will be 200 degrees. If we now suppose them to be mixed together, the temperature of the mixture will be half the excess of the hotter above the colder; or the ice will be cooled 100 degrees, and the antimony heated 100. The one half of the heat, therefore, which was contained in the ice, previous to the mixture, will be communicated to the antimony: from which it is manifest, that, after the mixture, the ice and antimony must contain equal quantities of absolute heat.

To place this in another light, it has been proved, that the same quantity of heat which raises a pound of ice six degrees, will raise three pounds of antimony six degrees. From which it is inferred, that the same quantity of heat which raises the ice 200 degrees, or any given number of degrees, will raise the antimony an equal number of degrees\*.

A pound of ice, therefore, and three pounds of antimony, when at the same temperature, contain equal quantities of absolute heat. But, it appears from the third general fact, that three pounds of antimony contain three times as much absolute heat, as one pound of antimony; and hence the absolute heat of a pound of ice, is to that of a pound of antimony, as three to one.

Again, if a pound of ice at 32, be mixed with a pound of antimony at 12, the temperature of the mixture will be 27; the ice will be cooled five degrees, and the antimony heated 15; or the change produced in the sensible heat of the ice, will be to that produced in the sensible heat of the antimony, as one to three. But it was before proved, that the absolute heat of a pound of ice, is to that of a pound of antimony, as four to one. From which it is evident, that the comparative quantities of absolute heat,

in

\* It is here supposed, that the ice continues in the same form.



in equal weights of ice and antimony, are reciprocally proportionable to the changes which are produced in their sensible heats, when they are mixed together at different temperatures \*.

Thus it appears, that the comparative quantities of absolute heat in bodies may be determined, by mixing them together as above, and observing the changes which are produced in their sensible heats. This rule, however, does not apply to those substances, which, in mixture, excite sensible heat or cold by chymical action.

From the reasoning which was employed to establish the above proposition, it follows, that equal weights of heterogeneous substances, as air and water, having the same temperature, may contain unequal quantities of absolute heat. There must, therefore, be certain essential differences in the nature of bodies, in consequence of which *some* have the power of collecting and retaining the element of fire, in greater quantities than *others*. These different powers are called in the following pages, the *capacities* of bodies for containing heat. Thus, if we find by experiment, that a pound of water contains three times as much absolute heat, as a pound of antimony, the capacity of water for containing heat, is said to be to that of antimony, as three to one.

## S E C T.

\* I have thus endeavoured briefly to establish the truth of the above doctrine, as a necessary introduction to the experiments which follow, but the more full and complete illustration of it I shall leave to Dr. Black and Dr. Irvine.

This discovery opens a wide field for investigation, as by means of it we are enabled to estimate the comparative quantities of absolute heat in bodies, and to determine with certainty and accuracy, the various proportions in which the element of fire is distributed throughout the different kingdoms of Nature.



## S E C T. II.

**H**A V I N G premised these general facts, I shall now lay before the reader my Experiments on Animal Heat, and the Inflammation of Combustible Bodies. \*

It was observed above, that sensible heat has a constant tendency to diffuse itself equally over all bodies, till they are brought to the same temperature. From this property of heat, it is manifest, that those animals which have a higher temperature than the medium in which they live, must be continually communicating heat to the surrounding bodies. Since therefore, in the animal kingdom, there is a constant dissipation of heat, it follows, that there must be a proportionable supply of this element, to repair the waste. For, if the animal body had not the power of exciting or collecting heat, it would soon arrive at the temperature of the ambient medium.

With a view to discover the nature of this power, I made a variety of experiments, in the summer, 1777, on animal, vegetable, and mineral substances;  
C
some

\* I think it proper to observe, that I was led to the consideration of this subject, in consequence of having attended the chymical Lectures of the learned and ingenious Dr. Irvine of Glasgow. And it is a tribute of justice which I owe to this philosopher, to acknowledge, that the solution which he has given of Dr. Black's celebrated discovery of latent heat, or of the heat which is produced by the congelation of water, spermaceti, bees-wax, metals, &c. suggested the views which gave rise to my experiments.

I must also add, that Dr. Irvine, from the solution which he has given of the above phenomena, and from the general fact, that heterogeneous bodies have different capacities for containing heat, concluded, that there was a difference between the absolute heat of fixed and atmospherical air. It remained to ascertain this difference, and to determine by experiments, whether fixed or atmospherical air, contained the greater quantity of absolute heat.



some of which I shall now relate, as I think they have led to the true source, from whence the heat of animals, and the heat which is produced by the inflammation of combustible bodies, is derived.

I must first observe, that experiments for determining the comparative quantities of absolute heat in bodies, are liable to three causes of inaccuracy. 1. When the substances to be compared, are mixed together, a certain time is required for the heat to pass from the warmer to the colder body, till they arrive at the same temperature. If they be *intimately* mixed, and the vessel be a little agitated, a minute is generally sufficient. During this interval, a part of the heat is carried off by the surrounding atmosphere. It therefore becomes necessary, to calculate the heat which is thus lost in the first minute.

A very simple and ingenious rule was laid down for this purpose, by the illustrious Sir Isaac Newton.

Considering the heat in a body, as the excess whereby it is warmer than the surrounding medium, he supposed, that the quantities of heat lost in small portions of time, would always be proportionable to the heats remaining. Thus, if a body were 180 degrees hotter than the atmosphere, the quantity of heat which it would lose in a given moment, would be double of that which it would lose in an equal portion of time, if it were only 90 degrees hotter than the atmosphere. And, therefore, if the times be taken in arithmetical progression, the decrements of heat will be in geometrical progression. But, it has been observed by Dr. Martine, that this rule is not to be admitted without some restriction. He has inferred from a variety of experiments, that the differential decrements are in a proportion somewhat greater than the inherent quantities of heat; and, there-



therefore, that the quantities of heat lost, are partly equable, and partly in geometrical progression\*.

This observation of Dr. Martine, it must be allowed, is well founded. But when the body, which is to be the subject of an experiment, transmits heat very fast, and its temperature is much greater than that of the ambient medium, the error produced, by calculating according to Sir Isaac Newton's rule, is so very inconsiderable, that it may be neglected.

On the contrary, when the experiment is made in a vessel that transmits heat very slowly, and the heat of the substance to be examined is not much greater than that of the atmosphere, the quantities of heat lost in any two small successive portions of time, approach so nearly to an equality, that the difference cannot be distinguished by the nicest thermometer. That this observation is well founded, appears from the following experiment :

A pound of water in an earthen vessel, being raised to 120, the temperature at the end of

1	minute was	—	—	119
2	—	—	—	118
3	—	—	—	117
4	—	—	—	116
5	—	—	—	115
6	—	—	—	114
7	—	—	—	113

In the experiments which I shall hereafter relate, the order of cooling was observed for several minutes ; and, in general, the heat which was lost in the first minute, was calculated according to Sir Isaac Newton's rule, from the series of numbers determined by observation.

I must here observe, that I believe Dr. Irvine was the first who applied this rule of Sir Isaac Newton,

\* See Martine's Essays.



to calculate the heat lost during the first minute, in experiments for determining the absolute heat of bodies.

2. If, in making these experiments, the substance which has the greatest heat, be mixed with that which has the least, in a cold vessel, a part of the heat will be communicated to the vessel: But if the experiment be made in a warm vessel, the cold substance will receive heat, not only from the warm substance, but from the vessel in which it is contained.

The most effectual method of avoiding this cause of inaccuracy, is first to determine the capacity of the vessel for receiving heat, compared with that of one of the substances to be examined.

The relative capacity of the vessel, in which some of the following experiments were made, compared with that of water, were thus determined.

The air in the room being	—	61
A pound of water at	—	168
was poured into an earthen vessel at	—	68
The temperature of the water at the end of		
1 minute was	—	155
2	—	150
3	—	145

To discover the heat communicated to the atmosphere in the first minute, say as 84 to 89 so is 94 to a fourth proportional, which gives 99.5. From which it appears that 5.5 were carried off by the air in the first minute, adding 5.5 to 155 we have 160.5 for the true temperature of the water and of the vessel.

Subtracting this from 168, we have 7.5 for the quotient. The water was therefore cooled by the vessel 7.5, and the vessel was heated by the water 92.5. And since the vessel received this heat from the water, it is manifest, that the same quantity of heat,



heat, which changes the temperature of a pound of water, 7.5 will change the temperature of the vessel 92.5. And by a parity of reasoning, the same heat which raises the water one degree, will raise the vessel  $12\frac{1}{3}$ . If, therefore, in any experiment, we find that the vessel has received  $12\frac{1}{3}$  of heat from a pound of water, we may be sure that the separation of this heat, has cooled the water one degree.

3. In experiments for determining the absolute heat of bodies, water is generally the standard. When the body which is to have its heat compared with that of water, transmits heat very slowly, it frequently happens, that the different parts of the mixture, cannot be brought precisely to the same temperature for several minutes. Thus I find that in experiments upon vegetables and the calces of metals, there is a considerable difference, at the end of the first minute, between the heat of the mixture at the surface, and that at the bottom of the vessel; arising partly from the tendency of the warmest part of the water to remain at the surface, and partly from the difficulty with which the above-mentioned substances transmit heat. This may be remedied in some degree, by mixing the bodies together intimately, and agitating the mixture briskly. But by much agitation, the heat is carried off suddenly and irregularly, which makes it difficult to calculate the heat that is lost in the first minute.

The method which I apprehend, least liable to error, is to agitate the mixture moderately, and at the end of the first minute, to take the arithmetical mean between the heat at the surface, and that at the bottom of the vessel; and if in a variety of experiments, in different circumstances, we find that the result is nearly the same, we may be sure that we have approached very near the truth.

With



With these precautions, the following experiments were made, to determine the absolute heat of some of the most common vegetable and animal substances, compared with that of water.

### EXPERIMENT I.

Air in the room	—	—	69		
A pound of wheat at	—		66		
was mixed with a pound of water at			166		
The mixture being agitated for a short time,					
The temperature at the end of					
	surface	bottom	medium		
1 minute was	138	—	128	—	133
2 —	134	—	125	—	129½
3 —	131	—	122	—	126½
4 —	127	—	120	—	123½

The mean temperature of the mixture at the end of one minute was 133, at the end of two minutes 129½, at the end of three minutes 126½: And therefore the heat carried off by the air in the first minute, being calculated according to Sir Isaac Newton's rule, was very nearly 3½. If we add this to 133, we shall have 136½ for the true temperature of the mixture. It was proved that the capacity of the vessel for receiving, heat, was to that of the water as 1 to 12½. In the experiment which we are now considering, the vessel was raised from 66 to 133, or 67.5; dividing this by 12½, we have nearly 5.5 for the quotient. From which it appears, that the water was cooled by the vessel 5.5, or the vessel separated 5.5 degrees from the water. The true temperature of the mixture was 136½, subtracting this from 166, we have 29½ for the remainder. The water was therefore cooled 29½ by the wheat and the vessel together. But we have shown that it was cooled 5.5 by the vessel; it was therefore cooled 24 by the wheat. But the wheat was raised from 66 to 136½ or 70.5. It follows,



follows, that the same quantity of heat which will change the temperature of water 24, will change that of wheat  $70\frac{1}{2}$ . Therefore the absolute heat of water is to that of wheat, as  $70\frac{1}{2}$  to 24, or very nearly as 2.9 to 1.

## EXPERIMENT II.

Air in the room — — 60.

One pound of oats, having the hulls  
taken off, at — — 61,

was mixed with one pound of water, at 161;

The temperature of the mixture at the end of

	surface		bottom		medium
1 minute was	127	—	123	—	125
2 —	125	—	122	—	$123\frac{1}{2}$
3 —	121	—	121	—	121
4 —	118	—	118	—	
5 —	114	—	116	—	
6 —	111	—	115	—	

To 125, the mean temperature of the mixture, adding  $2\frac{1}{2}$  for the heat carried off by the air in the first minute, we have  $127\frac{1}{2}$  the true temperature of the mixture.

By calculating, as in the preceding experiment, it appears that the water was cooled by the vessel nearly 5.4. The oats were raised from 61 to  $127\frac{1}{2}$  or 66 $\frac{1}{2}$ ; subtracting  $127\frac{1}{2}$  from 161, we find that the water was cooled by the oats and the vessel together  $33\frac{1}{2}$ ; but it was cooled 5.4 by the vessel; it was therefore cooled 28.1 by the water: and hence the absolute heat of water is to that of oats, as 66.5 to 28. , or as 2.36 to 1, that is, nearly, as  $2\frac{1}{3}$  to 1.

## EXPERIMENT III.

Air in the room — — 59.

One pound of beans at — — 60,

was mixed with one pound of water at 160

The



The temperature of the mixture at the end of

	surface		bottom		medium
1 minute, was	119	—	113	—	116
2 —	117	—	109	—	113
3 —	116	—	107	—	111½
4 —	113	—	106	—	109½
5 —	111	—	105	—	108
6 —	109½	—	104½	—	107
7 —	107½	—	104	—	106¾
11 —	101	—	101	—	101

To 116 adding 3 degrees for the heat carried off by the air in the first minute, we have 119 for the temperature of the mixture. It appears, from calculation, that the water was cooled by the vessel 4.8. The beans were raised from 60 to 119, or 59 degrees, subtracting 119 from 160, we find that the water was cooled, by the beans and the vessel together, 41 degrees. But it was cooled by the vessel 4.8. It was therefore cooled by the beans 36.2. And hence the absolute heat of water is to that of beans as 59 to 36.2, or nearly as 1.6 to 1.

#### EXPERIMENT IV.

Air — — — 61.

A pound of barley at — — 60,  
was mixed with a pound of water at 160;

The temperature of the mixture at the end of

	surface		bottom		medium
1 minute was	126	—	120	—	123
2 —	122	—	115	—	118½
3 —	119	—	113	—	116
6 —	109	—	109	—	109

To 123, the mean temperature, adding 4½ for the heat carried off by the air in the first minute, we have 127½ the true temperature of the mixture. The vessel was raised by the water 66½, dividing this by 12½, we have 5.3 for the quotient; by which it appears



appears that the water was cooled by the vessel 5.3. The barley was raised from 60 to  $127\frac{1}{2}$  or  $67\frac{1}{2}$ . The water was cooled by the vessel and the barley together, from 160 to  $127\frac{1}{2}$ , or  $32\frac{1}{2}$ . But it was cooled 5.3 by the vessel. It was therefore cooled 27.2 by the barley: and hence the absolute heat of water is to that of barley as 67.5 to 27.2, or nearly as 2.4 to 1.

### EXPERIMENT. V.

Air	—	—	60.
One pound of the lungs of a sheep at			59,
was mixed with a pound of water at			149 ;
The mixture at the end of			
	surface	bottom	medium
1 minute was	112	108	110
2 —	109	106	$107\frac{1}{2}$
3 —	106	105	$105\frac{1}{2}$
4 —	103	103	103

Adding 2.5 for the heat carried off by the air in the first minute, we have 112.5 for the true temperature of the mixture,

The vessel was raised from 59 to 112.5, or 53.5. Dividing this by  $12\frac{1}{2}$ , we have nearly 4.3 for the quotient. From which it appears, that the water was cooled by the vessel 4.3. The flesh was raised from 59 to 112.5, or 53.5. The water was cooled by the flesh and the vessel, from 149 to 112.5, or 36.5; but it was cooled 4.3 by the vessel; it was, therefore, cooled 32.2 by the flesh. And hence the absolute heat of water is to that of flesh, as 53.5 to 32.2; or, nearly as 1.3 to 1.

### EXPERIMENT VI.

Air	—	—	60,
One pound of milk at	—	—	60.
was mixed with a pound of water at			160 ;
D			The



The temperature at the end of

	surface		bottom		medium
1 minute was	109	—	107	—	108
2 —	106	—	104	—	105
3 —	103	—	102	—	102½

To 108, the mean temperature, adding 2.8, we have 110.8, the true temperature of the mixture.

The vessel was raised by the water 50.8. The water was therefore cooled by the vessel 4.1. But it was cooled by the milk and the vessel together 49.2. It was consequently cooled by the milk 45.1.

The milk was raised from 60 to 110.8, or 50.8. And hence the absolute heat of water is to that of milk, as 50.8 to 45.1; or, nearly as 1.1 to 1.

## EXPERIMENT VII.

Half a pound of water at — 47,  
was mixed with a half pound of blood at 98;

Having agitated the vessel, the heat was in a short time nearly equally diffused over the whole, and the mixture continuing fluid for the space of two minutes, its temperature at the end of

1 minute was	—	71	
2 —	—	70	coagulated.
3 —	—	70	
4 —	—	70	
5 —	—	70	
6 —	—	69	

The blood, which was the subject of this experiment, was procured from a sheep, by dividing the veins and arteries of the neck; and appeared by its colour to be a mixture of venous and arterial blood, though consisting chiefly of the latter. The capacity of the vessel in which this experiment was made, for containing heat, was to that of the water, as 6 to 94; or, nearly as 1 to 15.6.

The



The mixture cooled (if we except the time of its coagulation) at the rate of one degree in a minute. Adding one degree for the heat lost in the first minute, we have 72 for the heat of the mixture. The vessel was raised by the blood, from 47 to 72, or 25 degrees. The blood was therefore cooled by the vessel nearly 1.6: it was cooled by the water and the vessel together 26 degrees. It was therefore cooled by the water 24.4. The water was raised 25 degrees. And hence the absolute heat of a mixture of venous and arterial blood is to that of water, as 25 to 24.4.

These experiments prove, in general, that flesh, milk, and vegetables, contain less absolute heat than water, and water less than blood. Blood, therefore, contains a greater quantity of absolute heat, than the principles of which it is composed.

The remarkable accumulation of heat in this fluid, led me to suspect, that it absorbs heat from the air, in the process of respiration. And in this suspicion, I was much confirmed by the following considerations:

1. Those animals which are furnished with lungs, and which continually inspire the fresh air in great quantities, have the power of keeping themselves at a temperature considerably higher than the surrounding atmosphere. But animals that are not furnished with respiratory organs, are very nearly of the same temperature with the medium in which they live.

2. Among the hot animals, those are the warmest, which have the largest respiratory organs, and which consequently breathe the greatest quantity of air in proportion to their bulk. Thus, the respiratory organs of birds, compared with their size, are more extensive than those of any other animal; and birds have the greatest degree of animal heat.



3. In the same animal, the degree of heat is in some measure proportionable to the quantity of air inspired in a given time. Thus, we find that animal heat is increased by exercise, and by whatever accelerates respiration.

From these considerations, I was naturally led to a more particular examination of this subject: the result of which, is comprehended in the following propositions:

### P R O P O S I T I O N I.

Atmospherical air contains a greater quantity of absolute heat, than the air which is expired from the lungs of animals; and the quantity of absolute heat contained in any kind of air that is fit for respiration, is very nearly in proportion to its purity, or to its power in supporting animal life.

Before I proceed to the direct proof of this proposition, it is necessary to consider the nature of the change which the air undergoes in the lungs. It is well known, that the air expired from the lungs, occasions a precipitation in lime water. A part of it, therefore, consists of fixed air. It has been found, by Dr. Priestley, that the residuum of this air, is a mixture of atmospherical, and what he has called phlogisticated air, a species of air which occasions no precipitation in lime water, but which extinguishes flame, and is noxious to animal life.

That the fixed air produced in respiration, depends upon a change, which the atmospherical air undergoes in the lungs, is, I think, evident, from the following facts.

Air is altered in its properties by phlogistic processes, and though many of these processes are totally different from each other, yet the change produced in the air, is in all cases very nearly the same. It is diminished in its bulk. It is rendered incapable of main-



maintaining flame, and of supporting animal life. And, if we except a very few instances, where the fixed air is absorbed, it universally occasions a precipitation in lime water. We have therefore reason to believe, that there is no instance of a phlogistic process in nature, which is not accompanied with the production of fixed air.

It may be supposed, indeed, that this air is discharged from the substance which furnishes the phlogiston.

In *some* cases, a part of the fixed air may proceed from this source, as in various instances of combustion and putrefaction. But there are other cases, in which we are certain, that the fixed air is derived from a change produced in the atmospherical air.

Thus, air diminished by the burning of alcohol and sulphur, occasions a precipitation in lime water. The same effect is produced, when atmospherical air is diminished by nitrous air, and when it is exploded with inflammable air; and yet it is not found that brimstone, alcohol, nitrous or inflammable air, contain fixed air.

But Dr. Priestley has given us the most decisive proof of this fact, in the following experiment. Having caused the electric spark to pass through a glass tube, the lower part of which contained some water tinged with turnsol, he observed, that the blue colour of the liquor was, in a few minutes, changed to red, and that the included air was diminished in its bulk, and rendered highly noxious. He likewise observed, that when the spark was taken in air over lime water, the lime was precipitated\*.

Since, therefore, a quantity of fixed air, is, in these processes, produced by a change in the atmospherical air, we may conclude by induction, that the

\* See Dr. Priestley's experiments and observations upon air, Vol. I.



the same effect is produced in every other phlogistic process.

Thus it appears, that in respiration, atmospherical air is converted into fixed and phlogisticated air. It is therefore necessary, in order to determine the truth of the proposition, that we should compare the absolute heat of fixed and phlogisticated air, with that of atmospherical air.

The following experiments were made to determine the heat of these different species of air.

### E X P E R I M E N T I.

Air in the room — 52.

A bladder containing a pint of atmospherical air at 102, was immersed in a pint of water at 52.

The heat of the water at the end of

	surface	bottom
1 minute was	53	52½
2	53	52½
3	53½	53
4	53½	53½

In the above experiment, the air and the water do not seem to have been brought to the same temperature, till the end of 4 minutes. That this was really the case, is proved by the following experiment.

### E X P E R I M E N T II.

Air in the room 64. A pint of water was taken, the temperature of which, was 63 at the surface, and 62½ at the bottom. A pint of atmospherical air confined in a bladder, was raised to 163. The bladder containing the air being immersed in the water the heat was determined by two thermometers, one of which was placed near the surface of the water, in contact with the bladder and the other near the bottom.

At



At the end of 1 minute the thermometer at the surface was  $67\frac{1}{2}$ , thermometer at the bottom  $62\frac{1}{4}$

2 minutes	65	—	63
3 —	65	—	$63\frac{1}{4}$
4 —	$64\frac{1}{2}$	—	$63\frac{3}{4}$
5 —	$64\frac{1}{2}$	—	$63\frac{2}{3}$

In this experiment, which was made with the same bladder that was used in the former, the heat of the air in the bladder near the surface, exceeded the heat of the water for several minutes. And hence we may perceive the reason why, in the first experiment, the temperature of the water rose gradually at the surface and the bottom, till the end of 4 minutes; for during that time, it continued to receive heat from the air which had been immersed in it.

In that experiment, the heat communicated to the water by the air, and the bladder which contained it, was  $1\frac{1}{4}$ . Of this quantity of heat, the portion which was yielded by the bladder, will be seen by the experiment which follows :

### E X P E R I M E N T III.

A pint of water was taken at 52. The bladder having been dried and freed from air, was raised to 102, and being immersed in the water, the heat of the water at the end of

	surface	bottom
1 minute was	$52\frac{1}{4}$	52
2 —	$52\frac{1}{4}$	$52\frac{1}{4}$

We must allow, therefore, one quarter of a degree, in the experiment in question, for the heat imparted by the bladder. And hence it follows, that *one* degree was communicated by the air.

To discover, from the above experiment, the absolute heat of atmospherical air compared with that of water, we may consider what would have been the effect produced, if atmospherical air contained the  
same



same absolute heat with water. In that case, if the air were only the one hundredth part as dense as water, and were raised 100 degrees above the temperature of the water, it would communicate to it nearly one degree of heat. If it were only the one eighth hundredth part as dense, and were raised 100 degrees above the heat of the water, it would communicate to it nearly the one eighth part of a degree. If it were raised only 50 degrees above the heat of the water, it would communicate the one sixteenth part of a degree.

Now, the density of atmospherical air is to that of water, in a proportion somewhat less than that of 1 to 800. If, therefore, in the experiment in question, the atmospherical air had contained the same absolute heat with water, it would have communicated to the water, nearly the one sixteenth part of a degree of heat. But it communicated to it one entire degree of heat. Atmospherical air must therefore contain at least 16 times as much absolute heat as water.

Dr. Irvine has pointed out a general rule, by which the comparative quantities of absolute heat in bodies may be estimated, when the quantities of matter, and the changes produced in the sensible heats, are unequal. In that case, the quantities of absolute heat, are reciprocally as the changes in the sensible heats, multiplied into the quantities of matter.

By the help of this rule, the ratio of the heat of atmospherical air to that of water, may be more accurately calculated in the following manner.

It is evident, that, as equal bulks of air and water were taken, if the densities of the water and air had been equal, the absolute heat of the air would have been to that of the water, as 1 to 49, which  
is



is the reciprocal proportion of the changes produced in the sensible heats.

Again, if the changes produced in the sensible heats had been equal, the absolute heats would have been reciprocally as the quantities of matter. It follows, that neither being equal, the absolute heats are in the compound ratio, of the sensible heat gained by the water, to that separated from the air, and of the quantity of water to that of the air.

The experiment was made in a quart pewter vessel, the capacity of which for receiving heat was to that of the water very nearly as 1 to 16. The quantity of water was 16 ounces; and therefore, the heat received by the vessel was equal to that which would have been received by the one sixteenth part of 16 ounces, or by one ounce of water. The water and vessel together were consequently equal to 17 ounces of water; and the specific gravity of air being to that of water in the exact proportion of 1 to 862; it follows that the quantity of matter contained in 17 ounces of water is to that contained in a pint of air, nearly as 915 to 1. Hence the absolute heat of air is to that of water in the compound ratio

of	915	to	1, and
of	1	to	49

From which it appears, that the quantity of heat contained in the former of these elements, is to that contained in the latter, as 915 to 49, or nearly, as 18.6 to 1.

In calculating this experiment, no allowance was made for the heat carried off by the external air; for as the temperature of the water, after the air was immersed in it, exceeded that of the atmosphere, only  $1\frac{1}{4}$ , the portion of heat which was thus carried off was so very small, that it may be neglected.



I must farther observe, that, from the imperfection of thermometers, and from the difficulty of judging by the eye of one fourth or one third of a degree, perfect accuracy in experiments of this kind is not to be expected.

The experiment, however, for determining the heat of atmospherical air, has been frequently repeated—every repetition has tended to confirm the general conclusion, and from the result of a variety of trials, I have the greatest reason to believe that the ratio of the heat of atmospherical air to that of water, as deduced from the above experiment, does not exceed the truth.

I propose, in future, to endeavour to ascertain, with as much accuracy as possible, the heat of the different species's of air, by a greater variety of trials, and by thermometers constructed for the purpose

I next proceed to determine the heat of fixed and phlogisticated air.

#### EXPERIMENT IV.

Air in the room 52. A pint of fixed air, extricated from chalk by the vitriolic acid, was confined in a bladder, and raised to 104. A pint of water was taken at 54. The bladder containing the air being immersed in the water, the temperature of the water at the end of

	surface		bottom
1 minute was	54	—	54
2	54½	nearly,	54½ nearly
3	54½	—	54½

The bladder in which this air was contained being freed from air, and raised 50 degrees above the temperature of a pint of water, communicated to the water, one fourth of a degree.

This experiment, was frequently repeated with the same result.

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## E X P E R I M E N T V.

Air in the room 67. A pint of air, elevated from rosin by heat, was confined in a bladder, and was raised to 104.

A pint of water was taken at 64. The bladder containing the air, being immersed in the water, the temperature at the end of

			surface	bottom
1	minute was	—	64 $\frac{1}{2}$	64
2	—	—	64 $\frac{1}{2}$	64
3	—	—	64 $\frac{1}{2}$	64
4	—	—	64 $\frac{1}{2}$	64 $\frac{1}{4}$

The bladder in which the air was contained, being freed from air, communicated to a pint of water in the same circumstances, half a degree of heat.

To obtain this air, a small quantity of rosin, was put into a gun barrel, the end of which was heated red hot. When the rosin was violently inflamed, fresh air was blown into the touch hole, and the fumes immediately beginning to issue copiously, a flaccid bladder was fixed upon the end of the barrel.

The air which was thus obtained, was partly inflammable; when it was forced upon a candle, it burned with a pale bluish flame. It consisted chiefly of fixed and phlogisticated air.

## E X P E R I M E N T VI.

A pint of air elevated from tallow, as in the former experiment, and confined in a bladder, was raised to 113.

A pint of water was taken at 63. The bladder containing the air being immersed in the water, the temperature, at the end of

E 2

1 mi-



		surface		bottom
1 minute was		63 $\frac{1}{4}$	—	63
2	—	63 $\frac{1}{2}$	—	63 $\frac{1}{4}$
3	—	63 $\frac{1}{2}$	—	63 $\frac{1}{2}$
4	—	63 $\frac{1}{2}$	—	63 $\frac{1}{2}$

The bladder being then freed from air, and immersed in a pint of water, in the same circumstances, raised the water half a degree.

This air was also partly inflammable, but consisted chiefly of fixed and phlogisticated air.

From these experiments it appears, that the quantity of sensible heat communicated by fixed and phlogisticated air, to an equal bulk of water, (the difference of temperature being 50) is so small, that it cannot be measured by the thermometer. But we have seen, that in the same circumstances, a pint of atmospherical air communicates one degree of heat to a pint of water. From hence we may conclude, with certainty, that the absolute heat of atmospherical air is greater than that of fixed or phlogisticated air.

The specific gravity of fixed air was found by the honourable Mr. Cavendish, to be to that of water, as 1 to 511. If, therefore, it had contained the same absolute heat with water, it would have communicated to the water nearly the eleventh part of a degree. If it had contained less absolute heat than water, it would have communicated less than the eleventh part of a degree. But such minute variations of heat cannot be distinguished by the thermometer; and, therefore, from this experiment, we can draw no precise conclusion, with regard to the comparative heat of water and fixed air.

It is well known, that a great quantity of fixed air is contained in the crude calcarious earth. Chalk and limestone contain more than a third of their weight of this species of air. It has been proved  
by



by Dr. Black, that, when these substances are deprived of the fixed air, with which they are combined in their natural state, they are converted into quicklime; and as no sensible heat or cold is produced, by the separation of fixed from the calcarious earth (as I shall afterwards endeavour to show,) it is possible, by comparing the heat of the crude calcarious earth with that of quicklime, to ascertain, with accuracy, the absolute heat of fixed air.

The following experiments were made to determine the heat of chalk and quicklime.

#### E X P E R I M E N T VII.

A pound of chalk at	—	58,
was mixed with a pound of water at		158;
The temperature of the mixture at the end of		
1 minute was	— — —	135,
2 — — —	— — —	134.

Adding one degree for the heat carried off by the air in the first minute, we have 136 for the temperature of the mixture.

The chalk was raised from 58 to 136, or 78 degrees.

The water was cooled by the chalk and the vessel together 22 degrees. It was cooled nearly 2 degrees by the vessel—it was, therefore, cooled 20 degrees by the chalk. And hence the heat of water is to that of chalk, as 78 to 20, or as 3.9 to 1.

It is to be observed, that no sensible heat was produced by mixing equal parts of chalk and water together, when they were both at 57, which was the temperature of the air in the room.

The absolute heat of quicklime cannot be ascertained with accuracy, by making water the standard, as these substances, when mixed together, produce much sensible heat.

For



For this reason, I endeavoured to determine the absolute heat of quicklime, by mixing together equal quantities of chalk and quicklime at different temperatures. But, to determine, whether the experiment could be made with accuracy in this way, I first made the following experiment on chalk.

### E X P E R I M E N T VIII.

Air	—	—	64.
Half a pound of powdered chalk at			64
was mixed with an equal weight powdered chalk at			164;
The mixture at the end of			
1 minute was	—	—	116
2	—	—	114
3	—	—	112
4	—	—	111½
5	—	—	111
6	—	—	110½
7	—	—	110½
8	—	—	110

The vessel in which this experiment was made, was heated to 114, previous to the mixture. Hence it appears, that equal quantities of chalk, at different temperatures, being mixed together; the temperature of the mixture, at the end of two minutes, was half the excess of the hotter above the colder.

From a variety of trials, I have found that chalk heats and cools very slowly. And this seems to have been the reason, why the hot and cold chalk were not brought to a common temperature, till the end of the second minute.

### E X P E R I M E N T IX.

A pound of quicklime at	—	61
was mixed with a pound of chalk at		161.

The



The mixture at the end of

1 minute was	—	—	110
5	—	—	110
6	—	—	111
7	—	—	111½
8	—	—	112
9	—	—	111½
10	—	—	111
11	—	—	110½
12	—	—	110

The vessel was heated to 111, previous to the mixture.

In this experiment we find, that when the chalk and quicklime were mixed together, the thermometer at first fell to 110; afterwards it gradually rose to 112, which it never exceeded; the mixture then cooled at the rate of half a degree in a minute.

If, therefore, we take 112 for the temperature of the mixture, we shall probably be very near the truth. At least we shall not make the heat of quicklime greater than it really is. For tho' some sensible heat seems to have been produced by the mixture, yet the whole effect which the production of sensible heat can have in this calculation, is to diminish the heat of quicklime, compared with that of chalk.

Taking 112 for the common temperature, we have 49 for the heat separated from the chalk, and 51 for that gained by the quicklime. And hence the absolute heat of chalk is to that of quicklime as 51 to 49.

That cold is not produced by the separation of fixed air from the calcarious earth, I endeavoured to satisfy myself in the following manner.

If cold were produced by the separation of these substances, heat would be produced by their union. This conclusion is not conjectural. It is founded up-  
on



on an induction of facts. It is an inference drawn from what in similar cases actually takes place in nature. Thus cold is produced by the evaporation of water, and heat by the condensation of vapour. Like effects have been observed by the ingenious Dr. Black, in a very great variety of natural phenomena. And as no instance can be shown to the contrary, we may safely conclude, in general, that when a body produces cold in consequence of a change of form, it will produce heat when it returns to its former state.

To discover whether sensible heat is produced by the union of quicklime with fixed air, I exposed an ounce and a half of quicklime at 66, to the vapour arising from a mixture of chalk and the vitriolic acid. In a few minutes, the thermometer in the mixture rose to 88; the heat of the vapour was 82; and the thermometer in the quicklime stood at 78.

If sensible heat had been produced, in this experiment, by the union of the quicklime with the fixed air, the heat of the quicklime would have been greater than that of the vapour.

I have also found, that no sensible heat is produced, when quicklime is precipitated from lime water, by fixed air.

We may therefore conclude, with great probability, that sensible heat is not produced by the union of fixed air with the calcarious earth. At least it is certain, from these experiments, and from a variety of phenomena, that if heat is at all produced by the combination of these substances, the quantity is so very inconsiderable, that it cannot affect the conclusions, which are contained in the following pages.

As heat therefore, is not produced by the union of these substances, we may conclude that cold is not produced by their separation, and consequently during this process, they will not absorb heat from the  
surround-



surrounding bodies. Hence it may be inferred, that the quantity of heat contained in the earth and air when separated, is not greater than the heat which they contained previous to their separation.

The following is a brief illustration of the truth of this conclusion.

It is found by experiment, that the same heat which raises the regulus of antimony one degree, will raise the calx of antimony only the one third of a degree.

If, therefore, we suppose that the regulus, when at the common temperature of the atmosphere, contains 200 degrees of heat, and if we conceive it to be suddenly calcined, the heat contained in the regulus, will raise the calx, only the one third of 200 degrees, or 66 degrees and  $\frac{2}{3}$ ; the latter will therefore during the calcination absorb 133 degrees and  $\frac{1}{3}$  of heat. And hence the calx is found to contain three times as much absolute heat as the regulus.

In like manner, if the fixed air and calcarious earth when disunited, contained a greater quantity of absolute heat than when combined, the separation of these substances would necessarily be attended with the absorption of heat. But we have proved that no heat is absorbed during their separation. We may therefore conclude, that, when, a given quantity of chalk, is resolved into its principles, by converting it into quicklime and fixed air, the absolute heat of the quicklime and fixed air taken together, is not greater than the heat which was originally contained in the chalk\*.

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\* This conclusion is farther confirmed by the ingenious Dr. Irvine's discoveries with regard to the cause of the phenomena of latent heat. As these discoveries, however, have not been communicated to the world, I have not taken the liberty to point out their connection with this part of my subject.



From these data, the absolute heat of fixed air compared with that of chalk, may be calculated in the following manner.

It has been proved that the absolute heat of chalk is to that of quicklime, as 51 to 49, or nearly as 25 is to 24. We shall suppose that chalk contains one third of its weight of fixed air; and that the absolute heat, contained in the quicklime and fixed air which are produced by the calcination of a given quantity of chalk, is equal to that which was contained in the chalk, previous to its calcination. If we conceive the whole heat in the chalk divided into 25 equal parts, the heat contained in the quicklime, after the separation of the fixed air, will be to the original heat of the chalk, as 16 to 25. For if the quicklime were equal in quantity to the chalk, its heat would be to that of the chalk as 24 to 25. But as it is only equal to two thirds of the chalk, it will be as 16 to 25. And since the heat of the quicklime and fixed air taken together, is equal to the original heat of the chalk, the heat of the fixed air will be equal to the difference between the heat of the chalk and quicklime, or it will be represented by the difference between 16 and 25. That is, the heat contained in the fixed air, after the separation, will be to the original heat of the chalk, as 9 to 25, the quantity of matter in the fixed air being one third of that in the chalk. And therefore taking equal quantities of chalk and fixed air, the heat of the fixed air will be to that of the chalk, as 9 multiplied by three, or as 27 to 25, or as  $1\frac{2}{25}$  to one.

It has been already shown that the heat of chalk is to that of water, as 20 to 78, or as one to 3.9. But the heat of fixed air is to that of chalk as  $1\frac{2}{25}$  to one; therefore the heat of fixed air is to that of water, nearly as 1 to 3.6.

From



From these principles, we may determine the comparative heat of fixed and atmospherical air. The absolute heat of atmospherical air is to that of water, as 18.6 to one. The absolute heat of water is to that of fixed air as 3.6 to 1. The heat of atmospherical air is therefore to that of fixed air, as 18.6 multiplied by 3.6 to 1; or very nearly as 67 to 1.

### EXPERIMENT X.

Air in the room — — 52.

Fifteen ounces of water were taken at 51;

A quantity of dephlogisticated air, equal in bulk to 10 ounces of water, was raised to 101;

The bladder containing the air being immersed in the water, and the ball of the thermometer being kept in contact with the bladder for the first two minutes, the temperature at the end of

1 minute was 57,

2 — 55.

The thermometer being then removed from the bladder, the water at the end of

3 minutes was 54,

4 — 54,

5 — 54,

6 — 54.

And this was found to be the heat of the water, at the centre, as well as at the surface. The bladder in which this air was contained, communicated to the water, in the same circumstances, the one fourth of a degree, as nearly as could be judged by the eye.

Fifteen ounces of water, being heated in the same vessel, 2 degrees above the temperature of the atmosphere, cooled, in 20 minutes, 1 degree, or 1 fourth of a degree in 5 minutes. If, therefore, we allow the heat imparted by the bladder, for that



which was carried off by the atmosphere in the first 5 minutes, we have 3 degrees for the heat communicated to the water, by the dephlogisticated air.

The specific gravity of dephlogisticated air, was found, by Dr. Priestley, to be to that of atmospherical air as 187 to 185. Its specific gravity is consequently to that of water, nearly as 1 to 852. But the bulk of the water, in the above experiment, was one third greater than that of the air. Since, therefore, if equal bulks had been taken, the water would have been to the air as 852 to 1; it follows, that as the water was one third greater in bulk than the air, the quantities of matter were as 1278 to 1. The heat received by the vessel was equal to that which would have been received by one ounce of water. The water and vessel together were therefore equal to 16 ounces of water; and the quantity of matter in 16 ounces of water being to that contained in 10 ounce measures of dephlogisticated air, as 1363 to 1, it follows, that the absolute heat of dephlogisticated air, is to that of water in the compound ratio of 1363 to 1, and of 3 to 47, or, as 87 to 1.

To obtain this air, a quantity of red lead was moistened with yellow spirit of nitre, and the salt being dried and put into a glass vessel, the air was separated by an intense heat, and caught in bladders. It appeared to be of a very pure kind, as a candle burned in it with a crackling noise, and with a bright and vivid flame.

From this experiment, compared with experiment the 1st, it appears, that the absolute heat of dephlogisticated air, is to that of atmospherical air, as 87 to 18.6, or nearly as 4.6 to 1. And Dr. Priestley, whose discoveries on this subject are deservedly much admired, has proved that its power in supporting



ing animal life, is 5 times as great as that of atmospheric air.

We have, therefore, upon the whole, sufficient evidence for concluding, that atmospheric air contains a greater quantity of absolute heat, than the air which is expired from the lungs of animals; and that the quantity of absolute heat contained in any kind of air that is fit for respiration, is very nearly in proportion to its purity, or to its power in supporting animal life.

## PROPOSITION II.

THE blood which passes from the lungs to the heart, by the pulmonary vein, contains more absolute heat, than that which passes from the heart to the lungs, by the pulmonary artery.

As the former is the blood which is returned by the veins in the aortic system, and the latter is that, which, in the same system, is propelled into the arteries, I shall call the first venous, and the last arterial blood.

The following experiments were made to determine the heat of venous and arterial blood.

## EXPERIMENT I.

Air in the room	—	—	68
Half a pound of water, averdupoise weight,			
at	—	—	53,
was mixed with half a pound and 400 grains			
of arterial blood at	—		102.
The mixture at the end of			
1 minute was	78,		
2	—	77 $\frac{1}{2}$	nearly,
3	—	77 $\frac{1}{2}$	when it coagulated,
4	—	77 $\frac{1}{2}$	

EXPE-



## E X P E R I M E N T II.

Half a pound of water, averdupoise weight,  
at — — — 53½

was mixed with nine ounces and a half, and  
14 grains of venous blood, at 99⅓

The mixture at the end of

1 minute was 76,

3½ — 76 when it coagulated,

8 — 76,

9 — 75½.

In making these experiments, it was necessary to use as much expedition as possible, that the heat of the mixture might be determined previous to the coagulation; and, therefore, the water was first accurately weighed.—Half a pint of blood was taken from the carotid artery of a sheep, for the first experiment, and from the jugular vein for the second: the heat of the mixture was then ascertained by the thermometer, and the weight of the blood was determined at the conclusion of the experiment.

We learn from these experiments, that the specific gravity of venous blood is greater than that of the arterial. For the measures were nearly equal, and the weight of the former was found considerably to exceed that of the latter. The arterial blood appeared also to be much more fluid than the venous; and we have seen, that when they were mixed with equal quantities of water, the venous blood was somewhat later in coagulating, than the arterial.

To determine the heat of arterial blood, from the former of the above experiments, we may observe, that as, in this experiment, the blood was poured upon the water, a small portion of heat was lost in its passage through the air. I have found by a subsequent trial, that the quantity of heat which was thus lost, was very nearly one degree. If this heat had been added to the mixture, it would have raised it



it nearly half a degree; and as the mixture, previous to its coagulation, cooled at the rate of one fourth of a degree in a minute, we may add, at least, half a degree for the heat lost in the first minute; which gives  $78\frac{1}{2}$  for the temperature of the mixture.

This experiment was made in a pint pewter vessel, the capacity of which, for receiving heat, was to that of the water, nearly as 16 to 1. The quantity of water was eight ounces; the heat received by the vessel, was consequently equal to that which would have been received by the one-sixteenth part of eight ounces, or by half an ounce of water. It follows, that the effect of the vessel and the water together, was equal to that which would have been produced by eight ounces and a half of water.

The temperature of the mixture was  $78\frac{1}{2}$ : subtracting this from 102, we have  $23\frac{1}{2}$  for the heat separated from the blood. The water and the vessel were raised from 53 to  $78\frac{1}{2}$ , or  $25\frac{1}{2}$ . The quantity of blood was eight ounces and 400 grains averdupoise, or 3899 grains. The water and the vessel together were equal to eight ounces and a half of water, or to 3717 grains. And, therefore, the heat of arterial blood is to that of water, in the compound ratio of eight ounces and a half to eight ounces and 400 grains, and of twenty-five and a half to twenty-three and a half, or as 103 to 100; consequently the heat of water is to that of arterial blood, as 100 to 103, or nearly as 97.08 to 100.

In the second experiment, adding half a degree for the heat lost in the first minute, we have  $76\frac{1}{2}$  for the temperature of the mixture.

The blood was cooled from  $99\frac{1}{2}$  to  $76\frac{1}{2}$ , or nearly 22.83. The water and vessel were raised from  $53\frac{1}{2}$  to  $76\frac{1}{2}$ , or 23 degrees. The quantity of venous blood was  $9\frac{1}{2}$  ounces and 14 grains averdupoise, or 4168 grains. The water and vessel were together  
equal



equal to  $8\frac{1}{2}$  ounces of water. Therefore the heat of venous blood is to that of water, in the compound ratio of  $8\frac{1}{2}$  ounces to  $9\frac{1}{2}$  ounces and 14 grains, and of 22.83 to 23, or as 100 to 112.

Putting A for arterial blood, V for venous, and W for water, the ratio of the heat of venous to that of arterial blood, is determined in the following manner :

V.	W.	A.
97.08	100	112.

Therefore V: A:: 97.08: 112, or nearly as 10 to 11 $\frac{1}{2}$ . Thus it appears, that the blood which passes from the heart to the lungs, by the pulmonary artery, contains less absolute heat than that which passes from the lungs to the heart by the pulmonary vein.

### P R O P O S I T I O N III.

THE capacities of bodies for containing heat, are diminished by the addition of phlogiston, and increased by the separation of this principle.

As bodies, when inflamed, appear to emit light, and give out heat, from an internal source, and as those bodies only are combustible, which contain the phlogiston in a considerable quantity, it has been an opinion generally received among philosophers, that this principle is either fire itself, or intimately connected with the production of fire. If this were true, bodies, when united with phlogiston, would contain a greater quantity of fire, or of absolute heat, than when separated from it: metals would contain more absolute heat than their calces; and sulphur more than the vitriolic acid. But that the contrary is the fact, as stated in the above proposition, appears from the following experiments.

E X P E-



## E X P E R I M E N T I.

Air in the room	—	64.
Half a pound of water at	—	62,
was mixed with half a pound of tin at	162 ;	
The mixture at the end of		
	surface	bottom
1 minute was	68 —	68,
2 —	68 —	68.

## E X P E R I M E N T II.

Half a pound of water at	—	62,
was mixed with half a pound of the grey calx		
of tin at	— —	162 ;

The mixture at the end of

	surface		bottom		medium
1 minute was	68	—	72	—	70,
2 —	68 $\frac{1}{4}$	—	70	—	69 $\frac{3}{8}$ ,
3 —	68 $\frac{1}{2}$	—	69 $\frac{1}{2}$	—	69,
4 —	69	—	69	—	69.

These experiments were made in a pewter vessel, the capacity of which, for receiving heat, was determined thus :

Half a pound of water at	—	160,
was poured into the vessel at	—	60 ;

The temperature of the water at the end of

1 minute was	—	150,
2 —	—	146,
3 —	—	142.

Adding four degrees for the heat carried off by the air in the first minute, we have 154 for the common temperature of the water and the vessel. The absolute heat of the vessel, therefore, is to that of the water, as 6 to 94, or as 1 to 15 $\frac{2}{3}$ . That is, the heat contained in the vessel, was equal to the heat contained in the  $\frac{1}{15\frac{2}{3}}$  part of eight ounces of water. Or nearly equal to the heat contained in half an ounce of water.



In the first experiment, the tin was cooled 94 degrees, and the water heated 6. Since, therefore, the tin heated eight ounces of water, and the vessel which was equal to half an ounce of water, six degrees, it follows, that it would have heated eight ounces and a half of water six degrees. And hence the absolute heat of tin is to that of water, in the compound ratio of 6 to 94, and of 8.5 to 8, or as 1 to 14.7.

In this calculation, no allowance has been made for the heat which was lost in the first minute. For, though a portion of heat must have been separated from the tin by the air, when it was mixed with the water, yet the quantity was so very small, that it may be neglected. If one degree of heat were thus separated, it would not have raised the mixture more than one-fourteenth part of a degree. And it appears, that, after the tin and water were brought to a common temperature, the mixture cooled so very slowly, that the heat which was lost, could not be measured by the thermometer. If, however, we were to add one-fourth of a degree for the heat imparted to the air in the first minute, the absolute heat of tin would be to that of water, nearly as 1 to 14.1.

In the second experiment, the mean temperature of the mixture, at the end of one minute, was 70. It cooled nearly at the rate of one-fourth of a degree in a minute. Adding therefore one-fourth of a degree for the heat lost in the first minute, we have  $8\frac{1}{4}$  for the heat communicated to the water and the vessel, and  $91\frac{1}{2}$  for that separated from the calx. But the quantity of the calx was eight ounces. The water and the vessel were together equal to eight ounces and a half of water; and, therefore, the heat of the calx, is to that of water, in the compound ratio of 8.25 to 91.75, and of 8.5 to 8, or  
as



as 1 to 10.4. Hence the absolute heat of the calx of tin is to that of tin, as 14.7 to 10.4.

### EXPERIMENT III.

A quarter of a pound of water at — 55,  
was mixed with a quarter of a pound of calx of  
iron at — — — 155 :

The mixture at the end of

	surface		bottom		medium
1 minute was	75	—	77	—	76,
2 —	72	—	76	—	74,
3 —	71	—	74	—	73½
4 —	71½	—	72½	—	72,
5 —	71½	—	71½	—	71½.

During five minutes, the mixture cooled nearly at the rate of a degree in a minute.

Adding one degree for the heat lost in the first minute, we have 77 for the mean temperature of the mixture. The water, therefore, was heated 22 degrees, and the calx cooled 78.

The quantity of the calx was four ounces. The water and the vessel together were equal to four ounces and a half of water. And, hence, the heat of the calx is to that of water, in the compound ratio of 22 to 78, and of 4.5 to 4; or, nearly as 1 to 3.1.

The heat of iron was found, by Dr. Black, and Dr. Irvine, to be to that of water, as one to eight. I have since repeated this experiment with nearly the same result\*.

It appears, therefore, that the absolute heat of the calx of iron is to that of the metal, as 8 to 3.1.

\* I must farther observe, that, before I made the experiments which are recited in this section, the heat of lead and tin, in their metallic state, had also been determined by the above mentioned philosophers.



## E X P E R I M E N T IV.

Air in the room — — 61,  
 Half a pound of water — 58,  
 was mixed with half a pound of lead 158;  
 The mixture at the end of

	surface		bottom
1 minute was	62 $\frac{1}{2}$	—	62 $\frac{1}{2}$ ,
2 —	62 $\frac{1}{2}$	—	62 $\frac{1}{2}$ .

## E X P E R I M E N T V.

Air in the room — — 61,  
 One half pound of water — 60,  
 was mixed with half a pound of red lead 160;  
 The mixture at the end of

	surface		bottom		medium
1 minute, was	64 $\frac{1}{2}$	—	67	—	65 $\frac{3}{4}$ ,
2 —	64 $\frac{1}{2}$	—	66 $\frac{1}{2}$	—	65 $\frac{1}{2}$ ,
3 —	65	—	66	—	65 $\frac{1}{2}$ ,
4 —	65	—	65 $\frac{1}{4}$	—	65 $\frac{1}{8}$ ,
5 —	64 $\frac{1}{2}$	—	64 $\frac{1}{2}$	—	64 $\frac{1}{2}$ .

The heat received by the vessel, in the fourth experiment, was equal to that which would have been received by one half ounce of water. The quantity of water was eight ounces. The water and the vessel together were equal to eight and a half ounces of water: and, therefore, the quantity of lead being eight ounces, the absolute heat of lead is to that of water, in the compound ratio of 4.5 to 95.5, and of 8.5 to 8, or nearly as 1 to 19.9.

In experiment fifth, the mean temperature of the mixture, at the end of one minute, was 65 $\frac{3}{4}$ . During five minutes, it cooled nearly at the rate of one fourth of a degree in a minute. Adding, therefore, one fourth of a degree for the heat lost in the first minute, we have 66 for the temperature of the mixture.

The



The calx was cooled 94; the water and the vessel were heated 6; and hence the absolute heat of the calx is to that of water, in the compound ratio of 6 to 94, and of 8.5 to 8, or as 1 to 14.7.

It follows, that the absolute heat of the calx is to that of lead, as 19.9, to 14.7.

### EXPERIMENT VI.

Air in the room — — 58.

One fourth of a pound of water at 59, was mixed with one fourth of a pound of a compound, consisting of equal parts of the calces of lead and tin, at 159.

The mixture at the end of

	surface		bottom		medium
1 minute was	65	—	69	—	67,
2 —	63	—	67	—	65½,
3 —	64	—	66	—	65,
4 —	66	—	65½	—	65½,
5 —	65	—	65¼	—	65⅞,
6 —	65	—	65	—	65.

The mean temperature of the mixture, at the end of one minute, was 67. It cooled two degrees in six minutes; adding therefore, one third of a degree for the heat lost in the first minute, we have  $67\frac{1}{3}$ , or 67.3, nearly for the true temperature of the mixture.

The calx was cooled 91.7. The water heated 8.3. The absolute heat of the calx therefore is to that of water, in the compound ratio, of 8.3 to 91.7, and of 4.5 to 4, or as 1 to 9.8.

It is to be observed that these metals calcine more perfectly when mixed, than when separate.

### EXPERIMENT VII.

Air — — — 64,  
 Half a pound of water at — 63,  
 was mixed with half a pound of the regulus  
 of antimony at 163,  
 The



The mixture at the end of

1	minute was at the surface and bottom	70,
2	— — — —	68½,
3	— — — —	68.

### EXPERIMENT VIII.

Air	— — —	61,
One fourth of a pound of water at		58½,
was mixed with one fourth of a pound of		
calx of antimony at		158½,

The mixture at the end of

	surface		bottom		medium
1 minute was	72½	—	77	—	74
2 —	71½	—	74½	—	73
3 —	71	—	72	—	71½
4 —	70¾	—	71	—	70⅞

In Experiment VII. the mixture cooled at the rate of half a degree in a minute. The water and vessel were heated 7.5. The regulus was cooled 92.5. The heat of the regulus, therefore, is to that of water in the compound ratio, of 7.5 to 92.5, and of 8.5 to 8, or nearly as 1 to 11.6. In Experiment VIII. the mean temperature of the mixture was 74¾. It cooled in 4 minutes, nearly at the rate of one degree in a minute. Adding therefore one degree for the heat lost in the first minute, we have 75¾ for the true temperature; consequently the absolute heat of the calx of antimony, is to that of water in the compound ratio of 17¾ to 82¾, and of 8½ to 8, or nearly as 1 to 4.5, and hence the absolute heat of the calx of antimony, is to that of the regulus, as 11.6 to 4.5. By similar experiments, it may be demonstrated, that the vitriolic acid contains more absolute heat than sulphur. We may therefore conclude, in general, that bodies, when joined to phlogiston, contain less absolute heat than when separated from it; and consequently, that, in the former case their capacities for contain-



containing heat are diminished, and in the latter, increased.

It follows, that if phlogiston be added to a body, a quantity of the absolute heat of that body will be extricated; and if the phlogiston be separated again, an equal quantity of heat will be absorbed.

The calx of antimony, for example, contains nearly three times as much absolute heat as the regulus: when, therefore, by the addition of phlogiston, the calx is revived, it will lose two thirds of its absolute heat, and, on the contrary, when the regulus is by calcination deprived of its phlogiston, the calx will recover the heat which it had formerly lost.

In this point of view, the separation of heat from a body, by means of phlogiston, and the reabsorption of it, when the phlogiston is again disengaged, seems to be analogous to the separation of air from earths and alkali's, by means of an acid, and the reunion of these substances with this element, when the acid is separated. If the vitriolic acid, for instance, be added to a mild alkali, the fixed air will be extricated, and will fly off in the form of an elastic vapour; if phlogiston be added to a metallic earth, a portion of the absolute heat will be separated, and will fly off in the form of sensible heat; when the acid is again separated from the alkali, the latter recovers the air which it had lost, and when the phlogiston is again disengaged from the metallic earth, the earth reabsorbs the heat which had formerly escaped from it.

Heat, therefore, and phlogiston, appear to be two opposite principles in nature. By the action of heat upon bodies, the force of their attraction to phlogiston is diminished; and by the action of phlogiston, a part of the absolute heat, which exists in all bodies as an elementary principle, is expelled.



## S E C T. III.

**F**ROM the facts which have been established by the above experiments, the following explanation may be given of animal heat, and of the heat which is produced by the inflammation of combustible bodies.

I. *Of Animal Heat.*

It has been proved, that the air which is expired from the lungs of animals, contains less absolute heat than that which is inhaled in inspiration. It has been shown, particularly, that, in the process of respiration, atmospherical air is converted into fixed air; and that the absolute heat of the former is to that of the latter, as 67 to 1.

Since, therefore, the fixed air which is exhaled by expiration, is found to contain only the one sixty-seventh part of the heat which was contained in the atmospherical air, previous to inspiration, it follows, that the latter must necessarily deposit a very great proportion of its absolute heat in the lungs. It has moreover been shown, that the absolute heat of florid arterial blood, is to that of venous, as  $11\frac{1}{2}$  to 10. And hence, as the blood which is returned by the pulmonary vein to the heart, has the quantity of its absolute heat increased, it is evident that it must have acquired this heat in its passage thro' the lungs. We may conclude, therefore, that, in the process of respiration, a quantity of absolute heat is separated from the air and absorbed by the blood.

That heat is separated from the air in respiration, is farther confirmed by Experiment X. Prop. I. from which experiment, compared with Dr. Priestley's discoveries, it is manifest, that the power of any species of air in supporting animal life, is nearly  
in



in proportion to the quantity of absolute heat which it contains, and is consequently proportionable to the quantity which it is capable of depositing in the lungs.

The truth of this conclusion, will perhaps appear in a clearer light, from the following calculation, by which we may form some idea of the quantity of heat yielded by atmospherical air, when it is converted into fixed air, and also of that which is absorbed, during the conversion of venous into arterial blood.

We have seen that the same heat, which raises atmospherical air one degree, will raise fixed air nearly 67 degrees. And, consequently, that the same heat, which raises atmospherical air any given number of degrees, will raise fixed air the same number of degrees, multiplied by 67. In the Petersburg experiment, the heat was diminished 200 degrees below the common temperature of the atmosphere. We are, therefore, certain that atmospherical air, when at the common temperature of the atmosphere, contains at least 200 degrees of heat. Hence, if a certain quantity of atmospherical air, not in contact with any body that would immediately carry off the heat, should suddenly be converted into fixed air, the heat which was contained in the former, would raise the latter 200 degrees multiplied by 67, or 13400 degrees. And the heat of red hot iron being 1050, it follows, that the quantity of heat, which is yielded by atmospherical air, when it is converted into fixed air, is such, (if it were not dissipated) as would raise the air so changed to more than 12 times the heat of red hot iron.

If, therefore, the absolute heat which is disengaged from the air in respiration, were not absorbed by

H

the



the blood, a very great degree of sensible heat would be produced in the lungs.

Again, it has been proved, that the same heat which raises venous blood 115 degrees, will raise arterial only 100 degrees; and, consequently, that the same heat, which raises venous blood any given number of degrees, will raise arterial, a less number, in the proportion of 100 to 115, or 20 to 23. But we know that venous blood contains at least 230 degrees of heat. Hence, if a certain quantity of venous blood, not in contact with any body that would immediately supply it with heat, should suddenly be converted into arterial, the heat which was contained in the former would raise the latter only  $\frac{20}{23}$  of 230 degrees, or 200 degrees; and consequently the sensible heat would suffer a diminution, equal to the difference between 230 and 200, or 30 degrees. But the common temperature of blood is 96; when, therefore, venous blood is converted into arterial in the lungs, if it were not supplied by the air, with a quantity of heat proportionable to the change which it undergoes, its sensible heat would be diminished 30 degrees, or it would fall from 96 to 66.

That a quantity of heat is detached from the air and communicated to the blood in respiration, is moreover supported by the experiments which have been brought in proof of the third Proposition; from which it appears, that, when bodies are joined to phlogiston, they lose a portion of their absolute heat, and that when the phlogiston is again disengaged, they reabsorb an equal portion of heat, from the surrounding bodies.

Now it has been demonstrated, by Dr. Priestley, that, in respiration, phlogiston is separated from the blood and combined with the air. During this process, therefore, a quantity of absolute heat must  
neces-



necessarily be disengaged from the air, by the action of the phlogiston; the blood, at the same moment, being left at liberty to unite with that portion of heat, which the air had deposited,

And hence animal heat seems to depend upon a process, similar to a chemical elective attraction. The air is received into the lungs, containing a great quantity of absolute heat. The blood is returned from the extremities, highly impregnated with phlogiston. The attraction of the air to phlogiston, is greater than that of the blood. This principle will, therefore, leave the blood to combine with the air. By the addition of the phlogiston, the air is obliged to deposit a part of its absolute heat; and as the capacity of the blood is at the same moment increased by the separation of the phlogiston, it will instantly unite with that portion of heat which had been detached from the air.

We learn from Dr. Priestley's experiments, with respect to respiration, that arterial blood has a strong attraction to phlogiston: It will consequently, during the circulation, imbibe this principle from those parts which retain it with least force, or from the putrescent parts of the system: And hence the venous blood, when it returns to the lungs, is found to be highly impregnated with phlogiston. By this impregnation, its capacity for containing heat is diminished. In proportion, therefore, as the blood which had been dephlogisticated by the process of respiration, becomes again combined with phlogiston, in the course of the circulation, it will gradually give out that heat which it had received in the lungs, and diffuse it over the whole system.

Thus it appears, that, in respiration, the blood is continually discharging phlogiston and absorbing heat; and that in the course of the circulation, it



is continually imbibing phlogiston and emitting heat.

It may be proper to add, that, as the blood by its impregnation with phlogiston, has its capacity for containing heat diminished; so, on the contrary, those parts of the system from which it receives this principle, will have their capacity for containing heat increased, and will consequently absorb heat.

Now, if the changes in the capacities, and the quantities of matter changed in a given time were such, that the whole of the absolute heat separated from the blood were absorbed, it is manifest, that no part of the heat, which is received in the lungs, would become sensible in the course of the circulation.

That this, however, is not the case, will, I think, be evident, from the following considerations.

We know that sensible heat is produced by the circulation of the blood; and we have proved by experiment, that a quantity of absolute heat is communicated to that fluid in the lungs, and is again disengaged from it, in its progress thro' the system. If, therefore, the whole of the absolute heat, which is separated from the blood, were absorbed by those parts of the system from which it receives the phlogiston, it would be necessary to have recourse to some other cause, to account for the sensible heat which is produced in the circulation. But, by the rules of philosophizing, we are to admit no more causes of natural things, than such as are both true, and sufficient to explain the appearances; for nature delights in simplicity, and affects not the pomp of superfluous causes\*.

We may, therefore, safely conclude, that the absolute heat which is separated from the air in respiration,

\* See Newton's Principia b. iii. p. 202.



tion, and absorbed by the blood, is the true cause of animal heat.

It must nevertheless be granted, that those parts of the system which communicate phlogiston to the blood, will have their capacity for containing heat increased; and therefore, that a part of the absolute heat which is separated from the blood will be absorbed.

But from the quantity of heat, which becomes sensible in the course of the circulation, it is manifest that the portion of heat which is thus absorbed, is very inconsiderable.

It appears, therefore, that the blood, in its progress thro' the system, gives out the heat which it had received from the air in the lungs; a small portion of this heat is absorbed by those particles which impart the phlogiston to the blood; the rest becomes redundant, or is converted into moving and sensible heat.

I shall hereafter show, that the heat which is produced by this process, is similar to that which is produced by the inflammation of combustible bodies, with this difference, that, in the latter instance, the fire is separated from the air, in the former, from the blood.

#### *Of the Inflammation of combustible Bodies.*

From the above experiments we learn, that atmospherical air contains much absolute heat; that when it is converted into fixed and phlogisticated air, the greater part of this heat is detached; and that the capacities of bodies for containing heat, are diminished by the addition of phlogiston, and increased by the separation of it. From hence we infer, that the heat which is produced by combustion, is derived from the air, and not from the inflammable body.

For



For inflammable bodies abound with phlogiston, and contain little absolute heat; atmospherical air, on the contrary, abounds with absolute heat, and contains little phlogiston. In the process of inflammation, the phlogiston is separated from the inflammable body, and combined with the air; the air is converted into fixed and phlogisticated air, and at the same time gives off a very great proportion of its absolute heat, which, when extricated suddenly, bursts forth into flame, and produces an intense degree of sensible heat. We have found by calculation, that the heat which is produced by the conversion of atmospherical into fixed air, is such, if it were not dissipated, as would be sufficient to raise the air so changed, to more than twelve times the heat of red hot iron. It appears, therefore, that in the process of inflammation, a very great quantity of heat is derived from the air.

It is manifest on the contrary, that no part of the heat, can be derived from the combustible body. For the combustible body during the inflammation, being deprived of its phlogiston, undergoes a change similar to that which is produced in the blood, by the process of respiration; in consequence of which, its capacity for containing heat is increased. It, therefore, will not give off any part of its absolute heat, but, like the blood in its passage thro' the lungs, it will absorb heat.

The calx of iron, for example, is found to contain more than twice as much absolute heat, as the iron in its metallic form; from which it follows, that, in the process of inflammation, the former must necessarily absorb a quantity of heat, equal to the excess of its heat above that of the latter. Now, from whence does it receive this heat? It cannot receive it from the iron. For the quantity of heat in  
the



the calx, is more than double of that which was contained in the iron, previous to the calcination.

But in the burning of iron, the phlogiston is separated from the metal, and combined with the air; and it has been proved, that, by the combination of phlogiston with air, a very intense heat is produced. From hence it is manifest, that, in the inflammation of iron, the atmospherical air is decomposed, a very great proportion of its absolute heat is separated, part of which is absorbed by the calx, and the rest appears in the form of flame, or becomes moving and sensible heat.

We may conclude, therefore, that the sensible heat which is excited in combustion, depends upon the separation of absolute heat from the air by the action of phlogiston.

In confirmation of this conclusion, it may be proper to add, that, (if we except the change that the air undergoes in the process of respiration, in which the heat is absorbed) the sudden conversion of atmospherical, into fixed and phlogisticated air, is invariably accompanied with the production of sensible heat. Thus sensible heat is produced when common air is mixed with nitrous air, when it is exploded with inflammable air, when it is diminished and rendered noxious, by putrefaction, by combustion, and by the electric spark. If the quantity of air which is changed, by these processes, in a given time, be very great, the change is attended with much light, with a vivid flame, and with intense heat; but if the alteration in the air be slow and gradual, the heat passes off imperceptibly to the surrounding bodies.

It appears, upon the whole, that atmospherical air contains, in its composition, a great quantity of fire or of absolute heat. By the separation of a portion of this fire in the lungs, it supports the temperature



perature of the arterial blood, and this communicates that *pabulum vitæ*, which is so essential to the preservation of the animal kingdom. And, finally, by a similar process, it maintains those natural and artificial fires, which are excited by the inflammation of combustible bodies.

Assuming this doctrine as true, I shall next endeavour to shew, that it affords an easy solution of the most remarkable facts, relating to animal heat and combustion.

## S E C T. IV.

### *Of the principal Facts relating to Animal Heat.*

**T**H E above doctrine explains the reason why the breathing animals have a higher temperature than those which are not furnished with respiratory organs; for it has been proved that the former are continually absorbing heat from the air: And, it is probable, that, to provide an apparatus for the absorption of heat, was the chief purpose of nature, in giving to so great a part of the animal creation, a pulmonary system, and a double circulation.

We have shown that animal heat, and the inflammation of combustible bodies, depend upon the same cause, that is, upon the separation of absolute heat from the air, by the action of phlogiston.

The quantity of air phlogistified by a man in a minute, is found, by experiment, to be equal to that which is phlogistified by a candle, in the same space of time. And hence a man is continually deriving as much heat from the air, as is produced by the burning of a candle.

It is remarked by naturalists, that the cold animals have also the power of keeping themselves at a temperature,



perature, somewhat higher than the surrounding medium. To account for this, we may observe, that animal heat depends, indirectly, upon a change produced in the air by respiration, and directly upon a change which the blood undergoes in the course of the circulation.

In consequence of the tendency of the system to putrefaction, the blood is impregnated with phlogiston, and by this impregnation is obliged to give out a part of its absolute heat. The source from which it is again supplied with heat, in such of the cold animals as are not furnished with lungs, can only be determined by experiment. It is probable, that, in those animals, the aliment contains more absolute heat than the blood. If this be the case, the blood will be supplied with heat from the aliment.

2. From the experiments in heated rooms, it appears, that the animal body has, in certain situations, the power of producing cold, or of keeping itself at a lower temperature than the surrounding medium.

This power has been attributed by some philosophers to the evaporation from the surface of the body; and indeed it must be allowed, that the increased evaporation, will have a very considerable influence in diminishing the heat. But the experiments, which have been related above, point out another cause, which, I apprehend, conspires in producing the same effect.

By the heat of the surrounding medium, the evaporation from the lungs is increased. Now it may be shown, that if the evaporation from the lungs be increased to a certain degree, the whole heat which is separated from the air, will be absorbed by the aqueous vapour.

I From



From the calculation in Sect. III. page 58, it appears, that the capacity of the blood for containing heat, is so much increased in the lungs, that if its temperature were not supported by the heat which is separated from the air, in the process of respiration, it would fall from 96 to 66. Hence if the evaporation from the lungs be increased to such a degree, as to carry off the whole of the heat that is detached from the air, the arterial blood, when it returns by the pulmonary vein, will have its sensible heat greatly diminished, and will, consequently, absorb heat from the vessels which are in contact with it, and from the parts adjacent. And thus the very same process which formerly supplied the animal with heat, will now become the instrument of producing cold, and the quantity of cold produced, will be in proportion to the velocity of the blood through the lungs, and the fulness and frequency of the respiration.

Hence also we may perceive the reason, why the heat of animals is nearly the same, in all parts of the earth, notwithstanding the very great variations in the heat of the atmosphere, arising from the vicissitudes of the weather, and the difference of season and climate.

The quantities of heat lost by bodies, when heated and placed in the cold air, are in proportion to the excess of their sensible heat, above that of the surrounding atmosphere. The heat of the human body is very nearly, at all seasons of the year, 96; and, consequently, other circumstances continuing the same, the quantity of heat lost in a given time, when the air is at 36, will greatly exceed that which is lost in an equal portion of time, when it is at 66. It is therefore, necessary, that, in the former case, a greater quantity of heat should be absorbed from the air to supply the waste.

To



To account for this, we may observe, that, by the tonic and stimulant powers of cold, the vigour of the animal body is increased. The vessels on the surface are constricted. The blood is determined to the lungs. The pulse and the respiration are rendered full and frequent. And hence, as the cold advances in winter seasons, and in northern climates, the quantity of heat absorbed from the air is proportionably increased.

In summer, the blood is determined to the surface, the velocity of the circulation through the lungs, is diminished, and hence, a proportionable diminution in the quantity of heat absorbed. We may add to this, that, if the quantity of heat absorbed by the vapour, which is exhaled in respiration, be so great, as that the remaining portion of the heat deposited by the air, is not sufficient to support the temperature of the arterial blood, some degree of cold will be produced in consequence of the change which the blood undergoes in the lungs.

As animals are continually absorbing heat from the air, if there were not a quantity of heat carried off, equal to that which is absorbed, there would be an accumulation of it in the animal body. The evaporation from the surface, and the cooling power of the air, are the great causes which prevent this accumulation. And these are alternately increased and diminished, in such a manner, as to produce an equal effect. When the cooling power of the air is diminished by the summer heats, the evaporation from the surface is increased; and when, on the contrary, the cooling power of the air is increased by the winter colds, the evaporation from the surface is proportionably diminished. The influence of this cause in preserving the equality of



heat in animals, was first suggested to me by my ingenious friend, Mr. Cleghorn.

3. Among different animals, those are the hottest, which breathe the greatest quantity of air in proportion to their bulk; and in the same animal, the degree of heat is in some measure proportionable to the quantity of air inhaled in a given time.

These varieties appear to be the necessary consequence of the general fact, that the heat of the breathing animals is derived from the air. For if animal heat depends upon a change which the air undergoes in the lungs, it is evident, that, all other circumstances being equal, the greater the quantity of air which is changed in a given time, the greater must be the heat produced.

In exercise, by the action of the muscles, the venous blood is returned in greater quantities than usual, from the extremities, to the right auricle of the heart. By the action of the heart it is determined to the lungs. The respiration is accelerated; the velocity of the circulation is increased; and hence, a proportionable increase in the quantity of phlogiston discharged, and the quantity of heat absorbed.

The cold stage of fevers is preceded by languor, a sense of debility, and a diminution in the action of the heart and arteries. The respiration is small, the pulse is weaker than natural—the quantity of blood which passes thro' the lungs, in a given time, is diminished—and hence, less phlogiston will be discharged from the blood, and, consequently, less heat will be separated from the air.

In the progress of the cold stage, a spasm is formed upon the surface\*. By the constriction of the vessels on the surface, the blood is determined to the heart. The heart is stimulated to more frequent  
and

\* See Cullen's first Lines of the Practice of Physick.



and violent contractions. The velocity of the blood through the lungs is increased—the respiration is accelerated; and hence a greater quantity of heat will be absorbed.

We may observe, that the absorption of heat, and the accelerated velocity of the blood through the lungs, will act and react upon each other, in such a manner, as that the heat will have a constant tendency to increase. For the accelerated velocity of the blood occasions a greater absorption of heat; and the increased absorption of heat, by stimulating the heart and arteries to more frequent and powerful contractions, will again accelerate the velocity of the blood, which will still farther increase the absorption. And therefore, the heat will continue to be accumulated, till counteracted by the operation of some other cause. From the sudden diminution in the weight of the body, notwithstanding the quantity of watery fluids that are taken in, and the obstruction of the urinary secretion, it appears, that, in the hot stage of fevers, there is a very great evaporation from the surface; we may, therefore, conclude, that this is one of the means which Nature employs, for moderating the heat, and restraining the violence of the disease.

Another cause which prevents the accumulation of heat, is the cooling power of the external air. We have already observed, that the quantities of heat lost by a body in a given time, are in proportion to the excess of its heat, above that of the surrounding medium.

If, therefore, the sensible heat of the body increase, while the temperature of the air continues the same, the quantities of heat carried off by the latter, in a given time, will be proportionably increased.

In putrid fevers, to the accelerated velocity of the blood through the lungs, there is added a putrescent  
state



state of the system; in consequence of which, the air inhaled in inspiration, will be more copiously supplied with phlogiston, than when the body is in a sound and healthy state. If, in the latter instance, the air which is received into the lungs, were completely saturated with phlogiston, the quantity of heat separated from it, would always be proportionable to the quantity of air inhaled in a given time. But it appears from experiment, that the air which is expired by a healthy animal, is not completely saturated with phlogiston. It is capable of being farther diminished by nitrous air, and not more than the eighth part of it consists of fixed air. The quantity of heat, therefore, which is separated from the air in respiration, will be partly in proportion to the quantity of air inspired, and partly to the quantity of phlogiston discharged from the blood, in a given time.

In fevers of the putrid kind, as the solid and fluid parts of the system are in a putrescent state, and, consequently, retain their phlogiston with less force, a greater quantity of this principle will be discharged from the lungs, the air will be more copiously supplied with it in the process of respiration, and will, therefore, impart to the blood a greater proportion of its absolute heat. To these causes it is probably owing, that the heat of the human body never rises so high as in putrid fevers.

4. Topical inflammation is accompanied with redness, with tumour, and with unusual heat\*. From the throbbing of the vessels, and from microscopical observations, it appears, that the velocity of the blood through the part inflamed, is accelerated; and it is manifest, that a tendency to putrefaction must be produced by the violent reaction, and by the stagnation of the serous matter which is sometimes effused into the adjoining cellular texture. It has been  
already

\* See Cullen's first Lines of the Practice of Physick.



already observed, that the arterial blood has a strong attraction to phlogiston, and that by its union with this principle, in the course of the circulation, it is obliged to give out that heat which it had received in the lungs. In the state of health, the velocity of the blood through the different parts of the system, and the quantities of phlogiston with which it is supplied in those parts, are adjusted to each other in such a manner, that the heat is equally diffused over the whole. But, if by any irregularity, the balance be destroyed; if, by the increased action of the vessels, the blood be urged with greater violence than usual through any particular part, or, in consequence of a greater tendency to putrefaction, be more copiously supplied with phlogiston, it is manifest, that a greater quantity of heat will be extricated in that part, in a given time. This heat will stimulate the vessels into more frequent and forcible contractions, by which the velocity of the blood, and the consequent extrication of heat will be still farther increased. On this principle we may probably account for the partial heats which are produced by topical inflammations, and for those which arise in hectic and nervous diseases.

It will hereafter appear, that the heat is accumulated in topical inflammation, by the increased velocity of the blood through the part inflamed, in the same manner as it is accumulated upon the fuel, in combustion, by directing a stream of fresh air into the fire.

*Of the principal Facts relating to the Inflammation of Combustible Bodies.*

1. We have proved, that the sensible heat in combustion is derived from the air, and depends upon the separation of absolute heat from this element,



ment, by the action of phlogiston. From hence it is evident, that when the air, in which an inflamed body is confined, is saturated with phlogiston, and deprived of the greater part of its absolute heat, the source of inflammation will be exhausted, and the flame will necessarily be extinguished. And this explains the reason, why a constant succession of fresh air is necessary to inflammation, as well as to the support of animal life.

2. It is highly probable, from Dr. Priestley's Experiments, that dephlogisticated air contains less phlogiston, than any other species of air. And this conclusion will, I think, be farther confirmed, if we compare *his* Discoveries with the Experiments which have been related above.

The substances from which dephlogisticated air is obtained, either contain in their natural state very little phlogiston, or they are such as have the greater part of their phlogiston separated, by the force of fire, and by the action of the nitrous acid.

We have seen that phlogiston diminishes the absolute heat of bodies; that dephlogisticated air abounds with absolute heat; that when a certain quantity of phlogiston is added to this species of air, so as to reduce it to the state of common air, its absolute heat is proportionably diminished; and that when a still greater quantity is added, so as to convert it into fixed and phlogisticated air, it is, in a great measure, deprived of its absolute heat. From all which it appears, that dephlogisticated air contains much absolute heat, and little phlogiston. As the sensible heat which is produced by combustion, depends upon the separation of absolute heat from the air, by the action of phlogiston, it is manifest that the less phlogiston, and consequently the more absolute heat any species of air contains, the longer it must contribute to the support of flame, as well as to the preservation of  
animal



animal life. Agreeably to this conclusion, we find, from Experiments I. and X. Prop. i. Sect. ii. that dephlogisticated air contains nearly five times as much absolute heat as common air. And Dr. Priestley has shown, that five times as much sensible heat is produced by the conversion of it into fixed and phlogisticated air, as by that of common air; for a candle will continue to burn five times as long in the former, as in the latter species of air: Add to this, that it burns with a much more bright and vivid flame; and as the quantity of phlogiston is increased, the brightness and vivacity of the flame diminish, till at length the air becomes saturated with this principle, and the flame is extinguished.

3. As the sensible heat, in combustion, depends upon a change produced in the air, by the phlogiston, which is separated from the inflammable body; it is manifest, that (all other circumstances being equal) the intensity of the heat, must be in proportion to the quantity of air which is changed in a given time. And hence the heat may be increased to a very great degree, if a stream of fresh air be directed upon the fuel, by bellows or by the blow pipe.

We have found, that by the conversion of atmospherical into fixed air, a quantity of heat is disengaged, which would be sufficient to raise the air so converted to more than twelve times the heat of red hot iron. And indeed the degree of heat, excited by the inflammation of combustible bodies, would not be less than this, if the fire that is thus extricated, were applied to the fixed air alone, and were to remain in the same concentrated state, in which it is at first separated from the atmospherical air.

But as sensible heat has a constant tendency to an equal diffusion, it will instantly flow from the point inflamed, and spread itself over the surrounding bodies.



dies. It will be accumulated upon the fuel, absorbed by the vapour, and communicated to the atmosphere: And from the principles which have been established above, it is manifest, that the same heat which raises fixed air 13400 degrees, would raise an equal quantity of atmospherical air, only the  $\frac{1}{67}$  part of 13400, or 200 degrees. This explains the reason, why the heat is so intense in the flame of a candle, and is so greatly diminished at the smallest distance from the flame.

4. Tho' it is highly probable that all bodies have their capacities for containing heat changed in consequence of the addition or separation of phlogiston, yet from the experiments which have been recited above, it is manifest, that the degree of this change is very different in different bodies. We have seen that the capacity of the calx of iron is to that of iron, as 3.1 to 1; that the capacity of minium is to that of lead, as 19.9 to 14.7; that the capacity of the calx of antimony is to that of the regulus, as 11.6 to 3.9; and that the capacity of arterial blood is to that of venous, as  $11\frac{1}{2}$  to 10.

It appears, moreover, that different quantities of phlogiston, are required to saturate different bodies. Some of the metals abound more with this principle than others. The quantity of phlogiston which is required to saturate dephlogisticated air, is much greater than that which will saturate common air; and a greater quantity of phlogiston is required to saturate common air, than an equal weight of arterial blood.

From these facts it follows, that when phlogiston passes from one body to another, the changes in the capacities of the bodies for containing heat will be different, and unequal quantities of matter will be changed in a given time. Thus, in respiration, the phlogiston is separated from the blood and combin-  
ed



ed with the air. The diminution which is produced, by this process, in the capacity of the air for containing heat, is greater than the increase in that of the blood; and as more phlogiston is required to saturate atmospherical air, than an equal weight of arterial blood, the quantity of the latter which is changed in a given time, will be greater than that of the former.

When two contiguous bodies, at the same moment, have their capacities for containing heat respectively increased and diminished, if the changes be such, that the whole heat separated from the one is absorbed by the other, no sensible heat or cold will be produced.

I shall proceed to determine the cases in which this will happen, having first pointed out some of the chief circumstances, by which, the sensible heat of bodies, the capacities for containing heat, and the absolute heat contained, may be distinguished from each other.

The capacity for containing heat, and the absolute heat contained, are distinguished as a force from the subject upon which it operates. When we speak of the capacity, we mean a power inherent in the heated body; when we speak of the absolute heat, we mean an unknown principle which is retained in the body, by the operation of this power; and when we speak of the sensible heat, we consider the unknown principle as producing certain effects upon the senses and the thermometer.

The capacity for containing heat may continue unchanged, while the absolute heat is varied without end. If a pound of ice, for example, be supposed to retain its solid form, the quantity of its absolute heat will be altered, by every increase or diminution of its sensible heat: but as long as its form continues the same, its capacity for receiving heat will not be



affected by an alteration of temperature, and would remain unchanged, though the body were wholly deprived of its heat.

The alterations which are produced in the sensible heats of different bodies, by given quantities of absolute heat, are *greater* or *less*, according as the body, to which the heat is applied, has a *less* or *greater* capacity for containing heat. Thus it has been proved, that if a quantity of heat be added to a pound of water, which is sufficient to produce an increase in its temperature as 1, the same quantity of heat being added to a pound of antimony, will produce an increase as 4. The body, therefore, which has the less capacity for containing heat, has its temperature more augmented by the addition of a given quantity of absolute heat, than that which has the greater. Hence the *sensible* heat of a body depends partly upon the quantity of its *absolute heat*, and partly upon the nature of the body in which this heat is contained, and consequently the *sensible heat* may be varied, either by a change in the nature of the body itself, or by a change in the quantity of its *absolute heat*. If the variation of sensible heat arises from the first of these circumstances, it follows, that, in the same body, the *sensible heat* may vary, though the *absolute heat* continues the same\*.

The following proportions obtain, with respect to the capacities, the sensible heats, and the quantities of absolute heat.

The capacities of bodies for receiving heat, are considered as proportionable to the quantities of absolute heat which they contain, when the quantities of matter are equal, and the temperature are the same. Calling, therefore, the sensible heat *S*, the  
capa-

\* See the observations on fixed and atmospherical air, venous and arterial blood, page 57.



capacity  $C$ , and the absolute heat  $A$ , if the quantities of matter, and the sensible heats be given, the capacities will be as the absolute heats; or if  $S$  be given,  $A$  will be as  $C$ . Again, it has been shown, that, in the same body, if the form remain unchanged, or, in other words, if the capacity be given, the quantity of absolute heat will be in proportion to the sensible heat. That is, if  $C$  be given,  $A$  will be as  $S$ . Since, therefore, if  $S$  be given,  $A$  will be as  $C$ , and if  $C$  be given,  $A$  will be as  $S$ , it follows that if neither be given,  $A$  will be as  $S \times C$ . Therefore,  $C$  will be as  $\frac{A}{S}$ , and if  $A$  be given,  $C$  will be reciprocally as  $S$ . Consequently, if the capacities be reciprocally as the sensible heats, the quantities of absolute heat will be equal. Thus, if the capacity of the calx of antimony be to that of the regulus as 3 to 1, and if the sensible heat of the former be to that of the latter 1 to 3, the calx and regulus will contain equal quantities of absolute heat \*.

It appears, therefore, that in heterogeneous bodies, if the sensible heats be different, the *capacities* may vary, though the *absolute heat* be the same. And it is manifest from the foregoing experiments, that, in such bodies, if the capacities be different, the *absolute heats* may vary, though the *sensible heat* be the same.

These observations being premised, the cases in which no sensible heat or cold will be produced, by the passage of phlogiston from one body to another, may be determined in the following manner.

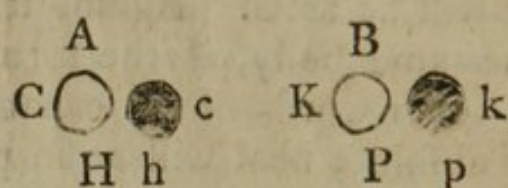
## PROP O.

\* The sensible heat is here supposed to be computed from the point of total privation



## P R O P O S I T I O N I.

LET there be two bodies, A and B, which have their capacities for containing heat changed at the same moment, the capacity of A being diminished, and that of B increased; let the capacity of A before the change, be denoted by C, and after the change, by c; the capacity of B before the change by k, and after the change by K. Then  $C - c$  will be the difference of the capacities of A before and after the change; and  $K - k$  the difference of the capacities of B. It is affirmed, that, if the temperatures and the quantities of matter changed in a given time, be equal, the differences of the capacities will be as the differences of the absolute heats.



For the capacities of bodies for receiving heat, are estimated (as was observed above) by the comparative quantities of absolute heat which they are found to contain, when the quantities of matter are equal, and the temperatures are the same. The temperature and the quantities of matter, therefore, in A and B being equal, the capacities will be directly as the absolute heats. If the absolute heat of A be double that of B, the capacity will be double, if triple, triple, &c. And therefore, calling the absolute heats of A and B before the change, H and p, and after the change h and P, it will be  $C : c :: H : h$ . And by conversion  $C - c : c :: H - h : h$ . For the same reason  $K - k : k :: P - p : p$ . But because the quantities of matter in A and B are equal, and the bodies before and after the change are conceived to be brought to the same temperature, it will be,  $c : k :: h : p$ .

Since



Since therefore  $C - c : c : k : K - k$

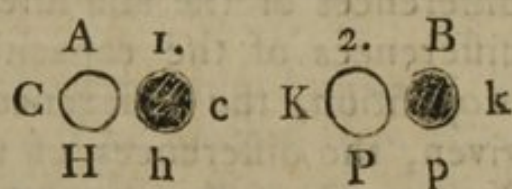
$H - h : h : p : P - p$ , by equality  
 $C - c : K - k : : H - h : P - p$ .

That is, the differences of the capacities are as the differences of the absolute heats, the temperatures and the quantities of matter being equal.

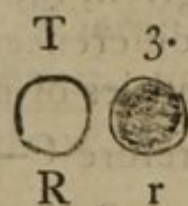
## PROPOSITION II.

IF the differences of the capacities be equal, the differences of the absolute heats will be as the quantities of matter\*.

The same things remaining as above, if the quantities of matter in A and B be equal,  $C - c : K - k : : H - h : P - p$ .



Let the quantity of matter in A be increased, in any proportion, as in Fig. 3, and, after the increase, let it be called T. Let the absolute heats be denoted by R and r, and the quantities of matter in A (or B) and T, by q and Q respectively.



Since the capacities of A and T are equal, the absolute heats before the change, will be as the quantities of matter. Therefore  $H : R : : q : Q$ . For the same reason the absolute heats, after the change, will be as the quantities of matter. That is,  $h : r : : q : Q$ . Therefore,  $H : R : : h : r$ , and  $H - h : R - r : : h : r$ . But  $h : r : : q : Q$ . Therefore  $H - h : R - r : : q : Q$ . But  $H - h$  is equal to  $P - p$ . Therefore  $P - p : R - r : : q : Q$ . And hence the differences of

\* In this and the following proposition, the bodies are supposed to be brought to the same common temperature before and after the change.



of the capacities being equal, the differences of the absolute heats will be as the quantities of matter.

### PROPOSITION III.

IF the differences of the absolute heats be equal, the differences of the capacities will be reciprocally as the quantities of matter.

For, by the first proposition, the  $C \bigcirc c$  quantities of matter being given, the  $H \ h$  differences of the absolute heats are directly as the differences of the capacities; and, by the second proposition, the differences of the capacities being given, the differences of the absolute heats are as the quantities of matter; it follows, that neither being given, the differences of the absolute heats, are as the differences of the capacities, multiplied into the quantities of matter. That is,  $H-h$  is as  $C-c \times Q$ . Therefore  $C-c$  will be as  $\frac{H-h}{Q}$ ; and if  $H-h$  be given,  $C-c$  will be reciprocally as  $Q$ . Consequently if the differences of the absolute heats be equal, the differences of the capacities will be reciprocally as the quantities of matter.

COR. I. It is, therefore, required, in order that neither heat nor cold should be produced, that the differences of the capacities should be reciprocally as the quantities of matter changed in a given time. For in that case, by the converse of this proposition, the differences of the absolute heats will be equal.

COR. II. See the Fig. Prop. I. If, therefore, the diminution in the capacity of A, be to the increase in that of B, in a greater proportion, than the quantity of matter in B to that in A, the whole of the heat which is separated from A will not be absorbed



forbed by B, a part of it will become redundant, or will be converted into moving and sensible heat.

COR. III. The differences of the capacities being reciprocally as the quantities of matter, if the quantities of matter changed in a given time be equal, the differences of the capacities will be equal.

COR. IV. See the fig. Prop. I. If the quantities of matter changed be equal, and if the heat of A, after the change, be equal to that of B previous to the change, that is, if  $h$  be equal to  $p$ , in order that neither heat nor cold should be produced, it is required that  $P$  should be equal to  $H$ . For in that case  $H - h = P - p$ .

COR. V. The same things being supposed, if  $h$  be less than  $p$ , then  $P$  must be greater than  $H$ , and  $P$  will be equal to  $H + p - h$ . For in that case  $H - h$  will be equal to  $P - p$ .

COR. VI. If  $h$  be greater than  $p$ ,  $P$  must be less than  $H$ , and  $P$  will be equal to  $H - h + p$ . For subtracting  $h - p$  from  $H$ , we have  $H - h + p = P$ . And, therefore,  $P - p = H - h$ .

The rule which is expressed in the first corollary, applies to the separation of heat from the air, and the absorption of it by the blood, in the process of respiration. For as the sensible heat in the lungs, is not greater than in the other parts of the body, it is manifest, that the whole of the heat which is separated from the air must be absorbed. And therefore the changes produced by the passage of the phlogiston, from the blood to the air, are such, that the difference of the capacities of venous and arterial blood, is to the difference of the capacities of fixed and atmospherical air, as the quantity of air changed in a given time, is to that of blood: in which case, by the above corollary, no part of the heat will become redundant. The opposite changes, therefore, which the air and the blood undergo in



the lungs, precisely balance each other. For as the quantity of blood, which is altered by respiration, in a given time, is much greater than that of air, so the change which is produced in the air, during this process, is proportionably greater than that which is produced in the blood.

The rule which is expressed in the second corollary, applies to the sensible heat produced in the course of the circulation, and to that which is produced by the inflammation of combustible bodies. As we find, for example, that a part of the heat which is disengaged from the blood, in its progress through the system, becomes redundant, we may conclude, that the diminution in the capacity of the blood, is to the increase in the capacity of those parts of the body from which it receives the phlogiston, in a greater proportion, than the quantity of matter in the latter to that in the former: in which case, the whole of the heat separated from the blood will not be absorbed; a part of it will be converted into moving and sensible heat.

That this rule is also applicable to the inflammation of combustible bodies, appears from the following experiments and observations.

In the burning of oil, the phlogiston is separated from its former basis, and combined with the air. The air is converted into fixed and phlogisticated air—the oil into vapour. By this process, the capacity of the air for containing heat is diminished, and that of the oil increased. And, therefore, from the first and third Corollaries, it follows, that if, in the inflammation of oil, equal quantities of air and oil were changed in a given time, and if the difference of the capacities of oil and the vapour of oil, were equal to the difference of the capacities of fixed and atmospherical air, the whole heat separated from the air, would be absorbed by the vapour.

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The difference of the capacities of fixed and atmospheric air is 66. The capacity of oil is to that of fixed air, nearly as three to one ; and, therefore, by the fifth corollary, if the whole heat separated from the air were absorbed, and if the quantities of air and oil changed in a given time, were equal, the capacity of the vapour of oil would be equal to  $67 + 3 - 1$ . It would be to that of atmospheric air, as 69 to 67. But we have seen, in the above Experiments, that a pint of atmospheric air communicates one degree of heat to a pint of water, the difference of temperature being fifty ; and that, in the same circumstances, the quantity of sensible heat communicated by a pint of the vapour, produced by the inflammation of oil, is so small, that it cannot be measured by the thermometer.

In the second place, supposing that atmospheric air has a greater capacity for containing heat, than the vapour of oil, if the quantity of oil changed in a given time, were proportionably greater than that of air, in this case, as appears from the third Proposition, the whole heat separated from the air, would be absorbed.

The comparative quantities of air and oil, which were changed by combustion, in a given time, were determined in the following manner :

A candle, weighing nearly three ounces, and burning with a large wick, was found to lose a fourth of an ounce in twenty-four minutes, or five grains in a minute. Now, if a candle consumes a gallon of air in a minute, and if the one-eighth part of this consists of fixed air ; it follows, that about eight grains of fixed air, will be produced by the burning of a candle, in a minute. From which it appears, that the quantity of air changed in a given time, is much greater than that of oil ; and it has been already proved, that the diminution in the capacity of the air for containing heat, is also greater than



the increase in that of the oil ; we may, therefore, conclude, that only a small part of the heat separated from the air will be absorbed ; the rest will be converted into sensible heat.

To place this in another light : if, during the inflammation of oil, the whole heat separated from the air, were absorbed by the vapour, it would follow, that the oily vapour mixed with the fixed and phlogisticated air, which are produced by the inflammation of a grain of oil, would contain as much absolute heat, as an equal quantity of atmospheric air, mixed with a grain of oil. Now it is well known, that when a candle is suffered to burn out in air, the whole mass of fixed and phlogisticated air and oily vapour, is contained in less space, than the atmospheric air, previous to the inflammation. And yet it appears, from Experiment I. and VI. Prop. I. Sect. II that a pint of this compound, contains much less absolute heat than a pint of atmospheric air.

I have made several experiments to determine the quantity of air which is phlogisticated by the calcination of iron ; and have reason to believe, that it is at least equal to the quantity of metal calcined ; from which we may calculate the heat produced by that process.

The capacity of atmospheric air is to that of fixed air, as 67 to 1 ; or as 147.4 to 2.2 : The capacity of fixed air is to that of iron, nearly as 2.2 to 1 ; and the capacity of the calx of iron is to that of iron, nearly as 2.5 to 1.

Calling, therefore, the capacity of atmospheric air, 147.4, and that of fixed air, 2.2, it appears from Cor. 6. Prop. III. that if the whole heat separated from the air, were absorbed by the calx, the capacity of the calx would be equal to  $147.4 - 2.2 - 1 = 146.2$ . The heat of the calx would consequently be



be to that of the metal as 146.2 to 1. But it is as 2.5 to 1; and hence, the heat which becomes redundant during the calcination of iron, is to the original heat of the iron, as 143.7 to 1; and it is to that of the calx as 143.7 to 2.5, or as 57.4 to 1.

It has been before shown, that bodies, when at the common temperature of the atmosphere, contain at least 200 degrees of heat. During the inflammation of iron, therefore, a quantity of heat becomes redundant, which would be sufficient to raise the calx 200 degrees, multiplied by 57.4, or 11480 degrees. And hence we may account for the heat which is produced by the percussion of flint and steel. A particle of the metal is struck off by the force of the flint. The phlogiston is separated from this particle, and is left at liberty to combine with the air, in consequence of which a quantity of fire is disengaged from the latter; and the heat which is produced during this process, is so intense, that the particle of the metal, which is struck off, is converted into glass.

Upon the whole, there is sufficient evidence for concluding, in general, that the heat in combustion, depends upon the separation of fire from the air, by the action of phlogiston; that a part of this fire is absorbed by the body, which supports the inflammation; and that the rest becomes redundant, or is converted into sensible heat. It follows, therefore, that the quantity of fire which is separated from the air, will be in proportion to the quantity of phlogiston which is joined to it in a given time; and the degree of sensible heat which is produced, will be greater or less, according as a less or a greater quantity of this fire is absorbed by the body, which discharges the phlogiston.

In the inflammation of alcohol and sulphur, a very great proportion of the fire which is detached from the air, is imbibed by the aqueous and sulphureous



reous vapour ; and, therefore, alcohol and sulphur burn with a pale and weak flame. On the other hand, those inflammable bodies which produce little vapour, or which produce a vapour that is capable of absorbing but little heat, as pit-coal, oil, wax, phosphorus, burn with a strong and vivid flame ; for, in these cases, a great part of the fire which is yielded by the air, is converted into sensible heat.

I have thus endeavoured to account for the phenomena of combustion and of animal heat, from the general principle, that the capacities of bodies for containing heat, are diminished by the addition of phlogiston, and increased by its separation.

On this principle, a variety of phenomena may be explained, besides those which have been already mentioned.

When the nitrous acid is mixed with oil of turpentine, the phlogiston is separated from the oil, and combined with the acid ; the latter is forced to give out a portion of its absolute heat ; part of which is absorbed by the basis of the oil, and the rest becomes redundant, or is converted into sensible heat. If the sensible heat be increased to a certain degree, the phlogiston will suddenly combine with the air, in consequence of which a great quantity of fire will be extricated, and the whole will explode, with a vivid flame, and with intense heat.

It is probable, that the vapour of the pure nitrous acid contains as much absolute heat as atmospheric air ; for the power of the former in maintaining flame, is nearly as great as that of the latter. In the deflagration of nitre, the acid is converted into vapour ; which being at the same moment combined with the phlogiston of the coal, the fire is instantly disengaged, an elastic fluid is suddenly expanded, and a loud explosion produced.

The



The ingenious Mr. Bewley has given the following explanation of the spontaneous accension of phosphorus. He supposes, with Dr. Priestley, that atmospheric air contains the nitrous acid, as a constituent principle; and observing that much heat arises from the sudden combination of phlogiston, with this acid; he concludes that the phlogiston of the phosphorus is capable of decomposing the air; and that by the union of the phlogiston with the aerial acid, a degree of heat is produced sufficient to inflame the phosphorus.

The experiments which have been recited above, seem to prove, as Mr. Bewley has supposed, that the production of heat is the necessary consequence of the combination of phlogiston with air: But these experiments appear moreover to shew, that this heat arises from the separation of a quantity of fire, which was contained in the air as a constituent principle, and which, in the process of combustion, is detached from it, by the action of phlogiston.

Dr. Priestley, has proved that the electric fluid is capable of communicating phlogiston, to atmospheric air, and of converting it into fixed and phlogisticated air. This fact explains the cause of the heat which is produced by the electric spark.

If a great quantity of electric matter be suddenly disengaged from a cloud or from the earth, it will extricate a proportionable quantity of fire in its passage through the air; and thus we may account for the sudden rising of the thermometer in the time of thunder and lightning.

It is found, by experiment, that the phenomena of an earthquake may be imitated by a mixture of iron filings and brimstone, made into a paste with water, and buried in the earth. May not the heat  
which



which is thus produced be explained in the following manner?

The attraction of the phlogiston to the acid of the sulphur, will be diminished both by the attraction of the iron to this acid, and by that of the water. In the degree of heat which is necessary to the inflammation of sulphur, atmospherical air is capable of separating the phlogiston from the vitriolic acid. Is it not probable that by the assistance of the iron and the water, it may be capable of producing this effect in the common temperature of the atmosphere? If this be the case, it follows, that by the action of the air, which is diffused through the substance of the earth, upon the phlogiston of the sulphur, and by that of the iron and water upon the acid, the sulphur will be decomposed; the air will unite with the phlogiston; the iron with the acid; a quantity of fire will be disengaged from the former, and an inflammable elastic fluid from the latter; and hence a commotion will be excited, accompanied with noise and the eruption of flame, resembling the phenomena of an earthquake.

May not a similar mixture of sulphureous and metallic bodies be produced in consequence of the changes which take place in the bowels of the earth? May not these bodies be brought into contact with the water and the atmospherical air which are diffused through the earth's substance, or lodged in cavities beneath its surface? By the action of the air upon the phlogiston, and of the water and the ore upon the acid, may not the sulphur be decomposed, as in the mixture of iron filings and brimstone? In which case a quantity of fire will be disengaged, and an elastic vapour produced, the latter of which, by its sudden expansion, will excite a commotion in the bowels of the earth, and will at length force its way through the superincumbent strata.

If



If much combustible matter be lodged in the regions where the subterraneous fires have been kindled, and if this matter be mixed with atmospherical air, or with substances, which, by the application of heat, produce a fluid that is capable of maintaining fire, the inflammation may be augmented to a prodigious degree, and the rarified vapours may carry along with them in their ascent, a great quantity of ignited materials abounding with phlogiston, by the exposure of which, the phlogiston will be discharged, and the flame extended through a large tract of air.

In this manner we may probably account for volcanos, those awful instances of combustion which are exhibited by nature in the fossil kingdom.\*

It appears, upon the whole, that a variety of important effects are produced in the universe in consequence of the mutual opposition of phlogiston and fire.

Vegetables are elaborated by the assistance of heat and moisture, from the elements of earth, air, and water, and by the action of the solar light, the principles of which the vegetable tribe is composed, are intimately combined with phlogiston, and are obliged to resign a portion of their absolute heat. In combustion, the phlogiston is disjoined from its vegetable basis, and is combined with the air; and thus those artificial fires are maintained which are so necessary in the economy of human affairs. In like manner, by the powers of animal life, the phlogiston is separated from the blood and discharged by respiration, in consequence of which a quantity of fire is absorbed from the air, and is communicated to the animal kingdom.

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\* The same doctrine seems to afford an easy solution of the heat which is produced by fermentation and putrefaction.



The air which was tainted by combustion and respiration, is again purified by the growth of vegetables †; and if this effect be produced by the separation of phlogiston, it follows that vegetation will restore to the air that heat which had been detached from it in the processes of respiration and combustion; and thus the principles of phlogiston and fire, by the medium of atmospherical air, will be continually circulating through the animal and vegetable kingdoms.

It may be proper to observe, that the doctrine which is advanced in the preceding pages, with respect to the cause of animal heat and of combustion, is the result of the general fact, that the changes which are produced in the temperatures of different bodies, by the application of given quantities of heat, are different; or, that, the quantities of matter being equal, the same quantity of heat which raises *one* body a certain number of degrees, will raise *another* a greater or a less number, according to the nature of the body to which it is applied. This fact appears to have been sufficiently verified by experiment; and, therefore, the consequences which have been deduced from it, must, I apprehend, be considered as well founded, whatever be the hypothesis which we adopt concerning the nature of heat.

For this reason, I have not entered into the enquiry, which has been so much agitated among the English, the French, and the German philosophers, whether heat be a *substance* or a *quality*. It is true, I have, in some places, made use of expressions, which seem to favour the former of these opinions. But my sole motive for adopting this language, was because it appeared to be more simple and natural, and more consonant to the facts which had been established by experiment. At the same time, I am  
per-

† See Dr. Priestley's Experiments on Air.



persuaded, it will be found to be a very difficult matter to reconcile many of the phenomena with the supposition, that heat is a quality. It is not easy to conceive, upon this hypothesis, how heat can be separated from bodies, by the addition of phlogiston, or how the absolute heat can be augmented by the separation of this principle; how the quantity of heat in the air can be diminished, and that in the blood, increased, by respiration, though no sensible heat or cold be produced.

Whereas, if we adopt the opinion, that heat is a distinct substance, or an element *sui generis*, the phenomena will be found to admit of a simple and obvious interpretation, and to be perfectly agreeable to the analogy of nature.

Fire will be considered as an elementary principle which enters into the composition of all known bodies. In consequence of the addition of phlogiston, a portion of the fire will be detached, in the same manner as the nitrous acid, is detached, by the vitriolic, from an earth or alkali, and therefore respiration and combustion will be truly chymical processes, in which, by the exchange of fire and phlogiston, a double decomposition will take place, and two new compounds will be formed; the blood, or the inflammable body, parting with phlogiston and receiving fire, and the atmospherical air parting with fire and receiving phlogiston.

I may add, in the last place, that, if fire be considered as an element, which is capable of uniting chymically with bodies, a table may be formed, exhibiting the respective attractions of phlogiston and fire.

As phlogiston separates from bodies, a part of their absolute heat, it should, be placed at the head of the first column: And as we do not know of any substances, that attract this principle with greater force than the earths of the perfect metals, these, perhaps,



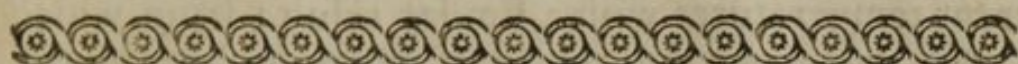
should stand immediately under phlogiston. In great degrees of heat, atmospherical air separates phlogiston from all inflammable bodies, and in the common temperature of the atmosphere, from nitrous air and phosphorus.

If, therefore, the attractions be arranged as they take place in consequence of the application of heat, under the earths of the perfect metals should stand dephlogisticated and atmospherical air; after which should be placed, the bases of all the inflammable bodies, disposing them according to the degrees of heat which are necessary to their inflammation, when combined with phlogiston; and at the foot of the column, should stand nitrous air.

Fire should be placed at the head of the second column; and if the attractions of bodies to this principle, be proportionable to the quantities of it, which they are found to contain, when the quantities of matter are equal; under fire should stand dephlogisticated and atmospherical air—the vapour of the nitrous acid, and probably of some other fluids—arterial blood, water, &c.

It is manifest, however, that much time, and a series of accurate experiments, would be required in order to the complete investigation of this subject.





## A P P E N D I X.

**I**T may not be amiss to give the Reader a brief account of the Experiments referred to in page 10, which were instituted by Mr. De Luc, with a view to determine the question, whether the thermometer be an accurate measure of heat; or, in other words, whether the expansions of the fluid contained in the thermometer, be in proportion to the quantities of heat applied?

It was laid down as a principle, by this philosopher, that if equal quantities of the same fluid be mixed together at different temperatures, the heat will be equally divided between them, from which it was concluded, that a thermometer being immersed in the warmer substance, and also in the colder, previous to the mixture, if its expansions were in proportion to the quantities of heat applied, it would point, after the mixture, to the arithmetical mean, or to half the difference of the separate heats.

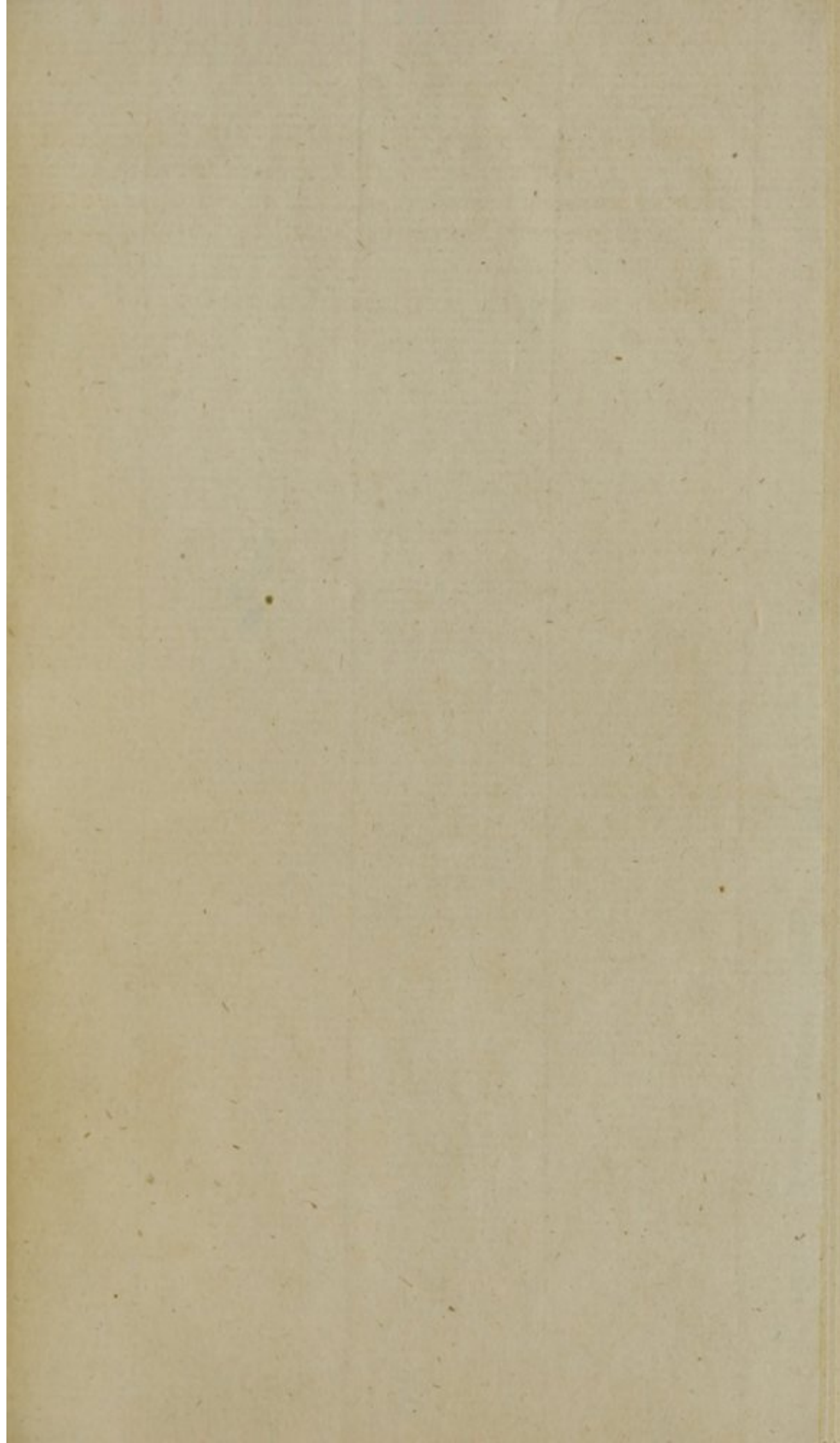
Proceeding upon this principle, he found that when a given quantity of water at 32, was mixed with an equal quantity of the same fluid at 212, the mercurial thermometer, being immersed in the mixture, pointed to 180, or to the arithmetical mean between 32 and 212: but the spirit of wine thermometer,



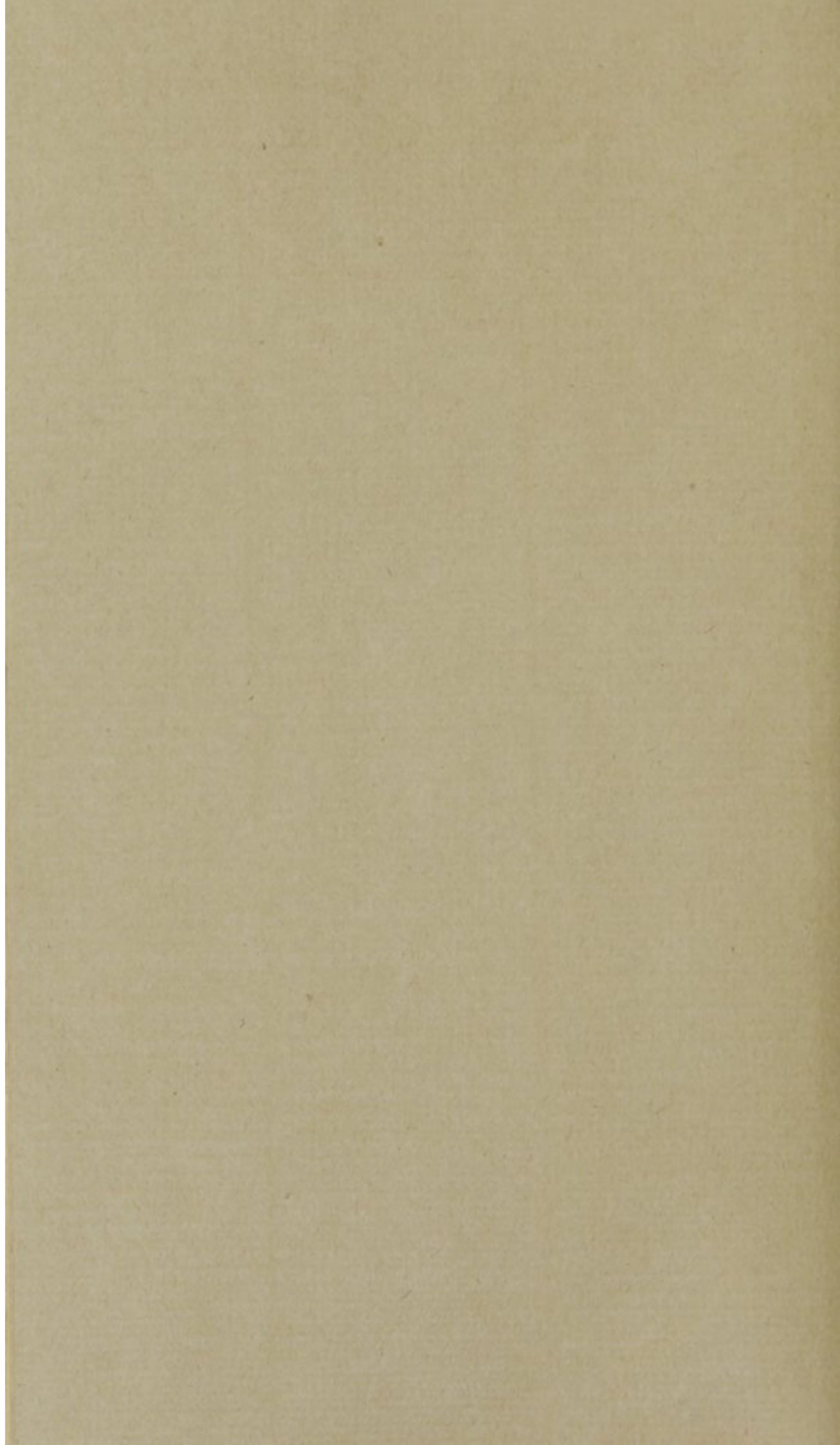
mometer, in the same circumstances, did not point  
 to 180. These Experiments were repeated at dif-  
 ferent temperatures with a similar result; from which  
 it was inferred, that alcohol expands irregularly,  
 but that the expansions of mercury correspond, pre-  
 cisely, to the quantities of heat applied.

F I N I S.











Leaves washed and deacidified with  
magnesium bicarbonate. Resewed.  
New all-rag end paper signatures.  
Unbleached linen hinges. Hand  
sewed headbands. Rebound in  $\frac{1}{4}$   
Russell's oasis morocco with hand  
marbled paper sides and vellum  
corners. Leather treated with  
potassium lactate and neat's foot  
oil and lanolin. December, 1979.

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Med. Hist.

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