

Experiments and observations on animal heat, and the inflammation of combustible bodies : being an attempt to resolve these phenomena into a general law of nature / by A. Crawford, M.D. F.R.S. L. and E. and member of the philosophical societies of Dublin and Philadelphia.

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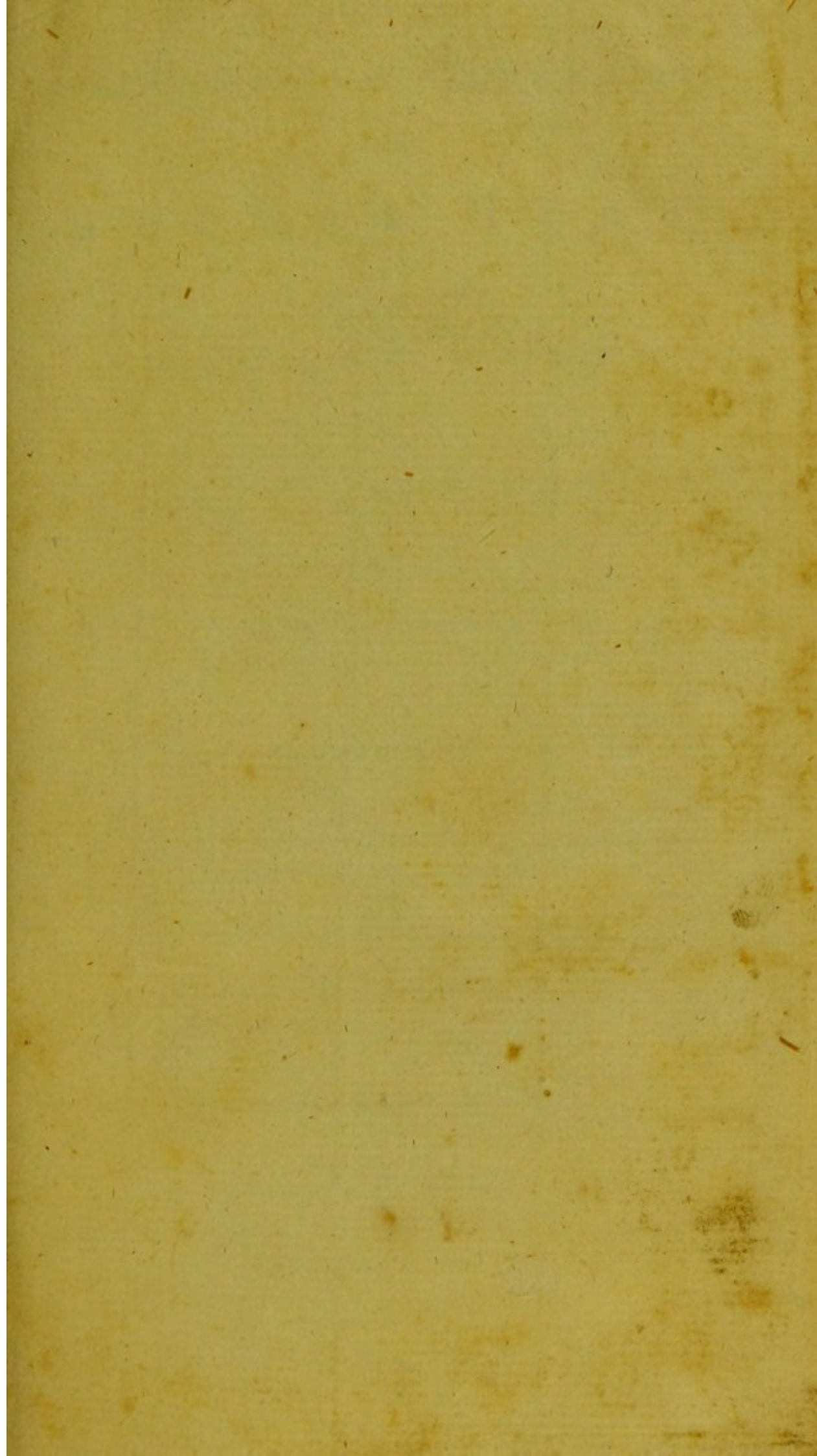


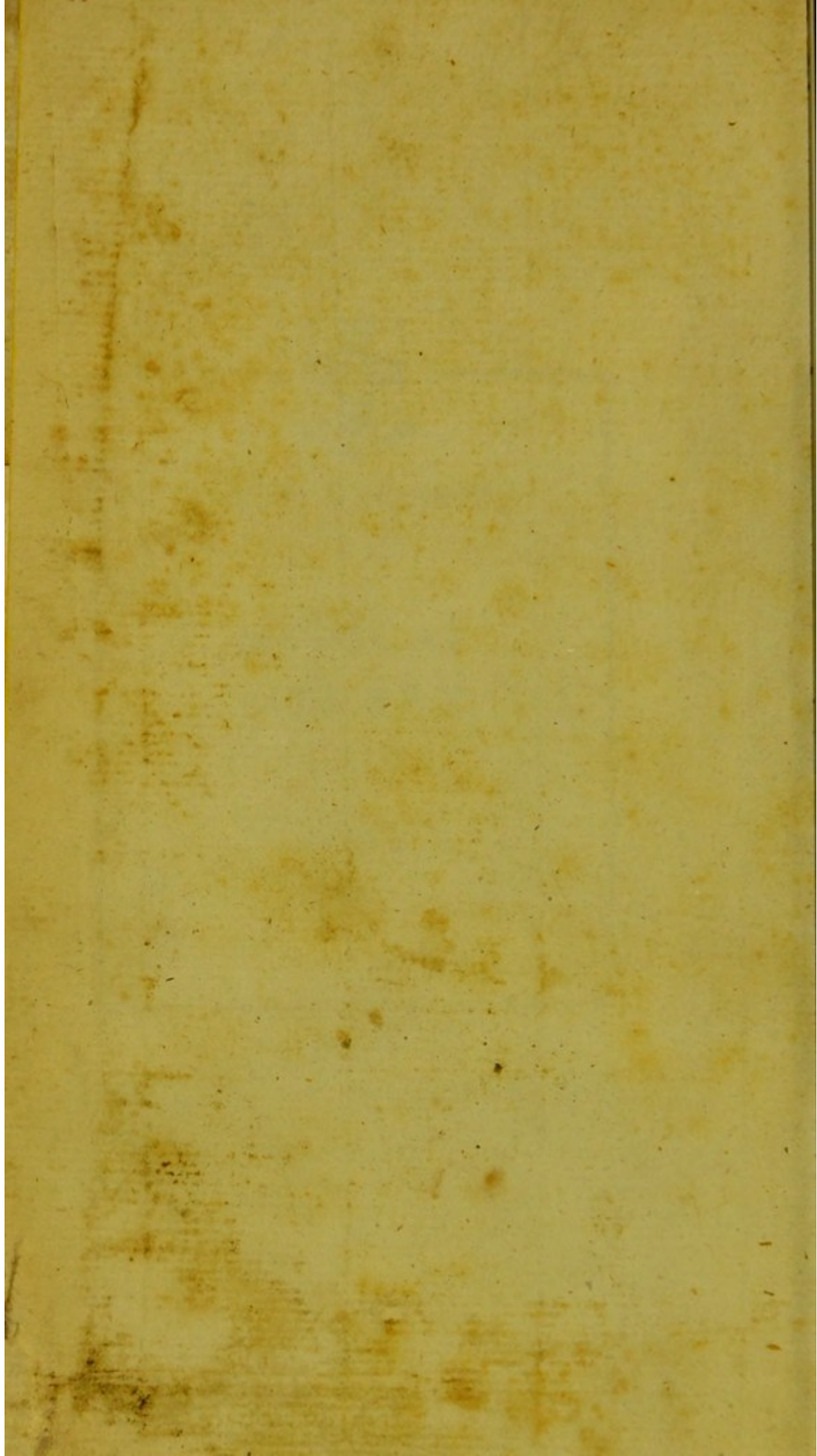
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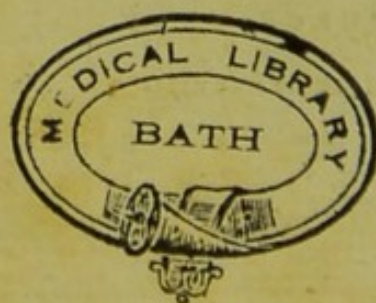
An Attempt to Resolve these PHENOMENA into a
GENERAL LAW of NATURE.

BY A. CRAWFORD, M.D. F.R.S. L. AND E.
AND MEMBER OF THE PHILOSOPHICAL SOCIETIES OF
DUBLIN AND PHILADELPHIA.

THE SECOND EDITION,
WITH VERY LARGE ADDITIONS.

L O N D O N:
PRINTED FOR J. JOHNSON, N^o 72, ST. PAUL'S
CHURCH-YARD.

M.DCC.LXXXVIII.



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TO
RICHARD KIRWAN, Esq.
THESE
EXPERIMENTS AND OBSERVATIONS
ARE INSCRIBED,
AS A MARK OF RESPECT AND
ESTEEM,
BY HIS MOST SINCERE FRIEND,
AND
OBLIGED HUMBLE SERVANT,
THE AUTHOR.

TO

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ADVERTISEMENT.

THE Experiments which gave rise to the following explanation of Animal Heat and Combustion, were made at Glasgow in the summer of the year 1777. They were communicated, in the autumn of that year, to Dr. Reid, Mr. Wilson, and Dr. Irvine. In the beginning of the ensuing session, they were made known to many of the Professors and Students in the University of Edinburgh; and in the course of the winter, they were explained by the Author, in the Royal Medical Society.

A brief account of these Experiments was laid before the public in the first edition of this work, which appeared in the year 1779.

As the trials recited in that publication had been made under many disadvantages, I soon afterwards found, upon a careful repetition of them, that I had fallen into considerable mistakes in my conclusions respecting the quantities of heat contained in the permanently elastic fluids.

I perceived, however, that these mistakes did not affect the explanation which I had

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

given of Animal Heat and Combustion. For though it appeared that the excess of the capacity of dephlogisticated, above that of fixed air, was not so great as I had at first imagined; yet, from an extensive series of experiments, which were made with the most scrupulous attention to accuracy, it was evident that the difference of the capacities of those fluids was such as to afford an adequate explanation of the phenomena.

I have hitherto been induced to defer the publication of the result of my enquiry, that I might have an opportunity of giving the subject a more thorough investigation, and of establishing the leading facts upon as solid a foundation as possible. I was sensible that to correct errors, is the best method of apologising for them; and that though the free communication of discoveries is essential to the progress of knowledge, yet it is of much greater importance to the interests of science, that facts should be well ascertained, than that they should be speedily published.

Many other causes have concurred to retard the publication of this Work. I might here mention the interruptions which have

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

arisen from want of health, from the avocations of a profession that engaged a great share of my attention, and from the difficulty of procuring the apparatus requisite for nice and delicate experiments. I am persuaded, however, that it is unnecessary to employ much time in apologising for a delay, which the greater part of my Readers will probably rather approve than condemn.

A general conviction now prevails, that it is by the method of experiment and induction alone, that we are permitted to interpret the characters which the Deity has impressed upon his works; and those who are in the habit of cultivating natural knowledge, well know that this method of searching into truth, necessarily requires much labour and patient investigation.

Though I have used my utmost endeavours to establish the facts, which I now venture to submit to the public eye, upon a solid foundation; yet I am far from meaning to insinuate that they are free from error. In an attempt to determine the relation which takes place between such subtle principles as air and fire, we can only hope for an approximation to the truth.

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

I believe, however, that no considerable mistakes will now be found, in the experiments recited in the following pages; and it is hoped that the candour of the public will ascribe the errors that may still remain, to those imperfections in the senses and in thermometers, which make it difficult to estimate, with precision, minute differences, and which must ever prevent us from arriving at perfect accuracy.

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E R R A T A.

Page	Line
16	7, <i>for places, read place.</i>
31	5, <i>for by 10lb. read to 20lb.—for .8 read 6.</i> 6, <i>for 100.9, read 101.3.</i>
53	16, <i>for 5, read V.</i>
71	7, <i>for 6, read VI.</i>
72	2, <i>at the bottom, dele that.</i>
85	12, <i>for V. read VII.</i>
134	1, <i>for was, read is.</i>
233	16, <i>for +, read ×, for ×, read +, and for +, read ×.</i>
256	5, <i>after absolute heat, insert in equal weights.</i>
270	12, <i>for 1.57, read 1.55.</i>
304	8, <i>for 5, read 4.</i> 20, <i>for 5, read 4.</i>
316	1, <i>for e f g h, read e f g n.</i>

EXPERIMENTS

AND

OBSERVATIONS

UPON

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 *.

Philosophers, who have applied themselves to this subject in modern times, have generally appropriated the word *heat* to the unknown principle; and in the language of

* See Dr. Reid's Inquiry into the Human Mind, p. 78.

science, its signification, as applied to that principle, is much more extensive than in common language. For, in common language, it expresses such a degree of the unknown external cause, as will produce a given effect upon the senses; but in the philosophical acceptation, it has been used to express the external cause in the abstract, without regard to the peculiar effects which it may produce. For the sake of greater precision, in the following treatise, I shall, with the ingenious Dr. Irvine of Glasgow, call the latter *absolute heat*, and the external cause considered as having a relation to the effects which it produces, I shall call *relative heat*.

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

The effects by which heat is known and measured are three, and therefore relative heat may admit of three subdivisions.

1st,

1st, This principle is known by the peculiar sensations which it excites in animals.— Considered as exciting those sensations, it is called sensible heat.

2dly, It is known by the effect which it produces upon an instrument that has been employed to measure it, termed a thermometer. This is called the temperature of heat in bodies.

3dly, It has been found by experiment that in bodies of different kinds, the quantities of absolute heat may be unequal, though the temperatures and weights be the same. Thus it will appear from the experiments recited in the following pages, that if a pound of water and a pound of diaphoretic antimony have a common temperature, the quantity of absolute heat contained in the former, is nearly four times that contained in the latter. When the principle of heat is considered relatively to the whole quantity of it contained in bodies that are of different kinds, but which have equal weights and temperatures, I shall term it comparative heat. If, for example, the temperatures and weights being the same, the whole quantity of heat in water be four

times as great as that in antimony, the comparative heats of these substances are said to be to each other as four to one.

Sensible heat is measured by the intensity of the sensation which it excites. Temperature, by the expansion of the fluid in the thermometer: and comparative heat is measured by the alterations of temperature, which equal quantities of absolute heat produce in bodies that have equal weights.

Thus we say that two bodies have the same sensible heat, when the organ of feeling is equally affected by them; and that the same body has a greater or less degree of sensible heat, according as it produces a greater or less effect upon that organ.

In like manner bodies are said to have the same temperature, when they produce equal expansions in the thermometer, and the same body is said to have a higher or lower temperature, according as a greater or less expansion is produced by it in that instrument.

It will hereafter appear, that the comparative heats of bodies which have equal weights and temperatures, are greater or less, in proportion as less or greater alterations are produced in their temperatures by equal quantities

tities of absolute heat.—Thus it is found, that the same quantity of heat which raises a pound of water one degree, is sufficient to raise a pound of mercury 28 degrees. From which it follows, as will be hereafter proved, that the comparative heat of water is to that of mercury as 28, to one—and consequently, the alterations which are produced in the temperatures of bodies, by given quantities of absolute heat, may properly be applied as a measure of their comparative heats; the alterations of temperature and the comparative heats being reciprocally proportional to each other.

Sensible heat depends partly upon the temperature, and partly upon the state of the organ of feeling; and, therefore, if a variation be produced in the latter, the sensible heat will be different, though the temperature continue the same. Thus water at the temperature of 62 degrees of Fahrenheit, appears cold to a warm hand immersed in it; but on the contrary, that fluid will appear warm, if a hand be applied to it which has a lower degree of heat than 62. For this reason the thermometer is a much more accurate measure of heat than the senses of animals. As long however as the organs

remain unchanged, the sensible heat is in proportion to the temperature. And, therefore, these terms have generally been considered as nearly synonymous. It has been observed, by my learned friend Dr. Reid, in a very accurate communication, with which he favoured me on this subject, that “until the ratio between one temperature and another be ascertained by experiment and induction, we ought to consider temperature as a measure which admits of degrees, but not of ratios: and consequently ought not to conclude, that the temperature of one body is double or triple to that of another, unless the ratio of different temperatures were determined. Nor ought we to use the expressions of a double or triple temperature, these being expressions which can convey no distinct meaning, until the ratio of different temperatures be determined.” By not attending to these circumstances, philosophers have been led into mistakes, in their calculations respecting the heat of comets at certain distances from the sun.

When we examine the comparative quantities of heat in different bodies, we may consider them either as having equal *weights*
and

and temperatures, or equal *magnitudes* and temperatures; and the results of these comparisons will be different, in proportion to the differences of the specific gravities of the substances compared. Thus it will be found, that the comparative heat of water, is to that of an equal volume of mercury, nearly as two to one; but in equal weights of these substances, the comparative heats are nearly as 28 to one. In the following treatise the comparative heats have been calculated from equal weights of the substances compared; this method appearing to be less liable to objection than the calculations from equal magnitudes, because the latter requires a more perfect knowledge of specific gravities than has hitherto been attained.

It is proper to add, that as equal weights of heterogeneous substances are found to contain unequal quantities of absolute heat, there must be certain essential differences in the nature of bodies; in consequence of which, *some* have the power of collecting and retaining that element in greater quantity 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 four times as much absolute heat as a pound of diaphoretic antimony when at the same temperature, the capacity of water for containing heat is said to be to that of antimony as four to one.

The temperature, the capacity for containing heat, and the absolute heat contained, may be distinguished from each other in the following manner.

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 temperature, we consider the unknown principle as producing certain effects upon 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

tered by every increase or diminution of its sensible heat: but it will hereafter be proved, that as long as its form continues the same, its capacity for receiving heat is not affected by an alteration of temperature, and would remain unchanged, though the body were wholly deprived of its heat.

It was before observed, that the changes which are produced in the temperatures of different bodies, by equal quantities of absolute heat, are *greater* or *less* according as the body to which the heat is applied has a *less* or a *greater* capacity for containing heat. 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 temperature 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 temperature 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 temperature arises from the first of these circumstances, it follows, that in
the

the same body the temperature may vary, though the absolute heat continues the same. If, for example, a body of a given weight be supposed to have a capacity as one, a quantity of absolute heat as 10, and a temperature which computed from the point of total privation is also as 10; and if the capacity of the body be conceived to be suddenly doubled, the same quantity of absolute heat which formerly raised it to the temperature of 10, will now be sufficient to raise it only to the temperature of five. This will afterwards more clearly appear, from the experiments and observations on fixed and atmospheric air, venous and arterial blood*.

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

The capacities of bodies for receiving heat, are considered as proportional to the quantities of absolute heat which they contain, when the quantities of matter are equal, and the temperatures are the same. Calling therefore the temperature T , the ca-

* It is proper to observe, that the general fact respecting the influence of a change of capacity upon the temperature of bodies, was first discovered by Dr. Irvine.

capacity C , and the absolute heat A , if the quantities of matter and the temperatures be given, the capacities will be as the absolute heats—or if T be given, A will be as C . Again, in the same body, if the capacity be given, the quantity of absolute heat will be in proportion to the temperature, computed from the point of total privation, and measured by a thermometer, the expansions of which are proportional to the increments of heat. That is, if C be given, A will be as T .—Since, therefore, if T be given, A will be as C , and if C be given, A will be as T ; it follows, that if neither be given, A will be as $T \times C$. Therefore C will be as $\frac{A}{T}$, and if A be given, C will be reciprocally as T . Consequently, if the capacities be reciprocally as the temperatures, the quantities of absolute heat will be equal. Thus if the capacity of the calx of antimony be to that of the regulus, as three to one, and if the temperature of the former, computed from the point of total privation, be to that of the latter as one to three, the calx and regulus will contain equal quantities of absolute heat.

Fire,

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 phænomena as proceeding 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.

* Vide Boerhaave, Martine, M^cQuer, &c.

I. Heat has a constant tendency to diffuse itself over all bodies, till they are brought to the same temperature. Thus it is found by the thermometer, that if two bodies, of different temperatures, are mixed together or placed contiguous, the heat passes from one to the other till their temperatures become equal; 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.

From this property of heat it follows, that the various classes of bodies throughout the earth, if they were not acted upon by external causes, would at length arrive at a common temperature, when the heat would become quiescent: in like manner, as the waters of the ocean, if not prevented by the winds, and by the attractions of the sun and moon, would come to an equilibrium, and would remain in a state of rest. But as causes continually occur in nature to disturb the balance of heat, as well as that of the waters of the ocean, those elements are kept in a constant fluctuation.

II. Heat

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

From the interesting experiments which were made on cold, by my ingenious friend Mr. Wilson, we learn, that at Glasgow in the winter of the year 1780, the thermometer on the surface of snow, sank 25 degrees below the beginning of Fahrenheit's scale.

We are told by Dr. Pallas, that in the Deserts of Siberia, during a very intense frost, the mercury was found congealed in thermometers exposed to the atmosphere, and a quantity of that fluid, in an open bowl placed in a similar situation, at the same time became solid. The decisive experiments which have been made by Mr. Hutchins at Hudson's Bay prove, that the freezing point of mercury is very nearly 40 degrees below the zero of Fahrenheit. From which it follows, that at the time of Dr. Pallas's observation, the atmosphere in Siberia must have been cooled to minus 40. By a paper which has been lately transmitted to the Royal Society we are informed, that the spirit of wine thermometer in the open air, at Hudson's Bay, during the winter 1785, fell to minus

42; and from the same communication we learn, that by a mixture of snow and vitriolic acid, the heat was so much diminished, that the spirit of wine sank to minus 80, which is 112 degrees below the freezing point of water.

Hence it is manifest, that heat is contained in considerable quantities in all bodies, when at the common temperature of the atmosphere. It is plain, however, that the quantity of heat inherent in each individual body is limited. This I think must be admitted, whatever be the hypothesis which we adopt concerning the nature of heat, whether we conceive it to be a force or power belonging to bodies, or an elementary principle contained in them. For those who consider heat as an element, will not suppose that an unlimited quantity of it can be contained in a finite body—and if heat be considered as a force or power, the supposition that finite bodies are actuated by forces which are infinite, is equally inadmissible.

To place this in another light, we know that bodies are universally expanded by heat, excepting in a very few instances, which do not afford a just objection to the general

fact; because in those instances, by the action of heat, a fluid is extricated that previously separated the particles from each other. Since, therefore, heat is found to expand bodies in the temperatures which fall within the reach of our observation, we may conclude that the same thing takes place in all temperatures. It will hereafter appear, that in mercury and in some other fluids, the expansions are proportional to the quantities of heat applied; from which it follows, that the quantities of heat in bodies are limited, because an infinite heat would produce an infinite expansion.

It is manifest that the number of degrees of sensible heat, as measured by the thermometer and estimated from the beginning of the scale, must be the same in all bodies which have a common temperature. For by the first general fact it is proved, that heat has a constant tendency to diffuse itself uniformly over bodies, till their temperatures become equal. From which it may be inferred, that if a quantity of heat were added to bodies absolutely cold, the same uniform diffusion would take place; and that if a thermometer, altogether

gether deprived of its heat, were applied to such bodies, it would be equally expanded by them, the whole of the sensible heat which they had acquired being indicated by that expansion.

III. If the parts of the same homogeneous substance have a common temperature, the quantity of absolute heat will be proportional to the bulk or quantity of matter. Thus the quantity of absolute heat, in two pounds of water, is double 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, and their capacities for receiving heat will be the same; and, therefore, the quantities of absolute heat which they contain will be proportional to the bulk or quantity of matter.

The truth of the general fact, that bodies of the same kind, having equal temperatures, contain quantities of heat proportional to their bulk, is not only manifest from the simplicity of the operations of nature, but may moreover be confirmed by an induction from the experiments recited in the follow-

ing work ; as the intelligent reader will readily perceive.

IV. The dilatations and contractions of the fluid in the mercurial thermometer, are nearly proportional to the quantities of absolute heat, which are communicated to the same homogeneous bodies, or separated from them, as long as they retain the same form. Thus the quantity of heat required to raise a body four degrees in temperature, by the mercurial thermometer, is nearly double that which is required to raise it two degrees, four times of that required to raise it one degree, and so in proportion.

The proportions which obtain between the increments of heat in the same homogeneous bodies, and the expansions of the various fluids that have generally been employed in thermometers, were first investigated by Mr. De Luc. It was laid down as a principle by this ingenious philosopher, that if equal quantities of hot and cold water be mixed together, the difference of heat will be equally divided between them. From which it was concluded, that a thermometer being immersed in the hot water, and also in the cold, previously to the mixture, if its expansions

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, added to the less or subtracted from the greater. Proceeding upon this principle, he found that when a given quantity of water at 45.5 of Fahrenheit, was mixed with an equal quantity of the same fluid at 200.75, the mercurial thermometer immersed in the mixture indicated somewhat less than the arithmetical mean, the deviation being not more than two degrees below that point. This experiment was repeated at different temperatures with a similar result. From which he inferred, that the mercurial thermometer is nearly an accurate measure of heat.

As Mr. De Luc's experiments, with the conclusions deduced from them, constitute a fundamental part of the doctrine of heat, I have repeated them with as much attention as possible.

The thermometers which I employed were mercurial, and had each degree of Fahrenheit divided into ten equal parts, being reduced to a common scale, by comparing them with a very accurate standard.

EXPERIMENT I.

The air in the room being 61.5, a quantity of water weighing thirteen pounds ten ounces and an half, was heated in a slight tinned iron vessel that had a cover of the same metal closely adapted to it, a thermometer being inserted in the center of the cover by means of a cork. When the water was raised to the desired temperature, it was gently agitated, that every part of it might be brought to the same heat, the thermometer immersed in it pointing precisely to 120.6. An equal quantity of cold water, the parts of which were also brought by agitation to a common temperature, was mixed with the warm, by pouring it into the tinned vessel in which the latter was contained.

When the mixture was reduced by agitating it with a wooden rod to a mean heat, its temperature, at the end of one minute, was 89.8—at the end of six minutes, the agitation being continued, it was found to be 89.4. Allowing therefore .066 for the heat lost in the first minute, we have 89.866 for the true temperature of the mixture. If
the

the thermometer, at the moment of immersion, had indicated the exact arithmetical mean, it would have stood at 89.8.

This experiment was repeated with the following result.

EXPERIMENT II.

The air in the room being 62, twelve pounds of water at 59.9 were mixed as before with an equal quantity of the same fluid at 119.8. The temperature of the mixture, reduced by agitation to a mean heat, was 89.8 at the end of one minute.

An agitation as nearly equal as possible being continued, the temperature at the end of five minutes was found to be 89.4. Allowing therefore .08 for the heat lost in the first minute, we have 89.88 for the true temperature of the mixture. The exact arithmetical mean in this experiment was 89.85.

EXPERIMENT III.

The air in the room being 59, twelve pounds of water at 56.1 were mixed as before with an equal quantity of water at

C 3

156.25.

156.25. The mean temperature of the mixture at the end of

1 Minute was 106.1

5 Min. ——— 105.3

Allowing therefore .16 for the heat lost in the first minute, we have 106.26 for the true temperature.

In this experiment, which was repeated twice with the utmost care, and with very nearly a similar result, the arithmetical mean between the separate heats was 106.2.

It appears, therefore, that in the three preceding trials, the true temperature was a little greater than the arithmetical mean. As in these trials the mixture was made in a warm vessel, it is manifest, that the deviation from the arithmetical mean is partly to be attributed to the heat received from the vessel.

The heat imparted by the vessel was determined by the experiment which follows.

The air in the room being 62, and the vessel being raised to 136, twelve pounds avoirdupois of water at 61.9 were poured into it. The water being agitated, its temperature in

1 Minute was 62.7

5 Min. ——— 62.8

The

The tinned vessel was heated by including it in a similar vessel made of copper, the size of which was such that the surfaces of the vessels were nearly in contact with each other. Thus circumstanced, they were placed in a large tub, and were surrounded with water at 136; the access of the external air being excluded by a broad pan which covered them, and which contained water nearly of the same temperature. By this contrivance the tinned vessel was entirely surrounded with warm water, and received from it an uniform heat, without being in contact with it.

It appears from the experiment, that the vessel was cooled by the water 73 degrees and .2; and that the water was heated .9 of a degree. And since the water received this heat from the vessel, it is manifest, that the same quantity of heat which changes the temperature of twelve pounds of water .9 of a degree, will change the temperature of the vessel 73 degrees and $\frac{2}{10}$. And by a parity of reasoning, the same heat which raises the water one degree, will raise the vessel 81.4. If therefore, in any experiment, we find that 81 degrees and $\frac{4}{10}$ of heat have been separated

from the vessel by twelve pounds of water, we may conclude, that this heat has raised the water one degree. Hence the heat communicated by the vessel in the preceding experiments may be determined. Thus in the third experiment the heat of the warm water and of the vessel was 156.25 ; the true temperature of the mixture was 106.26. The temperature of the vessel was therefore sunk by the cold water 49.99. The increase which was produced in the temperature of the cold water by this heat may be determined by the following proportion. If 81.4 give 1, what will 49.99 give ? By which it appears, that the water in the experiment in question received from the vessel .61 of a degree of heat. Subtracting this from 106.26, which was the true temperature of the mixture, we have 105.65 for the temperature which would have been indicated by the thermometer, if no heat had been received from the vessel. The deviation from the arithmetical mean in this experiment was therefore .55.

In Experiment I. the vessel was cooled 30.734. To discover how much heat the cold water received from the vessel, say, if

81.4

81.4 give 1, what will 30.734 give? Hence the heat imparted by the vessel was .377, which subtracted from the true temperature 89.866, gives 89.489 for the temperature, which would have been indicated by the thermometer if no heat had been received from the vessel.

In Experiment II. from a similar calculation it appears, that if heat had not been communicated by the vessel, the temperature indicated by the thermometer would have been 89.513. Hence in the former experiment the deviation from the arithmetical mean was .311; in the latter it was .337.

EXPERIMENT IV.

Air in the room	-	54
Eight pounds of water at		52.4
Mixed with an equal quantity		
of water at	- -	81.9
Temperature of the mixture		
in 1 min.	- -	67.1 plus
Three minutes	- -	67.1
The arithmetical mean in this		
experiment was	-	67.15

In Experiment I. page 20, the quantity of water was twelve pounds, and in that experi-

experiment the heat received from the vessel was .61.

To find how much would have been received, if the quantity of water had been only 8 lb. say, as 12 to .61, so inversely is 8 to a fourth proportional, which gives .91 for the heat, which the vessel would have imparted to 8 lb. of water.

But in the former experiment the vessel was cooled by the water very nearly 50 degrees. In this experiment it was cooled nearly 15, say, as 50 to 15, $:: .91$ to a fourth, which gives .27 for the heat received from the vessel.

The true temperature was 67.15, which was also the exact arithmetical mean. Hence the deviation from the arithmetical mean was .27.

EXPERIMENT V.

Eight pounds of water	-	98.8
Mixed with 16 lb. of ditto	-	54.6
The mixture in one min.	-	69.1 plus
————— five ———	-	69 plus

When the mixture is said to be 69.1 plus, the meaning is, that the excess of its temperature

perature above 69 is a little more than $\frac{1}{10}$. In which case it may be supposed to be $\frac{1}{10}$, plus $\frac{1}{20}$, and the temperature, as indicated by the thermometer, will then be 69.15. Allowing $\frac{1}{50}$ of a degree for the heat lost in the first minute, we have 69.17 for the true temperature.

From the original heat of the vessel 98.8

Take the true temperature - 69.17

We have - - - 29.63

for the heat separated from the vessel.

To discover how much the temperature of the cold water was increased by this heat, say as 81.4 to 1, so is 29.63 to a fourth, which gives .36 for the heat, which would have been communicated by the vessel to twelve pounds of water.—But the quantity of water was 16 pounds, therefore say as 12 to .36, so inversely is 16 to a fourth.—Whence it appears, that the heat imparted to the water by the vessel was .27, which, subtracted from the true temperature, gives 68.9 for the temperature, which would have been indicated by the thermometer if no heat had been received from the vessel.

The difference between the temperatures of the warm and cold water was 44.2. The quantity

quantity of cold water was to that of warm water as 2 to 1, dividing the difference of temperature into three equal parts, if the capacity of water were permanent, and the mercurial thermometer an accurate measure of heat, the warm water should have been cooled two of these parts, and the cold water heated one. Hence the temperature of the mixture should have been 69.3.

It was before shewn, that if no heat had been received from the vessel, the temperature indicated by the thermometer would have been 68.9.

EXPERIMENT VI.

Eight pounds of water at	-	120.8
Mixed with sixteen pounds of		
ditto at	-	51.8
Temperature of the mixture		75

In this experiment, the heat imparted by the vessel was .42 of a degree. We have therefore 74.58 for the temperature, which would have been indicated if no heat had been received from the vessel.

The temperature, by calculation, is 74.8.
Hence, when equal quantities of warm and
cold

cold water are mixed together, the mercurial thermometer is found in all cases to indicate very nearly the arithmetical mean. And, moreover, when the warm and cold water are mixed in different proportions, the results correspond very nearly with the temperatures, as determined by calculation.

I next endeavoured to discover, by similar experiments, whether the same law obtains when equal quantities of linseed oil are mixed together at different temperatures.

EXPERIMENT VII.

Air in the room	-	56
Ten pounds of linseed oil at	-	145
Mixed with an equal quantity of linseed oil at	-	55.5
The mixture in one minute was		101.7
————— two —————		101.5
————— three —————		101.3
————— four —————		101.1
————— five —————		100.9

In this experiment great attention was required, in order that the heat might be uni-

uniformly diffused over the oil, previously to the mixture, as well as subsequent to it.—For when that fluid is exposed to heat, the warmer particles have a much greater tendency to ascend to the surface than those of water in similar circumstances. And, consequently, in the above experiment, a brisk agitation was necessary to promote a uniform diffusion.

If two tenths of a degree be allowed for the heat lost in the first minute, the true temperature of the mixture will be 101.9. Hence the warm oil and vessel were cooled 43.1, the cold and warm oil being mixed in the vessel in which the latter was contained. The vessel was the same with that used in the former experiments.

The capacity of water is to that of oil very nearly as 2 to 1.—If therefore 81.4 degrees of heat, when separated from the vessel, raise a given quantity of water one degree, the separation of the same heat would raise an equal quantity of linseed oil two degrees.

In the experiment in question the vessel was cooled 43.1.—To discover how much the oil was raised by this heat, say as 81.4
to

to 2, so is 43.1 to a fourth, which gives very nearly one degree for the heat that would have been imparted by the vessel to 12lb. of linseed oil, and consequently the heat communicated by 10lb. was nearly .8 of a degree. Hence we have 100.9 for the temperature, which would have been indicated if no heat had been communicated by the vessel.—The arithmetical mean is 100.2.

This experiment was thrice repeated with the greatest attention; and the mean result gave nearly .5 for the excess of the temperature indicated by the thermometer above the arithmetical mean.

It is probable, that this deviation was in some measure owing to the tendency of the particles of the warm oil to ascend to the surface, in consequence of which the difficulty of bringing the several parts to a uniform heat, and of marking the mean temperature, was very great. And, for the same reason, the results of the several experiments with that fluid could not be brought to correspond so nearly as the results of those which were made with water. If, however, we suppose the experiments to be exact, and the thermometer

meter to have been an accurate measure of heat, it will follow that the capacity of linseed oil is a little augmented by an increase of temperature.

The reader will perceive, that in the foregoing experiments with water, when equal portions of this fluid, having different degrees of sensible heat, were mixed together, the temperature of the mixture, as indicated by the mercurial thermometer, was a little less than the arithmetical mean.

This irregularity Mr. De Luc accounts for in the following manner: He supposes, that the contractions of mercury have an increasing progress compared with the diminutions of heat in bodies—that a double diminution of heat, for example, will produce more than a double contraction; a triple diminution more than a triple contraction. In consequence of which the mercurial thermometer, in experiments similar to those recited above, must indicate less than the arithmetical mean.

Mr. De Luc has, however, himself observed, in a paper, with which he some time ago favoured me on this subject, that we
cannot

cannot determine with certainty from those experiments, the relation which the expansions of mercury bear to the increments of heat. For when we infer the agreement between the dilatations of mercury and the increments of heat from such experiments, we take it for granted, that the capacity of water for receiving heat, continues permanent at all temperatures between the freezing and boiling points. This, however, should not be admitted without proof.

It is proper to add, that if the mercury in the experiments in question had indicated the precise arithmetical mean, we could not from thence infer with certainty, that the dilatations of this fluid are proportional to the increments of heat. For such alterations may be conceived to take place in the capacity of water, and in the contractions or dilatations of mercury, in consequence of a change of temperature, as would account for the phenomenon, though the expansions of mercury and the increments of heat were not proportional to each other. If, for example, unequal augmentations of capacity are produced in water by equal increments of heat ;

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and

and if unequal contractions are produced in mercury by equal diminutions of heat, these contrary irregularities may be conceived to be so adjusted as precisely to balance each other, in consequence of which a mercurial thermometer introduced into equal quantities of hot and cold water mixed together, would point to the arithmetical mean.

As these experiments, therefore, are not decisive, I have endeavoured to discover the relation between the expansions of mercury and the increments of heat, by a mode of trial which appears less liable to objection.

In the course of my experiments on heat, I observed, that if the vapour of boiling water be made to flow in a continued stream through a pipe of any length or dimensions, every part of that pipe will be raised to the boiling point; and, moreover, that if the extremity of a pipe, leading from a covered vessel containing boiling water, be inserted into another covered vessel, which has two small apertures for the purpose of suffering the steam to enter and to escape, every part of the latter vessel will be kept at the temperature of boiling water, provided the vapour

pour be introduced in such a quantity, as that a portion of it shall escape uncondensed. In reflecting on this fact, it occurred to me, that a method might be devised of combining the freezing and boiling points of water, so as to obtain an intermediate temperature that should be constant. With that intention, I contrived the following apparatus; see plate I. fig. 1.

A C represents a double vessel consisting of two cylinders, A B C D, and *l m n o*, which are joined at the superior extremity (*l o*), the space between them being compleatly closed, excepting at the two apertures C and B, into the former of which the steam of boiling water enters, and the latter suffers it to escape.

In this vessel, therefore, there are two compartments, one of which is the space intercepted between the interior and exterior cylinders, and the other is that included within the interior cylinder, which is open at (*l o*), and closed at the bottom (*m n*): The steam that passes from the boiler G H through the pipe G C, into the space between the cylinders which forms the exterior compartment, keeps every part of that space at

the boiling point, as long as a portion of uncondensed vapour issues from the pipe B.

Upon the vessel A C is inverted another double vessel E D, consisting of the interior and exterior cylinders E A D F, and $r p s q$, which are joined at the lower extremity $p q$. In this vessel, therefore, there are also two compartments, one of which is formed by the space between the cylinders, which is open at its superior extremity E F, and the other by that included within the interior cylinder; which being closed at its superior extremity ($r s$), and open below, communicates with the interior compartment of the lower vessel. The exterior compartment of the upper vessel is filled with pounded ice, which is consequently applied to the external superficies of the interior cylinder, as well as to its bottom, ($r s$), being introduced in such a quantity as compleatly to occupy the cavity $e p r s q f$.

The two interior cylinders $l m n o$ and $p r s q$, are equal in size. Between the superior and inferior vessels, for the purpose of interrupting the sudden communication of the heat, is placed a thin flat annular piece of cork, on which the superior vessel rests, the diameter

diameter of the cork being equal to the breadth of the inner compartment.

The exterior cylinder A E F D of the upper vessel, is continued as far as the points A and D; and the outer surface of this vessel, being broader than that of the inferior one, by the above-mentioned continuation, a cavity is formed, which is filled with plaster of Paris; and thus the two vessels are cemented together. Through a perforation in the cork placed between the vessels, a small tube passes, one extremity of which opens at the point *o*, into the compartment formed by the interior cylinders; and the other extremity extending through the plaster of Paris, reaches to a small distance beyond the outer cylinder of the superior vessel. In this tube a thermometer (*t v*) is inserted, which has the mercury contained in a thin cylinder, nearly equal in length to the transverse diameter of the inner compartment.

As the interior cylinders are equal in size, and as the lower vessel is kept in all its parts at the boiling, and the upper vessel at the freezing point, it may be inferred, that the thermometer (*t v*), which passes horizontally through the center of the space intercept-

ed between the cylinders, and which is consequently exposed to equal surfaces at 32 and at 212, will acquire a degree of heat that is precisely the arithmetical mean between those points; and if the fluid in the thermometer indicate likewise the arithmetical mean, we may conclude, that the expansion and the heat acquired are proportional to each other.

The vessel that has the temperature of 32, is placed over that which is at the boiling point, in order that an uniform diffusion of the heat may be produced, by the cold air descending from above, and intimately mixing with the warmer air below*.

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* Upon a principle similar to that which has been employed in the apparatus described above, I have contrived an instrument, by means of which fixed temperatures may be obtained at all the intermediate degrees of heat between the freezing and boiling points of water.

This instrument, like that which has been now described, consists of two double vessels, one of which is inverted upon the other, the superior being kept at 32 by means of pounded ice, and the inferior at 212 by the steam of boiling water. The interior cylinders of these vessels are formed of brass polished in the inside; and in each cylinder a moveable piston is placed; that which is inserted in the upper being kept at 32, and that in the lower at 212. Hence it is manifest, that by changing
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The relation between the expansions of mercury and the real increments of heat, as determined by this apparatus, will appear from the following experiment.

EXPERIMENT VIII.

The cylindrical mercurial thermometer described above, was inserted by means of a cork in the tube *g*, and the middle point of the cylinder containing the mercury was made to coincide as nearly as possible with the

the relative positions of the pistons, the proportions of the surfaces at 32 and at 212, which are intercepted between them, may be varied at pleasure.

When each piston is raised to the upper extremity of the cylinder in which it is contained, we shall then have the whole of the superior cylinder, which is at 32, exposed to the action of the inferior piston at 212. If we wish to have a degree of heat lower than this, two brass plates, having the temperature of 32, may be introduced horizontally at the bottom of the upper cylinder, and may be gradually made to slide over the surface of the lower piston, until at length they meet. Each of these plates should consist of two parallel plates, which are placed at a little distance from each other, and which are joined at their extremities by cross plates, the space intercepted between them being filled with pounded ice, for the pur-

the center of the space included within the interior cylinders. The exterior compartment of the upper vessel was then filled with snow at 32, and the outer compartment of the lower vessel was raised by the steam of boiling water to 212. In a short time the thermometer rose to 121, where it remained without variation during the space of 15 minutes, the experiment being then discontinued. When this experiment was made, the barometer stood at 29.6. The boiling point of the thermometer had been taken when the barometer was at 30 inches.

pose of retaining them at the fixed temperature of 32. By this means, a smaller portion of the surface of the lower piston will be exposed to the upper cylinder, in proportion as the plates approach towards each other, and when they meet, it will be wholly covered by them. The air in the upper cylinder will be then reduced to the freezing point, as it will be entirely surrounded by surfaces at 32. A similar apparatus may be employed to raise the air in the lower cylinder, by regular gradations, to the temperature of 212.

It is manifest that, upon the same principle, by means of the freezing, and boiling, points of other substances, instruments may be made for exhibiting fixed degrees of heat, which shall comprehend a much larger scale than the instrument now described.

In Experiment III. the deviation of the mercury from the arithmetical mean was .55 of a degree; the difference of temperature between the hot and cold water, previously to the mixture, having been nearly 100 degrees. Hence we may infer, that if the difference of temperature had been 180, the deviation would have been .99, which coincides very nearly with the result of the foregoing experiment.

When this experiment was repeated with unequal surfaces, the central heat was not found accurately to vary with the surfaces; the effect produced by the smaller surface being comparatively greater than that produced by the larger. For when the surface at 32 was so diminished as to become one half of that at 212, the heat at the centre, as might be expected, was increased; but the *increase* of the central heat was less than the diminution of the cooling surface. On the contrary, when the surface at 212 was diminished till it became one half of that at 32, the *diminution* of the central heat was less than that of the heating surface. The reason of which seems to be, that the superior and inferior surfaces have greater power
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in heating or in cooling the air than the lateral surfaces. For a current of warm air is continually ascending, by the lateral surfaces, and a contrary current of cold air is descending through the centre, as I have found by experiment; and the force with which these currents are impelled against the superior and inferior surfaces, by increasing the points of contact, increases the heating and cooling powers. In these experiments the capacity of the lesser vessel was diminished by shortening its lateral surface: hence the perpendicular altitudes of the two vessels were unequal, but their breadth was the same, and consequently the superior and inferior surfaces, on which the heating and cooling power principally depended, being equal, the heats did not vary in proportion to the variation in the size of the vessels. It is proper to add, that the refrigerating power of the colder surface was found to be somewhat greater than the heating power of the warmer, in similar circumstances. The cause of this difference it is difficult to determine; but from whatever cause it may have arisen, it leads to the following conclusion: since it is found that in similar circumstances,

circumstances,

cumstances, when the surfaces are unequal, the refrigerating power of the colder, is greater than the heating power of the warmer surface, it may be inferred, that if the surfaces be made to approach towards equality, the effect produced by the colder surface will not in any intermediate point be less than that produced by the warmer. Hence we may conclude with certainty, that when the surfaces become equal, the real heat applied to a thermometer that passes horizontally through the centre of the apparatus, will not be greater than the true arithmetical mean between the freezing and boiling points.

It is possible, however, that it may be a little less; for the refrigerating power of the ice in melting, may be greater than the heating power of the steam during its condensation. But it is, I think, evident, that the deviation produced by this cause, supposing it to exist, must be inconsiderable: for in order that two substances should transmit heat unequally, it is necessary either that their magnitudes, conducting powers, or capacities for heat should be unequal, or that the differences between their heats, and those of the substances with which they are in

contact, should be unequal. But in the above apparatus the heat passes from the steam to the ice, through substances which have equal magnitudes, capacities, and conducting powers. If, therefore, in the experiment in question, the heat had been transmitted unequally, the difference between the temperature of the superior cylinder and of the ice must have been greater or less than the difference between the temperature of the inferior cylinder and of the steam: that is, the inferior cylinder must have been more or less heated by the steam, than the superior cylinder was cooled by the ice. But as the cylinders are good conductors of heat, and are closely in contact with the ice and steam, which have fixed temperatures, it is evident, that they must acquire very nearly the temperatures of 32 and 212. If, however, from the greater refrigerating power of the ice, we conceive the difference between its temperature and that of the superior cylinder, to have been half a degree less than the difference between the temperature of the steam and of the inferior cylinder, the real heat at the centre would not be reduced by the operation

tion of this cause more than a quarter of a degree below the arithmetical mean.

The thermometer in Experiment VIII. stood at 121, which was one degree less than the arithmetical mean. And since it has been shewn, that the real heat, at the centre of the apparatus, in that experiment, was not greater than the arithmetical mean, and that it did not descend below it more than $\frac{1}{4}$ of a degree, we hence conclude, that the thermometer, in the experiment in question, did not deviate from the real heat more than *one* degree, nor less than $\frac{3}{4}$ of a degree.

It is proper to observe, that the boiling point of the cylindrical thermometer used in this experiment, was taken by immersing it in boiling water, when the barometer stood at 30 inches, the cylinder and a small part of the tube being covered with the water. Now if we suppose the expansions of mercury accurately to correspond with the real heats, it is manifest that a thermometer, which is thus graduated, must observe a decreasing progress, compared with the increments of heat. This clearly appears from the experiments and observations contained in Mr. Cavendish's excellent paper on the
method

method of graduating thermometers*. For let a thermometer, graduated as above, be conceived to be immersed in water at 32, the air in the room being also at 32, and let the tube be sunk to such a depth in the water, that the surface of the latter shall coincide with the 32d degree of the thermometer. If the temperature of the water be gradually increased, the thermometer being kept in the same position, a portion of the mercury will rise above the surface; and if at the same time the temperature of the air be not increased, that portion of the mercury which is exposed to its influence will have its expansion diminished.

Let the water, for example, be raised to 33, the air remaining at 32: it is manifest, that the mercury, which, by its ascension in the tube, is now exposed to the influence of the air, will be less expanded than if it were wholly immersed in the water†.

* See Philosophical Transactions, Vol. LXVI. p. 380.

† See the Report of the Committee for adjusting Thermometers, published in the Philosophical Transactions, Vol. LXVII. p. 516.

If the difference between the temperatures of the air and water be increased, the diminutions produced in the expansion of that part of the mercury which is surrounded by the air will likewise increase. And hence, in proportion as the thermometer rises, greater quantities of heat must be continually added to the water, in order to produce equal expansions in the mercury. The latter will therefore observe a decreasing progress, compared with the increments of heat.

Agreeably to this I have found, that when the cylinder and tube used in Experiment VIII. were completely surrounded with steam, according to the method recommended by Mr. Cavendish, the mercury rose nearly a degree and three fourths above the boiling point, which had been taken in the common method. And the same thermometer, at the intermediate division between the freezing and boiling points, stood nearly one degree lower than a thermometer which was graduated according to Mr. Cavendish's ideas, and which was completely immersed in water.

If allowance be made for this error, it will follow, that in Experiment VIII. the mercurial

curial thermometer indicated very nearly the arithmetical mean.

The thermometers employed in the experiments with warm and cold water, recited above, were reduced to the same scale, by comparing them with a standard, which, as well as that used in Experiment VIII. had the boiling point taken when the barometer stood at 30 inches, the bulb and a small portion of the tube being immersed in the water. These thermometers must, therefore, as was already shewn, necessarily observe a decreasing progress, compared with the real increments of heat. And hence we find, that, in the experiments alluded to, the thermometer which was employed to determine the temperature of the mixture, stood a little lower than the arithmetical mean. When this error was corrected, it appeared that the precise arithmetical mean was very nearly indicated in those experiments.

In making experiments on the heat of fluids, the cause of error now explained may be entirely avoided, by graduating the thermometer according to the method recommended by Mr. Cavendish, and by immersing it, in each experiment, to a depth
equal

equal to the elevation of the mercury. But in many experiments this is attended with great inconvenience, and in some it is impossible.

If the thermometer be graduated in Mr. Cavendish's method, and it be not in each experiment entirely immersed, the error will be increased: it will fall more below the real heat, than if a thermometer made in the common way had been employed.

It seems best, therefore, in experiments that do not admit of a total immersion, to employ thermometers graduated in the common method, and at the same time to find the boiling point by the method which Mr. Cavendish has proposed. And thus, as has been shewn by that philosopher, a scale of the errors may be formed, by which we shall be enabled to make a near approximation to the truth. But this scale of errors will not be perfectly accurate, unless in all experiments, the thermometer be immersed to the same depth, and the air have the same temperature that it had when the boiling point was originally taken. The inaccuracies, however, produced by changes of temperature in the air, if the experiments be not made in the extremes of

heat or cold, will be inconsiderable; and it is possible to make an additional scale of the errors arising from that cause, which may be employed in experiments that require very great accuracy.

The reader will observe, that there is a difference of nearly two degrees between the general result of the foregoing experiments, and those of Mr. De Luc on the same subject.

According to that philosopher, the mercurial thermometer, when it has acquired the degree of heat which is the arithmetical mean between the freezing and boiling points, stands at 38.6 of his scale, which is 118.85 of Fahrenheit's. By the experiments related above it stands at 121. The cause of this difference cannot easily be determined with certainty. Mr. De Luc himself supposes, that the real deviation from the arithmetical mean is not altogether so great as it appeared in his experiments, a small error being conceived to have arisen from the evaporation of the heated water, and allowance not having been made, excepting in one experiment, for the refrigeration produced by the external air. In the experiments with warm and cold

cold water recited above, particular attention was paid to these circumstances ; and as the thermometers which I employed were made with great care, and were reduced to a common scale, by comparing them with an accurate standard, I cannot help considering the results of these experiments as approaching nearly to the truth. And I am much confirmed in this conclusion, by their coincidence with Experiment VIII. which was made in such different circumstances, and in a manner so little liable to error.

The following experiment was made to determine the relation between the expansions of *water* and the increments of heat, by means of the apparatus used in Experiment VIII.

EXPERIMENT IX.

A water thermometer, that had the water contained in a thin flat tube, was inserted in the apparatus, the center of the tube being made to coincide with the center of the space intercepted between the interior cylinders, as in the former experiment.

In the course of a few minutes the water rose to 78, where it remained without variation.

For the purpose of making this thermometer, a tube was chosen which was nearly accurate, and the slight inequalities that occurred in it were compensated by the size of the divisions in the scale. The water, previously to its introduction into the tube, was freed from air, that it might bear the temperature of 212. It had the boiling point taken when the barometer was at 30, and the freezing point was found, by immersing it in water, cooled to 32.

The result of this experiment very nearly corresponds with that of Mr. De Luc on the same subject. For by Mr. De Luc's experiment, when the mercurial thermometer stood at 40 of his scale, (equal to 122 of Fahrenheit's) the water thermometer stood at 20.5 of his scale, which was equal to 78.125 of Fahrenheit's.

From Experiment VIII. it appears, that when the mercurial thermometer is exposed to a degree of heat that is intermediate between the freezing and boiling points of water, it indicates very nearly the arithmetical mean. Hence we may conclude, with
great

great probability, that the expansions of mercury are proportional to the quantities of heat applied, and consequently that the mercurial thermometer is an accurate measure of heat. And this conclusion is, I think, confirmed by the coincidence between the above-mentioned experiment, and those with warm and cold water formerly recited.

It moreover appears from Experiment IX. that the expansions of *water* do not correspond with the real increments of heat, the deviation from the arithmetical mean between the freezing and boiling points, in that experiment, being 44 degrees of Fahrenheit's scale.

5. The capacities of bodies for containing heat are nearly permanent, as long as they retain the same form.

The capacity of a body for heat is said to be permanent, when the same quantity of absolute heat which raises it one or two degrees, as measured by an equidifferential thermometer, at a given temperature, will raise it an equal number of degrees at all other temperatures. On the contrary, the capacity is said to be increased or diminished, by an alteration of temperature, when in consequence of such

an alteration a greater or a less quantity of absolute heat must be applied to it, in order to produce an equal effect upon the thermometer.

It has been proved by Experiment VIII. that the mercurial thermometer is nearly an accurate measure of heat; and from the experiments with warm and cold water, it appears, that when equal portions of this fluid are mixed together at different temperatures, the mercurial thermometer indicates very nearly the arithmetical mean. Hence it follows, that the capacity of water is permanent, in all the intermediate temperatures, between the freezing and boiling points. For if, by augmenting its sensible heat, the capacity of water were increased, an equidifferential thermometer, in experiments similar to those recited above, would point to *more*, and if diminished, to *less* than the arithmetical mean.

The truth of this will appear from the following propositions,

PROPOSITION I.

If two equal portions of the same fluid be mixed together at different temperatures, the
heat

heat will be equally divided between them ; or, in other words, the quantity of absolute heat imparted by the warmer to the colder fluid, will be half the difference of the separate heats.

There are three cases of this proposition :

1. The capacity of the fluid may be continually increased or continually diminished by an augmentation of temperature.
2. It may be alternately increased and diminished.
3. The capacity may remain permanent in all temperatures, while the fluid retains the same form *.

* In the two former of the above cases, the variation of capacity is supposed to be gradual. It is possible, however, that the capacity of a body may be *suddenly* varied by a change of temperature. Thus it has been proved by Dr. Irvine, that when water has arrived at the boiling point, it undergoes an instantaneous change of capacity in the moment of its conversion into vapour. But the supposition of a *sudden* variation of capacity cannot have place in the present instance ; because when the capacities of bodies are suddenly varied by an alteration of temperature, they are found to suffer a change of form, as appears from the discoveries of the above-mentioned philosopher. And consequently, if the temperature be altered while the form continues the same, the capacity must either be *permanent*, or it must be *gradually* increased or diminished.

Case 1. Let A and B be equal portions of any fluid, the capacity of which is conceived to be continually increased by a gradual augmentation of its temperature. Let the part A be supposed to have a higher temperature than the part B. It is affirmed, that if B and A be mixed together, and brought to a common temperature, A will communicate to B one half of the excess of its heat above the original heat of B.

For let the temperatures of A and B (see plate I. fig. 2 and 3.) be represented by the lines CH and LM; and let the ordinates uv , wx , yz , and ab , cd , ef , be drawn representing the capacities at different temperatures, the capacities at the point of total privation being expressed by the lines BC, KL; and let BG and KI be the lines joining the extremities of the ordinates.

It was observed above, that the quantities of absolute heat in bodies are proportional to the rectangles under the capacities and temperatures estimated from the point of total privation. If, therefore, the lines CH, LM, represent the temperatures, and the lines HG, MI, the capacities, the absolute heats will be represented by the rectangles
HR,

HR, MQ. When the bodies are mixed together, and brought to a common temperature, let the heat of A be diminished by the figure HER, and let that of B be increased by the figure OIP; and since the heat which is taken from A is the same with that which is added to B, it is manifest that HER must be equal to OIP. 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 FCSE, is equal to the figure OLPN. For the same reason the figure DT is equal to the figure MQ: consequently the remainder OIP or HER is equal to the remainder FAS. That is, A communicates to B one half of the excess of its heat above the original heat of B*.

* In the reasoning which has been employed to establish the above proposition, it is taken for granted, that no part of the heat which is absorbed by bodies, in consequence of an increase of capacity, enters into a chemical union with them.

The opinion of those philosophers who suppose that elementary fire is capable of chemically combining with bodies, does not appear to be founded upon decisive experiments. I shall endeavour to shew in the sequel, that we have no sufficient evidence for believing that bodies do, in any instance, enter into this peculiar mode of combination with that element.

If, therefore, any fluid have its capacity continually augmented by an increase of temperature, and if equal portions of this fluid be mixed together at different temperatures, the quantity of absolute heat imparted by the warmer to the colder fluid will be half the difference of the separate heats.

In like manner it may be proved, that if a fluid have its capacity continually diminished by an increase of temperature, and if equal portions of it be mixed together at different temperatures, the heat will be equally divided between them.

If the capacity be alternately increased and diminished by augmenting the temperature, as in the second case, the line joining the extremities of the ordinates which represent the capacities will be a waving line. If the capacity continue permanent, as in the third case, it will be a straight line. The demonstration of these cases is the same with that of the first. We may therefore conclude universally, that if two equal portions of the same fluid be mixed together at different temperatures, the quantity of absolute heat communicated by the warmer to the colder fluid, will be half the difference of the separate heats.

PRO-

PROPOSITION II.

If a fluid have its capacity continually increased by an augmentation of temperature, and if equal portions of that fluid, having different temperatures, be mixed together, the temperature of the mixture, as measured by an equidifferential thermometer, will be greater than the arithmetical mean: or in other words, the sensible heat imparted by the warmer to the colder fluid, will be more than half the difference of the separate heats.

For as the capacity of the fluid increases with the temperature, the quantities of absolute heat which are required to produce equal alterations of temperature will be continually increasing. Consequently, if the difference between the temperatures of the warmer and colder fluid be conceived to be divided into two equal parts, that is, if DF be equal to FH , (see plate I. fig. 2.) the quantity of absolute heat required to raise the fluid from D to F , will be less than that required to raise it from F to H . From which it follows, that if the line DH , representing the excess of the temperature of the warmer

warmer above that of the colder fluid, be conceived to be so divided in the point F, that the quantity of absolute heat which raises the fluid from D to F, shall be equal to that which raises it from F to H, HF must be less than FD. Hence if we take from the warmer fluid a quantity of heat, by which the excess of its absolute heat above that of the colder, shall be diminished one half, the difference of temperature will be diminished *less* than the half. In like manner it may be proved, that if we add to the colder fluid a quantity of absolute heat, equal to that which has been taken from the warmer, or to half the difference of the separate heats, its temperature will be augmented *more* than the half. But it was before shewn, that when the fluids are mixed together, the quantity of absolute heat which the warmer imparts to the colder, is precisely half the difference of the separate heats. From which it follows, that the diminution in the temperature of the warmer fluid will be less than the increase in that of the colder, and hence the temperature of the mixture will be greater than the arithmetical mean.

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By a similar mode of reasoning, it may be shewn, that if a fluid have its capacity continually diminished by an increase of temperature; and if equal portions of that fluid, having different degrees of sensible heat, be mixed together, the temperature of the mixture will be less than the arithmetical mean.

If the capacity were irregularly changed by an alteration of temperature, being in some parts of the scale increased, and in others diminished; and if the warmer fluid, previously to its mixture with the colder, were raised to a variety of different temperatures in a series of experiments similar to those recited above; in that case, an equidifferential thermometer would, in some experiments, indicate more, and in others less, than the arithmetical mean.

It appears, however, that in Experiments I, II, III, and IV. the arithmetical mean was very nearly indicated by the mercurial thermometer. And from Experiment VIII. it is inferred, that this instrument is nearly an accurate measure of heat. And since it has been shewn, that if the capacity of water were either *regularly* or *irregularly* varied

varied by an alteration of temperature, a thermometer which was an accurate measure of heat, would, in those experiments, have indicated more or less than the arithmetical mean, it follows, that the capacity of this fluid is permanent in all temperatures between the freezing and boiling points.

I may add, that the truth of these conclusions is much confirmed by the coincidence between the results of the above-mentioned experiments, and that of experiment VIII.

For if we suppose the capacity of water to vary in different temperatures, the results of the first seven experiments will oblige us to have recourse to the very improbable supposition, that these variations are precisely balanced by contrary variations in the expansions of mercury. And again, if we imagine the expansions of mercury to vary, the result of the 8th experiment obliges us to suppose, that this variation is balanced by a deviation of the true heat, at the center of the apparatus used in that experiment, from the arithmetical mean; which is also very improbable. But that a deviation from the true heat should moreover take place in the center of this apparatus,

paratus, precisely equal to the supposed variations in the capacity of water, implies such a coincidence of improbable events, that I think the contrary may be considered as certain.

The permanency of the capacity of water being proved, and the mercurial thermometer being shewn to be an accurate measure of heat, the influence of a change of temperature upon the capacities of other bodies, may be determined by comparing them with water as a standard. To illustrate this, let equal bulks of water and iron be supposed to be mixed together, the latter having a higher temperature than the former by fifty degrees. Let them be conceived to be mixed a second time, the excess of temperature in the iron being 100 degrees ; and let the sensible heat lost by the iron in the first instance be equal to that gained by the water. If the capacity of iron be permanent, the sensible heat which it loses in the second instance will also be equal to that which the water receives ; but if its capacity be increased, the diminution of temperature in the iron will be less, and, if diminished, *greater* than

than the augmentation of temperature in the water.

I have mixed most of the metals and their calces with water at different temperatures ; and from the general result of my experiments on this subject, I have the utmost reason to believe, that the capacities of bodies, while they retain the same form, are not liable to be varied by a change of temperature. For though many of the trials did not correspond accurately with that supposition, yet they agreed more nearly with it, than with any other. From which we may infer, that the supposition is true.

It is upon a mode of reasoning similar to this, that most of our conclusions are founded, respecting the existence of general laws. Astronomers, for example, do not conclude, that the heavenly bodies move in elliptical orbits, because their observations indicate that path precisely, but because they approach more nearly to it, than to the circular, or to any other line of motion. At the same time it must be acknowledged, that though perfect accuracy cannot be attained, yet in proportion as experiments make a nearer approximation to that point, the inferences deduced

duced from them will be *more* certain, and *less* labour will be required in tracing them to first principles.

From the foregoing experiments and observations, we may conclude in general, that the capacities of bodies which retain the same form, are permanent in the intermediate temperatures, between the freezing and boiling points of water. Hence we may infer by induction, that the same law obtains throughout the whole of the scale of heat.

The reliance which we are disposed to place upon the evidence of inductive reasoning, is confirmed by the universal experience of mankind, which proves that the operations of nature are simple and uniform, that similar effects proceed from the agency of similar causes, and that all the phænomena of the universe, as far as they fall within the reach of human observation, are governed by general laws. Thus if it be found by experiment and observation, that the forces of the sun's attraction to some of the planets are inversely as the squares of the distances, we infer, from the simplicity of the operations of nature, that this law prevails throughout the whole of the planetary system, and

governs the motions of all the bodies that revolve around the sun as a center. In like manner if it be found, in those temperatures which come within the reach of our observation, that while the forms of bodies continue the same, the capacities are permanent, we conclude by induction, that this is a general law of nature, which extends throughout the whole of the scale of heat, even to the point of total privation.

As this conclusion is founded upon an extensive series of experiments, and as it affords a simple interpretation of the phenomena, it may, I think, be considered as a legitimate induction. And if any deviation from the general law should afterwards appear in particular instances, it must be admitted with such exceptions as occur *.

By the permanency of the capacities of bodies for heat, we are enabled to account for the regularity with which they give off heat when placed in a cold medium.

The laws of the refrigeration of heated bodies were first investigated by Sir Isaac Newton. This illustrious philosopher supposed, with great probability, that the quan-

* See Newton's Optics.

tities of heat lost by a body in small portions of time, would always be proportional to the excess of its temperature above that of the surrounding medium. Thus, if the heated body were 180 degrees warmer than the atmosphere, the quantity of heat which it would lose in a given moment, would be double that which it would lose in an equal portion of time, if it were only 90 degrees warmer than the atmosphere. From which it follows, that if the times were taken in arithmetical progression, the decrements of heat would be in geometrical progression, and the heats remaining, considered as the differences between the temperature of the body and of the external air, would also observe the same law.

Dr. Martine has shewn, that this rule, though nearly accurate, is not to be admitted without some restriction. From *his* experiments, compared with those of Muschenbroke, it appears, that the decrements are in a proportion somewhat greater than the remaining heats, and consequently the quantities of heat lost are partly equable, and partly in geometrical progression.

It appears, therefore, that bodies are cooled according to the same fixed laws : from which it follows, that we must either admit the capacities to be permanent, or must have recourse to the very improbable supposition, that the changes of capacity produced by alterations of temperature, are precisely similar in all the various classes of natural bodies.

For if the changes of capacity were dissimilar, the laws of refrigeration would vary in different substances. To illustrate this, let B and C be two bodies, the former of which has its capacity uniformly diminished, and the latter uniformly increased by a diminution of temperature. If these bodies be heated and placed in a cold medium, the refrigeration of the first will be proportional to the *joint effect* of two causes which act in conjunction with each other : for B will be cooled both by the refrigerating power of the ambient medium, and by the absorption of heat arising from the increase of its capacity : the refrigeration of the second, on the contrary, will be proportional to the *difference* between two causes which act in opposition to each other, for C will be continually deprived of heat by the cooling power of the
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surrounding medium, and it will be continually supplied with heat from an internal source, in consequence of the diminution of its capacity. Hence the cooling power of the ambient medium, will in the first instance be aided by the absorption of heat; and in the second instance, it will be counteracted by the exhalation of that principle. The quantities of heat lost therefore by B, in small intervals of time, will be in a *greater* proportion than the heats remaining, considered as the differences between its temperature and that of the external air; and the quantities lost by C will be in a *less* proportion than the heats remaining *. The laws of refrigeration in B and C will therefore be different. But these differences are not found to take place in nature.

It has moreover been proved, that the laws of the refrigeration of water, are the same with those which obtain in the cooling of other fluid as well as solid substances. And

* It is here supposed agreeably to the rule laid down by Sir Isaac Newton, that if the capacities were permanent, the quantities of heat carried off by the surrounding medium in small successive intervals of time, would be in geometrical progression.

since it has been shewn by experiment, that the capacity of water for containing heat is permanent, we may infer, that in other substances, while the forms are unchanged, the capacities are also permanent.

Future experimentalists may perhaps discover some exceptions to this law. From the cold produced by the sudden expansion of aerial fluids, it is probable, that the capacities of those substances are a little increased by an augmentation of temperature; but it will be shewn in the sequel, that if such exceptions to the general law do exist, the effects which they produce are very inconsiderable.

It was formerly proved, 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 form being supposed to remain unaltered, the quantities of absolute heat will be in proportion to the temperatures, as measured by an equidifferential thermometer, and com-

puted from the point of total privation. Hence if the temperature of a body, estimated as above, 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.

6. When in consequence of exposure to heat, bodies that were previously solid are converted into fluids, they absorb a quantity of heat, which is necessary to their existence in a fluid form, but which does not increase their temperature: on the contrary, when the same bodies are re-congealed by exposure to cold, they part with the heat which in the process of melting they had formerly absorbed.

This important fact was observed by Mr. De Luc, in the years 1755 and 1756, as we learn from his work on the atmosphere, and from his late publication on meteorology. It appears to have been discovered nearly at the same time by Dr. Black of Edinburgh, who had no opportunity of becoming acquainted with what Mr. Luc had done, and who taught it publicly in his Chemical Lectures, as early as the year 1757 or 1758. It is proper to add, that Dr. Black was the first

who compleatly established this discovery by a series of ingenious and decisive experiments. These experiments have lately been communicated to the public by Mr. De Luc, in his excellent Treatise on Meteorology; and as they will throw much light on the subject of the following dissertation, I shall lay a brief account of them before the reader*.

If a pound of water at 32, be mixed with an equal quantity of that fluid at 172, by an equidifferential thermometer, the temperature of the mixture will be 102, which is the arithmetical mean between the heat of the warm water and of the cold. But if a pound of ice at 32, be mixed with a pound of water at 172, the temperature of the mixture will be 32. Hence it appears, that ice and water at the freezing point are very differently affected by heat. For in the first experiment, a quantity of heat which raised the thermometer 70 degrees; passed from the warm water into the cold, and by this heat the temperature of the cold water was increased 70 degrees. In the last experiment, a quantity

* A very accurate account of the Doctrine of Latent that has been also published by Mr. Wilkie, in the Swedish Transactions. See Acta Succ. 1772, p. 97.

which

which raised the thermometer 140 degrees, passed from the warm water into the ice, in consequence of which the latter was melted, but its sensible heat was not increased, the temperature of the mixture being 32. Whence it follows, that, in the melting of ice, 140 degrees of heat are absorbed, which do not produce any effect upon the thermometer, and which, as far as respects that instrument, may be said to exist in the water in an insensible state.

The heat which the water absorbs, when it acquires its fluid form, is again separated from it by congelation, as appears from the following experiment. If a pound of water at 32, be mixed with an equal quantity of ice at 4, nearly $\frac{1}{3}$ of the water will be frozen, and the temperature of the mixture will be 32. In this experiment the ice is raised from 4 to the freezing point: it is therefore evident, that by the congelation of nearly $\frac{1}{3}$ of a pound of water, a quantity of heat is evolved, sufficient to raise a pound of ice 30 degrees. It is proper to observe, that the quantity of water frozen in this experiment, is somewhat less than $\frac{1}{3}$ of a pound. Let it be conceived, however, for the sake of illustration,

tration, to be precisely that quantity. It is manifest, that if, by the congelation of $\frac{1}{3}$ of a pound of water, a quantity of heat be detached, sufficient to raise the ice in the above experiment 28 degrees, by the congelation of a pound of water, a quantity of heat would be detached, sufficient to raise it five times 28 degrees. Allowance being made for the fractions, which have been neglected in this illustration, and for the different capacities of ice and water, it will appear, that the heat which is extricated by the congelation of water, is precisely equal to that which is absorbed by the melting of ice. It is, indeed, perfectly agreeable to the simplicity of nature, that the heat which bodies absorb in consequence of a change of form, should re-appear when they undergo a contrary change.

Hence we may perceive the reason of the following phænomenon, which I believe was first observed by Dr. Black. If water that is freed from air, and which is perfectly at rest be exposed to the atmosphere when it is colder than 32, it will sink several degrees below the freezing point, and at the same time will retain its fluid form, no part of it being congealed.

gealed. If it then be slightly agitated, a portion of it will suddenly become solid, and the temperature of the mixture, now consisting of ice and water, will rise to 32.

The reason is obvious: water exposed to a degree of cold below 32, does not freeze without the concurrence of a slight agitation. When, therefore, water that had been previously cooled below 32, is agitated, a part of it freezes, and the portion that becomes solid gives off a quantity of heat, sufficient to raise the whole to the freezing point.

If the quantity of water, and the temperature to which it was reduced, previously to its agitation, be known; and if the quantity that is congealed, upon shaking it, be determined, the degree of cold necessary to freeze the whole may be found.

For the sudden freezing of a part of the water in this experiment, arises from its having been reduced to a lower temperature than 32, prior to its agitation: and since effects are proportional to their adequate causes, it is manifest, that if the temperature of the water be more and more diminished in different experiments, the quantity of ice produced by the agitation, will always be
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proportional to the previous decrement of the temperature below the freezing point. That is, if a decrement of temperature, as 10, below 32, freeze a quantity of the water, as 1, a double quantity will be frozen by a double decrement, and so in proportion. Let the quantity of water be called Q , let the degree of cold necessary to freeze the whole be S , let any other degree of cold less than S be d , and let the ice produced by that degree be m . Then $m : d :: Q : S$; and if m , d , and Q , be given, S will also be given, for it will be equal to $\frac{dQ}{m}$. That is, if the

quantity of ice produced as above by any given degree of cold, be determined by experiment, the degree of cold necessary to freeze the whole, may be found by calculation. Whence the comparative heats of ice and water will also be given: for the comparative heat of water will be to that of ice, as the degree of cold necessary to freeze the whole, is to the heat absorbed by water in the melting of ice.

It has also been proved by Dr. Black, that when water is converted into vapour, a quantity of heat is absorbed, which is neces-

fary to the existence of the vapour in the form of an elastic fluid, but which does not increase its temperature.

This ingenious philosopher was first led to the discovery of the absorption of heat by aqueous vapour, in consequence of an unexpected appearance which occurred in an experiment made with water at a high temperature. A quantity of that fluid having been raised in Papin's digester to a temperature many degrees above the boiling point, was suffered to communicate with the external air by opening a stop cock, upon which a part of it was instantly converted into vapour, and the water at the same moment sank to 212. Whence it was concluded, that the vapour produced in the experiment absorbed a quantity of heat from the water, by means of which the temperature of the latter was suddenly reduced to the boiling point. The truth of this conclusion was afterwards fully established by the same philosopher.

The following experiments appear to prove it decisively.

If eight pounds of the filings of iron at 212, be mixed with a pound of water at 32, the temperature of the mixture will be nearly

122; the iron will be cooled 90 degrees, and the water heated 90. But if eight pounds of iron filings at 300, be mixed with a pound of water at the boiling point, the temperature of the mixture will be 212, and a part of the water will be suddenly converted into vapour. If a very sensible thermometer be suspended in such a manner as to be in contact with the vapour that is thus produced, the latter will also be found to have the temperature of 212. In this experiment, 88 degrees of heat are separated from the iron, by which a portion of the water is elevated in the form of vapour, but its sensible heat is not increased, the mixture having the temperature of 212, and the vapour that escapes during the experiment being also at the boiling point. Hence, as the heat separated from the iron is not communicated to the water, it must necessarily be absorbed by the vapour. In this conclusion we are confirmed by observing, that the heat which is thus absorbed is again separated from the vapour when it is condensed.

We are informed by Mr. De Luc, in his Treatise on Meteorology, that the quantity of heat produced by the condensation of a
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given weight of vapour, as determined by the experiments of Mr. Watt, would be sufficient to raise an equal weight of a non-evaporal substance having the same capacity with water, 943 degrees*.

From the heat which is evolved in the freezing of water, we may perceive the reason why that fluid, when it is exposed to a degree of cold lower than 32, and is at the same time gently agitated by the wind, ceases to cool as soon as it arrives at the freezing point, its temperature remaining stationary, until the whole is congealed. For it is well known, that 32 is the point at which water, in a state of gentle agitation, becomes solid. When, therefore, it is reduced to that point, it begins to freeze, and consequently to evolve heat.

As the evolution of heat in this instance immediately depends upon the congelation, it is manifest, that the quantity of heat evolved in a given time, will be in proportion to the quantity of water congealed. But the congelation of the water arises from the refrigerating power of the ambient medium;

* See *Idees sur la Meteorologie*, tome premier, p. 224.

the quantity of water congealed will therefore always be proportional to that power. If, for example, by exposure to a medium 10 degrees below the freezing point, a quantity, as 1, be frozen in a given time, by exposure to a medium 20 degrees below the freezing point, double that quantity will be congealed in an equal time.

Whence it appears that water, in congealing, is acted upon by two opposite powers ; it is deprived of heat, by exposure to a medium which has a lower temperature than 32 ; and it is supplied with heat, by the evolution of that principle in consequence of the congelation. As these powers are precisely equal to each other, the temperature of the freezing mixture must remain unvaried.

In like manner as if water at the same moment be permitted to enter into a vessel by one aperture, and to escape from it by another ; and if the quantity which escapes be equal to that which enters, the height of the water in the vessel will continue stationary.

Hence also we may perceive the reason why melting ice has a fixed temperature. For such is the nature of this substance, that
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it cannot acquire a greater heat than 32, without becoming fluid ; and it was proved, that the water which is formed by the melting of ice, absorbs, at the moment of its formation, a quantity of heat which does not increase its temperature.

When, therefore, ice is placed in a medium warmer than 32, as it cannot rise above that point without melting, the heat which it receives will be wholly employed in changing it into a fluid, and will consequently be rendered insensible.

To the operation of the same causes it is owing, that the temperature of water boiling, as well as of steam during its condensation, is stationary, the absorption of heat in these processes being equal to the supply. Hence it may be inferred in general, that if bodies, while they are undergoing a change of form, in consequence of exposure to heat or cold, have a fixed temperature, they either absorb or evolve heat. Moreover, if the temperature remain fixed, while the body is exposed to a cold medium, we may conclude that it is evolving heat ; and if, on the contrary, the temperature be unaltered, while it is ex-

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posed to a warm medium, we may infer that it is absorbing heat.

Dr. Black has found that many substances, as tallow, spermaceti, bees-wax, rosin, have a fixed temperature, when they are undergoing a change of form, by melting, or by congelation. Whence he infers, that during the former process they absorb, and during the latter, evolve heat.

From these facts and observations we may conclude universally, that *when bodies, in consequence of exposure to heat, have arrived at the melting or boiling points, they absorb a quantity of heat, which is necessary to their existence in the state of non-elastic fluids, or of vapour, but which does not increase their temperature: and, on the contrary, when vapours are condensed, or non-elastic fluids are congealed, they part with the heat which they had formerly absorbed.*

The following proportions obtain with regard to the congelation of fluids, and the melting of solids, as well as to the elevation and condensation of vapours.

It is manifest, that in the congealing of different portions of the same fluid, if the times of the congelation be equal, the quantities congealed will be directly proportional

to the power of the cooling cause, or to the velocity of the separation of heat. If, for example, a certain quantity of water be conceived to be frozen by a cooling cause, which separates the heat with a velocity as 1, it is evident that the velocity of the separation of heat being doubled, a double quantity will be frozen in an equal time.

Let T be the time of the congelation, Q the quantity of matter, and P the power of the cooling cause. If T be given, Q will be as P . Again, if the power of the cooling cause be given, the quantity congealed will be as the times of the congelation; that is, if P be given, Q will be as T ; and if neither be given, Q will be as $P \times T$, consequently T will be as $\frac{Q}{P}$, and if Q be given, T will be reciprocally as P . That is, if the quantities congealed be given, the times of the congelation will be inversely proportional to the power of the cooling cause; in other words, the time required to freeze a given quantity of the fluid, will be diminished in proportion as the power of the cooling cause is increased. Hence the congelation will be momentaneous, or the fluid will be entirely

changed into a solid at the same moment, if the power of the cooling cause be such as instantly to separate the whole of the heat evolved in the congelation. Moreover, if the velocity of the separation of heat be given, the times required to congeal equal quantities of different fluids, will be in proportion to the quantities of heat which they give off in the freezing process. If, for example, the heat given off by water in freezing be to that given off by the congelation of an equal quantity of spermaceti, as 2 to 1; and if the velocities of the separation of heat be equal, the time required to change the former into a solid will be double that which is required to produce the same change in the latter.

Hence the quantities of heat evolved by the freezing of different fluids may be inferred from the times of their congelation, when they are exposed to cooling causes, the powers of which are equal.

It was before proved, that if bodies retain the same form, their capacities for heat are not varied by a change of temperature. From the experiments of Dr. Irvine, there is the utmost reason to believe, that if by a
change

change of temperature the forms of bodies be altered, their capacities for heat are increased or diminished, in consequence of which they must necessarily absorb or evolve heat. And thus the celebrated discovery recited above, is traced to a general law of nature. I shall not, however, enter more particularly into this subject at present, but shall leave the farther illustration of it to the learned Philosophers to whom the honour of the discovery is due.

V. Unequal quantities of absolute heat are required to produce equal alterations of temperature in equal weights of heterogeneous bodies.

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 an equidifferential thermometer, it will be found that unequal quantities of absolute heat have been communicated to the water, and to 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 through 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 experiments, 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 Fahrenheit, 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 the arithmetical mean between the heat of the warmer and colder fluid.

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

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 the arithmetical mean.

On the other hand, if the mercury be hotter than the water, the temperature of the mixture will constantly be *less* than the arithmetical mean. 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 through 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 which are produced in their *sensible* heats, when they are mixed together at different temperatures*.

* 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.

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

If four pounds of diaphoretick antimony at 20, be mixed with one pound of ice at 32, the temperature of the mixture will be 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 four 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 four 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 the arithmetical mean between that of the warmer and colder substance. And since the capacities of bodies are permanent as long as they retain the same form, we infer, that the result would be similar, 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 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, previously 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 four pounds of antimony six degrees. And as the capacities of bodies, while they retain the same form, are not altered by a change of temperature, it follows that the same quantity

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

Again, if a pound of ice at 32, be mixed with a pound of antimony at 7, the temperature of the mixture will be 27; the ice will be cooled five degrees, and the antimony heated 20; 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 four. 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 equal weights of ice and antimony, having the same temperature, are reciprocally proportionable

portionable to the changes produced in their sensible heats, when they are mixed together at different temperatures*.

This conclusion appears to be justly deduced from the premises. The reasoning which has been employed to establish it, is founded upon the necessary proportion and connection between causes and effects, and is altogether independent of any hypothesis respecting the nature of heat.

It may be considered as a particular application of the following universal theorem. If equal causes, operating upon different classes of bodies, produce unequal effects, in order to the production of equal effects, unequal causes will be required: in other words, if the effects vary when the causes are equal, the causes will vary when the effects are

* 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.

equal;

equal; and if, in each class of bodies, the effect be proportional to its cause, the variations of causes and effects, in the different classes, will be reciprocally proportional to each other. The proof is the same with that of the foregoing proposition, substituting the term *effect* for temperature, and the term *cause* for absolute heat.

This reasoning will apply not only to the investigation of the comparative heats of bodies, but also to all other branches of knowledge in which the above-mentioned reciprocation of causes and effects takes place. It may be extended, for example, to menstruums and solvents in chymistry, to forces and the motions produced by them, in natural philosophy, as well as to the diminutions of forces, and the powers by which they are diminished. Thus let A and B be two bodies, and let C be a moving force; let this force be such, that the double, treble, quadruple, or $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, of it may be taken. The quantities of the moving force required to produce equal velocities in A and B, will be reciprocally as the velocities communicated to those bodies when the moving forces are equal. This necessarily follows from what
has

has been demonstrated by natural Philosophers respecting forces, and the velocities which they communicate. For let equal forces be applied to A and B, and let the velocity of the former be to that of the latter, as 2 to 1; since, by the second law of nature, in the motions communicated to the same body, the forces are as the velocities, if the velocity of A be conceived to be diminished the one half, or to become equal to that of B, the force required to produce this velocity will be diminished in the same proportion; consequently when A and B move with equal velocities, the force applied to the former will be half of that applied to the latter. Hence it appears, that if with equal moving forces, the velocities be as 2 to 1, with equal velocities, the moving forces will be as 1 to 2. The same reasoning will apply to menstruums and solvents.

Thus let A and B be two menstruums, and let C be a substance to be dissolved in them; let the degrees, by which A and B approximate to saturation by given additions of the solvent, be supposed to be known. If we find in the several intermediate degrees, between the points of saturation and of total privation,

privation, that the same quantity of the solvend which makes A approximate one degree towards saturation, will make B approximate two degrees, we may conclude that the dissolving power of the former is to that of the latter, as 2 to 1; and if A and B be both saturated, or have made equal approaches towards saturation, the quantity of the solvend contained in A will be double that contained in B.

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 chemical action.

S E C T I O N II.

Having 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

ly

ly 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; 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 heat in bodies, by mixing them together at different temperatures, are liable to several causes of inaccuracy. 1. When the substance

stances 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.

It was formerly remarked, that when a heated body is placed in a cold medium, the decrements of heat are partly equable, and partly in geometrical progression. If the body transmit heat very fast, and its temperature be much greater than that of the ambient medium, the decrements are nearly in geometrical progression, agreeably to the rule laid down by Sir Isaac Newton; the error produced by calculating according to this rule being so 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. When the decrements of heat were found to be sensibly different from each other, the heat lost in the first minute was calculated according to Sir Isaac Newton's rule, from the series of numbers determined by observation.

But in general the experiments were made under such circumstances, that the heat
passed

passed off very slowly; in which case the difference between the decrements is so small, as not to be distinguished by the eye. And, therefore, the heat lost in the first minute may, without any sensible error, be considered as equal to that lost in those which immediately follow.

I must here observe, that I believe Dr. Irvine was the first who applied the above-mentioned rule of Sir Isaac Newton, to calculate the heat lost during the first minute, in experiments for determining the comparative quantities of heat in bodies.

2. If the experiment be made by adding the warmer substance to the colder, the former will lose a part of its heat in passing through the air. To avoid this cause of inaccuracy, the experiment should be reversed: the colder substance should be taken at the temperature of the air in the room, and in that state being mixed with the warmer, it cannot, during its passage through the air, lose any part of its heat.

3. If 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 ex-

periment 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; and the capacity of the vessel being known, its influence upon the temperature of the mixture may be found. Let a pound of water, for example, be one of the substances, whose comparative heat is to be examined; and let the capacity of the water be to that of the vessel as 12 to 1. If in any experiment we find that the vessel has received 12 degrees of heat from a pound of water, we may be sure that the separation of this heat has cooled the water one degree.

4. In experiments for determining the comparative quantities of heat in 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.

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.

To remedy this cause of error, a moderate agitation should be used, and the experiment should be made, in such circumstances, that the mixture shall cool very slowly, in order that as small a portion of the heat as possible may escape, while its parts are acquiring a common temperature.

The mixture may be made to cool slowly, if the substances, that are to be mixed together, be taken in considerable quantity, and if the vessel in which the mixture is contain-

ed be furrounded on all sides with a covering that slowly transmits heat.

It is moreover evident, that the mixture will cool slowly if its temperature do not much exceed that of the external air. The excess of the temperature of the mixture above that of the surrounding air, all other circumstances being equal, will be in proportion to the difference between the temperature of the colder and warmer substance, the former being supposed, previously to the experiment, to have the same heat with the ambient medium. It is, therefore, proper, with a view to diminish the velocity of the refrigeration, that the warmer substance should not be raised to a very high temperature. On the contrary, if the difference between the heat of the warmer and colder substance be very small, those substances will suffer but little alterations in their temperatures when they are mixed together. But these alterations are the measures of the capacities; and if the measures of the capacities be greatly diminished, it is manifest that the unavoidable errors, which are produced by the imperfections of the senses, and of thermometers, will render the results of the experiments,

ments, and the calculations which are deduced from them, precarious.

Hence it follows, that in trials made with a view to determine the comparative heats of bodies by mixture, very small as well as very great differences of temperature should be avoided. There is, however, a certain limit between these extremes which will be least liable to error; and this limit is to be determined by varying the difference between the temperatures of the colder and warmer substance in different experiments, and observing those results which most nearly coincide with each other. If accurate thermometers be used, and if the other sources of fallacy be carefully avoided, the results of the trials, made with considerable variations of temperature, will be nearly the same, as appears from the following experiments:

EXPERIMENT I.

Air in the room	—	65
Thirty-six ounces troy of water, at	123	
were introduced as speedily as		
possible into an earthen vessel at	65.2	
The temperature of the water was measured		
by a thermometer, which had the mercury		
H 4		con-

contained in a small cylinder, that reached from the surface to the bottom.

At the end of

1	minute it was	—	116.2
2	—	—	115.1
3	—	—	114.5
4	—	—	114 minus.
5	—	—	113.5
6	—	—	113

The vessel in which this experiment was made weighed 30 ounces and $\frac{3}{4}$ troy. It was three inches and $\frac{5}{8}$ in diameter, and nine inches in depth.

From the experiment it appears, that the water lost in the course of the second minute a little more than a degree, and in the four succeeding minutes it cooled very nearly at the rate of half a degree in a minute. Hence it follows, that the water and the vessel had not acquired a common temperature till the second minute was expired, the earthy matter of which the vessel was composed being a bad conductor of heat, and its thickness being nearly one fourth of an inch. As the heat lost, when the progress of cooling became regular, was half a degree during each minute, we may safely conclude, that the
quantity

quantity of heat carried off by the air, in the first and second minutes, was nearly one degree. Adding therefore one degree to 115.1, which was the temperature of the water at the end of the second minute, we have 116.1 for the true temperature of the water and vessel. Subtracting this from 123 we have 6.9 for the remainder. The water was therefore cooled by the vessel 6.9. The vessel was raised from 65.2 to 116.1 or 50.9; and since it received this heat from the water, it is evident that the same heat which changes the temperature of 32 ounces of water 6.9, will change the temperature of the vessel 50.9. And, by a parity of reasoning, the same heat which raises the water one degree, will raise the vessel 7.37. The capacity of the water is consequently to that of the vessel as 7.37 to 1.

EXPERIMENT II.

Air in the room	—	65
Thirty-two ounces of water		
at	—	151.75
were poured into the earthen		
vessel at	—	65.25
		The

The temperature of the
water in

1 minute was	—	141 plus
2 minutes	—	139.75
3	—	138.5
4	—	137.5
5	—	136.5
6	—	135.5
7	—	134.5 plus
8	—	133.7 plus

After the third minute was expired, the progress of cooling was nearly one degree in a minute. If, therefore, we allow three degrees for the heat carried off by the air during the first three minutes, we shall have 141.5 for the true temperature.

Hence it appears, that the water was cooled by the vessel 10.25, and the vessel heated by the water 76.25. By this experiment the capacity of the water was to that of the vessel as 7.33 to 1.

EXPERIMENT III.

Air in the room	—	66.5
Thirty-two ounces of water at		179
were poured into the earthen vessel		
at	—	66.5
		In

In 1 min.			the temperature was	164.4
2	—	—		162.2
3	—	—		160.6
4	—	—		159
5	—	—		157.5
6	—	—		156 plus
7	—	—		154.6

After the third minute was expired, the progress of cooling was nearly 1.5 during each minute. If we allow 4.5 degrees for the heat lost in the first three minutes, we shall have 165.1 for the true temperature. The water was consequently cooled by the vessel 13.9, and the vessel heated by the water 98.6.

By this experiment, therefore, the capacity of the water is to that of the vessel as 7.09 to 1. Hence it follows, that when the difference between the temperature of the water and vessel previously to the experiment is increased, the capacity of the former compared with that of the latter, is apparently diminished. The reason is obvious: in proportion as the water is raised to a higher temperature, more heat escapes during its passage through the air; and if allowance be not made for this difference, a greater quantity of heat will appear to be separated by the vessel.

vessel. It is manifest, however, from the near coincidence of the results of the two first experiments, that the fallacy arising from this cause, when the water is not raised to a high temperature, is inconsiderable.

It was before observed, that the mixture will cool slowly if its temperature do not much exceed that of the external air. It may be proper to add, that if the colder substance be taken at a lower temperature than the air in the room, and the warmer substance at a higher; and if the difference of their heats be so proportioned, that the common temperature, after they are mixed together, shall be nearly the same with that of the air in the room; the fallacy, arising from the refrigeration of the mixture, will be altogether avoided, at the same time that a large scale may be obtained.

5. The volumes of the substances, which are to have their heats compared, should be as nearly equal as possible. For if the volumes be unequal, as the several parts of the warmer and colder substances cannot be brought into immediate contact with each other, the difficulty of diffusing the heat uniformly over the mixture will be increased.

Let

Let water, for example, and sand, be the substances which are to have their comparative heats examined. If the volume of the sand be less than that of the water, the depth of the latter, when they are mixed together, will exceed that of the former; and if it be greater, the reverse of this will take place. In both cases it will become more difficult to bring the sand and water to a common temperature.

If the volume of the water exceed that of the sand, and if, in repeating the experiment, the comparative bulks of these substances be varied, an equal degree of agitation being given to the mixture, the capacity of the water, compared with that of the sand, as determined by a thermometer which reaches from the surface to the bottom, will appear to diminish when the volume of the sand is diminished. This is proved by the following experiments:

EXPERIMENT I.

Air in the room	—	49
Thirty-eight ounces of common		
sand at	— —	53 $\frac{1}{2}$
		Mixed

Mixed with $16\frac{1}{2}$ ounces of wa-

ter at — — 149

The mixture being agitated for a
short time, its temperature in

1	minute was	—	117 $\frac{1}{2}$
2	—	—	117 $\frac{1}{2}$
3	—	—	117 $\frac{1}{4}$
4	—	—	116 $\frac{1}{4}$
5	—	—	116 $\frac{1}{4}$
6	—	—	115 $\frac{3}{4}$
7	—	—	115 $\frac{1}{2}$
8	—	—	115 plus
9	—	—	114 $\frac{3}{4}$
10	—	—	114 $\frac{1}{4}$ plus

EXPERIMENT II.

Air in the room — 61.5

Twenty-two ounces of com-
mon sand at — 62 $\frac{1}{4}$

Mixed with 22 ounces of
water at — 143 plus.

The mixture being agitated as
before, its temperature in

1	minute was	—	128 minus
2	—	—	127 $\frac{1}{2}$ minus
3	—	—	127 minus
			4 minutes

4 minutes was	—	—	126 $\frac{1}{4}$
5	—	—	125 $\frac{1}{4}$
6	—	—	124 $\frac{1}{4}$
7	—	—	123 $\frac{1}{2}$
8	—	—	122 $\frac{3}{4}$
9	—	—	122
10	—	—	121 $\frac{1}{4}$
11	—	—	120 $\frac{1}{2}$
12	—	—	120
13	—	—	119 $\frac{1}{2}$

The agitation given to the mixture in both experiments was nearly equal. I found from subsequent trials that the heat imparted to the air by the agitation was nearly one degree. The experiments were made in the same vessel, the heat of the mixture was measured by cylindrical thermometers, that reached from the surface to the bottom, a smaller thermometer being used for the first experiment than for the second. Allowing one degree for the heat lost during the first minute, the capacity of water will be to that of the sand, by the first experiment, as 5 to 1; and by the second as 4.7 to 1. Thus it appears, that in the foregoing experiments with sand and water, the capacity of the latter was

was apparently diminished, when the comparative bulk of the former was diminished.

This fallacy arises from the increased difficulty of bringing the water and sand to a common temperature. For as the capacity of water is greater than that of sand, when these substances are mixed together, the common heat of the mixture will be nearer to the original temperature of the water than to that of the sand. If the sand and water be not brought to a common temperature, in other words, if the temperature of the water at the surface exceed that of the sand at the bottom, a cylindrical thermometer, the length of which is equal to the depth of the mixture, will give a degree of heat that is intermediate between the temperature at the surface at the bottom.

But this intermediate temperature will be less than that which would be produced, by bringing the sand and water to a common heat; because as the capacities are different, if the parts of the mixture were reduced to a common heat, the increase in the temperature of the sand would be greater than the diminution in that of the water.

Hence

Hence the water will be seemingly more refrigerated, and the sand less heated; and consequently the relative capacity of the former will appear to diminish, and that of the latter to increase, in proportion as the heat is more unequally diffused over the mixture; and since the inequality in the bulk of the substances compared contributes to a more unequal diffusion of the heat, it follows, that an increase in the excess of the volume of the water above that of the sand, all other circumstances being equal, must be accompanied with an apparent diminution in the relative capacity of the water. Hence, by taking volumes perfectly equal, we shall have the limit at which the relative capacity of water will appear to be the greatest, and which will consequently give us a result that will approach nearest to the truth. For in that case the above-mentioned cause of fallacy, tending to make the *apparent* proportion of the capacity of water to that of sand less than the *real* proportion, will be most effectually avoided.

From the foregoing observations it follows, that, in experiments for determining the comparative heats of bodies by mixing

I them

them together at different temperatures, if all other circumstances be equal, the comparative heat of that which has the greater capacity will appear to diminish in proportion as the heat is more unequally diffused over the mixture. And as solid bodies have less capacities for heat than water, it likewise appears, that in experiments for ascertaining the capacities of such bodies by mixing them with this fluid, if the latter have the greater volume, the relative capacity of the water, as determined by a cylindrical thermometer which reaches from the surface to the bottom of the mixture, will be apparently diminished by an unequal distribution of the heat.

It is manifest, that, in all cases, the facility of promoting an uniform diffusion of the heat will be increased, by bringing the parts of the colder and warmer substances closely into contact with each other, which will be most easily accomplished by mixing them in equal volumes, and by subdividing the solids into minute parts.

If the causes of fallacy, now explained, be carefully avoided; if the substances, which are to have their heats examined, be used in
such

such quantities, and placed in such a situation, after they are mixed together, that they shall cool very slowly; if, at the same time, the difference of their temperatures be such as to afford a scale sufficiently large; if the heat be uniformly diffused over the mixture, and its temperature exactly ascertained, the results of different experiments will nearly coincide with each other, and the comparative heats will be determined with a degree of accuracy which will approach near to the truth.

It is proper to add, that though considerable variations should occur in the results of different experiments, yet the general conclusions deduced from them may, in many cases, be as certain as if they admitted of perfect precision. Thus if the capacity of the calx of iron, for receiving and communicating heat, be compared with that of iron; and if we find in a variety of trials that the proportion of the former to the latter is never greater than $4\frac{1}{2}$ to 1, and never less than $3\frac{1}{2}$ to 1, we may conclude with the utmost certainty, that the capacity of the calx considerably exceeds that of the metal; and moreover, if the experiments have been made with proper care, and have been frequently

I 2 repeated,

repeated, we shall have good evidence for believing that the exact ratio of the capacities of these substances is in some of the intermediate points between the extremes; that is, between the proportions of $3\frac{1}{2}$ to 1, and of $4\frac{1}{2}$ to 1. If, however, as was before remarked, the above-mentioned sources of fallacy be observed with attention, and carefully avoided, we shall have a near coincidence in the results of different trials.

With these precautions the following experiments were made to determine the comparative heats of some of the most common vegetable and animal substances, water being used as a standard.

EXPERIMENT I.

Air in the room	—	64
Twenty-two ounces of dried wheat	—	63.7 plus
Mixed with 20 ounces of water at	—	120
Temperature of the mixture in		
2 minutes was	—	101.7 plus
3	— —	101.7
4	— —	101.6
		5 mi-

5 minutes	—	101.6
6	—	101.5
7	—	101.5
8	—	101.5 minus
9	—	101.5 minus
10	—	101.4 minus
11	—	101.3
12	—	101.3
13	—	101.2 plus
14	—	101.2
16	—	101

The parts of the mixture being brought by agitation to a common temperature, the mean heat at the end of the 22d minute was 98.5.

The mixture was agitated during the first minute by means of a rod 12 times: its temperature was measured by a thermometer which reached from the surface to the bottom: the quantities of wheat and water were so proportioned that the volumes should be nearly equal. The cold wheat was poured into the vessel containing the warm water. The vessel was placed on a slip of flannel, and its mouth was covered with flannel, to prevent the access of the air in the course of the experiment.

It appears, that the mixture from the end of the second to that of the ninth minute lost three-tenths of a degree, and from the end of the ninth to that of the sixteenth, it lost six-tenths of a degree. The velocity of the refrigeration was therefore accelerated: for which the following causes may be assigned: when a body, that transmits heat slowly is exposed to a medium colder than itself, it cools more quickly at the surface than at the center; in consequence of which, a difference of temperature will arise in those parts of the mixture. This difference will, for a certain length of time, continue to increase, and during that time the progress of cooling at the center will be accelerated. For it is manifest, that, in the cooling process, the particles at the surface are continually receiving heat from those at the center, and giving off heat to the external air. During the first moments of the refrigeration, as there is but little difference between the temperature of the particles at the center and of those at the surface, the transmission of the heat from the former to the latter is very slow. But the particles at the surface will at this time
cool

cool most speedily, because the difference between their temperature, and that of the surrounding air, is greater than at any other period of the refrigeration. Since, therefore, the particles at the surface, in the beginning of the cooling process, receive heat slowly from the center, and are, at the same time, quickly deprived of heat by the external air; the separation of that principle from those particles, will, during this part of the process, exceed the supply. The difference between the temperature at the center and at the surface will consequently increase, and the velocity of the refrigeration at the center will be accelerated. But the rate of cooling at the surface is retarded, in proportion as the body approaches more nearly to the temperature of the ambient medium; and this retardation will gradually operate to diminish the rate of cooling at the center.

Hence, in the foregoing experiment, the refrigeration at the center was slow at first, and was gradually accelerated for a certain length of time, till it arrived at a maximum, after which it began to be retarded.

From the difference between the heat at the surface and at the center, we may ac-

count for the sudden diminution of temperature when the mixture was agitated at the end of the 16th minute. For the thermometer, previously to the agitation, indicated the heat at the center which was the warmest part of the mixture. And, therefore, when the several parts were brought nearly to a common heat, the thermometer sank $2\frac{1}{2}$ degrees.

It will hereafter appear, that when the parts of the mixture were previously brought to a temperature perfectly uniform, it lost by an agitation equal to that given to it, in these instances, half a degree. Allowing, therefore, half a degree for the heat separated by the agitation in the course of the 22d minute, we shall have 99 for the mean temperature at the end of the experiment; but the mean heat at the termination of the 2d minute was 101.7, hence the heat imparted to the air in 21 minutes was 2.7. If we suppose the mean rate of cooling during that time to be uniform, a supposition, which as the refrigeration was very slow, will be nearly accurate, we shall have .12 for the heat lost in each minute.

Con-

Consequently we have 101.94 for the mean heat at the end of the first minute; and if half a degree be added on account of the heat lost by the agitation at the beginning of the process, the true temperature will be 102.44.

The original heat of the wheat was	63.7
That of the water	— 120
The wheat was therefore raised	38.74
And the water cooled	— 17.56

In this experiment the cold wheat was introduced into the vessel containing the warm water. It will be proved in the sequel, that the heat communicated by the vessel was equal to that which would have been communicated by .84 of an ounce of water.

It appears, therefore, that the wheat cooled 20 ounces of water, and the vessel which was equal to the .84 of an ounce, 17.56 degrees. From which it follows, that it would have cooled 20.84 ounces of water, 17.56 degrees. The quantity of wheat was 22 ounces. Hence, according to a rule which was first proposed by Dr. Irvine, and which will be more particularly explained hereafter, the comparative heat of water is to that of wheat, in the compound ratio of 38.74 to 17.56, and of 22 to 20.84, or as 2.3 to 1.

I next

I next endeavoured to determine the capacity of the vessel used in the foregoing experiment, compared with that of water.

In order to avoid the inaccuracy arising from the heat lost by pouring the warm water into the cold vessel, the greater heat was given to the latter, which was raised to a fixed temperature by the following contrivance. It was before observed, that if the vapour of boiling water be made to flow in a constant stream through a pipe of any length or dimensions, every part of that pipe will be raised to the boiling point. It is, therefore, evident, that if a small spiral pipe be made to wind round a large cylindrical vessel at a little distance from it, and be kept at 212 by a stream of aqueous vapour, the vessel will be gradually heated, till at length it arrive at a certain point below 212, where it will continue without variation as long as the vapour passes through the pipe, and the temperature of the air in the room remains unchanged. In figure 4, plate I, the reader will see a sketch of an apparatus contrived on this principle. A B C D represents a tinned vessel nine inches deep, and $5\frac{1}{2}$ inches wide, environed by the spiral pipe A E B, placed
at

at the distance of a quarter of an inch from it. This pipe having performed one complete revolution around the vessel, terminates at the point B, where the steam is suffered to escape. Into the spiral pipe, the tube H G is inserted by means of a cork at one of its extremities G, and at the other extremity H, it is fixed to the boiler H K. In the tube H G is inserted the cock R, for the purpose of occasionally interrupting the communication between the boiler and the spiral pipe, and a cock is fixed to the boiler at P, through which a part of the steam is suffered to escape, when necessary. To the spiral pipe, at the point *o*, is joined the crooked pipe *o u b*, and to this is joined another *b v y*, that is connected with the circular pipe *a d*, placed at a little distance from the bottom of the vessel A B C D. The perpendicular portion *u b* of the crooked pipe *o u b*, is of such a size as to be capable of slipping into the corresponding pipe *b v*, the former being adapted to the latter in such a manner, that it may be introduced into it to a greater or to a less depth. And hence the circular pipe *a d* may be made to approach towards the bottom of the vessel, or to recede from it at pleasure.

The

The superior surface of the vessel is covered by a lid. This is heated by the small spiral pipe *be*, which is joined at the point *b* to the cock *F* by a moveable joint, similar to that described above. The cock is foldered to the pipe *FG*. Hence the spiral *eb* being moveable upon the extremity of the cock, may likewise be made to approach towards the lid, or to recede from it. It is manifest, that by this contrivance the heat communicated at the superior surface, and at the bottom of the vessel may be so adjusted, as to be precisely equal to that communicated at the lateral surface. The passage of the steam, through the pipe *be*, may be occasionally interrupted by turning the stop-cock *F*; and the pipe may be separated from the cock, when it is necessary to remove the lid of the vessel, in order to introduce the fluid or solid substance, which is to be the subject of the experiment.

bklm represents a similar vessel, (fig. 5.) surrounded by three spiral pipes *ac*, *df*, *eg*. The middle and inferior *df*, *eg*, having performed five revolutions each in contact with the cylindrical superficies of the vessel, and one around the bottom, terminate near to the center.

The superior pipe *a c* being made to cross the other two twice, revolves only three times around the cylindrical surface, after which having made a circular revolution around the bottom, it also terminates near to the center.

Into the middle and inferior pipes, which are separated from each other nearly the one-fourth of an inch, are inserted the stop-cocks *o* and *n*, in consequence of which the steam, that is admitted by the conical vessel *a b*, may be made to pass either through the superior pipe alone, or through the superior and inferior, or through the three at pleasure. It is plain, that by this contrivance, three different degrees of heat may be communicated to the vessel *b k l m*, each of which will be nearly fixed, being liable to be varied only by a change in the temperature of the air in the room.

The vessel which was to be the subject of the experiment, and which was made of tinned iron, was $7\frac{5}{8}$ inches deep, $4\frac{1}{2}$ wide, and weighed exactly 10 ounces and two scruples troy. It was placed upon a slip of flannel in the vessel *b k l m*, (fig. 5.) which may be called a steam bath; the extremity of the latter vessel being closed by a cork that was made

made to fit it accurately. In this cork a thermometer was fixed that had the degrees marked upon the tube, and which reached to such a depth as to be in contact with the bottom of the interior vessel.

The steam was then made to pass through the superior spiral pipe, in consequence of which the thermometer gradually rose to 138.5, where it remained with very little variation.

The air in the room being - 64

A quantity of water, weighing 40

ounces troy, was taken at - 64.5

Its temperature being examined by a very accurate thermometer, that had each degree divided into 10 equal parts, distinctly visible to the naked eye.

The interior vessel was then suddenly removed from the steam bath; the water was poured into it, and was agitated for a short time, after which its temperature was examined by the same thermometer that had been employed to determine its heat previously to the experiment, and was found to be as follows :

1 minute - 65.8

2 - 65.9

5 - 65.9½ very nearly 66.

During

During the course of this experiment, the water was kept in a state of gentle agitation, that all its parts might have an equal temperature. It is manifest, that a small quantity of heat must have been lost by the vessel in the short space of time which was employed to remove it from the steam bath. The heat that was thus lost may be nearly estimated by the experiment which follows :

Air in the room	-	$68\frac{1}{4}$	nearly
Forty ounces of water		68.15	very nearly
The vessel was placed in the			
steam bath A B C D,			
(fig. 4.) and raised to		109.25	
The water being poured			
ed into the vessel, its			
temperature in			
1 minute was	-	68.9	
2	-	68.9	plus
3	-	69	minus
4	-	69	minus
7	-	69	

Hence we have 1.5 for the heat communicated to the water in the first experiment, and .85 for that communicated in the second. In consequence of which the comparative heat of the water will be to that of the vessel by

the former experiment as 48 to 1, and by the latter as 47.3 to 1. In the latter case, therefore, the ratio of the heat of the water to that of the vessel appears to be less than in the former. The reason is obvious: when the vessel is removed from the steam bath, for the purpose of receiving the cold water, it must necessarily lose more heat in proportion as it was previously raised to a higher temperature. It is manifest, however, from the near coincidence of the results of the two experiments, that the quantity of heat lost in either case must have been very small. This experiment was frequently repeated, different degrees of heat being given to the vessel previously to the introduction of the water. The following are the results of four of the most accurate trials.

Comparative heat of water to that of the vessel by the

1st trial as	—	48 to 1
2	—	47.4 to 1
3	—	47.3 to 1
4	—	47 to 1

From the mean result of these trials it appears, that the comparative heat of the vessel was to that of the water as 1 to 47.4.

The

The quantity of heat therefore contained in the vessel was equal to that contained in .84 of an ounce of water.

EXPERIMENT II.

Air in the room	—	52
Twenty-two ounces of dried wheat at	—	53.5
Mixed with 20 ounces of water at	—	126.75

the mixture being agitated briskly and uniformly, with an iron rod, 12 times; and its temperature being measured by the same thermometer used in the preceding experiment, in

1 minute it was	—	101
2	—	101
3	—	101

The mixture being again agitated 12 times, its temperature at the end of the

4th minute was	—	$100\frac{1}{2}$
5	—	$100\frac{1}{2}$ minus
9	—	100

K

Being

Being again agitated 12
times, at the end of the

10th minute it was	99 $\frac{1}{2}$
12 — — —	99 $\frac{1}{2}$ minus

It is proper to observe, that in agitating the mixture, a motion as nearly equal as possible should be communicated to its several parts. If this circumstance be not attended to, a variation will be produced in the results of different trials.

In this experiment the cold wheat, as before, was poured into the vessel containing the warm water. The vessel rested on a wooden table, which was covered with flannel. Its mouth was closed with a cork, to prevent the access of the external air, and the thermometer was introduced into the mixture through an aperture in the cork.

It appears, that when the mixture was agitated at the end of the 3d minute, it lost half a degree. The same thing happened when it was again agitated at the end of the 9th minute. Hence we may infer, that a brisk agitation, continued 12 times, was sufficient to bring the mixture to a common heat. For it will be proved by a subsequent experiment that if this were not the case, more
heat

heat would have been apparently lost in the second repetition of the agitation than in the third. As the agitation given to the mixture, in these instances, was equal to that which it had suffered at the beginning of the experiment, it follows that we may allow very nearly half a degree for the heat lost in the course of the first minute; for though the heat, separated by the agitation at the beginning, must have been a little greater than that which was afterwards separated; yet as the heats lost at the end of the 3d and 9th minutes were so nearly equal, that the difference could not be distinguished by the eye, we may safely conclude, that the difference between those lost at the end of the 3d and 1st minutes, was so small that it may be neglected. Allowing, therefore, half a degree for the heat separated in the first minute, we have 101.5 for the true temperature of the mixture.

The original heat of the

wheat was	—	53.5
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That of the water was		126.75
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It follows, that the wheat

was raised	—	48	degrees
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And the water cooled		25.25
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K 2

In

In this experiment the cold wheat was introduced into the vessel containing the warm water. The heat received from the vessel was equal to that which would have been received from .84 of an ounce of water. Hence by this experiment the comparative heat of water is to that of wheat as 2 to 1.

I have selected the foregoing experiments from a variety that were made with the same substances, because they give the greatest and least results, when the mean of those which were most accurate was taken, the capacity of water appeared to be to that of wheat as 2.1 to 1, which is probably near to the truth.

The reader will perceive that the ratio of the comparative heats of water and wheat, as deduced from these experiments, is considerably different from that which was published on the same subject in the first edition of this work. I was not then aware of some of the causes which tend to render the conclusions from such experiments erroneous.

In the experiment on wheat formerly published, I endeavoured to ascertain the temperature of the mixture by thermometers placed at
the

the surface, and at the bottom; and as the mixture, in the course of the first minute, was not sufficiently agitated to produce an uniform diffusion of the heat, it became difficult to adjust the relative positions of the thermometers, in such a manner as to determine the mean temperature with accuracy. The substances were also mixed in a wide-mouthed vessel, which exposed a large surface to the air, they therefore cooled rapidly; for these reasons a considerable inaccuracy arose in the calculation of the comparative heats of wheat and water, as deduced from that experiment.

I must, however, observe, as an apology for this inaccuracy, that it did not arise from an error in the statement of facts. I am convinced from the result of subsequent trials, that a series of facts nearly similar, would occur to others who should take the trouble to repeat the experiments in similar circumstances.

I do not pretend to affirm, that in repeating such experiments, a perfect coincidence of results can be obtained. In the work which I formerly published, I remarked, that a change in the temperature of the air in the

room ; a variation in the time that was 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. But I can safely affirm, that nearly similar results will be obtained when the experiments are repeated with a proper attention to these sources of error ; and the mistake in the calculation of the comparative heat of wheat, as determined by the above-mentioned experiment, principally depended upon a fallacy in the deduction of the common temperature from facts, which were observed with attention, and stated with fidelity to the public,

It was before remarked, that in the 2d experiment, if the water and wheat had not been brought to a common temperature in the course of the first minute, less heat would have been apparently lost in the second repetition of the agitation than in the third. This is proved by the experiment which follows,

E x =

EXPERIMENT III.

Air in the room	—	56
Twenty-two ounces of dried wheat at	—	55
were mixed with 20 ounces of water at	—	126.75

The circumstances being the same as in the preceding experiments, excepting that the substances were not agitated when they were mixed together, the temperature in

1 minute was	—	100.25
2	—	100.75
3	—	101 plus
4	—	101.5
5	—	102 minus

The mixture being then agitated 12 times, the temperature at the end of the

6th minute was	—	103 plus
7	—	103 plus
8	—	103
9	—	103
10	—	103
11	—	102.75

12	—	—	102.75 minus
13	—	—	102.5
14	—	—	102.5 minus
15	—	—	102.25
16	—	—	102.25 minus
17	—	—	102
18	—	—	102
19	—	—	102 minus
20	—	—	101.75
22	—	—	101.5
24	—	—	101
26	—	—	100.5
28	—	—	100
30	—	—	99.5
32	—	—	99
34	—	—	98.5
36	—	—	98
38	—	—	97.25 $\frac{1}{2}$
40	—	—	96.75
42	—	—	96 plus
44	—	—	95.5

Thus it appears, that when the warmer and colder substances were not agitated at the beginning of the experiment, the thermometer continued to ascend during the first five minutes, after which the mixture being agitated

tated it suddenly rose one degree. When I first observed this appearance on mixing water and wheat together, I supposed that it proceeded from the production of sensible heat, in consequence of a chemical union between these substances. But I was soon led to correct that opinion, because I found that the same effect was produced when water and sand were mixed in similar circumstances. By varying the experiment, and by examining the temperature of the mixture at the surface and at the bottom, I discovered that the ascension of the thermometer depended upon the unequal distribution of the heat, and upon the difference between the capacities of wheat and water.

For as no agitation was given to the mixture at the beginning of the experiment, the water and wheat were not brought to a common temperature, in the course of the first minute; and as the thermometer reached from the surface to the bottom, it indicated a degree which was intermediate between the temperature of the wheat and water. It was before observed, that this intermediate heat must be less than the common

mon temperature *, because the capacity of the warmer being greater than that of the colder substance, when they were brought to a common heat, the increase in the temperature of the latter would be greater than the diminution in that of the former. The thermometer, therefore, continued to ascend until the mixture was brought by agitation in the sixth minute to a common heat, after which it began slowly to descend. If the temperature of the wheat were greater than that of the water, the reverse of this would take place; for in that case, the mixture being agitated as above, the thermometer would suddenly sink several degrees.

By experiments similar to those which have been now recited, I endeavoured to determine the comparative heats of most of the farinaceous vegetables, and of some of the animal substances that are commonly used in aliment. The results are comprized in the following table.

Table of the comparative heats of vegetable and animal substances, water being the standard.

* See the observations on the experiments with sand and water, page 112.

Common water	—	1.000
Horse beans	—	0.502
Rice	— —	0.506
Wheat	— —	0.477
Oats having the hulls taken off		0.416
Peas	— —	0.492
Unhulled barley	—	0.421
Lean of the beef of an ox		0.74
Hide of an ox with the hair		0.787
Lungs of a sheep	—	0.769
Fresh milk of a cow		0.999
Arterial blood of a dog		1.03

In the course of my experiments with farinaceous vegetables, I found that when some of those substances were reduced to powder, and intimately mixed with warm water, they produced sensible heat. This will appear from the experiment which follows.

EXPERIMENT IV.

Air in the room	—	62
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Twenty-two ounces of wheat coarsely powdered, being mixed with 18 ounces of warm water, and the mixture being reduced by agitation to a common heat, the temperature

rature as measured by a thermometer which reached from the surface to the bottom

Was in 2 minutes	—	—	$94\frac{1}{2}$
3	—	—	$94\frac{1}{2}$
4	—	—	$94\frac{1}{2}$ plus
5	—	—	95 minus
6	—	—	95 plus
7	—	—	$95\frac{1}{4}$
8	—	—	$95\frac{1}{2}$
9	—	—	$95\frac{1}{2}$ plus
10	—	—	$95\frac{3}{4}$
11	—	—	$95\frac{3}{4}$ plus
12	—	—	96 minus
13	—	—	96 minus
14	—	—	96 minus
15	—	—	96
16	—	—	96 plus
21	—	—	96 plus
26	—	—	96
31	—	—	$95\frac{3}{4}$

The wheat swelled in the course of this experiment, and towards the conclusion was so much enlarged, as not to be completely covered by the water.

From the gradual ascension of the thermometer during the space of twenty minutes, it

it is manifest, that sensible heat must have been produced.

Agreeably to this I have found, that if wheat and warm water be mixed together, and suffered to cool, and if the mixture be heated a second time, and again suffered to cool, the progress of the refrigeration, in the latter instance, will be much slower than in the former; which proves, that the water having remained for a considerable time in contact with the wheat, is at length enabled, by the assistance of a moderate degree of warmth, to contract a chemical union with it, and to produce sensible heat.

Sensible heat is likewise produced by mixing the powder of beans, peas, rice, oats, and barley, with water raised to 120.—My original intention in reducing these substances to a coarse powder, was to bring them into such a state, as that they should speedily and intimately mix with water, this circumstance being of importance to the accurate determination of their comparative heats.

They were pulverized by the following process. Each of them was first ground into flour; it was then made into a thin cake with water, and baked in an oven till it became
crisp;

crisp ; after which it was coarsely powdered by beating it in a mortar.

The coarse powder of the farinaceous vegetables which is thus procured, will readily mix with water ; whereas a considerable time, and much agitation, is required to mix the flour of these substances with that fluid.

When the powders obtained by this process, were intimately mixed with water, and raised to a moderate degree of warmth, they were found, as was before remarked, to produce sensible heat ; which rendered the results of the experiments made with a view to determine their comparative heats in some measure fallacious. I, therefore, had recourse to the method which I had formerly adopted, of mixing those substances with water in their entire state ; because under these circumstances, they either do not produce sensible heat, or the quantity produced is so small, as not materially to affect the result of the experiment ; and the comparative heats may, by this mode of trial, be determined with a considerable degree of accuracy, if the precautions formerly mentioned be properly attended to, and particularly if the warmer and colder substances, when they are mixed
toge-

together, be placed in such a situation, as that they shall cool very slowly. For, in that case, the heat lost by the agitation necessary to bring them to a common temperature, will be inconsiderable.

The foregoing experiments prove in general, that flesh, milk, and vegetables, contain less absolute heat than water, and water less than arterial blood. Arterial 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

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:

PROPOSITION I.

The quantity of absolute heat contained in pure air, is diminished by the change which it undergoes in the lungs of animals, and the quantity of heat in any kind of air that is fit for respiration, is nearly proportional 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 produced in the air by the process of respiration. It is now generally agreed among Philosophers, that the air of the atmosphere principally consists of two different species of permanently elastic fluids, which have been called by Dr. Priestley phlogisticated and dephlogisticated air.

If a portion of atmospherical air be exposed in an inverted jar to water, which has had its air separated by boiling, it will be resolved into the two fluids mentioned above; the purer part of it will be attracted by the water, and the noxious phlogisticated part will remain in the vessel unabsorbed.

Moreover, if phlogisticated and dephlogisticated air be mixed together in certain proportions, they will compose an elastic fluid which has a very near, if not a perfect resemblance to common air. These therefore appear, both by the synthetic and analytic mode of trial, to form the constituent parts of the atmosphere, the dephlogisticated air composing nearly one fourth of the whole, and the remaining three fourths consisting chiefly of phlogisticated air.

Of these constituent principles, it is the purer part alone which is affected by the respiration of animals. This I think is evident from the following experiments.

If an animal be placed in a given quantity of atmospherical air, confined in a jar inverted over water, after a certain length of time, it will fall into convulsions and expire; the air will be diminished nearly one fifth of its bulk, and that portion of it which remains in the jar will be found to consist of phlogisticated, mixed with a very small quantity of pure air, the latter having been rendered by its diffusion through the former, unfit for ministring any longer to the respiration of the animal. If the experiment be made over lime water, the liquor will become turbid, a precipitation will take place, and the quantity of calcareous earth precipitated, will be in proportion to the diminution of the air. If the trial be made over mercury, no sensible diminution will take place; but if a small quantity of caustic alkali be introduced, it will be changed into the mild alkali, and the air will be diminished in the same degree, as if the experiment had been made over water.

If

If the jar inverted over mercury, contain dephlogisticated instead of atmospherical air, and if the animal placed in it be removed as soon as it begins to sicken, and the caustic alkali be introduced, a much more considerable diminution will take place, and by the repeated introduction of the animal and of the alkali, almost the whole of the dephlogisticated air may be made to disappear*. Hence it follows, that it is the dephlogisticated part of the atmosphere, which ministers to the support of animal life, and which is altered by the action of the lungs.

It is extremely probable, that this alteration consists in the conversion of pure into fixed air, by the union which the former contracts with inflammable air, or with its basis, in the process of respiration. That such an union takes place in the lungs, will, I think, appear from the following considerations.

It is well known, that the blood undergoes a remarkable change of colour when circulating in a living animal, for the vivid

* Vide Memoire sur La Chaleur, par Mons. Lavoisier & De la Place, lû a l'Académie Royale des Sciences, le juin 2s. 1783. p. 52.

arterial blood, in its passage through the capillaries to the venous system, acquires a deep and livid hue, and again resumes its light and florid colour in the lungs. Dr. Priestley has proved, that similar alterations are produced in the colour of the blood by exposure to pure and inflammable air. For when vivid arterial blood is exposed to inflammable air, or to any other species of aerial fluid, known to contain that principle or its basis, it speedily acquires the deep and livid hue of venous blood. And, on the contrary, when venous blood is exposed to pure air, it acquires the light and florid colour of arterial; and these alterations are produced in an equal degree, when a thin bladder is interposed between the air and blood. The same philosopher has proved, that the air and blood which are employed in these experiments, undergo opposite changes, for pure air is vitiated by exposure to venous blood; and on the contrary, inflammable air is absorbed, and phlogisticated air improved by exposure to arterial blood: from the latter circumstances it appears, that arterial blood has an attraction to inflammable air, or to its basis.

That

That inflammable air will produce a change of colour in the blood, when introduced into the veins of a living animal, appears from the following experiment, which was tried by my ingenious friend Dr. Hamilton.—Three ligatures were made on the jugular vein of a cat, and the portion of the vein contained between two of them, having been previously emptied of its blood, was filled with inflammable air. The air being confined in this situation, by closing the puncture through which it was introduced, the middle ligature was removed, in consequence of which, the blood that was contained in the part of the vein intercepted between the middle and the third ligatures, was suffered to mix with the inflammable air. After an hour had elapsed, the blood being drawn from the vein, was found fluid, and had acquired a colour almost as dark as ink. At the time when this trial was made, a quantity of blood, as nearly equal as possible to the former, was likewise intercepted, during the space of an hour, between two ligatures that were tied on the crural vein of the same animal. This blood being drawn from the vein, was found in some measure coagulated,

but not to so great a degree, as to be immiscible with water. To these portions of blood, equal quantities of water were added; when it appeared, that the portion which was exposed to the inflammable air, imparted a much deeper tincture to the water, than that which had been drawn from the crural vein. Hence it appears, that when inflammable air is introduced into the veins of a living animal, it increases the livid colour of the blood, and at the same time diminishes its tendency to coagulation. The inflammable air used in this experiment, was obtained from iron filings, dissolved in the vitriolic acid. It cannot be doubted, that the same effect would be produced, by exposing the blood to that species of inflammable air, which is obtained from animal substances.

Since, therefore, the arterial blood undergoes the same change of colour in the capillaries, that it suffers by exposure to inflammable air; since it has an attraction to that fluid; and since the separation of inflammable air from animal substances is promoted by heat, and by the tendency of the juices to putrefaction; we may I think safely conclude, that the change
which

which the blood undergoes in the capillaries, arises from its impregnation with this principle. And as the absorption of inflammable air, or of its basis, is the cause of the change, which is produced in the colour of the blood, during its circulation through the capillaries ; we may also conclude, that when the blood again recovers its florid colour in the lungs, the inflammable principle is detached. In this conclusion we may rest with the greater certainty, because the venous blood, in the lungs, is exposed to the action of that species of air, which in other phlogistic processes, is known to combine with the inflammable principle. In the process of respiration, therefore, the pure air, which is received into the lungs, combines with a portion of the inflammable air contained in the venous blood.

The substances that are the result of the union between these fluids, in phlogistic processes, differ from each other, because inflammable air, which is one of the ingredients, is of different kinds. Thus the inflammable air obtained by the solution of metals, in the vitriolic acid has, in several respects, distinct properties, from that which is obtained, by

exposing animal and vegetable substances to heat in close vessels. The former has a much less specific gravity than the latter; the first, when a lighted candle is dipped into a jar filled with it, explodes; the second burns with a lambent flame; the residuum which arises from the combustion of the one, with a certain proportion of pure air, in close vessels, is water; but that which arises from the combustion of the other, in similar circumstances, is fixed air. It is evident, that the fixed air found in the latter case, must either have pre-existed in the inflammable air, or it must have been produced by the union of this fluid, or of some part of it, with dephlogisticated air. From Dr. Priestley's experiments it appears, that it does not universally pre-exist in the inflammable air, for in some experiments, in which pure and inflammable air were mixed together, and fired by the electric spark, the quantity of fixed air, which was the result of the inflammation, was considerably greater than the entire quantity of inflammable air used in the process*. Hence it is manifest, that either the whole

* See Priestley's Experiments relating to various Branches of Natural Philosophy, vol. III. p. 272.

or a part of the fixed air found in those experiments, must have been produced by a union of the inflammable air, or of some principle contained in it, with the dephlogisticated air.

It appears therefore, that fixed air is produced by the combination of dephlogisticated with lambent inflammable air, or with some of its constituent parts. It was before shown, that the last mentioned species of air, is united to the blood in the capillaries, and is again separated from it, in the process of respiration. And since it is found, that fixed air is exhaled by expiration, a portion of the pure air being at the same time made to disappear, there is I think the utmost reason to believe, that the fixed air, which is the result of this process, is produced by the union of the pure and inflammable air, which come into contact with each other in the lungs. To this conclusion it should not be objected, that these fluids, when mixed together, do not suddenly combine, unless an ignited body be applied to them; because we know from the experiments of Dr. Priestley, that inflammable air, without the assistance of flame, is capable, in its nascent state, of contracting an union with pure air.

As it has been proved, however, by the very ingenious experiments of Mr. Cavendish, that water is the result of the union between pure air, and the inflammable air which is obtained from metals, by the vitriolic acid; it is possible, that a portion of the pure and inflammable air, which meet in the lungs, may undergo that peculiar mode of combination by which water is produced. And this opinion will be rendered more probable, if we consider that though the fixed air, which was the product of the above-mentioned experiments that were made by Dr. Priestley, considerably exceeded the inflammable air, yet it was not equal to the sum of the two airs, which were employed in those experiments. From these considerations, I think we may conclude, that the pure air received into the lungs, combines with the inflammable principle, which is extricated from the blood, and that by this combination it is partly converted into fixed air, and partly into aqueous vapour *.

* After I had written the above, I found that Dr. Higgins maintains the same opinion. See experiments on acetous air.

In order therefore to determine the truth of the proposition, it must be proved, that pure air contains a greater quantity of absolute heat than the fixed air and aqueous vapour which are exhaled from the lungs †.

I have examined with great care the comparative quantities of heat contained in these fluids, and shall now proceed to communicate the result of my enquiry.

In the former edition of this work I observed, that from the imperfection of thermometers, and the difficulty of judging by the eye of the fraction of a degree, perfect

† Some philosophers have maintained, that the dephlogisticated air is absorbed by the blood. But this opinion does not appear to be supported by any decisive experiments. Admitting it however to be true, it will not affect the general doctrine which I have endeavoured to establish.—Because if dephlogisticated air be absorbed, it must be separated again by some of the emunctories: for if this were not the case, there would be an accumulation of it in the animal body. But it is not found that any of the secretions contain dephlogisticated air. This fluid, therefore, supposing it to be received into the blood, must necessarily be altered in its properties; and it seems to be evident, that by coming into contact with the putrescent parts of the system, it would be changed into fixed air, or into water, and would consequently have the quantity of its absolute heat diminished.

accuracy

accuracy in experiments for determining the heat of air, was not to be expected; and added, that I proposed in future to endeavour to ascertain with as much exactness as possible, the comparative quantities of heat in the different kinds of air, by a greater variety of trials, and by thermometers constructed for the purpose. I have since repeated my former experiments with as much accuracy as possible, and by giving a more minute attention to the circumstances which influenced their results, I have endeavoured to make a nearer approximation to the truth. In consequence of this, the reader will find, that the quantities of heat in the various kinds of air, as determined by my late experiments, are very different from those recited in my former publication. It will appear, however, that the general facts on which the theory is founded are true, and I am persuaded, that, with the candid and judicious, the novelty of the subject, and the extreme difficulty of the undertaking, will in some measure plead my apology for the errors in that work.

Before we can arrive at much precision in experiments, we must be acquainted with
the

the various causes which may render their results fallacious; and in a new and obscure branch of science, a knowledge of these causes can only be obtained by repeated trials and attentive observation.

The peculiar difficulty of determining with precision the comparative heats of the different kinds of air, arises partly from the fugitive nature of heat, partly from the rarity of the permanently elastic fluids, in consequence of which not more than fifteen or twenty grains of them can conveniently be employed in an experiment, and partly from the changes that they are liable to undergo, with respect to purity and the quantities of moisture which they contain.

When the experiment is made by introducing a few grains of warm air confined in a bladder into an equal bulk of water, as the specific gravity of the latter so greatly exceeds that of the former, it is manifest that very little heat will be imparted to the water. For the same reason, if different elastic fluids be employed, the differences in the quantities of heat communicated will be minute; and in that case, as from the imperfections of our senses, and of thermometers, every
expe-

experiment is necessarily attended with some degree of inaccuracy, the errors of the observations will bear a considerable proportion to the differences which are to be ascertained ; the results will therefore be liable to much uncertainty.

A variety of expedients were tried with a view to obviate this difficulty ; a few of which I shall briefly relate, as they enabled me to ascertain some of the most important circumstances that require attention, in trials for determining the heat of air, and led to a mode of making the experiment which appears to be decisive. My first expedient was as follows :

In the side of a crooked glass tube, that was of such a size as to contain nearly half an ounce of iron filings, a small aperture was made, through which a very sensible thermometer was introduced into the centre of the tube, being kept in its place by means of a cork closely adapted to the aperture. A bladder having a brass cock tied to its neck, was occasionally fixed to the lower end of the tube. The air, which was to be the subject of an experiment, being confined in this bladder, was raised to the required temperature

ture in a Dutch oven, and was then forced through the filings contained in the tube, by a press which had a weight applied to it. Thus a considerable quantity of warm air was made to pass through a small quantity of cold iron filings. It is proper to observe, that the bladder was surrounded by several folds of flannel, in order that the heat might be retained in the air, during the short time which was employed in forcing it through the tube. As complete a separation as possible was made, between the bladder and tube, by means of thick flannel.

With this apparatus I made the following experiments, which, as they tend to throw light upon the subject of the present enquiry, seem not unworthy of attention, though they do not lead to any direct conclusion respecting the object which I had principally in view.

EXPERIMENT I.

Fourteen scruples of dry iron filings being introduced into the crooked glass tube, and the thermometer in the centre standing at $49\frac{1}{2}$; nearly 40 ounce measures of atmospheric

rical air were heated in the bladder to 110, and made to pass through the filings by means of the press and weight mentioned above. In one minute and seven seconds nearly, the thermometer stood at 57. It was therefore raised $7\frac{1}{2}$ degrees. I continued to repeat this experiment for the greater part of the day, with the same bladder, and found, that the heat communicated to the filings gradually diminished, till at length the result was as follows. The thermometer in the centre being $48\frac{1}{2}$ nearly, 40 ounce measures of atmospheric air were raised to 118, and passed through the filings as above. In one minute, the thermometer stood at $50\frac{1}{2}$. It was therefore now raised only 2 degrees. Though I repeated the last experiment with all the attention I was capable of, yet I found the filings in such a state, that not more than 2 degrees were communicated by the hot air which was forced through them. I then removed the filings from the instrument, and perceived by their adhering to each other, and to the inner surface of the tube, that they had collected a considerable quantity of moisture. The filings and tube being made perfectly dry, and the experiment again repeated

ed on the ensuing day, the thermometer was raised as at first more than 7 degrees.

The air being made to pass repeatedly through the same filings, the quantity of heat which it communicated, gradually diminished, and the filings were found to have contracted moisture, as in the former experiments.

That the moisture imparted to the filings in these instances was derived from the bladder, is proved by the experiments which were made with the following apparatus. See plate 1. fig. 6.

A copper worm H K was fixed in a wooden vessel F G, which was occasionally filled with warm water, or with melted spermaceti; one extremity of the worm M, terminated in a pipe which passed through the side of the vessel, and which was so contrived, as to be capable of slipping into a brass tube connected with the bottom of the small glass vessel A B, that contained iron filings, the pipe and tube being made to fit each other with such exactness, as to be air tight. By a similar contrivance, the other extremity of the worm was joined to the bladder C D, in which the air was contained. At the point E, a thermometer was inserted in the worm, to such a
M depth,

depth, that the current of air must necessarily strike against its bulb. To the upper extremity of the glass vessel A B containing the filings, was fixed a crooked tube A L, the farther end of which was introduced into the mouth of an inverted glass jar R S, that was placed in the wooden vessel P Q. To this jar a scale was applied, by the help of which the quantity of air employed in the experiment was accurately measured.

It is manifest, that the air expelled from the bladder by means of the press formerly mentioned, would in passing through the worm acquire the heat of the surrounding fluid, and having imparted its heat to the cold filings contained in the glass tube A B, would afterwards be received into the inverted jar R S, that had been previously filled with water.

I was at first induced to contrive this apparatus, hoping that I should have been enabled to determine by it, the comparative heats of the different kinds of air. In that expectation, however, I was disappointed. But, as was before observed, the trials that were made with it, clearly proved the effect of the moisture derived from the bladder in
influ-

influencing the results of such experiments. It may not be improper briefly to relate, the experiments by which this was ascertained:

EXPERIMENT II.

A quantity of melted spermaceti at 107, having been poured into the vessel F G, nearly 28 ounce measures of atmospherical air included in the bladder, were made to pass by means of the press and weight through the worm and filings.

In this experiment, the air in the bladder had the same heat with that in the room. During its passage through the worm, it acquired the heat of the melted spermaceti; as appeared from the thermometer employed to indicate its temperature. This heat it imparted to the cold iron filings, which were raised by it, in one minute, four degrees.

EXPERIMENT III.

The melted spermaceti being again introduced into the vessel, 28 ounce measures of atmospherical air, included in the same bladder, which had not been used for a consi-

derable time, were gradually raised in a Dutch oven to the temperature of 95. The air was then forced through the worm and filings, and the heat imparted to the latter, in one minute, was 8 degrees.

In both these experiments, the air had acquired the temperature of the melted spermaceti, as was evident from the thermometer inserted in the worm; and from the difference of the heat imparted to the filings, it is manifest that, in the second experiment, the warm bladder must have communicated moisture to the air contained in it; but in the first, as it had not been previously heated, it was not in a condition to produce that effect. Hence we may perceive the cause of the gradual diminution of the heat, in the experiments recited in page 159.

The bladder used in these experiments, had imbibed moisture in consequence of its exposure to the external air. When it was first heated, it communicated a portion of this moisture to the air contained in it, and the air in these circumstances, being filtered through the cold dry filings, the vapour which was mixed with it was condensed, the filings became moist, and much sensible heat was pro-

produced: but in the succeeding experiments, as the bladder was more perfectly dried, in consequence of being more frequently heated, it communicated a smaller quantity of moisture to the air which it contained; and consequently the latter, in passing through the damp iron filings, gradually parted with less and less aqueous vapour, till at length it would not any longer give off moisture, but would rather be in a condition to absorb it.

Agreeably to this I have found, that when the experiment was made by heating air confined in a bladder, and immersing it in cold water, the heat imparted to the water was greater or less, in proportion as the bladder communicated to the air which it contained, a greater or a less quantity of moisture.

Of this I shall mention the following instances.

Fourteen ounce measures of atmospherical air were introduced into a small bladder, which weighed very nearly a drachm and an half, and which had been perfectly dried, having been previously freed from its oil, by washing it carefully in soap and warm water.

The temperature of the room

being — — 50,

M 3

The

The air contained in the
 bladder was gradually raised
 in a Dutch oven to 101,

And was suddenly immersed
 in 18 ounces of water,
 which had the tempera-
 ture of — — 48.6,

In three minutes the water
 was — — 49.1 nearly.

The air, therefore, with the bladder in
 which it was confined, communicated to the
 water .5 of a degree nearly.

But if great care was not taken to free the
 bladder from moisture, the heat imparted to
 the water in similar trials, was not less than
 8 or 9 tenths of a degree. And when blad-
 ders somewhat larger were used, as in the
 experiments which were formerly published
 on this subject, the quantity of heat com-
 municated was more than a degree.

It appears, therefore, that these experi-
 ments are liable to great variations, from the
 moisture imparted by the bladder to the air
 which it contains. When the trial was made
 with the apparatus described above, the re-
 sult was also affected by the moisture which
 the iron filings might contain; and as the
 air

air necessarily flowed through a portion of a cold pipe, in its passage to the crooked tube, much heat was dissipated before it came into contact with the filings. It is manifest, that the calculation of the heat which was thus lost, must be attended with much uncertainty.

Finding that these methods of making the experiment, were liable to so many sources of fallacy, I next endeavoured to ascertain the relative capacities of the various kinds of air, by heating them in Florence flasks, and immersing them in water; the temperature of the air being measured by a very sensible mercurial thermometer, introduced into the centre of the flask through a cork adapted to its mouth, and that of the water, by similar thermometers, which had each degree of Fahrenheit's scale divided into ten equal parts.

In my first attempts to determine the comparative heats of the permanently elastic fluids by this apparatus, the Florence flask was filled with water previously to the introduction of the air. But I soon found, that the quantity of moisture which adhered to the internal surface of the vessel, rendered

the result of the experiment precarious. For this reason, in my subsequent trials, the flask, before the air was introduced into it, was filled with mercury.

From these trials it clearly appeared, that dephlogisticated air communicated more heat to water, than fixed or phlogisticated air, in similar circumstances.

It was found, however, from the errors produced by variations in the relative positions of the flask, and of the thermometers, and from the difficulty of introducing the former into the water, with equal velocities, in different experiments, that the comparative heats of the permanently elastic fluids, could not be determined by this mode of trial with sufficient accuracy.

It may be proper to add, that in some of the experiments which were instituted in the course of this enquiry, I employed a spirit of wine thermometer, that had each degree divided into an hundred equal parts distinctly visible by the naked eye. The preference was given to that fluid for these experiments, as it is much more expansible by heat than mercury. With this instrument, I hoped that I should have been able to have distinguished

guished minute variations of heat; but it did not answer my expectations.

For when thermometers are constructed in such a manner, that the ball or cylinder in which the fluid, that measures the heat is contained, shall bear so great a proportion to the size of the bore, as that each degree may be subdivided into many equal parts, what is gained in the magnitude of the scale, is lost in the diminished sensibility of the instrument. When the above-mentioned thermometer, for example, was immersed in water that had a higher temperature than itself by $\frac{1}{8}$ of a degree, it did not acquire the heat of the water, in less than a minute and a half. And when the same thermometer, together with a very sensible mercurial one, were placed in a small vessel containing water to which heat was applied externally by the hand, the mercury rose several degrees higher than the spirit of wine.

Since, therefore, large thermometers, which are made with spirits of wine, are very defective in point of sensibility; and since it appears, that when they are applied to a substance which has a variable heat, they never indicate its true temperature; they are totally

tally improper for nice experiments, in which minute variations of heat are to be ascertained *.

I likewise made trial of air thermometers, with a view to determine the comparative heats of the permanently elastic fluids; but these, though very sensible, did not answer the purpose, being deficient in point of accuracy.

In the course of my experiments on this subject, fixed and phlogisticated air were found to undergo a remarkable change, when exposed to heat in bladders.

This will appear from the experiments which follow.

Air compleatly noxious was obtained by forcing fresh air into the touch-hole of a gun barrel, the butt-end of which containing a small quantity of rosin, was exposed to a red heat.

The air thus produced was received, by means of a crooked tube fixed to the mouth of the barrel, in an inverted jar filled with

* It will be shewn in the sequel, that a method was discovered of employing, with advantage, large mercurial thermometers, to determine the comparative heats of the permanently elastic fluids.

water. Being carefully examined by Dr. Priestley's test, it was found, that two measures of it mixed with one of nitrous air, occupied the space of three measures. A quart and half a pint of this air, was then introduced into a thin bladder, surrounded with a covering of flannel, and was gradually raised in a Dutch oven placed before a fire in the room, to the temperature of 130. After which, it was very nearly as good as common air, two measures of it, and one of nitrous air, occupying the space of two measures *. It is necessary to observe, that this experiment was made in a very thin hog's bladder, which had been previously well rubbed.—If the experiment be made in a thick bladder, that has not been previously rubbed, it will not succeed.

A similar improvement was made in fixed air by exposure to heat in bladders. It is proper, however, to remark, that in the lat-

* When air newly obtained from rosin, in the manner above described, was exposed to heat, a penetrating smell was perceived to issue from the bladder in which it was confined, but this effect was produced in a much slighter degree, when the air, previously to its exposure to heat, had been kept for a considerable time in contact with water.

ter species of air, the change was effected with greater difficulty; more heat, and a longer time being required to produce it.

I shall observe in the last place, that if air confined in a bladder, and heated as above, be immersed in water, which is nearly of the same temperature with the atmosphere, less heat will be imparted, all other circumstances being equal, than if it had been immersed in water several degrees below the temperature of the atmosphere; a portion of the heat communicated, in the latter instance, being evidently derived from the external air, in consequence of the agitation given to the water, by immersing in it a body that has a large surface. The error arising from this cause may be estimated by the following experiment.

Air in the room 75.5,

A pint of water in a tin vessel being found, when tried by a very accurate thermometer, that had each degree divided into ten equal parts, to have the temperature of 74.5,

And a pint of atmospherical air, which was contained in a bladder, and which had precisely the

same

same heat with the air in the room, being suddenly immersed in this water, the latter was raised to

74.6,

Agreeably to this I have observed, that if phlogisticated air, which had been made 50 degrees hotter than the air in the room, was immersed in water one degree below the temperature of the atmosphere, it would communicate more heat, by nearly one-tenth of a degree, than if it had been immersed in water, only half a degree below the temperature of the atmosphere.

The water used in the above experiment, had been raised to 75.1, after which it had remained in the room more than an hour; during this time it was cooled by evaporation to 74.5, and at this point it remained stationary, the air in the room being 75.5.

In a former experiment, the air in the room being 72.5, and the same thermometer being introduced into a pint of water that was raised in the same vessel to 72.5, the temperature of the water was found to fall, in a short time, to 72, the air remaining at 72.5.

The

The last experiment was repeated with a similar result, when the air was at 65.

This seems to prove, that the evaporation from the surface of water, is not always in proportion to the heat of the external air; but is sometimes the same at different temperatures, and different when the temperatures are the same.

From these facts and observations it will appear, that the following circumstances require attention, in experiments for determining the comparative heats of the permanently elastic fluids.

1. The portions of the aerial fluids that are to be subjected to the experiment, should be free from moisture: or at least the quantities of moisture which they contain, should be as nearly equal as possible.

2. The trials should be made in such a manner, that those fluids may undergo no change during the experiment.

3. If the experiment be made by immersing air in water, the temperature of the latter, previously to the immersion, should be somewhat lower than that of the air in the room; and it is of great importance, that the difference between the heat of the surrounding

rounding atmosphere, and of the water, before the air is introduced into it, should be the same in different experiments.

4. The air should be introduced into the water as speedily as possible ; and in different experiments, the velocities of introduction should be equal.

5. In order to magnify the differences between the heats communicated by the various kinds of permanently elastic fluids, it is proper to raise them to a high temperature, previously to their immersion in the water ; for the heats imparted by those fluids, and consequently the differences of the heats which they communicate, will be nearly in proportion to the excess of their temperatures, above that of the water in which they are immersed.

This, however, must be understood with some limitation ; for when the temperature of the air that is to be employed in the experiment is very high, a part of the water will be converted into vapour at the moment of immersion, and much heat will be lost. If oil be used as a standard instead of water, the raising of the air to a very high degree of heat, will moreover be subject to this inconvenience, that a considerable time will be required,

required, after immersion, to bring the air and oil to a common temperature. I have found in general, that the air may be raised with advantage, nearly to the heat of boiling water.

6. It is of importance to employ thermometers, which are very sensible, and which at the same time have as large a scale as possible. These qualities are with difficulty combined in the same instrument. They may, however, be in some measure united by using small mercurial thermometers with very fine bores.

Of the different fluids that have hitherto been tried for the purpose of measuring minute variations of heat, mercury is decidedly the best; for air is defective in point of accuracy, and all other fluids, excepting air, are inferior to mercury in sensibility.

7. In different experiments the thermometer should be kept precisely at the same distance from the bladder or vessel containing the air, and from the surface and bottom of the water.

8. It is of great importance that the air, and the fluid to which it imparts its heat, should speedily acquire a common temperature;

ture; and, on the contrary, that the fluid to which the heat is imparted, should give off that heat as slowly as possible to the surrounding atmosphere.

With these circumstances in view, I contrived an apparatus for determining the comparative heats of the different kinds of air, the outlines of which are expressed in plate II.

AB, CD, represent two slight brass cylindrical vessels, of equal weights and capacities, terminating in the small tubes *ls*, *rk*, at the extremities of which are fixed the brass cocks *s* and *k*. These cocks are joined to the cross bar MN, which consists of two elastic brass plates that are screwed together at the middle, but are capable of being separated from each other, to a little distance, at their extremities; in consequence of which the necks of the cocks, that are fixed to the stems of the brass vessels, can be slipped in between them, where they are lodged in grooves formed for receiving them, and are made fast by small screws that pass through the extremities of the plates. To the middle of the cross bar is fixed a square socket, the inner surface of which is finely polished, and which is of such a size as to be accurately adapted to the

N

polished

polished upright bar EF, and at the same time easily moveable upon it. At equal distances from the upright bar are placed the tinned vessels GH, IK, that have a little more than double the capacity of the brass cylinders. The distances of the vessels and cylinders from the upright bar are so adjusted, that when the cylinders are made to descend, they shall be directed by the bar and socket precisely into the centres of the tinned vessels, and when, in descending, they have arrived at $\frac{1}{8}$ of an inch from the bottoms of those vessels, they are stopped by means of a pin fixed in the upright bar at the point T. The cylinders being formed for the purpose of receiving the airs, which were to have their comparative heats determined, were made very slight, in order that the quantities of the airs, might bear as great a proportion as possible, to the weight of the vessels in which they were contained. With a view to diminish the inaccuracy unavoidably arising from the heats communicated by the cocks *s*, *k* and by the cross bar MN, the tubes *ls*, *rk*, were made very slight, and of a considerable length; the distance from the cocks to the brass cylinders, being four inches
and

and seven-eighths. From the length and slightness of the tubes, it was found that the brass cylinders could not be kept sufficiently steady, in the centres of the tinned vessels, by the cross bar MN. For this reason another bar OP was added, a lateral view of which is seen in figure 1st, plate III.

a b represents a narrow brass plate of a considerable thickness, to which is fixed the socket *o p*, moveable upon the upright bar formerly described. The two thin plates *c n*, *d k*, are riveted to *a b*, at the points *o* and *p*, and their extremities *l* and *m* are formed into grooves which are adapted to one half of the circumference of the stems of the brass cylinders, to the other half of which are closely applied two brass pieces, which are also grooved, and which are fastened to the plates by the screws *r* and *s*. The two male screws, *g* and *h*, are passed through the extremities of the thick plate *a b*. These are introduced into two corresponding female screws in the thin plates *c n*, *d k*, by means of which, it is evident that the latter may be made to approach nearer to the former, or to recede to a greater distance from it. This motion is communicated to the stems of the brass cylinders,

ders, by means of which they are adjusted, in such a manner, as that, when they are introduced into the water, they shall be accurately in the centres of the tinned vessels, where they are kept steadily in their proper positions. The superior and inferior cross bars are connected together by a thin plate of brass, which passes from the socket of the former to that of the latter, and which is made fast by means of screws. The screw that fixes this plate to the socket of the inferior bar, is of such a length, as, when screwed home, to extend to a small distance beyond its inner surface, by which means the apparatus, in descending along the upright bar, may be fastened to any part of it.

To each of the tinned vessels two very small mercurial thermometers are fixed, which are of different lengths, the longer reaching as far as the centre of the vessel, the shorter extending a little lower than the surface.

The thermometers *ab* and *cd*, (see the figure, plate II.) which indicate the heat in the vessel *GH*, are applied to a thin plate of ivory *ef*, which has two scales divided upon it, one for each thermometer. This plate is fixed to the vessel by means of a clamp *gb*,
the

the shank of which (*g b*) consists of two thin elastic brass plates, united at the top, and separable from each other at the lower extremity. For the purpose of admitting the thermometers, the projecting part *o v*, having the figure of a paralelopiped, was added to the tinned vessel. The exterior side of this projection, is received between the plates of the shank described above. These plates are closely applied to it by means of their elasticity, in consequence of which the clamp is steadily retained in its position, at the same time that it admits of a considerable degree of elevation or depression. Hence the depth to which the thermometers are sunk, may be varied at pleasure; and in order that this depth may be accurately known, a scale of equal parts is divided upon the exterior plate of the shank. Through the upper part of the shank is passed a small male screw *S*, similar to that which is used in adjusting the focus of a telescope. This is introduced into a corresponding female screw inserted in the ivory plate, which has the thermometers fixed to it; and as that plate is suspended in the clamp by a pin, around which it has a free motion, it is manifest that by turning the above-men-

tioned screw, the thermometers may be made to approach nearer to the centre of the vessel, or to recede farther from it.

For the purpose of admitting a third thermometer, which was used in some experiments, a small female screw was foldered obliquely near to the bottom of each of the tinned vessels. Through this the thermometer was introduced into the vessel, and was made fast by means of a male screw cemented to its stem. These screws pressed upon each other by flat surfaces, which were ground with such accuracy, as to be oil or water tight, without the help of leathers. Two small mercurial thermometers *l* and *r* (see fig. plate II.) were introduced by means of screws into the centres of the brass cylinders, for the purpose of measuring the heats of the airs which they contained. These thermometers have the degrees divided upon their stems by a diamond.

The following method was employed to free the air from moisture, and to introduce it into the brass cylinders: see plate I. fig. 7. *a b c d* represents a pewter syringe, which is occasionally filled with water, and inverted in a vessel containing the same fluid. The syringe be-
I
ing

ing placed in this situation, the air, which is to be the subject of the experiment, is introduced into it by means of a glass funnel. When the air is introduced, a small pipe *g b*, which has the cock *g* fixed to it, is screwed to the extremity of the syringe below the surface of the water. The cock being open, the piston (*n o*) is forced a little downward, to expel the water with which the pipe *g b* is unavoidably filled, when it is screwed to the syringe. Upon this the cock being closed, and the syringe being raised from the water, it is screwed to the worm *k l*, that is fixed in the wooden vessel *i m*, and is surrounded with a mixture of snow and salt.

A flaccid bladder previously freed from air, by rolling it with packthread, and having a brass cock *p* firmly attached to its neck, is screwed to the extremity of the worm *l*. The communication through the worm is then opened by turning the cocks *p* and *g*; and the air is gradually forced from the syringe into the bladder, in which it is confined by shutting the cock. The bladder containing the air is next separated from the worm, and is screwed to the extremity of the cock, which is fixed to the stem of the brass cylinder, the

latter having been previously exhausted by an air pump; the communication is now opened by turning the cocks, and the air rushes from the bladder into the cylinder.

The worm is surrounded with a mixture of snow and salt, that the air, by the cold to which it is exposed in its passage, may be as much as possible freed from moisture. The experiments which have been made to prove that ice evaporates, cannot properly be objected to this method of separating moisture from the air. For my friend, Mr. Wilson, has discovered by statical experiments, that at very low temperatures, no evaporation takes place from the surface of snow *.

In trials for determining the comparative heat of air, by means of the apparatus now described, it is of great importance that the worm, the bladder, and brass cylinder, should be perfectly dry; and this circumstance being properly attended to, I have not found any difference to take place, between the heat

* It has been proved by Wallerius, that the evaporation of ice is greatest at the moment of congelation; and that it diminishes when the cold is increased. See Wallerius *Memoir de l'Academie de Suede*, 1746. The experiments of Wallerius on this subject are confirmed by those of Muschenbroke.

communicated to water by air, which, previously to its introduction into the cylinder, had been cooled to a low temperature, and by that which had been introduced at the temperature of the air in the room. It is proper, however, that the airs should be introduced at a temperature somewhat lower than that of the water in the tinned vessels; because we are then certain that they do not contain any aqueous vapour, capable of being condensed by the fluid in which they are immersed.

From Saussure's experiments it appears, that the quantity of aqueous vapour attracted by the air of the atmosphere, when at 65 of Fahrenheit, is very small, a cubic foot of air requiring not more than eleven or twelve grains of vapour to bring it from the state of perfect dryness to that of extreme moisture. Hence it follows, that not more than $\frac{1}{3}$ of a grain would be necessary to saturate with aqueous vapour, 37.9 cubic inches of air, which is equal to the solid content of one of the cylinders. It has also been proved by the same Philosopher, that minute quantities of water are sufficient to saturate the other permanently elastic fluids. When we find, therefore,

fore, that the aërial fluid, which is to be the subject of our experiment, is at any of the intermediate degrees, between the points of saturation and extreme dryness, we are sure that it contains a very small quantity of moisture.

The brass cylinders were heated by the following contrivances, see plate III. figure 2. A B represents a quadrangular tinned vessel, that has the two smaller vessels C D, E F, fixed to its bottom by means of dovetails, along which they may be made to slide, and may be so adjusted that their distances from each other, shall be equal to the distances of the cylindrical vessels in which the air is contained; their size being such that they shall be nearly accurately filled by those vessels, when the latter are introduced into them. It is, therefore, manifest that warm water contained in the large vessel A B, and coming into contact with the external surfaces of the smaller vessels C D, E F, will necessarily communicate a part of its heat to those vessels, and to the cylinders which are placed in them. If the cylinders had been heated by being immediately immersed in warm water, a quantity of moisture would have adhered to their
sides,

sides, which would have produced an inaccuracy in the experiment.

In constructing the above described apparatus, which was made by Messrs. Nairne and Blunt, great care was taken that the brass cylinders should be precisely equal in weight, dimensions, and capacity. Their weight is 7 ounces, 3 drams, 38 grains troy each; the length is 8 inches and $\frac{7}{8}$; the breadth 2 inches $\frac{3}{8}$ near to their upper extremity, and 2 inches $\frac{5}{8}$ near to the bottom. They are each of them capable of containing twenty ounces Troy of water, and consequently the solid content is 37.91 cubic inches. The tinned vessels are also equal to each other in weight and dimensions. They are covered with thick flannel for the purpose of confining, as much as possible, the heat imparted to the water by the cylinders.

The bulbs of the thermometers were not more than $\frac{2}{10}$ of an inch in diameter, in consequence of which they were very sensible; and their bores were so minute as to admit of a large scale, each degree of Fahrenheit being divided into ten equal parts, distinctly visible to the naked eye. It was found a very difficult undertaking to graduate four thermometers

mometers of so small a size, with such precision, as accurately to correspond with each other. After many attempts, in the course of several months, it was at length accomplished. They were so exactly adjusted, as not to differ from each other more than the $\frac{1}{10}$ of a degree in any part of the scale, excepting in a small portion of it near to the beginning, where the variation was $\frac{2}{10}$ of a degree. That the difference might be still farther diminished, a scale of the errors was formed, by immersing the thermometers in a vessel containing a large quantity of warm water, and by accurately examining the contractions in each thermometer, which corresponded to the diminutions of heat in the water.

I shall now proceed briefly to relate the experiments which were made with this apparatus. The following was tried with a view to examine the accuracy of the instrument.

EXPERIMENT I.

The cylinders having been previously made perfectly dry, and having been exhausted by means of an air pump, equal quantities of atmospherical air were introduced into them.

them. They were placed in the water bath formerly described, and were exposed to the action of boiling water for ten minutes.

Into each of the tinned vessels G H, I K, see fig. plate II. was introduced 20 ounces Troy of water, having very nearly the same temperature with the air in the room. The water bath and cylinders being now removed from the fire, were placed upon a stand below the table, on which the upright bar and tinned vessels were fixed. The cylinders were then instantly raised from the bath, and were introduced, by means of the socket and upright bar, into the centre of the water in the tinned vessels, where they were kept fast by a screw applied to the bar. Having been suffered to remain in this situation for five minutes, they were removed; the water in the vessels during a few seconds was equally agitated; after which the temperatures being examined by the central thermometers, with the help of a magnifier, were found to correspond within less than the $\frac{1}{20}$ of a degree. This experiment was several times repeated with a similar result, the cylinders being suffered to remain immersed in the water, during the space of eight, twelve, and sixteen minutes

minutes successively. The cylinders were heated in a room adjoining to that in which the tinned vessels containing the water were placed; because a fire in the latter, by disturbing the equality of its temperature, would have rendered the experiment liable to inaccuracy.

The tinned vessel and brass cylinder, placed towards the right hand during the examination of the thermometers, may be distinguished by the letter R; those towards the left by L.

EXPERIMENT II.

The cylinder L was filled in the manner described above with dephlogisticated air, the purity of which, when it was examined, at the conclusion of the experiment, was such that one measure of it, mixed with two of nitrous air, occupied the space of one measure.

Fresh atmospheric air was introduced into the cylinder R.

Twenty ounces Troy of water was contained in each of the tinned vessels.

The air in the room was 58.1.

The water in the vessels being agitated for a few seconds, the temperature in R was found to be 57.8; that in L, 57.8.

The

The cylinders having been previously exposed, in the water bath, to the action of boiling water, for 10 minutes, were suddenly introduced, as in the former experiments, into the tinned vessels. They were kept in this position for 14 minutes, during which time the temperatures, examined by the central thermometers, were as follows :

	R		L
1 min.	58.8	a little more	59.1
2 -	59.1	—	59.3
3 -	59.2	—	59.4 a little less
4 -	59.2	—	59.4
5 -	59.3	—	59.5 a little less
6 -	59.3	—	59.5 very nearly
7 -	59.3	—	59.5
10 -	59.4	—	59.6 a little less
14 -	59.5	—	59.7 a little less

The cylinders being then removed, and the water agitated for a short time, the general temperature in the vessel R was a little less than 60; that in L 60.2.

The air in the room was 60.25, very nearly.

This experiment was repeated with the following result.

E x-

EXPERIMENT III.

Air in the room — 58.6

Water in each of the

tinned vessels - 59.5 very nearly.

Dephlogisticated air, having the same degree of purity with that used in the preceding experiment, was introduced into the cylinder L, and atmospherical air into R.

The cylinders were then heated, and immersed in the tinned vessels, in the manner described above; and the temperatures being accurately examined by the central and superficial thermometers, were as follows.

Min.	R		Surface.	L		Surface.
	Centre.			Centre.		
2	61.1	<i>p.</i>	62.9	61.2	<i>p.</i>	63.1
3	61.1	<i>p.</i>	62.9	61.3	<i>m.</i>	63.1
4	61.2	<i>m.</i>	62.8 <i>p.</i>	61.4	<i>m.</i>	63.1
5	61.2	<i>p.</i>	62.8 <i>p.</i>	61.4	<i>m.</i>	63 <i>p.</i>
6	61.2	<i>p.</i>	62.8 <i>p.</i>	61.4		63 <i>p.</i>
7	61.2	<i>p.</i>	62.8	61.4		63 <i>p.</i>
8	61.2	<i>p.</i>	62.8	61.4		63 <i>p.</i>
9	61.2	<i>p.</i>	62.7 <i>p.</i>	61.4	<i>p.</i>	62.9
10	61.2	<i>p.</i>	62.7 <i>p.</i>	61.4	<i>p.</i>	62.9
11	61.2	<i>p.</i>	62.7	61.4	<i>p.</i>	62.9
12	61.2	<i>p.</i>	62.7	61.4	<i>p.</i>	62.9

N. B. The letter *p*, placed after some of the figures, signifies, *a little more*, and the letter *m*, *a little less*. These letters were added when the excess or defect did not amount to more than the $\frac{1}{10}$ of a degree.

The

The cylinders being removed, and the water a little agitated, the general temperature in R was ——— 61.5 *p*.

In L — — — 61.7 *p*.

The air in the room was a little less than — 61.

The reader will perceive that, in the course of these experiments, the temperature of the air in the room was so much increased, as to be nearly equal, at the end of 12 or 14 minutes, to the temperature of the water in the tinned vessels.

This was effected by the heat of the persons who were present, and by candles that were brought into the room at the beginning of the experiment.

If the temperature of the air in the room were not increased nearly in proportion to that of the water, the difference between the heats imparted by the dephlogistified and atmospherical air, would gradually diminish in the course of the experiment, and would at length entirely vanish. For though the tinned vessels were enveloped with coverings that were slow conductors of heat, yet when the air in the room had a lower temperature than the water in which the cylinders were immersed, the escape of the heat could not
O be

be entirely prevented; because we do not know any substance in nature which is altogether impervious to that principle: and hence it is manifest that if the portions of water, to which the airs in the cylinders imparted their heat, were placed in a cooling medium, the fluid in the vessel, which had the highest temperature, would most speedily give off its heat to the surrounding bodies. The excess of the heat communicated by the pure air, above that communicated by the atmospherical, would therefore gradually diminish, and would at length vanish.

It is evident that, in the foregoing experiments, most of the causes of error formerly enumerated, were avoided.

The effects of moisture were obviated by making the cylindrical vessels perfectly dry, and introducing the airs into them at a low temperature; as the two kinds of air were exposed for the same length of time to the action of boiling water, they must necessarily have acquired the same degree of heat; being raised from the bath, and introduced into the water at the same moment, they must have been immersed with equal velocities; the portions of water in the tinned vessels, previously to the immersion, had nearly the same temperature

temperature with the air in the room; and the heats imparted to them were indicated by accurate thermometers, which were very sensible, and had a large scale. After the cylinders were immersed, the temperature of the air in the room was gradually augmented, in such a manner, as to be nearly equal to the temperature of the water at the end of the experiment; and hence the errors which might proceed from an inequality in the refrigeration of the water, were in a great measure avoided. The fallacy arising from the heats imparted by the vessels, in which the airs were contained, was also obviated by this apparatus. For as the cylinders were perfectly equal in temperature, weight, and capacity, the portions of heat communicated by them to the water, must have been equal, and must therefore have balanced each other. Whence it is evident that the difference observed, in the above experiments, must be attributed solely to the difference between the heats imparted by the dephlogisticated and atmospheric air.

The following experiment proves, that if atmospheric air be contained in both cylinders, and if, at the beginning of the experiment, the water in one of the tinned vessels

have a higher temperature than that in the other by $\frac{2}{10}$ of a degree, this difference will exist during the space of 12 minutes, and will appear at last in the water after the cylinders are removed.

EXPERIMENT IV.

Atmospherical air was introduced into both cylinders.

The air in the room was	-	59.5,
The water in the vessel R	-	60.4,
that in L	-	60.6.

The cylinders, heated as before, were suddenly immersed in the water, and the temperatures at the centre and surface were examined during the space of 12 minutes.

		R		L		R		L
	Min.	Centre.		Centre.		Surface.		Surface.
In	1	61.6 <i>p.</i>		61.7		63.9 <i>m.</i>		63.9
	2	61.9 <i>m.</i>		62		63.9 <i>m.</i>		63.9
	3	61.9		62 <i>p.</i>		63.9 <i>m.</i>		63.9
	4	61.9		62.1 <i>m.</i>		63.9 <i>m.</i>		63.9
	5	61.9		62.1 <i>m.</i>		63.8 <i>p.</i>		63.9
	6	61.9		62.1 <i>m.</i>		63.8		63.9
	7	61.9		62.1 <i>m.</i>		63.8		63.9
	8	61.9		62.1		63.7 <i>p.</i>		63.9 <i>m.</i>
	9	61.9		62.1		63.7		63.9 <i>m.</i>
	10	61.9		62.1 <i>p.</i>		63.6 <i>p.</i>		63.8 <i>p.</i>
	11	61.9 <i>p.</i>		62.1 <i>p.</i>		63.6 <i>p.</i>		63.8 <i>p.</i>
	12	61.9 <i>p.</i>		62.1 <i>p.</i>		63.6 <i>p.</i>		63.8.

The

The cylinders being removed, and the water agitated, the general temperature in R was — — 62.3,
 that in L was — — 62.5.

At the conclusion of the experiment, the temperature of the air in the room was nearly a degree lower than that of the water in the tinned vessels.

The reader will perceive that, in the second experiment, the temperature of the water, before the cylinders were immersed in it, was a little less than that of the air in the room; in the third its temperature was a little greater; and in the fourth the water had not only a higher temperature, at the beginning of the experiment, than the surrounding air; but the heat of the latter, at the conclusion, was nearly a degree below that of the former.

These seem to have been the principal reasons, why, in the second experiment, more heat was imparted to the water by the cylinders, than in the third, and in the third more than in the fourth.

It is, however, manifest that the quantity of heat communicated must in some measure depend upon the velocity with which the cylinders are immersed in the water, and it is

difficult, in different experiments, to immerse them with velocities that are precisely equal.

In making these experiments, it is necessary that the cylinders, when they are raised from the water bath, should be moved as speedily through the air as possible; and at the same time, as soon as they have approached to the water, in the tinned vessels, they should be introduced with a moderate degree of velocity; for if the velocity with which they are introduced be very great, the impulse given to the water will be such as to force a part of it from the vessels, and the warmer and colder particles at the surface and centre being intermixed, will produce an irregularity in the heats indicated by the thermometers.

It appears from the above experiments, that when dephlogisticated air was contained in the cylinder L, and atmospherical air in R, the former communicated a greater quantity of heat to an equal bulk of water than the latter, by $\frac{2}{10}$ of a degree; and that the purity of the dephlogisticated air, which produced this effect, was such that one measure of it, mixed with two of nitrous air, occupied the space of one measure.

As these experiments are of a very delicate nature, in order still farther to guard against fallacy, they were reversed by introducing dephlogisticated air, nearly of the same purity with the above, into the cylinder R, and atmospherical air into L. It was then found, all other circumstances being the same as in the former experiments, that the pure air in the cylinder R imparted more heat to the water, than the atmospherical air in L, by $\frac{2}{16}$ of a degree.

I varied this experiment, by introducing hot oil instead of boiling water into the vessel used for heating the cylinders; and having thus raised the cylinders, one of which contained pure, and the other common air, to 300, I suddenly immersed them in equal portions of water, that had nearly the temperature of the air in the room; and the thermometers being accurately examined by my friend, Dr. Percival, during several minutes, the difference of the heats, imparted by the pure and common air, was found to be nearly $\frac{3}{16}$ of a degree.

The tinned vessels, in which the water was contained, were larger than those used in the preceding experiments, the quantity of water

in each being 32 ounces, 6 drams, and 28 grains Avoirdupoise. Though this experiment was made with great care, yet it was supposed that the result was not altogether accurate, because, upon immersing the cylinders in the water, a hissing noise was heard, and a small quantity of vapour was observed to ascend from each vessel. It does not however appear, that the inaccuracy produced by the ascent of the vapour in this instance was considerable; for if a just allowance be made for the heat which escaped from the cylinders, during their passage through the air, when they were raised from the bath, for the purpose of introducing them into the tinned vessels, the ratio of the heats of pure and of common air, as determined by this experiment, will appear to be very nearly the same with that determined by those which were formerly recited.

Though the results of the foregoing experiments appear to be decisive, yet in order to ascertain the comparative heats of the aërial fluids with greater precision, it was thought proper, in the experiments which I shall now relate, to substitute oil as a standard instead
of

of water ; the former having a less capacity for heat, and consequently having its temperature more augmented by the addition of a given quantity of that principle, than the latter.

It is proper to observe, that when experiments, similar to those recited above, are made with oil, the variations of heat produced by the immersion of the cylinders, continue for a much greater length of time, than when they are made with water. The reader will perceive that these variations could not be exactly ascertained, by placing the thermometers in the middle point between the brass cylinders, and the interior surfaces of the tinned vessels ; for, in that case, those instruments would not indicate equal changes of heat, unless they possessed equal sensibilities ; but four thermometers equally sensible can scarcely be obtained ; for, in order that they should possess this quality, it is necessary that the quantities of mercury which they contain, and the thickness of their bulbs, should be precisely equal.

Now it is manifest that thermometers, unequally influenced by heat, cannot determine variable differences with precision, unless

less they be adjusted at distances from the heating bodies, proportional to their sensibilities. This adjustment was accomplished by repeatedly introducing the heated cylinders, containing equal quantities of atmospherical air, into the cold oil, and varying the distances of the thermometers, until the heats indicated by them were equal. After many trials made with the most solicitous attention, in which I was assisted by my friend, Dr. Hamilton, on whose accuracy I could place the greatest reliance, the central thermometers were at length adjusted with such exactness, that the heats indicated by them did not vary from each other more than the tenth of a degree, which will appear from the following experiment.

EXPERIMENT VI.

Air in the room $56\frac{1}{2}$.

One pound, four ounces and $\frac{3}{4}$ of an ounce Avoirdupoise of spermaceti oil, was introduced into each of the tinned vessels, the depth of the oil being a little more than half the height of the vessel, in consequence of which

which the central thermometers reached to a small distance below the surface of the oil, and were therefore compleatly immersed in it.

The temperature of the oil was a few degrees lower than that of the air in the room, in order that when it was heated by the cylinders, its mean heat might be nearly equal with that of the surrounding air. The portions of oil in the tinned vessels were equally agitated by small glass tubes, that the heat might be equally diffused over them; the agitation being continued for a few minutes until the thermometers became stationary. This was repeated several times, and the temperatures being observed, immediately before the cylinders were immersed, that in R was found to be 53.5; L 53.5.

The cylinders containing atmospherical air were heated in an adjoining room, by means of the water bath formerly described, which was kept at the boiling point for 10 minutes. The bath and cylinders being then removed into the room, in which the tinned vessels were placed, and the cylinders being speedily immersed in the oil, the temperatures, during the space of five minutes, were as follows:

Centre.

	Centre.		Surface.	
	R	L	R	L
1 Min.	53.6	53.6	58.4	60
2	54.3	54.4	60.7	61.3
3	54.8	54.7	61.1	61.3
4	55.3	55.2	61.1	61.3
5	55.8 <i>m.</i>	55.7	61.1 <i>p.</i>	61.3.

The air in the room at the end of the experiment was 60. As the two thermometers in each of the tin vessels were placed on the same ivory plate, having a double scale, the reader will perceive that when those at the centre were adjusted in such a manner as to indicate nearly equal heats, the superficial ones could not be so adjusted, unless the latter had possessed precisely the same degree of sensibility with the former; but as their sensibilities were different, the heats indicated at the surface, during the first two minutes, were very unequal; after which they approached to each other within the .2 of a degree.

I then made the following experiment, with the assistance of Dr. Hamilton, in the presence of my learned friend, Mr. Kirwan, and of the late much lamented Doctors Watkinson and Kerr.

EXPERIMENT VII.

The air in the room being 54, and the oil in R 51.3 plus; that in L 51.3; dephlogisticated air being introduced into R, and air phlogisticated by iron filings and brimstone into L; and the cylinders being heated as before, and immersed in the oil; the temperatures observed by Dr. Hamilton, during seven minutes, were as follows:

	Centre.		Surface.	
	R	L	R	L
2 Min.	52.1	52.3		
3 $\frac{1}{2}$ nearly	53.4	53.1	59.5	59.7
5	54.4	54	59.5	59.5
6	54.8	54.4	59.5	59.5
7	55.1	54.7 plus	59.4	59.4

The purity of the dephlogisticated air was examined at the conclusion of this experiment, a part of it being drawn from the cylinder by means of an exhausted glass vessel, when it was found that one measure of it, mixed with two of nitrous air, occupied the space of a little more than one measure. The phlogisticated air, in the course of the experiment, was nearly changed into atmospheric air.

This

This experiment was repeated with the following result.

EXPERIMENT VIII.

Air in the room 54.5.

Dephlogisticated air, nearly as pure as that used in the foregoing experiment, was introduced into the cylinder R, and common air into L.

The temperature of the oil in the vessel R was 51.3; that in L 51.1.

The cylinders being heated and immersed in the oil, the temperatures, indicated by the central and superficial thermometers, were,

	R	R	L	L
Minutes.	Centre.	Centre.	Surface.	Surface.
in 2	51.8	51.7		
3	52.6	52.3 <i>p.</i>		
4	53.6	53.2	58.8	58.9 <i>p.</i>
5	54.	53.5	58.8	58.8
6	54.3	53.8	58.7 <i>p.</i>	58.7 <i>p.</i>
7	54.5	54.2	58.7	58.7

In Experiment VI. the heat indicated by the central thermometer in the vessel R, at the end of five minutes, exceeded that in the vessel L by $\frac{1}{15}$ of a degree.

In

In Experiments VIIth and VIIIth the excess was a little less than $\frac{4}{10}$ of a degree, allowance being made for the difference of temperature indicated by the thermometers, in the vessels R and L, before the cylinders were immersed in them.

These experiments were frequently repeated with great care, and it was uniformly found, that when dephlogisticated air, which had the purity of 1, as indicated by Dr. Priestley's Test, was introduced into R, and atmospherical air into L, the heat imparted by the former to the oil, at the end of four minutes, was $\frac{3}{10}$ of a degree greater than that by the latter; the variation in the results of different trials being not more than the $\frac{1}{20}$ of a degree.

I next reversed the experiments, with pure and phlogisticated air, in the presence of Mr. De Luc, pure air being introduced into the cylinder L, and phlogisticated air into R. But in order that the results might be more decisive, the accuracy of the apparatus was first tried, by making the experiment with atmospherical air, in both cylinders, when it was found that the heats communicated to the central thermometers, did not
vary

vary from each other more than the $\frac{1}{10}$ of a degree.

This will appear from the following experiment, in which the variations of temperature were observed by Mr. De Luc, during the space of ten minutes.

EXPERIMENT IX.

Air in the room	—	54
The oil in the tinned vessel R		52.1
That in L	— —	52.3

The cylinders, containing atmospherical air, being heated as before, and immersed in the oil,

Minutes.	Centre.	Centre.
in 2	R was 53.3	L 53.5
4	54.5 <i>p.</i>	54.7
5	54.9 <i>p.</i>	55.1
6	55.1 <i>p.</i>	55.2 <i>p.</i>
8	55.4 <i>p.</i>	55.6 <i>p.</i>
9	55.5 <i>p.</i>	55.7

In this experiment, therefore, the difference of the heats communicated to the central thermometers did not exceed the $\frac{1}{10}$ of a degree; allowance being made for the excess of temperature in the vessel L, previously

ously to the immersion of the cylinders; and during the greater part of the time the heats communicated were perfectly equal.

EXPERIMENT X.

Air in the room	—	58
The oil in the vessel R	—	56.9
———— in L	—	56.9

Dephlogistified air being introduced into the cylinder L, and phlogistified air into R; and the cylinders being heated and immersed in the oil, the temperatures, as observed by Mr. De Luc, during 10 minutes, were as follows:

Minute.	Centre.	Centre.
1	R 57.2 <i>p.</i>	L 57.2 <i>p.</i>
2	57.7	57.9 <i>p.</i>
3	58.3 <i>p.</i>	58.6
4	58.9	59.2
5	59.3	59.5
6	59.7	59.9
7	59.9 <i>p.</i>	60.1 <i>p.</i>
9	60.1	60.3
10	60.3	60.4 <i>p.</i>

It is proper to observe, that the heats indicated by the thermometers at the surface in

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this

this experiment, were nearly equal; and the same thing happened in Experiment VII.

That it might be known, whether the airs suffered any change by exposure to heat in the cylinders, their purity was examined previously to the experiment, in the presence of Mr. De Luc, when it appeared that one measure of the dephlogisticated air, mixed with two of nitrous air, occupied the space of $\frac{3}{10}$ of a measure; and two measures of the phlogisticated air, mixed with one of nitrous air, occupied the space of 2.6. Being again examined, at the conclusion of the experiment, in the presence of the same philosopher, the purity of the dephlogisticated air was found to be so much diminished, that one measure of it, mixed with two of nitrous air, occupied the space of $\frac{8}{10}$ of a measure; and the phlogisticated air was nearly changed into atmospherical, two measures of it, with one of nitrous air, occupying the space of 2.15.

It appeared from subsequent trials, that the stop cocks, which were screwed to the ends of the cylinders, when exposed to heat, were not altogether air tight: for though they were made with such accuracy, that the cylinders, when cold, could be very nearly exhausted,

exhausted, yet when the latter were placed in the water bath, the heat which ascended along the stems, was not so speedily received by the stoppers, as by the cavities to which they were adapted; and hence the latter, expanding more speedily than the former, the exclusion of the external air became less perfect.

It appears from this experiment, that phlogisticated air has a much greater tendency to mix with the air of the atmosphere than pure air. For the difference of alteration, in those species's of air, did not arise from a difference in the stop cocks, because the same effects were produced when the experiment was reversed. The truth of this observation was moreover confirmed, by exposing pure and phlogisticated air to a moderate heat, in thin bladders, that had been previously well rubbed.

In the above experiment, the heat observed in the centre of the vessel L, at the end of four minutes, was nearly $\frac{3}{15}$ of a degree greater than that observed in R. After the 4th minute was expired, the difference began to diminish, and in the course of the six succeeding minutes it nearly vanished, the

excess of heat in L, at the end of that time, being only $\frac{1}{8}$ of a degree.

It was before observed, that, in the above experiments, the heats indicated by the superficial thermometers were nearly equal; the temperature of the oil, at the surface, being liable to be influenced by causes which could not with certainty be counteracted. The chief of these seemed to be, the tendency of the heated particles of oil to ascend to the surface; the facility with which those particles communicate their heat to the neighbouring bodies; the agitation given to the oil by the immersion of the cylinders; the heat imparted by the stems which connect the cylinders with the cross bars; the influence of the external air; and the difficulty of adjusting the superficial thermometers with such precision that they should indicate equal heats. But at the centre, the excess of heat communicated by the dephlogisticated air, whether it were contained in the vessel L or R, was very nearly invariable, as the experiment, when repeated with accuracy, never failed to succeed within the $\frac{1}{8}$ of a degree; and when water was used as a standard, instead of oil, the excess of heat imparted by the

the pure air appeared not only at the surface and centre, but was observed to exist after the cylinders were removed, and the several parts of the water were reduced by agitation to a common temperature.

It is proper to remark, that the difference of the heats communicated by dephlogisticated air to oil and water, as indicated by the central thermometers in these experiments, was less than might have been expected from the difference of the capacities of those substances. For the capacity of water is to that of spermaceti oil nearly as 2 to 1, from which it might be supposed, that the heats imparted would be as one to two. But they appear, in the foregoing experiments, to have been as 2 to 3. We have also seen, that the difference of heat at the centre, when the experiment is made in oil, vanishes more speedily than when it is made in water. These irregularities seem principally to be occasioned by the difference in the conducting powers of those substances with respect to heat, and by the greater tendency of the particles of warm oil to ascend, in consequence of which the heat is suddenly accumulated at the surface, and is speedily communicated to the neighbouring

P 3

bouring bodies. It is manifest, that the causes which diminish the differences of heat at the surface, will gradually produce a similar diminution at the centre.

When the experiment was made by introducing the cylinders into the tinned vessels, which contained atmospherical air alone, the effects produced by the sudden transmission of the heat were more evident; for, in that case, the difference of the heats communicated by pure and phlogisticated air could not at all be perceived at the surface, and at the centre it did not appear till after the expiration of many minutes; and though, at length, pure air evidently communicated more heat than phlogisticated air, yet the results of the different trials were so irregular, that no precise conclusions could be drawn from them.

The foregoing experiments prove, in general, that pure air communicates more heat to an equal bulk of oil or water than atmospherical air.

I next endeavoured to determine the comparative quantities of heat communicated by phlogisticated, inflammable, and fixed air.

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From the preceding experiments on pure and atmospherical air, it appears, that when the cylinders are immersed in oil, more minute differences of heat are indicated by the central thermometers, than when they are immersed in water: for this reason, in the experiments which follow, oil was used as a standard. It is of great importance to the success of the experiment, when this fluid is made the standard, that the heat, previously to the immersion of the cylinders, should be uniformly diffused over the oil, and should remain as nearly stationary as possible. But the heat of the oil cannot remain stationary, if the temperature of the air in the room vary; and in rooms which are exposed to the influence of the weather, a permanent temperature can scarcely be obtained. For the purpose of avoiding this cause of inaccuracy, the apparatus was placed in a vault below the surface of the earth, where the alterations of temperature were very slow, and the diffusion of heat nearly uniform.

It was before observed, that as the stop-cocks, which were fixed to the stems of the cylinders, were unequally heated, they did not sufficiently interrupt the communication

between the external air and that in the cylinders; in consequence of which the air was found to undergo a considerable change in the course of the experiment. This difficulty was obviated in the following manner: as soon as the air was introduced into the cylinder, the stop-cock was unscrewed from the extremity of the stem, and the cavity of the latter was accurately closed by a cork, after which the cock was again fixed to it. In consequence of this, when the apparatus was heated, the cork, by its elasticity, adapted itself to the increased capacity of the stem; and thus the interruption of the communication between the air in the cylinder and the external air, being rendered nearly complete, the former was found to suffer very little change from exposure to heat.

With these precautions I endeavoured to determine the capacity of phlogisticated air, the apparatus having been previously so adjusted, that when atmospherical air was contained in both cylinders, the heats indicated by the central thermometers were very nearly equal, as appears from the experiment which follows.

E x-

EXPERIMENT XI.

Air in the vault — 60.7

One pound, three ounces and two drams of spermaceti oil being introduced into each of the tinned vessels, the temperature in the vessel R, was 59.9; that in L, 60 plus.

The cylinders, containing atmospheric air, were heated in an adjoining vault by means of the water bath, and were suddenly immersed in the oil, upon which the central heats being accurately marked during the space of 10 minutes,

Minutes.

In 2	R was 60.2	L 60.2
3	60.9	61.
4	61.5	61.5 plus.
5	62. minus.	62.1 minus.
6	62.3 plus.	62.4 plus.
7	62.6 minus.	62.7
8	62.8	62.9
9	62.9	63.
10	63. plus.	63.1 plus.

Air in the vault 61.7.

E x-

EXPERIMENT XII.

Air in the vault	—	62.6
Oil in the tinned vessel R	—	62.1
In the vessel L	—	62.2

Air phlogisticated by hepar sulphuris was introduced into the cylinder R, and atmospheric air into L. The cavities of the stems of the cylinders were accurately closed by corks in the manner described above; the cylinders were then heated as before, and immersed in the oil; and the central heats being observed during six minutes,

Minute.

In 1	R was 62.2	L 62.3
2	62.5	62.6 <i>p.</i>
3	63.1 <i>p.</i>	63.3
4	63.7 <i>p.</i>	63.9 <i>p.</i>
5	64.2 <i>p.</i>	64.3 <i>p.</i>
6	64.5	64.6

Air in the vault at the conclusion of the experiment 63.3.

This experiment was repeated with the following result.

E x-

EXPERIMENT XIII.

Air in the vault	—	63.
Oil in the vessel R	—	62.3 <i>m.</i>
In L	—	62.3 <i>m.</i>

The cylinders, containing phlogisticated and atmospheric air as before, were heated and immersed in the oil, and the central heats being observed during eight minutes,

	R	L
In 1 Minute	62.4	62.4
2	62.7 <i>p.</i>	62.7 <i>p.</i>
3	63.4	63.5
4	64. <i>p.</i>	64.1
5	64.5 <i>m.</i>	64.5
6	64.8 <i>p.</i>	64.9
7	65.1	65.1 <i>p.</i>
8	65.3 <i>p.</i>	65.3

Two measures of the phlogisticated air, used in these experiments, having been mixed, previously to its introduction into the cylinder, with one of nitrous air, the measures of the test were 2.9. After the experiments were finished, a part of it was drawn from the cylinder, and being again examined by nitrous air, the measures of the test were found

found to be 2.8, very nearly. From which it appears, that very little alteration had been produced in it by exposure to heat in those experiments.

An interval of several days had elapsed between each of the experiments now recited. They were made in the month of July 1783, when the heat of the sun was gradually increasing, and at length became very intense. This accounts for the alteration in the temperature of the vault. It was found, in the course of eleven days, to suffer a change of 3 degrees.

From these experiments, compared with many others of a similar kind, which it is unnecessary to relate, it appears, that the difference of the central heats, imparted to the oil by atmospherical and phlogisticated air, was nearly $\frac{1}{10}$ of a degree.

Inflammable and fixed air were next examined, with great care, by the same apparatus. From the results of a variety of trials, compared with each other, it manifestly appeared, that the central heat communicated to the oil, by fixed air, was a little less than that communicated by atmospherical air, the difference being not so great as $\frac{1}{10}$ of a degree;

gree; and, on the contrary, when inflammable and atmospherical air were compared, the central heat imparted by the former was a little greater than that by the latter, the difference being, in this instance, also, less than the $\frac{1}{10}$ of a degree.

It may perhaps be doubted, whether such minute differences may not be liable to much uncertainty. With respect to this I would observe, that the $\frac{1}{10}$ of a degree on the scale of the thermometers, used in my experiments, is nearly as distinct as an entire degree of the thermometers that are commonly made with Fahrenheit's scale. And as each division can be nearly bisected by the eye, the $\frac{1}{20}$ of a degree may be distinguished with considerable precision. For this reason, I have ventured to lay the results of the foregoing experiments with inflammable, fixed, and atmospherical air, before the reader. From numerous trials, made with the utmost care, and variously diversified, the above-mentioned differences appeared to exist. There is, however, decisive evidence for believing, that the real differences do not exceed those which have been determined by these experiments, because the results of the several

several trials corresponded within the $\frac{1}{20}$ of a degree.

If, however, we should suppose, that the heats imparted by equal bulks of these three species of air were perfectly equal, it would not affect the doctrine contained in the following pages.

In the experiments recited above I have endeavoured to determine the *differences* of the heats imparted to water by fixed, phlogisticated, inflammable, atmospherical and dephlogisticated air. That the *entire* heats communicated by these fluids might be known, it was necessary to discover the quantity which was yielded by the cylinders employed for the purpose of immersing them in the water. This was determined by the experiments which follow.

EXPERIMENT XIV.

Air in the room	—	60.5
Water in the tinned vessel R		60. m.
In L	— —	60. m.

The brass cylinders, containing atmospherical air, being heated as before, and immersed in the tinned vessels, were suffered to remain

remain during the space of eight minutes; after which, being removed, and the water in both vessels being equally agitated, the temperature in R was found to be 62.2; that in L 62.2 *m*.

It appears, therefore, that when atmospheric air was in both cylinders, the heats imparted were nearly equal, the difference being not greater than the $\frac{1}{10}$ of a degree.

The quantity of water contained in each of the vessels was 20 ounces Troy.

In order to discover the quantity of heat, communicated by the air in this experiment, the water was suffered to enter into one of the cylinders in the following manner: the bottom of the cylinder R being opened, the spherical plate which closed it was introduced laterally, in such a manner, that the semi-circle, dividing its surface into two equal parts, should come into contact with the half of the circular extremity of the cylinder, to which it was fastened by folder. Hence the remaining half of the bottom being open, the water, when the cylinder was immersed in it, was suffered to enter freely, and the air at the same time was permitted to escape by the cock which was fastened to the upper extremity

extremity of the stem. In order that the cylinder, when its bottom was opened, might be compleatly furrounded by the water in the tinned vessel, it was necessary that the quantity of this fluid should be twice as great as that which was required to cover it when the bottom was closed, the water in the former case being permitted to enter into the cylinder, and in the latter being excluded. For this reason, in the following experiment, 40 ounces Troy of water were introduced into the tinned vessel R, and 20 ounces into L.

EXPERIMENT XV.

Air in the room — 62.

The temperature of the water in the vessel R, was 61.7 plus.

That in L — 61.8

The cylinders L and R, the latter being open above and below, and the former containing atmospherical air, were heated as in the foregoing experiment, and immersed in the water, where they were suffered to remain for eight minutes; being then removed, and the water in both vessels being equally agitated,

agitated, the general temperature in R was found to be 62.7° ; that in L 64° .

In this experiment the heat imparted by the cylinder R was one degree; that by L 2.2 , the quantity of water in the vessel R being 40 ounces, and that in L 20. If the quantity of water in the vessel R had been only 20 ounces, the heat communicated by the cylinder alone would have been nearly two degrees; and the heat imparted to the water in the vessel L, by the cylinder filled with atmospherical air being 2.2 , the difference gives nearly $\frac{2}{10}$ of a degree for the heat yielded by the air*.

In the above experiment the utmost attention was given to avoid the causes of fallacy, by which I had been deceived in the experiments on the capacities of the aërial fluids, recited in my former publication.

It must be observed, that those experiments were made under very unfavourable circumstances. They were my first attempts towards the discovery of truth in an obscure and intricate part of science, where the sources

* The reader will see the heat imparted by atmospherical air more accurately stated in page 234.

of error were numerous, and the means of avoiding them had not been ascertained. Differences, which arose from the condensation of greater or less quantities of moisture in the bladders that were employed in my experiments, I attributed to the different powers of the aërial fluids for communicating heat. My first experiment with atmospherical air was made with a fresh bladder, which is more disposed to attract moisture, and is dried with greater difficulty, than those which have been previously freed from their impurities by washing. This experiment was again repeated, after a considerable interval of time had elapsed, and the same bladder which had been accidentally purified from its salts and mucilage, by being kept for a considerable time under water, in the previous experiments, was again dried, and was used for the purpose of determining the heat imparted by fixed air. The thermometers which I employed were not sufficiently sensible, nor were their scales of such a size as to indicate minute variations of heat; and the immersion of the bladder and thermometer in the water being regulated by the hand, their relative positions

positions could not be adjusted with the accuracy which such delicate experiments required.

The heat imparted by the bladder was examined by rolling it with packthread, raising it to the desired temperature, and immersing it in water a little below the heat of the air in the room. I was not then aware, that, in these circumstances, a small portion of the surface of the bladder being in contact with the water, it communicates its heat more slowly than when it is distended with air.

The dephlogisticated air employed in the experiment, recited in my former publication, was obtained from red lead moistened with yellow spirit of nitre, which was exposed to heat in a Florentine flask, having a flaccid bladder, that was previously freed from air, tied to its neck. As it was my first attempt to procure dephlogisticated air, I had not observed that, in these circumstances, this fluid must be mixed with a considerable quantity of aqueous and nitrous vapour. I, therefore, did not deem it necessary, previously to the experiment, to pass the air through water, and transfer it into a dif-

ferent bladder; in consequence of which, when I attempted to determine its absolute heat, I was led into an error of nearly two degrees and a half.

Having determined the entire quantity of heat, yielded by atmospherical air, I next endeavoured to discover the heat which was lost by the cylinders, when they were raised from the bath, for the purpose of immersing them in the water. It was formerly observed, that in making the experiments recited above, the water bath, with the cylinders contained in it, was placed near to the bottom of the table on which the upright bar and tinned vessels were fixed; and that the cylinders, being raised from the bath, were directed into the vessel by means of the bar and socket. During this operation they were made unavoidably to pass through a portion of cold air, and from the flightiness of the cylinders, and the facility with which they transmitted heat, it is manifest, that the quantity imparted to the air must have been considerable. The heat which was thus lost was estimated by the experiment which follows:

E x-

EXPERIMENT XVI.

Air in the room — 63.2

Twenty ounces Troy of water
was introduced into the tin-
ned vessel L at — 61.9 *p*.

The cylinder L, containing atmospherical air, was exposed, as in former similar experiments, to a boiling heat in the water bath for 10 minutes.

The bath and cylinders were then placed on the same table with the tinned vessel L, in which the cold water was contained, a deal board being interposed between them to prevent the passage of the heat from the bath to the vessel, upon which the cylinder was instantly introduced into the vessel, and the bath removed: the cylinder being suffered to remain in this situation during six minutes, it was then raised from the vessel; the water was slightly agitated, and the general temperature at the end of

7 minutes was	—	64.5 <i>m</i> .
8	— —	64.5 <i>m</i> .
9	— —	64.5 <i>m</i> .

Q 3

The

The air in the room at the
conclusion of the experi-
ment was — 63.3

Hence the heat communicated to the water was 2.5 degrees.

In this experiment the cylinder, previously to its immersion in the water, came into contact with a very small quantity of cold air, and was made to pass through it with as much velocity as possible. Though these precautions were used to diminish the escape of heat from the cylinder, during its passage through the air, yet it was impossible entirely to prevent it. If we suppose that in the last experiment a quantity of heat escaped, which would have raised the water $\frac{1}{10}$ of a degree, we shall have 2.6 degrees for the whole of the heat communicated by the cylinder, and the air which it contained.

In Experiments XIV. and XV. the heat communicated to the water by the cylinder L, was only 2.2 degrees. In those experiments, therefore, the cylinder must have lost nearly $\frac{1}{6}$ of its heat during its passage through the air.

In Experiment XV. the cylinder R, having remained in the water bath for 10 minutes,

nutes, was raised to the temperature of 200, as appeared from the thermometer introduced into its centre.

The heat of the water in the tinned vessel, previously to the immersion of the cylinder, was 61.7. Hence the difference between the temperature of the water and cylinder was 138.3; of this quantity nearly $\frac{1}{8}$ was carried off by the external air. The remainder, or 115, plus 61.7, gives the true temperature of the cylinder previously to its immersion in the water. The temperature of the water, at the end of eight minutes, when the cylinder was removed from it, was 62.7 nearly. The cylinder was therefore cooled by the water 114 degrees, and the water heated by the cylinder one degree.

The weight of the water in the tinned vessel was 40 ounces Troy, and the capacity of the vessel for receiving heat was equal to that of one ounce of water plus, .23 of an ounce. The water and vessel were, therefore, equal in capacity to 41.23 ounces of water.

The heat, separated from the cylinder, was 114.25 degrees; and that received by the water was one degree, consequently the capacity of the water and vessel, or of 41.23

ounces of water, was to that of the cylinder as 114.25 to 1. If the quantity of water had been 114.25 ounces, the capacity of the cylinder would have been equal to that of one ounce of water; but the quantity of water was only 41.23 ounces. The capacity of the cylinder must, therefore, have been less than that of an ounce of water in the proportion of 41.23 to 114.25. That is, the capacity of the cylinder was equal to that of .3608 of an ounce of water.

Hence we may determine the heat, that would have been communicated by the cylinder, in the experiment in question, if the tinned vessel R had contained only 20 ounces of water, which was the quantity contained in the vessel L. For, according to Richman's rule, when two unequal quantities of water are mixed together at different temperatures, if the products of the masses, multiplied into the separate heats, be added together; and if this quantity be divided by the sum of the masses, the quotient will give the temperature of the mixture. That is, calling the masses M and m , and the separate heats C and c ; the temperature of the mixture will be

$$\frac{MC + mc}{M + m}.$$

It

It was proved, that the heat separated by the water from the cylinder R, in Experiment XV. was 114.25 degrees; and the heat of the water at the end of the 8th minute was 62.7 *p*. Adding this to 114.25, we have 176.95 for the temperature of the cylinder, at the moment of its immersion in the water. If we conceive the quantity of the water to be 20 ounces, and the vessel to be equal to one ounce plus .23 of an ounce of water, the vessel and water will be equal to 21.23 ounces. The temperature of the water was 61.7; and the cylinder was equal to .3608 of an ounce of water.

We have, therefore, according to Richman's rule,
$$\frac{21.23 + 61.7 \times .3608 + 176.95}{21.23 + .3608},$$
 equal to 63.626 for the temperature of the mixture; and the heat of the water, previously to the immersion of the cylinder, being 61.7, the difference gives 1.926 for the heat which would have been communicated by the cylinder, if the quantity of water in the vessel R, had been equal to that in L: that is, if it had been 20 ounces.

The heat imparted to the water in the vessel L was 2.2 degrees. Of this quantity of heat,

heat, it appears, that 1.926 was yielded by the cylinder; from which it follows, that the heat imparted by the air was .274. It is proper to observe, that there was an error of $\frac{1}{20}$ of a degree in this experiment; for when it was compared with others of a similar kind, the mean result gave .23 of a degree for the heat communicated by atmospheric air.

To discover from these experiments the relation, which the absolute heat of atmospheric air bears to that of water, we may consider what would have been the effect produced if this species of air had contained the 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 eight hundredth part as dense, and were raised 100 degrees above the heat of the water, it would communicate to it nearly the eighth part of a degree.

Now the density of atmospheric air is to that of water in a proportion somewhat less than that of 1 to 800; and, in the foregoing

experiments, the excess of the temperature of the air above that of the water was 115.5 degrees. 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 eighth part of a degree; but it communicated a little more than the fifth of a degree. Atmospherical air must, therefore, contain a greater quantity of absolute heat than water.

Dr. Irvine has pointed out a general rule, by which the comparative quantities of heat in bodies may be estimated, when the quantities of matter, and the changes produced in the temperatures are unequal. In that case the comparative quantities of heat are reciprocally as the changes in the temperatures 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 497.5, which is the reciprocal proportion

proportion of the changes produced in the temperatures.

Again, if the changes produced in the temperatures had been equal, the quantities of absolute heat would have been reciprocally as the quantities of matter. It follows, that neither being equal, the quantities of absolute heat 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 air.

It was before shown that the vessel and water were equal to 21.23 ounces of water; and the specific gravity of water being to that of atmospherical air in the proportion of 830 to 1: it follows, that the quantity of matter contained in 21.23 ounces of water, is to that contained in 20 ounce measures of air, as 881 to 1.

In Experiment XV. the temperature of the water in the vessel L, previously to the immersion of the cylinders, was 61.8; the heat of the cylinder, with the air contained in it, was 200; the difference was 138.2; and $\frac{1}{6}$ being allowed for the heat imparted to the external air, we have 115 nearly for the excess of the heat of the air in the cylinder, above
that

that of the water. The true heat of the cylinder, therefore, previously to immersion, was 176.8; the temperature of the water at the end of the experiment was 64. The air was consequently cooled by the water 112.8, and it was before shown that the water was heated by the air .23 of a degree. Hence the comparative heat of atmospherical air is to that of water in the compound ratio of 881 to 1, and of .23 to 112.8.

From which it appears, that the quantity of heat contained in the former of these elements, is to that contained in the latter, as 202.63 to 112.8, or as 1.79 to 1.

The comparative heat of pure air may be calculated as follows: It was before proved, that the two aërial fluids, pure and phlogistified air, mixed in certain proportions, compose the air of the atmosphere; and that of the atmospherical air, it is the purer part alone which is diminished by phlogistic processes. Hence the quantity of pure air, contained in common air, may be discovered by observing the diminution which these processes produce.

When nitrous and common air are mixed over water, in such proportions as to effect a compleat

compleat saturation, and the mixture is suffered to remain for a sufficient length of time, the whole of the nitrous air, and $\frac{1}{5}$ of the common air, disappear. When common air is exposed to a mixture of iron filings and brimstone, the mean diminution, as determined by the experiments of Dr. Priestley and Mr. Scheele, is 27 parts of an 100. In the former instance it is probable, that a small portion of the pure air is protected by the phlogisticated from the action of the nitrous air, as has been remarked by Mr. Kirwan; and in the latter case it is likely that the whole of the pure air is separated by the iron filings and brimstone, and that a part of the phlogisticated air also disappears, as it is well known that a small quantity of this fluid is capable of being absorbed by water. If, therefore, we take the mean result of the experiments with nitrous air, and with iron filings and brimstone, we shall have nearly 25 parts of an 100 for the true diminution. From which it appears, that nearly $\frac{1}{4}$ of the air of the atmosphere consists of dephlogisticated air.

In Experiment II. p. 190, the purity of the dephlogisticated air was such, that one measure

sure of it, mixed with two of nitrous air, occupied the space of one measure. From Dr. Priestley's experiments it appears, that two parts of nitrous air saturate very nearly one of dephlogisticated air; because when they were mixed in this proportion, the dephlogisticated air being very pure, not more than $\frac{3}{10}$ of a measure remained. See Priestley's Observations and Experiments on Air, vol. 4. sect. 25. When, therefore, one measure of dephlogisticated air, mixed with two of nitrous, occupy the space of one measure, it follows, that the dephlogisticated air must have contained two thirds of its bulk of pure air. For the quantity of air that disappears is two measures. Of these, one part must have been pure air, and two parts nitrous. The quantity of pure air, therefore, contained in the dephlogisticated air, must have been one third of two measures, or two thirds of one measure; and as the whole of the dephlogisticated air, originally subjected to the examination, was one measure, it is evident that it must have contained two thirds of its bulk of pure air. The part which remained undiminished by nitrous air, and unabsorbed by water, was phlogisticated air. It is proper
to

to observe, that the air had been placed, previously to its examination, a considerable time over water, to free it from the admixture of fixed air.

Thus it appears, that the dephlogisticated air, which was introduced into the cylinder L, in Experiment II. p. 190, contained two thirds of its bulk of pure air, and one third of phlogisticated air; but the common air, in the opposite cylinder R, contained three fourths of its bulk of phlogisticated air, and one fourth of pure air. In that Experiment we may conceive the phlogisticated air in L, amounting in volume to one third of the whole, to be balanced by an equal quantity of the same species of air in R. We may also conceive a portion of the pure air in L, to be balanced by a quantity of that fluid in R, amounting to one fourth of the whole; and the cylinders, being equal in weight and capacity, precisely balanced each other.

The entire quantity of pure air in L was $\frac{2}{3}$ of the whole, the quantity in R was $\frac{1}{4}$. The excess of the pure air in L, above that in R, was consequently .41, and the excess of the phlogisticated air in R, above that in L, was also .41. The experiment in question,

tion, therefore, gives us the difference between the heat imparted by .41 parts of pure air, and that imparted by an equal bulk of phlogisticated air. This difference was $\frac{2}{10}$ of a degree. Hence we may discover the quantities of heat, which would have been communicated to the water, if the cylinder R had been filled with phlogisticated air, and L with air perfectly pure. For let the heats, which would have been yielded by pure and phlogisticated air, be called x and y ,

$$\text{Then } \frac{41}{100} x - \frac{41}{100} y = .2$$

$$41 x - \frac{4100}{100} y = 20$$

$$4100 x - 4100 y = 2000$$

$$\text{Therefore } x - y = .487.$$

But it was shown that the entire quantity of heat imparted to the water by the common air, in the cylinder R, was .23; and this species of air contains $\frac{1}{4}$ of its bulk of pure air.

$$\text{Therefore } \frac{1}{4} x + \frac{3}{4} y = .23$$

$$x + \frac{12}{4} y = .92$$

$$4 x + 12 y = 3.68$$

$$\text{and } x - y = .487$$

$$12 x - 12 y = 5.844$$

$$\text{but } 4 x + 12 y = 3.68$$

R

there-

$$\text{therefore } 16 x = 9.524$$

$$\text{and } x = .595$$

$$\text{but } x - y = .487$$

$$\text{therefore } y = .108$$

Whence it appears that the entire quantity of heat, which would have been yielded by pure dephlogistified air, in the experiment in question, is .595; and that the quantity which would have been imparted by air compleatly phlogistified, is .108.

It was before observed, that as the temperature of the air in the room, in Experiments II. and III. was gradually encreased in proportion to the heat received by the water in the tinned vessels, the error arising from the irregularity of cooling was in a great measure avoided. But as the quantities of heat received by the water were unequal, it was impossible entirely to obviate this source of fallacy: for the air having nearly the same degree of heat with the water in the vessel which had the higher temperature, it is manifest, that the water in that which had the lower, would necessarily, in the course of the experiment, receive a small portion of heat from the surrounding air. If we suppose that, by this cause, the difference of
heat

heat in the vessels was diminished the $\frac{1}{300}$ part of a degree, we shall have very nearly .6 for the heat which would have been imparted by pure dephlogisticated air.

In Experiment II. the difference between the heats imparted by the dephlogisticated and common air, was a little more than .2 of a degree. In Experiment III. it was precisely .2. In that Experiment the heat of the water, in the tinned vessels, previously to the immersion of the cylinders, was 59.5. The cylinders were raised to 200, which was 140.5 degrees higher than the temperature of the water. The water was raised by the cylinder R, containing atmospherical air, a little more than one degree.

Comparing the result of this Experiment with that of Experiment XVI. it appears that the cylinder must have lost, during its passage through the air, nearly $\frac{1}{5}$ of the heat which it had received from the boiling water; but the heat which it received was 140. The loss was, therefore, 28.1: subtracting this from 140, and adding the remainder to the heat of the water in the tinned vessel, we have 171.9 for the true temperature of the cylinders at the moment of their immersion.

The general temperature of the water in the vessel L, at the end of the experiment, was 61.7° . The cylinder was therefore cooled by the water 110.2 ; and it has been proved, that if the cylinder had contained pure dephlogisticated air, the heat imparted by this fluid to the water would have been $.6$ of a degree.

The specific gravity of pure air is to that of common air, by Dr. Priestley's Experiments, as 187 to 185 . The specific gravity of the latter is to that of water as 1 to 830 ; consequently the specific gravity of pure air is to that of water as 1 to 821 . The quantity of air was 20 ounce measures. The water and vessel were equal to 21.23 ounces of water. Hence the quantity of air was to that of water as 1 to 872.49 . The comparative heat of water is, therefore, to that of pure air, in the compound ratio, of 110.2 to $.6$, and of 1 to 872.4 , or as 1 to 4.749 .

The comparative heat of phlogisticated air is calculated as follows: It has been already shown that the heat, which would have been imparted to the water, in the II^d and III^d Experiments, by air compleatly phlogisticated, is $.108$.

From

From the Experiments of Abbe Fontana it appears, that the specific gravity of phlogisticated air is to that of common air as 377 to 385. The specific gravity of the latter is to that of water as 1 to 830; therefore the specific gravity of phlogisticated air is to that of water as 1 to 847. The quantity of phlogisticated air was 20 ounce measures. The water and vessel were equal to 21.23 ounces of water. Hence the quantity of air was to that of water as 1 to 899.

The air in the cylinder R was cooled by the water, in Experiment III. 110.4; and it was proved that the water would have been heated by the air, if it had consisted wholly of phlogisticated air, .108; consequently the comparative heat of water is to that of phlogisticated air, in the compound ratio of 110.4 to .108, and of 1 to 899, or as 110.4 to 87, nearly; that is as 1.26 to 1.

The following is the calculation of the comparative heat of fixed air: It was shown, that in the experiments with the apparatus described above, which may be called an aerometer, the difference of the central heats, imparted by pure and common air to oil, was to the difference of the heats imparted by these

fluids to water, as 3 to 2. Hence we may conclude, that similar effects would be produced if the experiments were made with common and fixed air, in oil and in water; that is, the difference in oil would be one third greater than in water; but the mean result of my experiments, with fixed and common air in oil, gave a difference of .05 in favour of common air. The difference in water would therefore be .034. The specific gravity of fixed air was found, by the Hon. Mr. Cavendish, to be to that of common air as 1.57 to 1. Hence its specific gravity is to that of water as 1 to 553. The quantity of fixed air was 20 ounce measures; the water and vessel were equal to 21.23 ounces of water; the quantity of water was, therefore, to that of fixed air, as 587 to 1. From the foregoing experiments with the acrometer, it appears that the heat imparted, by common air, to an equal bulk of water, is .23; and we have seen that the heat communicated by that fluid is .034 parts of a degree greater than by fixed air. Hence the heat communicated to water by fixed air is .23—.034, or .196; and the heat separated from the air by the water being 110, it follows that the com-

comparative heat of fixed air is to that of water, in the compound ratio, of .196 to 110, and of 587 to 1, or as 115 to 110.

The comparative heat of inflammable air, as deduced from the foregoing experiments, may be calculated in the following manner.

The specific gravity of pure inflammable air was found, by Mr. Cavendish, to be to that of common air, as 1 to 12.

The inflammable air, which was employed in the experiments to determine the comparative heat of this fluid, was procured from iron filings dissolved in the vitriolic acid. It was obtained by a rapid effervescence, and was freed from the vitriolic acid air, with which it is commonly contaminated, by making it to pass through water. It was therefore nearly pure; but as the brass cylinder, into which it was introduced, was exhausted by an air pump, and as the air, previously to its introduction into the cylinder, was forced into a bladder by the syringe formerly described, it was unavoidably, during this process, mixed with a portion of common air. Allowance being made for the error arising from this cause, the specific gravity of the inflammable air used in the above-mentioned

experiments may be supposed to be to that of common air very nearly in the proportion of 1 to 10. But the specific gravity of common air is to that of water as 1 to 830. Hence the specific gravity of water was to that of the inflammable air in question, as 8300 to 1.

The quantity of inflammable air was 20 ounce measures Troy; the water and vessel were equal to 21.23 ounces of water. If the volumes of the air and water had been equal, the quantities of matter would have been in the ratio of 1 to 8300. But the volume of the water (the vessel being conceived to be equivalent to 1.23 ounces of this fluid) was to that of the air as 21.23 to 20. Hence the quantity of matter in the water was to that in the air as 8810 to 1.

The heat, imparted to water by inflammable air, is a little greater than that imparted by common air in similar circumstances, as I have found by repeated trials. But as the difference is very small, we shall probably approach near to the truth, if we suppose that the heats imparted by these fluids are equal. The heat therefore which inflammable air communicates to water, in experiments

ments similar to those recited above, will be .23.

Hence the comparative heat of the inflammable air, which was used in the above-mentioned trials, is to that of water in the compound ratio of 8810 to 1, and of .23 to 113.2, or as 17.9 to 1.

Admitting that the lightest inflammable air communicates as much heat to water, as an equal bulk of common air, in similar circumstances, its comparative heat may be calculated as follows. The specific gravity of this fluid is to that of water as 1 to 9960. The quantity of the water employed in the experiment, the vessel being reduced to its value in water, was, therefore, to that of the air, as 10567 to 1. The comparative heat of the lightest inflammable air is consequently to that of water in the compound ratio of 10567 to 1, and of .23 to 113.2, or as 21.4 to 1.

In the deductions from the foregoing experiments, it is taken for granted that the capacities of the airs are permanent, in the intermediate temperatures between the freezing and boiling points of water. But it was before observed, that these fluids probably
suffer

suffer a small alteration of capacity, in consequence of their rarefaction or condensation by exposure to heat or cold. It is, however, manifest that no change of capacity could have arisen from this cause in the experiments in question; because the airs were prevented from expanding, by the brass cylinders, in which they were included. It seems indeed evident, from the following facts and observations, that supposing the airs in those experiments to have been permitted freely to expand, the alteration in their capacities would have been inconsiderable.

It is well known that cold is produced when air is rarified in the receiver of an air pump, and heat when it is condensed. I have made many experiments to determine the quantities of heat and cold which are caused by the condensation and rarefaction of air; but I have not as yet been enabled to ascertain this point with a sufficient degree of precision. I have found, in general, that when one atmosphere was slowly introduced into the exhausted receiver of an air pump, a very sensible thermometer suspended in the receiver rose nearly 6.5 degrees. If the air was suddenly introduced, the increase of tem-

temperature was not more than 5.5; and it appeared that there was a certain limit in the velocity of introduction, which produced the greatest possible augmentation of temperature.

I also found, that the cold produced by exhausting the receiver, was nearly 5 degrees, and that during this operation the thermometer ceased to fall before the air was completely exhausted, being observed sensibly to ascend when the gage was at 27.

In these experiments I endeavoured to obviate the effects of moisture, by placing the receiver upon a leather which had its pores filled with tallow. Notwithstanding this precaution, however, a sensible precipitation of vapour was perceived in the exhausted receiver. But the error arising from that cause was avoided by repeating the experiments with an inverted glass decanter, which was fixed to the plate of the air pump, a thermometer being inserted by means of a screw into a cavity in the neck of the decanter.

When the experiments were made with this apparatus, the results were nearly the same with those of the preceding trials; the cold produced by the exhaustion of the air
being

being 5 degrees. It moreover appeared, that the cold generated by the rarefaction of phlogisticated air, was a little less, and by the rarefaction of dephlogisticated air, a little greater than by that of common air; but the difference was inconsiderable.

From the experiments of General Roy we learn, that the bulk of common air, at the temperature of 212, is to its bulk at the Zero of Fahrenheit's thermometer, as 14 to 10, nearly; that is, when it is raised from Zero to 212, its expansion is nearly one half of its original bulk.

The rarefaction of dephlogisticated air by heat is greater than that of common air. Let the bulk of the former of these fluids, when its temperature is raised 110 degrees, be conceived to be increased one half; and by that increase of bulk, let a quantity of heat be absorbed in consequence of an augmentation of capacity, which would raise the air three degrees.

In the experiments with the aerometer recited above, it appeared that the separation of 110 degrees of heat from the dephlogisticated air, raised the water .4 of a degree; and hence, by the separation of $110 + 3$, or of

113 degrees, the water would have been raised .4 plus $\frac{1}{100}$. That is, the separation of three degrees would have raised the water $\frac{1}{100}$ of a degree, which is a quantity so inconsiderable that it could not have been distinguished by the thermometer.

Having determined the comparative heats of atmospherical, pure, phlogisticated, fixed, and inflammable air, I next endeavoured to ascertain the comparative heat of aqueous vapour.

The comparative heat of aqueous vapour could not be determined by experiments similar to those recited above, because the vapour of boiling water condenses when it is reduced to a temperature below 212. I therefore had recourse to the following method in order to discover the capacity of that fluid.

It is now generally believed that aqueous vapour is composed of pure and inflammable air, which in the process of combustion intimately unite with each other, and at the same time give off a large quantity of elementary fire.

If, therefore, the whole quantity of absolute heat, contained in pure and inflammable air, be given, and if the heat which these fluids

fluids evolve, when, by the process of combustion, they are changed into aqueous vapour, be likewise determined, the quantity of absolute heat in the latter will be known. Hence, in order to discover the quantity of heat in aqueous vapour, it was necessary to determine the heat which is extricated from pure and inflammable air when they are burned together in close vessels. With a view to determine the heat produced by the combustion of these fluids, it was proposed to me by Dr. Priestley that they should be confined in a tinned or brass vessel surrounded with water, and should be fired by the electric spark, a thermometer being introduced into the water to indicate the heat that it received: in consequence of which I contrived the following apparatus for the purpose of making this experiment. (See plate III. fig. 3.) G K represents a brass vessel, at the lower extremity of which is fixed a stop cock, and at the upper extremity the apparatus commonly used for firing inflammable and dephlogisticated air by the electric spark. This apparatus consists of a crooked brass wire O P, inserted in a small glass tube *r*, to which it is cemented by plaster of Paris. The glass tube is,
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in like manner, cemented to a brass pipe through which it passes, the latter being fixed by a screw to the brass vessel at the point L. The immediate communication between the wire and vessel being thus interrupted by means of the glass tube, the electric spark, which enters at the upper extremity of the wire, sets fire to the air in its passage from the lower extremity Q to the side of the vessel.

The airs having been mixed in the proper proportions in a bladder, are introduced into the brass vessel previously exhausted by an air pump. The brass vessel is then introduced into the tinned cylindrical vessel A D, (see fig. 4) to the bottom of which it is screwed by means of a male screw formed on the extremity of the pipe of the stop cock, which, as has been already mentioned, is fixed to the lower end of the brass vessel. As this cock necessarily projects to a little distance from the pipe, through which the air passes, it is manifest that a small portion of vacant space will be interposed between the pipe and the lower extremity of the tinned vessel which is formed to receive it. For the purpose of filling this cavity, a piece of cork, equal to it

it in size, is divided longitudinally through the centre, and its sides are applied to the stop cock, being excavated in such a manner as to be closely adapted to every part of it. This is represented by N O, the lower extremity of the figure G K, see fig. 3.

Finally, a quantity of water, sufficient to cover the brass vessel, is introduced into A D, (see fig. 4) and the temperature is indicated by a thermometer, which has each degree of Fahrenheit's scale divided into ten equal parts. The tinned vessel is covered with thick flannel for the purpose of counteracting the refrigerating power of the surrounding air.

By means of this apparatus I made the following experiment :

Air in the room — 48.

A mixture of dephlogisticated and inflammable air, the volume of the latter being to that of the former as 2 to 1, was introduced into the brass vessel, which was screwed to the bottom of the tinned vessel, and surrounded with 16 ounces Troy, 2 drams, and 2 scruples of water. The water being suffered to remain in the vessel till the heat became very nearly stationary, its temperature, as indicated by a thermometer which had each degree

degree of Farhenheit's scale, divided into ten equal parts, was found to be 46.1 plus.

The airs were now fired by the electric spark, and the temperature

In 1 minute was	—	47.3
2	— —	47.9
3	— —	48.3
4	— —	48.4
5	— —	48.5
6	— —	48.6
7	— —	48.6
8	— —	48.6 plus.
9	— —	48.6 plus.

The brass vessel being now removed, and the water being agitated, for a little time, that the heat might be equally diffused over it, the general temperature was 48.6 plus. When this experiment was made the barometer stood at 30.6.

The purity of the dephlogisticated air, as measured by Dr. Priestley's Test, was .2. The inflammable air was obtained from iron filings dissolved in the vitriolic acid.

It is proper to observe, that as the brass vessel could not be compleatly exhausted by the air pump, a small portion of common air remained in it when the mixture of

dephlogisticated and inflammable air was introduced; a little common air was also unavoidably mixed with these fluids, when they were forced by means of a syringe into the bladder, previously to their introduction into the brass vessel. Though the inflammable air was obtained by a rapid effervescence, and was passed several times through water, for the purpose of purifying it, yet it could not be entirely freed from the vitriolic acid air, and other impurities with which it is commonly contaminated.

For these reasons, after the experiment was finished, a quantity of air remained in the brass vessel, which did not suffer any change by the deflagration. This quantity was estimated in the following manner: the brass vessel, at the conclusion of the experiment, being unscrewed from the tinned vessel, was instantly plunged into water, and the cock, which was fixed to its lower extremity, being opened, the water was suffered to rush into it. Having remained in this situation until no more water would enter, the cock was shut, and the vessel was weighed; after which the same vessel being compleatly filled with water,

ter, was again weighed: whence it appeared that 5 ounce measures Troy, and 3 dram measures of air, remained in the vessel at the conclusion of the experiment.

The original heat of the water in this experiment was 46.1 plus. At the end of nine minutes, when the brass vessel was removed, and the water was reduced by agitation to a uniform temperature, the thermometer stood at 48.6 plus. The heat imparted by the deflagration of the airs was therefore 2.5 degrees.

This experiment was frequently repeated, and the mean result gave 2.4 for the heat communicated to the water. Hence the number of degrees of sensible heat, which would have been communicated by the deflagration of the airs employed in these experiments, to an equal quantity of pure and inflammable air, mixed in the same proportions, may be determined as follows.

The weight of the brass vessel in which the airs were fired, was 23 ounces Troy, 1 dram, and 2 scruples, or 11140 grains. The capacity of brass is to that of water as 1 to 8.9. That is, 8.9 ounces of brass, or 4272 grains,

are equal in capacity to 1 ounce of water; and, consequently, 23 ounces, 1 dram, and 2 scruples, or 11140 grains, are equal in capacity to 2.607 ounces of water.

The capacity of the tinned vessel in which the water and brass apparatus were contained, was to that of 34 ounces Troy of water, as 1 to 21.4. In other words, the quantity of absolute heat contained in the vessel was equal to that contained in the $\frac{1}{21.4}$ part of 34 ounces of water, or equal to that contained in 1.58 ounces of water. The quantity of water employed in the experiment was 16 ounces Troy, 2 drams, and 2 scruples, or 16.33 ounces very nearly. The brass apparatus has been shown to be equal to 2.607 ounces of water, and the tinned vessel to 1.58 ounces. Hence the water, cylinder, and vessel were equal in capacity to 20.517 ounces of water.

It was the purer part of the inflammable air alone that was capable of uniting with the dephlogisticated air in these experiments.

The specific gravity of pure inflammable air is to that of common air as 1 to 12, and to that of dephlogisticated air nearly as 1 to

12.1.

12.1. If, therefore, the volumes of the two airs, used in the experiments in question, had been equal, their weights would have been as 1 to 12.1. But as the volume of the inflammable air was twice as great as that of the dephlogistified air, the former was to the latter in weight as 1 to $\frac{12.1}{2}$, or as 1 to 6.05. Hence the specific gravity of the compound, consisting of these two species's of air mixed in the above proportions, was to that of dephlogistified air, as $\frac{1}{2.574}$ to 1, and to that of inflammable air, as $\frac{1}{2.574}$ to $\frac{1}{12.1}$, or as 4.7 to 1; and the specific gravity of inflammable air being to that of water, as 1 to 9960, it follows that the specific gravity of water was to that of the compound as 2119 to 1.

The comparative heat of pure dephlogistified air is to that of water, as 4.75 to 1, nearly; and the comparative heat of pure inflammable air is to that of water, as 21.4 to 1. Hence the comparative heat of the compound was to that of water, as 7.11 to 1.

It was shown above that the quantity of air which remained in the brass vessel, at the conclusion of the experiment, was 5 ounce measures Troy, and 3 dram measures. It is

manifest that this air, which consisted chiefly of phlogisticated, mixed with a small portion of common air, did not suffer any change in its capacity during the deflagration.

This appears from the experiments of Mr. Cavendish, who has proved that pure dephlogisticated and inflammable air are almost wholly converted into water, when they are fired by the electric spark in close vessels; and that the quantity of air which remains in the vessel, at the conclusion of the experiment, is in proportion to the heterogeneous aërial fluids with which those airs are mixed previously to their combustion.

The entire quantity of air which filled the brass vessel was 16 ounce measures Troy, and 3 dram measures, very nearly; and the quantity which remained unaltered being 5 ounce measures, and 3 dram measures, it follows that the volume of the airs, which contracted a chemical union in those experiments, and which were converted into water, was 11 ounce measures. The quantity of water used in the experiments was 16.33 ounces; and it was before shown that the brass and tinned vessels were equal in capacity to 4.187 ounces of water; consequently the
relative

relative magnitudes of the air and water were as 11 to $16.33 + 4.187$, or as 11 to 20.517.

It has been already proved that the specific gravity of the compound of the airs employed in the experiment was to that of water as 1 to 2119. Hence the quantity of matter in 20.517 ounces of water was to that in 11 ounce measures of the compound, as 3952.3 to 1. We have seen that the water was raised, by the deflagration of the airs, 2.4 degrees. If the capacities of the airs and water had been equal, it is manifest that the same heat which raised 3952 parts of water, 2.4, would have raised one part of air 9484.8 degrees. But the capacity of the airs was to that of water as 7.11 to 1. The airs would therefore have been raised by the same heat only the $\frac{1}{7}$ part of 9484.8, or 1333 degrees. Hence the whole quantity of heat which the airs contain at a given temperature, as expressed in degrees of the thermometer, may be determined as follows.

It was shown, that the airs previously to their combustion contained a quantity of heat sufficient to raise them 1333 degrees. If we suppose, that when they were fired by the electric spark they gave off the whole of

the heat which they contained ; it will follow that the point of total privation, or the degree of cold to which they must be reduced in order to deprive them wholly of their heat, is 1333 degrees below the common temperature of the atmosphere. But a part of the heat that existed in these fluids was retained by the water, which was the product of their union ; the heat that was extricated from the airs being the difference between the entire quantity of heat which they contained, and that contained in an equal weight of water at the same temperature. We have before seen that the whole quantity of absolute heat, in the compound of pure and inflammable air, was to that contained in an equal weight of water at the same temperature as 7.11 to 1 ; and hence the difference between the absolute heat of the airs and of the water was $7.11 - 1$, or 6.11.

It has been proved that this difference was such as would have heated the compound of pure and inflammable air 1333 degrees. In order, therefore, to discover how much the temperature of the airs would have been raised by the heat which was retained by the water ;

ter; say if 6.11 give 1333 degrees, what will 1 give? Whence it appears, that the heat retained by the water would have heated the airs 218 degrees; adding this to 1333, we have 1551 degrees for the whole quantity of heat contained in the airs, as expressed in degrees of the thermometer, and estimated from the point of total privation *.

The reader will observe, that the foregoing calculation is deduced from the comparative heats of air, and of water, and not from those of air and aqueous vapour. For though, in the experiments recited above, the pure and inflammable air were at first converted into aqueous vapour; yet as the vessel was surrounded with water at a low temperature, and as a considerable quantity of air remained in it after the deflagration,

* It is a tribute of justice, which is due to Dr. Irvine, to acknowledge, that the mode of reasoning, which has been employed in the above enquiry, to determine the whole quantity of heat contained in air, is similar to that which he formerly employed to calculate the lowest degree of heat in bodies from the relative capacities of ice and water, and the quantity of heat given off by the latter when it is congealed. The ingenious Theorem, by which this curious question was solved, the reader will see explained in Mr. Nicholson's Treatise on Natural Philosophy.

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the vapour, in the course of the experiment, must necessarily have been condensed.

It is evident that, in this experiment, an imperfect vacuum was formed by the conversion of the pure and inflammable air into water; and some Philosophers have supposed that a vacuum may contain heat.

I endeavoured to determine the question respecting the capacity of a vacuum for heat by the following experiment: The cylindrical vessels of the aerometer, containing atmospheric air, were exposed for a considerable time, in the bath, to the action of boiling water, in order that the stop cocks, by having the heat uniformly diffused over them, might become air tight. The air was then exhausted from one of the cylinders by means of a crooked pipe, which was screwed upon it, and which communicated with an air pump; after which the cylinders having been exposed to heat in the bath for several minutes, were suddenly immersed in the tinned vessels, containing equal quantities of cold water. From this experiment, which was several times repeated, it clearly appeared that a vacuum has a less capacity for heat than atmospheric air; and that, if it does con-
tain

tain heat, the quantity is so minute that it could not be appreciated by my thermometers. Admitting, however, that a small quantity of heat may exist in the vacuum, which is formed by the deflagration of pure and inflammable air, in close vessels, it will follow, that the natural Zero is a little lower than that which has been derived from the foregoing calculation. And in that case the natural Zero, as determined by experiments with substances which do not undergo a considerable alteration of bulk, when they suffer a change of form, would be farther removed from the common temperature of the atmosphere, than the Zero which is deduced from experiments with pure and inflammable air. It is manifest, however, that the utmost care and accuracy would be required to determine this point with sufficient precision. In the mean time I think we may conclude with certainty, that pure and inflammable air, at the temperature of 50, contain nearly 1550 degrees of heat.

It was before shown that all bodies, which have a common temperature, must have the same number of degrees of sensible heat, as measured by an equidifferential thermometer,
and

and estimated from the point of total privation. If, therefore, the entire quantity of sensible heat contained in air, at the common temperature of the atmosphere, be 1550 degrees, it will follow that the number of degrees of heat, in all bodies at the same temperature, must likewise be 1550; and consequently this will be the point to which, if bodies were to be refrigerated, they would become absolutely cold.

Hence it is manifest, that if, by the foregoing experiments upon air, the true Zero, or point of absolute cold, has been justly determined, it should correspond with the Zero which is derived from similar experiments upon all other bodies that produce sensible heat or cold, in consequence of a change of form. Messrs. Lavoisier and De la Place have endeavoured to show that the Zero, or point of total privation, as determined by the changes of capacity, and the quantities of heat evolved from bodies, in various instances, is not uniform. But they admit that the apparent deviations from this law were not greater than such as might have arisen from unavoidable errors in their experiments. I have made many experiments with a view to determine
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this question; and I have uniformly found, that when bodies produced heat, in consequence of a change of form, their capacities were diminished; and that, on the contrary, when they produced cold, their capacities were increased. But I have not yet been enabled to bring my experiments to such a degree of accuracy as to determine, with certainty, whether the relation which the increase and diminution of capacity bears to the quantities of heat and cold produced be such as to give a uniform result for the point of total privation. The experiments were, however, by no means inconsistent with that law, and did not deviate from it more than might naturally be expected from the inaccuracy to which such experiments are liable.

Having ascertained the whole quantity of heat in pure and inflammable air, we may hence determine the capacity of aqueous vapour. It has been proved that pure air, at the common temperature of the atmosphere, contains 1550 degrees of heat; and it was shown that all other bodies must contain an equal number of degrees when at the same temperature. Water, therefore, at the common temperature of the atmosphere, will
like-

likewise have 1550 degrees of heat; and at the temperature of 212 it will have nearly 1650 degrees of heat. But the heat absorbed by aqueous vapour, as appears from the experiments of Mr. Watt, is 914 degrees. From which it follows, that the capacity of water is to that of aqueous vapour as 1650 to $1650 + 914$, or as 1 to 1.55. It was before proved that the capacity of water is to that of pure air as 1 to 4.749. The capacity of aqueous vapour is therefore to that of pure air as 1.57 to 4.749, or as 1 to 3, nearly.

Thus it appears, that dephlogisticated air contains a greater quantity of absolute heat than fixed or phlogisticated air, or aqueous vapour.

Hence the quantity of heat, in a mixture of pure and phlogisticated air, will be increased by augmenting the proportion of the former, and diminished by augmenting that of the latter. Agreeably to this we find, that the comparative heat of air perfectly pure, is to that of atmospherical, which consists of $\frac{1}{4}$ pure, and $\frac{3}{4}$ phlogisticated air, as 2.2 to 1.

The reader will observe, that the quantities of absolute heat in equal bulks of respirable

spirable air, having different degrees of purity, are not accurately proportional to the quantities of pure air which they contain; for the quantity of pure air, in a given portion of the purest dephlogisticated air, is four times as great as that contained in an equal bulk of atmospherical; and the comparative heat of the former is to that of the latter only as 2.2 to 1. The reason is obvious: phlogisticated air, which is one of the constituent parts of atmospherical, contains a considerable quantity of absolute heat.

If, however, from the absolute heat of any species of respirable air, we subtract the heat contained in that portion of it, which is not altered by the action of the lungs in respiration, the quantity of heat contained in the remainder will be nearly proportional to its power in supporting animal life. Thus, if from the heat of a given quantity of atmospherical air, we deduct the heat of the phlogisticated air which forms one of its constituent parts, and which is not altered by respiration; the quantity of heat in the remainder will be to that contained in a quantity of pure air equal in bulk to the atmospherical, as 1 to 4 nearly. And Dr. Priestley, whose

whose discoveries on this subject have been deservedly much admired, has proved, that the power of the latter, in supporting animal life, is nearly five times as great as that of the former.

It is proper to remark, that though the powers of different species's of respirable air, in contributing to the support of animal life, are nearly proportional to the quantities of pure air which they contain, yet they are not accurately in that proportion; for the quantities of pure air, contained in the purest dephlogisticated and in atmospherical air, are as 4 to 1; but the power of the first, in supporting animal life, is to that of the last as 5 to 1. The reason of this difference seems to be, that a small portion of the pure air, contained in the atmospherical, is protected from the action of the lungs by the phlogisticated air with which it is mixed, as Mr. Kirwan has suggested.

There is, therefore, upon the whole, sufficient evidence for concluding that the purer part of atmospherical air has its capacity for heat diminished by the change which it undergoes in the lungs of animals; and that, if from the absolute heat of any species of
respirable

respirable air not perfectly pure, we subtract the heat of those ingredients which are not altered by respiration, the quantity of heat contained in the remainder will be nearly proportional 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 propelled through the arteries in the aortic system, and the latter is that which in the same system is returned by the veins, I shall call the first arterial, and the last venous blood.

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

EXPERIMENT I.

Air in the room — 68

Half a pound of water, Avoirdupoise weight, at 53, was mixed with half a pound and 400 grains of arterial blood, at 102.

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The

The mixture at the end of

1	minute was	78,
2	—	77 $\frac{3}{4}$, nearly,
3	—	77 $\frac{1}{2}$ when it coagulated,
4	—	77 $\frac{1}{2}$.

EXPERIMENT II.

Half a pound of water, Avoirdupoise weight, at 53 $\frac{1}{2}$, was mixed with nine ounces and a half and 14 grains of venous blood, at 99 $\frac{1}{2}$.

The mixture at the end of

1	minute was	76,
3 $\frac{1}{2}$	—	76 when it coagulated,
8	—	76,
9	—	75 $\frac{1}{2}$.

In making these experiments it was necessary to use as much expedition as possible, that the heat of the mixture might be determined previously 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

the weight of the blood was determined at the conclusion of the experiment.

It may be proper to observe, that when the venous and arterial blood were first discharged from the animal, the former was much more fluid than the latter; 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 nearly half a degree; and as the mixture, previously 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 1 to 16. 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 an 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 Avoirdupoise, or 3899 grains; and, therefore, the heat of the arterial blood is to that of water, in the compound ratio of $8\frac{1}{2}$ ounces to 8 ounces and 400 grains, and of $25\frac{1}{2}$ to $23\frac{1}{2}$, 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

we have $76\frac{1}{2}$ for the temperature of the mixture. The blood was cooled from $99\frac{1}{3}$ 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 Avoirdupoise, or 4168 grains. The water and vessel were together 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 $V : A :: 97.08$ to 112, or nearly as 10 to $11\frac{1}{2}$

To determine whether the same change is produced in the blood of other animals, by the process of respiration, similar experiments were made with the venous and arterial blood of a dog. And the inaccuracy which might be supposed to have arisen in the former experiments, from the tendency of

the blood to coagulate, was avoided by mixing the water and blood in such quantities that the coagulation should not take place for many minutes. When nearly 2 ounces of venous blood was received from the jugular vein of a dog, and speedily mixed with 13 ounces of water, no tendency to coagulation was observed during the first three minutes. At the end of the 4th minute, the fluidity of the mixture seemed to be a little diminished; and at the end of the 6th minute it was completely coagulated.

When 1 ounce, 7 drams, and 2 scruples of arterial blood was taken from the carotid artery of a dog, and mixed with 13 ounces Troy of water, having the same temperature with that used in the foregoing experiment, a tendency to coagulation was perceived at the end of $3\frac{1}{2}$ minutes; and at the conclusion of the 5th minute, the coagulation was completed. By varying these experiments, I found that when one part of blood was mixed with nearly 12 parts of water, no coagulation took place for several hours; and thus the inaccuracy which might be supposed to arise from a tendency to coagulation, was obviated. From the results of a variety of trials,

trials, made with these precautions, it appeared that the comparative heat of the arterial blood of a dog was to that of the venous, as 114 to 100. In the experiments with the blood of a sheep, the comparative heats were as 115 to 100. These results so nearly coincide, that the difference may be supposed to have arisen from unavoidable errors in the experiments. It is not improbable, however, that in some animals, the excess of the comparative heat of the arterial above that of the venous blood, may be greater than in others.

From the foregoing experiments we may conclude, in general, that 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.

PROPOSITION III.

The comparative quantities of heat in bodies, supposed to contain phlogiston, are increased by the changes which they undergo in the processes of calcination and combustion.

By some Philosophers in the present age, the existence of the principle, which the Chemists have termed phlogiston, has been called in question.

It does not, however, appear that any evidence has hitherto been adduced which will warrant us in rejecting this principle. We know that a particular species of combustion is produced by the union of inflammable with the purer part of common air. We also know that inflammable air can be obtained from the greater part of combustible bodies, by exposing them to heat in close vessels. Hence this fluid or its basis exists in most of those bodies; and as they are not found to contain any other principle essential to their inflammation, it follows that inflammable air or its basis may justly be considered as the same thing with the phlogiston of the Chemists *. But it may be doubted whether the Chemists have not been deceived, in supposing the extrication of this principle

* See Mr. Kirwan's Paper on this subject. Philosophical Transactions, Royal Society, London, vol. 72, p. 195. See also the treatise which has lately been published, by the same Philosopher, in defence of the doctrine of phlogiston.

necessary

necessary in all cases to that process, as well as to the process of calcination.

If there is any other substance in nature, besides the inflammable principle, which is capable of combining with pure air; and if, in consequence of that combination, the air should suddenly suffer a great diminution in its capacity for heat, the phenomena of flame and of combustion would be produced.

Mr. Lavosier, who is the principal opponent of the doctrine of phlogiston, supposes that metals are simple substances, which in certain degrees of heat are capable of contracting an union with pure air, and that by this union they are converted into calces. That when the calces are again exposed to heat in contact with charcoal, the latter combines with the pure air, changing it into ærial acid; and thus the air being separated by the superior attraction of the charcoal, the metals are revived. Sulphur and phosphorus, are, in like manner, supposed to be simple substances, which during their combustion are changed into acids by combining with pure air.

This theory appears necessarily to lead to the following consequences. If the inflammable

mable air, which is produced by the solution of metals in acids, by that of sulphur in alkalis, and by the exposure of charcoal to heat, be not derived from the metals, the sulphur and the charcoal, but from the decomposition of water, as Mr. Lavoisier supposes, the quantity of inflammable air produced in these cases, should not be greater or less, than the quantity contained in the portion of water that is decomposed. It is now generally believed, that the quantity of inflammable air contained in water, is nearly $\frac{1}{8}$ of the whole, the remaining $\frac{7}{8}$ consisting of pure air. During the solution of metals in acids, Mr. Lavoisier conceives that the portion of the water, which consists of pure air, is united to the metallic salt, the portion which consists of inflammable air being at the same time suffered to escape. When the metallic salt is decomposed by a caustic alkali, the air that was formerly united to the salt adheres to the calx, and falls with it to the bottom of the vessel. The weight of the precipitate is consequently greater than the original weight of the metal.

Hence the quantity of water that is decomposed, admitting the truth of Mr. Lavoisier's

fier's theory, may be determined, for it will be $\frac{1}{5}$ greater than the excess of the weight of the calx above the original weight of the metal *. If, therefore, that excess be divided into five equal parts, and if the weight of the inflammable air be equal to one of those parts, we may conclude, with much appearance of probability, that it was wholly derived from the water. If it be greater, we may thence infer, with certainty, that a part of it must have been extricated from the metal during its solution; and if less, it will follow that the whole of the air united to the calx could not have been derived from the decomposition of water.

It appears, indeed, from Mr. Kirwan's late excellent work on phlogiston, that the hypothesis respecting the decomposition of water, is not supported by any decisive ex-

* Dr. Priestley's Experiments prove, that in some instances water is obtained by exposing the calces of metals to heat in contact with inflammable air. In those cases the quantity of water that was decomposed during the calcination of the metals, according to Mr. Lavoisier's Hypothesis, may be found, for it will be equal to the quantity that is reproduced,

periments;

periments; and, consequently, that the opinion of those who reject the antient doctrine of phlogiston rests on a very precarious foundation.

As the question, however, respecting the existence of the inflammable principle in metals, in charcoal, in sulphur, and in phosphorus, does not yet seem to have been determined with sufficient certainty, I shall not enter into it more particularly at present; but shall endeavour to show that, whatever be the nature of the change which bodies, supposed to contain phlogiston, undergo in the processes of calcination and combustion, the quantities of their absolute heat are increased. This will appear from the following experiments.

EXPERIMENT I.

Air in the room being	—	49.
16 ounces of water were introduced into a tinned vessel, and were raised to	—	127.
With this water 18 ounces of washed diaphoretic antimony were mixed at	—	49.1.
		The

The vessel containing the mixture was placed in the steam bath formerly described. It rested on a thin piece of cork which was applied to the bottom of the bath, and its mouth was closed with a cork to prevent the access of the external air. The bath was raised to the fixed temperature of 102.

The mixture being agitated in such a manner as to bring its several parts nearly to an equal heat, in two minutes its temperature (as measured by a cylindrical thermometer which reached from the surface to the bottom) was

	—	111.75,
6 minutes	—	111.75,
8	—	111.75, nearly.

By subsequent trials it was found that the agitation given to the mixture diminished its temperature nearly .33 of a degree. Adding therefore .33 to 111.75, we have 112.08 for the true temperature of the mixture.

In this experiment the cold antimony was introduced into the vessel containing the warm water. The heat imparted by the vessel was equal to that which would have been imparted by .85 of an ounce of water.

The

The water was cooled — 14.92.
 and the antimony heated — 62.58.

Since, therefore, the antimony cooled 16 ounces of water, and the vessel which was equal to .85 of an ounce of water, 14.92 degrees, it follows that it would have cooled 16.85 ounces of water 14.92 degrees. Hence the comparative heat of water is to that of diaphoretic antimony, in the compound ratio of 14.92 to 62.58, and of 16.85 to 18, or as 4.4 to 1, nearly.

As the calx used in this experiment was united to a considerable quantity of air, I next endeavoured to expel the air from it, as compleatly as possible, by moistening it with nitrous acid, and exposing it to a red heat, agreeably to the method recommended by Mr. Scheele. Its comparative heat, after this process, was found to be to that of water as 1 to 6; and the ratio of the comparative heat of regulus of antimony was, to that of the same fluid, as 1 to 15.5.

By similar experiments, I endeavoured to determine the comparative heats of most of the metals and their calces, the latter being examined both when combined with air, and
 when

when in a great measure freed from it by means of nitrous acid, and exposure to heat.

The results are comprehended in the following table :

Washed diaphoretic antimony		.22727
Washed diaphoretic antimony, having the air extricated by nitrous acid and fire, -		.16666
Regulus of antimony -		.06451
Yellow calx of lead having the air extricated * -		.06802
Lead - - -		.03520
White calx of tin -		.10869
White calx of tin, having the air extricated by nitrous acid and fire -		.099009
Tin - - -		.070422
Rust of iron - - -		.25000
Rust of iron, having the air ex- tricated by nitrous acid and fire, - - -		.16666
Iron - - -		.12696

* This calx was obtained from red lead by means of nitrous acid and fire. The comparative heats of the red lead and yellow calx were found to be nearly the same.

Calx of copper precipitated from
blue vitriol by an alkali, and
having the air extricated by
nitrous acid and fire, - .22727

Copper - - .11111

Brafs - - .11235

Calx of zinc precipitated from
white vitriol by an alkali, and
having the air extricated by
nitrous acid and fire, - .13698

Regulus of zinc - .09433

Water - - 1.00000

By similar experiments it may be proved
that sulphur contains less absolute heat than
the vitriolic acid, and oil less than water or
air *.

From the following trials, which were
several times repeated with the utmost care,
it appears that alcohol also contains less
absolute heat than water.

* This appears from Mr. Kirwan's Experiments,
the results of which have been communicated to the
public, in Mr. Magellan's Treatise on Elementary Fire.
According to these experiments the comparative heat of
water being — — 1.000

That of sulphur is — 0.183

Vitriolic acid, of a brown colour, — 0.429

Linseed oil — — 0.528

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EXPERIMENT I.

Air in the room	—	61.5.
Nine ounces of powdered chalk at	61,	
mixed with $10\frac{1}{4}$ ounces of spirit		
of wine at	—	139.

The mixture being agitated 20 times with the thin blade of a knife, and the temperature being examined by a cylindrical thermometer which reached from the surface to the bottom,

In 1 minute it was	120.75
2 —	120.5 nearly
3 —	119.75
4 —	119.

I have found that the parts of the mixture are most speedily brought to a common temperature when they are agitated by a thin flat substance. The blade of a knife is well calculated for this purpose.

From subsequent trials it appeared, that the heat lost by the agitation in the course of the first minute was one degree. Adding this to 120.75, we have 121.75 for the true temperature of the mixture.

The spirit was therefore cooled	17.25,
and the chalk heated	- 60.75.

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In the experiment the cold chalk was poured into the vessel containing the warm spirit of wine; and the capacity of the vessel being equal to that of .7 of an ounce of spirit, the spirit and vessel were together equal to 10.95 ounces. The quantity of chalk was nine ounces. Hence the comparative heat of chalk is to that of spirit of wine, in the compound ratio of 17.25 to 60.75, and of 10.95 to 9, or as 1 to 2.89. The specific gravity of the spirit used in this experiment was .857

The following experiment was made with spirit, the specific gravity of which was .784.

EXPERIMENT II.

Air in the room	—	60.
Nine ounces of powdered chalk at	62.8,	
mixed with 10.25 ounces of		
alcohol at	—	139.

The mixture being agitated 20 times with the thin blade of a knife, its temperature, as measured by a cylindrical thermometer, was

In 1 minute	—	118.75
2	—	118.5
3	—	118
4	—	117.
		Allow-

Allowing one degree for the heat lost by agitation in the course of the first minute, we have 119.75 for the true temperature.

The alcohol was therefore

cooled	—	19.25
and the chalk heated	56	degrees.

The cold chalk was introduced into the vessel containing the warm alcohol; and the capacity of the vessel being nearly equal to that of .8 of an ounce of alcohol, the spirit and vessel were together equal to 11.15 ounces. The quantity of chalk was nine ounces.

The comparative heat of chalk is therefore to that of alcohol in the compound ratio of 19.25 to 56, and of 11.15 to 9, or as 1 to 2.34.

EXPERIMENT III.

Air in the room	—	61.
Nine ounces of powdered chalk at	62.2,	
mixed with 10.5 ounces of		
water at	—	139.

The mixture being agitated as before 20 times, and its temperature being measured by the same thermometer that was used in the former experiments,

In 1 minute it was	-	124.75
2	—	124.5 plus,
3	—	123.75
4	—	122.75.

The capacity of the vessel was equal to that of .65 of an ounce of water, and the heat lost in the first minute was very nearly 1.2 degrees. Hence by this experiment the comparative heat of chalk is to that of water as 1 to 3.9.

The comparative heat of water is therefore to that of alcohol as 3.9 to 2.34, or as 1.66 to 1.

This experiment was repeated as follows:

Air in the room — 52.

Atmospherical air being contained in both tubes of the aerometer, 16½ ounces of spirit of wine (spec. grav. .826) was introduced into the tinned vessel L, and 20 ounces of water into R.

The temperatures of the water and spirit being very nearly equal, the aerometer was heated in the water bath, and immersed in the tinned vessels, where it was suffered to remain during 16 minutes; after which being removed, and the water and spirit being agitated for a short time, the former was

raised two degrees, the latter four. By this experiment the comparative heat of water is to that of the spirit as 1.5 to 1.

From the foregoing experiments it appears, in general, that metals contain less absolute heat than their calces, sulphur less than the vitriolic acid, and oil and alcohol less than air or water. Most of the metals, which were the subject of these experiments, are fixed substances, no part of them being elevated into vapour, in their calcination, except perhaps a small portion of inflammable air, which escapes during that process. Sulphur, oil, and alcohol, on the contrary, are wholly volatile on the application of fire. There are other substances, as wood and pit-coal, which, in the process of inflammation, are partly fixed and partly volatile. We cannot, by comparing these substances with their ashes, determine whether their capacities for containing heat are increased by combustion; because the ashes, which are the result of the combustion of a given quantity of them, bear a very small proportion to the whole. Thus it appears, from the Experiments of the learned and ingenious Bishop of Llandaff, that pit-coal contains one-third of its

weight of aërial and liquid matters, which may be separated by distillation; that dry heart of oak and fallow lose more than two-thirds of their weight in the same process, and that from 106 pounds Avoirdupoise weight of dry peeled oak, only 19 ounces of ashes were obtained. It is probable that a much smaller quantity would be produced by the combustion of an equal weight of green oak.

I have found that the ashes which were obtained from 56 pounds of Newcastle coal, weighed seven pounds six ounces. These ashes were not compleatly calcined. If they had been exposed to a strong heat, in a crucible, it is likely that the diminution in their weight would have been much more considerable.

Doctor Priestley has proved that charcoal, when moistened with a little water, and heated to a sufficient degree in close vessels, may be almost wholly resolved into heavy inflammable air; a fluid which consists partly of light inflammable air, and partly either of fixed air or of a substance that may be considered as its basis.

If, therefore, the capacities of wood and pit-coal be found by experiment to exceed
those

those of their ashes, we cannot from hence infer that the capacities of those substances are diminished by combustion. For it has been proved that the fixed matters in wood and pit-coal are combined with a large proportion of volatile principles, consisting chiefly of pure or of fixed air and water; and it appears, from experiment, that compounds, which consist of these elements, united to principles that resist the force of fire, have a greater capacity for heat than the latter in their uncombined state. We find, for example, from the foregoing table, that the calces of metals, when they are saturated with air, contain more absolute heat than the same substances, when the air is in a great measure extricated by nitrous acid, and exposure to fire.

I have also found that chalk contains more absolute heat than quicklime.

In order to determine the quantities of absolute heat in chalk and quicklime, I had recourse to alcohol as a standard. For the comparative heat of quicklime could not be ascertained with accuracy by making water the standard, as these substances, when mixed together, produce much sensible heat; but

when quicklime is mixed with alcohol, the heat produced during the first minute, is so small as not materially to effect the result of the experiment.

The comparative heat of quicklime, as determined by this method, will appear from the following trials.

EXPERIMENT I.

Air in the room	—	62.5.
Eleven ounces of powdered quick-		
lime at	—	66.2
mixed with 10.25 ounces of al-		
cohol at	—	139.

The mixture being agitated with the thin blade of a knife 20 times, its temperature

In 1 minute was	—	119.75
2	— —	120.5
3	— —	121 plus
4	— —	121.75
5	— —	122.75
6	— —	123.75
7	— —	124.75
8	— —	125.5
9	— —	126.5
11	— —	128

In

In 16 minutes	—	—	131
21	—	—	134.5
31	—	—	137.5
41	—	—	140
51	—	—	134.5

EXPERIMENT II.

Equal bulks of quicklime, and the same alcohol, being mixed together at the temperature of 62, the mixture

In 1 minute was	—	—	62.2
2	—	—	62.4
3	—	—	62.5
4	—	—	62.6
5	—	—	62.7
6	—	—	62.8.

The specific gravity of the alcohol used in these experiments was .784. Hence it appears, that more sensible heat is produced by mixing quicklime and alcohol when the latter is at a high temperature, than by mixing them when both are at a low temperature. It also appears, that, in the former instance, the quantity of sensible heat produced increases for a certain length of time, until it arrives

arrives at a maximum, after which it begins to diminish *.

Allowing $\frac{1}{4}$ of a degree for the sensible heat produced by the mixture, and one degree for that which was lost by the agitation, we have $\frac{1}{4}$ of a degree for the absolute quantity of heat separated in the course of the first minute. This being added to 119.75 gives 120 for the true temperature of the mixture. The capacity of the vessel was equal to that of .8 of an ounce of alcohol, the alcohol and vessel were therefore equal to 11.05 ounces; the alcohol was cooled 19 degrees, and the quicklime heated 53.8. The comparative heat of quicklime is, therefore, to that of alcohol in the compound ratio of 11.05 to 11, and of 19 to 53.8, or as 1 to 2.8.

It was before shown that the comparative heat of chalk is to that of alcohol, as 1 to 2.34. Hence the comparative heat of chalk

* It is manifest, that the production of sensible heat increased for a certain length of time in the above experiment. For though the ascension of the mercury in the thermometer was equal during several minutes, yet as the temperature was augmented, the quantity of heat carried off by the air, in a given time, was continually encreasing.

exceeds

exceeds that of quicklime in the proportion of 2.8 to 2.34, or in that of 1.15 to 1.

Mr. Kirwan has proved that the mild volatile alkali contains more absolute heat than the caustic. From the Experiments of this Philosopher it also appears, that the dephlogistigated nitrous and vitriolic acids have more absolute heat than the same acids when they are not dephlogistigated; and it is very probable that the dephlogistigation of those acids arises from the separation of inflammable, and the absorption of pure air. It moreover appears by experiment that the vitriolic acid, which probably derives its fluidity from water, contains more absolute heat, when it is combined with a large proportion of that fluid, than when it is united to a small proportion of it.

Hence we may conclude, in general, that when substances, which resist the force of fire, are united to pure air, aërial acid, or water, the capacity of the compound is greater than that of the fixed substance, in its uncombined state; and that the excess of the capacity of the compound, above that of the fixed substance, is, in some measure, proportional

tional to the quantity of the fluids with which the latter is combined.

If this be admitted, it will follow that the capacities of wood and pit-coal, which are compounds consisting of fixed elements united to a large proportion of aërial and aqueous particles, must necessarily be greater than those of the fixed parts that remain after these substances have been resolved into their principles by combustion *.

I have endeavoured, by a variety of trials, to ascertain the comparative heats of wood, of pit-coal, and of their ashes.

In the following table I shall give the reader a brief view of the results of those trials.

Dust of the pine tree	—	.50000
Charcoal	— —	.26315
Ashes of the same charcoal	—	.09090
Ashes of the elm tree	—	.14025
Pit-coal	— —	.27777
Cinders	— —	.19230
Ashes of cinders	—	.18552

* It is proper to observe, that the ashes of vegetables are also combined with a portion of air and water. But the quantity of those fluids, which is contained in the ashes, is much smaller than that which existed in the vegetables previously to their combustion.

In

In making these experiments, the solid body, reduced to a fine powder, was raised in a slight tinned iron vessel to the temperature of 139; the cold water was then suddenly poured upon it, and the vessel was placed in such a situation, that the surrounding air should have nearly the same temperature with the mixture. Different parts of the same room were chosen for this purpose; the water, before it was mixed with the solid, being placed in the colder part of the room, and the mixture in the warmer.

By this contrivance the loss of heat arising from the agitation, which was necessary to bring the parts of the mixture to a common temperature, was so small as to produce a very slight inaccuracy in the experiment.

As the experiment were made in the winter season, a sufficient variation of temperature was readily obtained in the same room, because the heat communicated by the solid to the cold water was not very considerable.

The solid was raised to the desired temperature by placing it in the steam bath formerly described; and the heat of the mixture was measured by a thermometer that had

had each degree of Farhenheit's scale divided into ten equal parts.

The trials, from which the results contained in the foregoing table have been calculated, do not exactly correspond with some experiments which were formerly communicated to the public on the same subject. But as they were several times repeated with great care, and were made in circumstances which admit of a considerable degree of accuracy, I cannot help thinking that they approach near to the truth.

These experiments prove that wood and pit-coal have more absolute heat than their ashes. We cannot however, as was before observed, infer from hence that the capacities of those substances are diminished by combustion, nor indeed can we conclude that the capacities of the fixed matters which they contain are diminished by that process. Notwithstanding the above experiments, it is possible that the fixed matters in pit-coal and vegetables, may have a less capacity for heat when they exist in those substances as constituent principles, than they have during their subsequent existence in the state of ashes. Thus, if we conceive the capacities of the

earthy and alkaline principles which exist in dry oak to be to that of the oak as 1 to 10, it will follow that the capacities of these principles are nearly doubled when they acquire the form of ashes in the process of combustion, because it is found that the ratio of the capacity of the ashes, produced by the burning of oak, is to that of the oak itself as 1 to 5.

If, however, we suppose, contrary to analogy, that the capacities of the ashes of pit-coal and vegetables are diminished by combustion; yet it is manifest that the sum of the capacities of the principles, into which those substances are resolved by that process, is increased. For fixed bodies, as was before shown, have a less capacity for heat than pure air, ærial acid, or water. When these substances enter into a chemical union with each other, it is very probable that the capacities of the fixed parts are increased, and that those of the volatile parts are diminished. We cannot, therefore, suppose that the fixed principles, which enter into the composition of wood, have a greater capacity for heat than the compound which consists of those principles combined with air and water. Let the capacities of the fixed principles be conceived,
for

for the sake of argument, to be equal to that of the compound. It has been shown that the capacity of dry oak is nearly five times greater than that of its ashes. Hence it will follow, upon this supposition, that the fixed parts in dry oak, when they are changed into ashes by combustion, undergo a diminution of capacity as 5; but it was proved that the fixed principles in dry oak compose not more than $\frac{1}{84}$ of the whole, the remaining 83 parts being, in the process of combustion, resolved into air and water. During this process the water is raised into vapour, and the air is expanded into a permanently elastic fluid. The capacities of the air, and of the aqueous vapours, which are thus obtained, are at least four times greater than that of dry oak. Consequently, when this substance is resolved into its principles by combustion, there is, in one part, a diminution of capacity as 5, and in 83 parts, an increase of capacity as 4; whence it appears that the sum of the capacities of the principles into which dry oak is resolved by that process, is greatly increased. The same reasoning will apply to the changes in the capacities of the elements derived from the

the combustion of other species of wood, as well as of pit-coal.

Hence we may conclude, in general, that the comparative heats of bodies, supposed to contain phlogiston, are increased by the processes of calcination and of combustion.

It follows, that when an inflammable body is deprived of its power of supporting flame, by the process of combustion, it absorbs a quantity of absolute heat, and when, by a contrary process, it again recovers its inflammability, an equal quantity of heat is detached. The calx of copper, for example, contains nearly twice as much absolute heat as copper. When, therefore, by exposing it to the action of fire, in contact with inflammable substances, the calx is revived, it will lose one half of its absolute heat; and, on the contrary, when the copper is again calcined, the calx will recover the heat which it had formerly lost.

From the foregoing observations and experiments it appears, that the capacities of bodies are diminished by their union with inflammable air, and increased by its separation. Pure air, for example, by its union with that principle, is converted into water,

and therefore its capacity for heat is diminished. The experiments of Mr. Kirwan prove that the capacity of the vitriolic acid is also diminished by phlogistication, and it is well known that this effect is produced by its union with inflammable air, or with substances containing that principle. It has moreover been shown, that by the peculiar combination of the heavy inflammable air with earth, and with water, in vegetables, and in pit-coal, the capacity of the compound for heat is diminished.

Hence we may conclude, in general, that bodies have their capacities for heat lessened by their union with inflammable air, and that when this principle is again detached, the elements, into which the body is resolved, have their capacities for heat increased; from which it follows, that in the former instance a quantity of heat will be separated from the body, and in the latter an equal quantity absorbed. In this point of view the separation of heat from a body, by means of the inflammable principle, and the re-absorption of it, when that principle is again disengaged, seems to be analogous to the separation of fixed air from earths and alkalis by means of

an acid ; and the re-union of these substances with this fluid 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 the inflammable principle be added to pure air, 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 fixed air which it had lost, and when the inflammable principle is again disengaged from the pure air, the air re-absorbs the heat which had formerly escaped from it.

PROPOSITION IV.

When an animal is placed in a warm medium, the colour of the venous blood approaches more nearly to that of the arterial, than when it is placed in a cold medium ; the quantity of respirable air which it phlogisticates, in a given time, in the former instance, is less than that which it phlogisticates, during an equal space of time, in the latter ; and the quantity of heat produced, when a given portion of pure air is altered by

the respiration of an animal, is nearly equal to that which is produced when the same quantity of air is altered by the burning of wax or charcoal.

That the difference between the colour of the venous and arterial blood, in a living animal, is diminished by exposure to heat, and increased by cold, appears from the following experiments.

EXPERIMENT I.

A dog, at 102, was immersed in water at 114; so much of his head being uncovered as to allow him a free respiration.

In 5 minutes the dog was 108, water 112

6 dog 109, water 112

11 dog 108, water 112,

the respiration having become very rapid.

13 dog 108, water 112,

the respiration being still more rapid.

In half an hour the dog was 109, water 112.

The animal was then in a very languid state.

Small quantities of blood being drawn from an artery, and from a contiguous vein, the

the venous blood was found to have undergone a remarkable change of colour. For in the natural state, the colour of the venous blood is a dark modena red; and that of the arterial a light scarlet. But after the animal, in the experiment in question, had been immersed in warm water for half an hour, the venous blood assumed very nearly the hue of the arterial, and resembled it so much in appearance, that it was difficult to distinguish between them.

It is proper to observe that the animal, which was the subject of this experiment, had been previously weakened by losing a considerable quantity of blood a few days before. When the experiment was repeated with dogs, which had not suffered a similar evacuation, the change in the colour of the venous blood was more gradual; but in every instance in which the trial was made, and it was repeated six times, the alteration was so remarkable, that the blood which was taken in the warm bath, could readily be distinguished from that which had been taken from the same vein before immersion, by those who were unacquainted with the circumstances or motives of the experiment.

To discover whether a similar change would be produced in the colour of the venous blood in hot air, a dog, at 102, was placed in air at 134. In 10 minutes the temperature of the dog was $104\frac{1}{2}$, that of the air being 130. In 15 minutes dog 106, air 130. A small quantity of blood was then taken from the jugular vein, the colour of which was sensibly altered, being much lighter than in the natural state.

I next endeavoured to ascertain the effect produced upon the colour of the venous blood, in a living animal, by exposure to cold. To determine this, a dog, at 100, was immersed in water at 45. In about a quarter of an hour a few ounces of blood were taken from the jugular vein, which was evidently much deeper in its colour than that which had been taken in the warm bath, and appeared to me, as well as to several other Gentlemen who assisted in the experiment, to be the darkest venous blood we had ever seen.

From the foregoing experiments we may conclude, that when an animal is placed in a cold medium, the venous blood assumes a
much

much darker hue than when it is placed in a warm medium.

These experiments seem to confirm the following opinion, which was first suggested by my very ingenious friend Mr. Wilson, of Glasgow. Admitting that the sensible heat of animals depends upon the separation of absolute heat from the blood, by means of its union with the phlogistic principle in the minute vessels, may there not be a certain temperature at which that fluid is no longer capable of combining with phlogiston, and at which it must of course cease to give off heat?

The following experiments prove that the air expired from the lungs of an animal, is more phlogisticated in a cold than in a warm medium.

EXPERIMENT II.

The air in the room being at the temperature of 61, a small Guinea pig was placed on a stand in an inverted glass jar that contained nearly five pints of common air, and was surrounded with water at $55\frac{1}{2}$. It was suffered to remain in this situation 42 mi-

nutes, when it discovered signs of great uneasiness: being removed from the jar, it fell into convulsions, but soon recovered. The air in the jar was then tried by mixing two measures of it with one of nitrous air, and the measures of the test were found to be $2.7\frac{1}{2}$ nearly; which showed that it was about $\frac{1}{4}$ as pure as common air.

EXPERIMENT III.

A Guinea pig of nearly the same size being placed in the same jar, containing common air as before, and surrounded with water at 104, expired in 42 minutes nearly.

The air in the jar being then tried by mixing two measures of it with one of nitrous air, the test was a little more than 2.5, or it was nearly half as pure as common air. The phlogification of the air, in the first experiment, was, therefore, twice as great as in the second.

Hence it appears, that the degree of purity in the air, necessary to the support of life, is greater when an animal is placed in a warm than in a cold medium; for in the second experiment the animal survived, though the
purity

purity of the air was diminished three-fourths. In the last, it was only diminished one half, and yet the animal expired.

It may likewise be proper to remark, that the symptoms of uneasiness discovered in the former experiment were much greater than in the latter. The eye in particular, in the one case, seemed to be much swoln, and became dim and languid; in the other, it retained its brightness to the last.

As it might be suspected that the superior purity of the air, in the latter experiment, did not arise from its having been respired at a higher temperature, but from some latent difference in the animals, the trials were repeated as follows.

EXPERIMENT IV.

A small Guinea pig was placed in an inverted jar, containing nearly five pints of common air, which was surrounded with water at 46.5. The animal was suffered to remain in this situation 36 minutes, during which time it did not discover signs of much uneasiness. The air in the jar being then examined

examined by means of nitrous air, the test was found to be 2.5, or it was one half as pure as common air.

EXPERIMENT V.

On the ensuing day the same Guinea pig, apparently in perfect health, was placed in the same jar surrounded with water at 102. It was removed at the end of 36 minutes without appearing to have received any injury. The air which remained in the jar being then examined by nitrous air, the test was found to be 2.2, or it was nearly $\frac{1}{2}$ as pure as common air.

As it might be imagined that, in these trials, a fallacy arose from the exposure of the animal, in the first instance, to cold, and in the second to hot air, the experiments were reversed as follows.

EXPERIMENT VI.

A small Guinea pig was placed in the same jar that was used in the former experiments. The jar was filled with common air as before, and surrounded with water at 102.

The

The Guinea pig being removed at the end of 36 minutes, and the air in the jar being examined by nitrous air, the test was 2.3 nearly.

EXPERIMENT VII.

The same animal, on the ensuing day, appearing to be in perfect health, was suffered to remain 36 minutes in the same jar, containing also common air, and surrounded with water at 52. The air being then examined as before, the test was 2.5.

From these experiments it appears, that when an animal is placed in a cold medium the quantity of air which it phlogisticates, in a given time, is greater than when it is placed in a warm medium.

The following apparatus was contrived with a view to determine the quantities of heat produced, when given quantities of air were altered by the burning of combustible bodies, and by the respiration of animals. See plate IV. fig. 1.

A C represents a triple vessel made of tinned iron, and consisting of an exterior vessel A B C D, 16 inches wide, and 19.4 deep; a
middle

middle vessel $efgb$, $11\frac{1}{2}$ wide, and $13\frac{1}{2}$ deep; and an interior vessel $abcd$, $9\frac{1}{2}$ wide, and $10\frac{1}{2}$ deep.

The middle vessel is supported by a circular plate of iron, at the distance of 2.75 inches from the bottom of the exterior vessel; and the distance of the cylindrical surface of the exterior, from that of the middle vessel, is also 2.75 inches. The space between these vessels is stuffed with fine down.

The interior vessel is supported by the pipes bf , ig , at the distance of .875 of an inch from the bottom of the middle vessel, and this is likewise the distance of the cylindrical surfaces of the vessels from each other.

The space included between these vessels is occasionally filled with water.

In the upper surface of the interior vessel there is an aperture five inches wide. To this aperture is soldered a flat ring of brass kl , which is covered by a plate of the same metal, the surface of the latter being adjusted to that of the former, with such accuracy, that, when they are screwed together, a piece of leather being interposed between them, they become air tight.

In

In the centre of the brass plate there is a circular cavity an inch and a half wide, in which a thin plate of glass is cemented. By means of this glass the light of an inflamed body, placed in the centre of the apparatus, becomes visible, and the focus of a lens may be occasionally directed through it into the interior vessel.

It was before observed that the interior vessel rests upon the pipes *bf*, *ig*. These open into it at the points *b* and *i*. The latter being bent upward, extends along the interior surface of the middle vessel, and when it has arrived at the point *n*, it is again bent at right angles, and terminates at *m*. To this extremity is screwed the crooked pipe *VZ*, which is fixed by cement to the inverted jar *EF*, the latter being supported in the tub *GH* by the cross bar *YO*. The communication between the jar *EF* and the interior vessel may be occasionally interrupted by the stop cock *V*.

The pipe *bf* extends along the opposite side of the middle vessel as far as the point *e*, where it is also bent at right angles, and terminates at *p*, in a conical brass pipe, which enters into a similar pipe at the extremity of
the

the crooked tube R U. These pipes are ground together with such accuracy as to be air tight. They are closely applied to each other by means of a screw.

The extremity R, of the crooked tube U R, is cemented into the neck of the inverted jar I K, which is placed in the tub L M; and a second crooked tube S T passes from the neck of the same jar into the tub P Q, where it terminates under the inverted jar N O.

When an experiment is to be made with this apparatus, water is introduced into the tub G H till it rises a little above the cross bar Y O, on which the inverted jar E F rests. The subject of the experiment is introduced into the interior vessel *a c*, and the aperture *k l* is covered by the brass plate, which is screwed down in such a manner as to be air tight. The space included between the middle and interior vessels is filled with water, and the upper surface of the exterior vessel is closely covered with a thick cushion, stuffed with fine down.

The inverted jar I K, and the tub L M, are filled with water, and likewise the tub P Q, and the inverted jar N O. The cock

U being shut, and V open, the diminution, which is produced in the air by the animal, or by the burning body, is measured by the rising of the water in the jar E F.

The quantity of heat produced is determined by the increase of temperature in the water that surrounds the interior vessel; and the escape of the heat from that fluid is prevented by the down with which it is encompassed.

The purity of the air in the interior vessel is examined as follows: The cocks V and T are shut, and U opened. A part of the water is removed from the tub L M, in consequence of which the water in the jar I K descends by its gravity; and in descending it draws a portion of air from the interior vessel *a c.* The cock U is then shut, and T opened, and the water being returned into the vessel L M, the air is forced by its pressure from the jar I K into the jar N O, whence it is introduced into the eudiometer.

With this apparatus I made the following experiments.

EXPERIMENT VIII.

The barometer being 30.4, and the air in the room 67.5, a quantity of water weighing 31 pounds, seven ounces Troy, was taken at the temperature of 67.65; the space included between the interior and middle vessels was filled with a portion of this water to such a height, that the upper surface of the fluid was nearly on a level with the mouth of the interior vessel. A small wax taper, which had been accurately weighed, was then lighted, and instantly placed in the centre of this vessel; the mouth of which was now covered with the brass plate, and the remaining part of the water was introduced. The surface of the latter rose nearly one inch above the interior vessel. The mouth of the exterior vessel was covered with a cushion of down.

Before this experiment was made the jars E F and I K had been separated from the vessel A C, and the nozzle of a pair of bellows had been fixed by means of a screw to the extremity of the pipe *n m*. Fresh air was thus introduced into the interior vessel, and the

the vitiated air escaped through the pipe *k f*. By this contrivance the taper was kept in a state of inflammation for half an hour, when the experiment was discontinued.

The water, in the course of the experiment, was, at short intervals, gently agitated by a wooden rod for the purpose of rendering the diffusion of the heat uniform; and the temperature of the air in the room was augmented nearly in the same degree with that of the water. The heat of the water was measured by a thermometer, that had each degree of Fahrenheit's scale divided into ten equal parts.

At the end of the experiment the thermometer, in the water, stood at 69.75, the air in the room being 69.5 nearly. The loss of weight in the wax taper was 26 grains.

Thus it appears, that by the combustion of 26 grains of wax, the temperature of 31 pounds seven ounces of water was raised 2.1 degrees.

To discover how much air was altered by the combustion of this quantity of wax, I made the following experiment.

Y

E x-

EXPERIMENT IX.

A large tub was filled with lime water. A piece of cork, which had a portion of the same wax taper that was used in the foregoing experiment fixed in its centre, was made to swim on the surface of this water. Four narrow slips of thin paper, applied to each other by their flat surfaces, were tied to the upper extremity of the taper, being of such a breadth as to surround one half of the wick. A smaller number of slips than four did not furnish a sufficient heat to kindle it; and it was found that the wick could not readily be inflamed unless a part of its surface were exposed to the air. The slips were made very dry by pressing them with a hot iron. They weighed a little less than a grain.

Over this taper a large cylindrical glass vessel was inverted, which had a scale of equal parts affixed to it. A tube of a considerable length, that was nearly bent double, had one of its extremities introduced into the inside of this vessel, the other extremity rising above the surface of the water on the outside. The vessel was now pressed
into

into the water, and the greater part of the air which it contained escaped through the tube, which was then withdrawn, and pure dephlogisticated air introduced in such quantity, that the surface of the water, in the inside of the jar, stood at the height of three inches from its inverted mouth.

For the purpose of determining the height of the water with greater exactness, the jar was removed from the tub, and was placed upon a table in a shallow vessel, the depth of which was little more than three inches; by this means the eye could be brought nearly to the same level with the point which was to be observed, and the surface of the water was rendered distinct by the reflection of the light from the bottom of the vessel. When this observation was made, one candle was placed behind the jar, and another before it; by the latter, the divisions of the scale were illuminated, and by the former, the light was thrown upon the surface of the water.

It is proper to observe, that when the point at which the water stood was marked, the fluid, which surrounded the outside of the jar, was raised to the same height with that in the inside, in consequence of which the

air in the jar had the same density with the external air; and the jar was kept in an horizontal position by means of a *spirit level*.

The point at which the water stood in the jar being thus accurately determined, the taper was then inflamed by throwing the focus of a lens upon the slips of paper that were fixed to its upper extremity, and was allowed to burn until the air in the jar was so far vitiated, as to be no longer capable of supporting the inflammation. It was now removed from the jar by making it to pass under the surface of the water, and having been carefully dried, it was weighed, when it was found to have lost eleven grains. The air in the jar was then introduced into large bottles, which were half filled with lime water, and being agitated until the fixed air was absorbed, the residuum was returned into the jar, which was placed in a vessel nearly three inches deeper than that formerly used, and the surrounding water being brought to a level with that in the jar, the internal surface stood at 5.75 inches. The diminution of the air was, therefore, such as to raise the water from 3 to 5.75, or 2.75 inches. The quantity of water which occupied

pied one inch in this jar was 15 ounces Troy. Hence the quantity of pure air, altered by the combustion of eleven grains of wax, was 41.25 ounce measures, or 78.2 cubic inches; and consequently, the quantity of air changed by the combustion of 26 grains of wax, was 97.5 ounce measures, or 184.84 cubic inches. When this experiment was made the thermometer, which measured the heat of the air in the room, was at - 70

The barometer at - 30.4.

It is of importance that the portions of wax employed in these experiments, should resemble each other as nearly as possible in their purity; and, therefore, parts of the same taper were used in both experiments. When the trials were repeated with different tapers, a variation was observed in the results: thus I found that when a species of wax, somewhat purer than that employed in the foregoing experiments, was inflamed in the above described apparatus, and was suffered to burn until the water was raised 2.15 degrees, the loss of weight in the wax was only 22.5 grains.

6.75 grains of this wax being burned in dephlogisticated air, the diminution of the

latter was nearly 30 ounce measures Troy, which gives 100.7 ounce measures for the quantity of air altered by the combustion of 22.5 grains.

EXPERIMENT X.

To determine the quantity of heat produced by an animal, a *Guinea pig* was introduced into the centre of the interior vessel described above, which was surrounded by 31 pounds seven ounces Troy of water, at the temperature of - 62

In half an hour the water being brought by agitation to an uniform heat, its tempera-

ture was - - 62.25

In an hour it was - 62.5

In an hour and a half - 62.75

In two hours - - 63.

The heat of the air in the room at the beginning of the experiment was 62; and its temperature was augmented nearly in the same degree with that of the water.

Hence the heat imparted to the water by the animal in two hours was 10 degrees by
my

my thermometer, or one degree by Fahrenheit's.

The quantity of air altered by the respiration of the animal, in that time, was determined as follows.

EXPERIMENT XI.

Part I. (see plate 4, fig. 2.)

The same *Guinea pig* was introduced into the glass jar A B, which was instantly inverted over water in the tub C D, and was made to rest upon the cross bar E F, the animal being supported by the stand G H. One extremity I, of the crooked tube K L M N O, was made to pass through the cross bar E F into the jar A B, where it rose a little way above the surface of the water; the other extremity P terminated under the mouth of the inverted jar Q R, which was filled with water, and was supported by a cross bar in the tub S T. As soon as the jar A B, containing the animal, was inverted over the water, a greater quantity of that fluid was introduced into the tub, by the pressure of which a part of the air was forced from the jar through the crooked tube, and

was received in the vessel Q R. The animal was suffered to remain in the jar 35 minutes; it was then removed by making it to pass under the surface of the water.

The air in the jar A B was agitated in large bottles with lime water until the fixed air was absorbed, and likewise that which had been received in the vessel Q R. The whole was then returned into the vessel A B, and the height of the water being determined with the precautions mentioned above, it was found to stand at 3.312 inches. When this experiment was made the air in the room was

The barometer - - 30.

In the experiment formerly related, that was made with a view to determine the quantity of air altered by the combustion of wax, the thermometer was at 70, the barometer at 30.4.

The variation in the heights of the barometer, in these experiments, was therefore $\frac{1}{16}$ of an inch. Hence, if the temperature of the air in both instances had been the same, the densities would have been as 76 to 75; and consequently the bulk of the air in Experiments VIII. and IX. in order to re-duce

duce it to the same density with the air in the Xth Experiment, must have been increased $\frac{1}{78}$ of the whole, or 1.3 ounce measures. But the temperatures varied from each other by 7.5 degrees. Supposing the air to be expanded $\frac{1}{483}$ of its bulk by one degree of heat, it follows, that if the heights of the barometer had been the same, the densities would have been as 64 to 65 nearly; or the bulk of the air in the preceding trials, in order to reduce it to the same density with the air in the last experiment, must have been diminished by $\frac{1}{85}$ of the whole, or 1.4 ounce measures. It was before shown that it must be increased 1.3 ounce measures on account of the difference in the heights of the barometer. If, therefore, in those experiments the barometer had stood at 30, and the thermometer at 62.5; the bulk of the air changed by the combustion would have been $97.5 + 1.3 - 1.4$, or 97.4 ounce measures.

EXPERIMENT XI. Part II.

The Guinea pig being introduced into the same jar, the latter was inverted over water; a part of the air was forced from it as before; the

the animal was immediately removed by making it to pass under the surface of the water; and the air that was expelled was again returned into the jar, when it was found that the water in the jar stood at 2.187 inches. Hence the diminution of the air by the respiration of the animal during 35 minutes, was such as to raise the water in the jar from 2.187 to 3.312 inches, or 1.125 inches.

As the quantity of water which occupied the space of one inch in this jar was 15 ounces Troy, it follows that, in the experiment in question, the air was diminished 16.875 ounce measures Troy, and consequently the quantity of pure air, altered by the respiration of the animal, in two hours, was 57.8 ounce measures Troy, or 109.5 cubic inches.

When I first tried this experiment, I introduced the animal into the inverted jar, by making it to pass under the surface of the water; but the result, in these circumstances, was liable to a considerable degree of inaccuracy. For when the animal is dry it alters a smaller quantity of air in a given time than when it is wet. In the latter instance cold

is

is produced by the evaporation from the surface of the animal, and by the experiments recited in page 312, it is proved that when an animal is placed in a cold medium, it phlogisticates a greater quantity of air, in a given time, than when it is placed in a warm medium.

I therefore found it necessary first to introduce the animal into the jar, and then, as speedily as possible, to invert the latter over water, the animal being supported by the stand as described above.

When the experiment is made in this way, a slight inaccuracy arises from the rarefaction of the air, by the heat which the animal communicates, during the short interval that elapses between its introduction into the jar, and the inversion of the latter over water; but this inaccuracy was corrected by Experiment IV. part 2. For when the Guinea pig was removed, in that experiment, the ascension of the water in the jar measured not only the space which the animal had occupied, but also the air that was expelled by the heat which it communicated.

It has been proved that the quantity of pure air, altered by the respiration of the animal,

animal, in two hours was 57.8 ounce measures Troy, and that the quantity of heat imparted to the water during the same period of time was one degree.

Hence it follows, that if an increase had been produced in the temperature of the water, amounting to 2.1 degrees, the quantity of air changed would have been 121 ounce measures. It was before shown, that when an equal quantity of heat was produced by the burning of a wax taper, the quantity of air altered was nearly 100 ounce measures.

A series of experiments, similar to those now recited, have been made by Messrs. Lavoisier and De la Place with a different apparatus. See *Memoire sur La Chaleur*, Lû a l'Académie Royale des Sciences, le 28 Juin, 1783.

According to the experiments related in this *Memoire*, when equal portions of air were altered by a Guinea pig, and by the combustion of charcoal, the quantity of heat excited in the former instance was to that in the latter as 13 to 10.3 nearly. It is proper to observe that, in those experiments, the quantity of air altered by the respiration of the animal was measured, by placing it in
air

air which had nearly the temperature of 60; and when the heat which it produced was determined, it was placed in a medium which had the temperature of 32 or 33. Now it has been proved, that when an animal is surrounded by a medium at a low temperature, it phlogisticates a greater quantity of air, in a given time, than when it is surrounded by warm medium. Hence we may conclude, that the quantity of air phlogisticated by the animal, in Mr. Lavoisier's apparatus for measuring heat, was greater than that which has been determined by his calculation. This cause of inaccuracy was suspected by the Authors of the Memoire.

I have repeated Mr. Lavoisier's experiments with charcoal, and have obtained the following results.

EXPERIMENT XII.

An oblong piece of charcoal was taken which weighed 73.8 grains. It was formed in such a manner that one of its extremities should taper until it nearly terminated in a point. The air having been expelled from it by exposure to heat, its conical extremity was

was set on fire by applying it to the flame of a candle, upon which it was instantly placed in the centre of the apparatus employed for measuring heat. Thus situated, it was kept in a state of inflammation by a current of fresh air, which was introduced into the interior vessel by means of the bellows and pipe described above.

The interior vessel was surrounded by 31 lb. 7 ounces Troy of water, which, at the beginning of the experiment, had the temperature of 62.7.

The air in the room was 62.8. At the end of 22 minutes, the water being reduced by agitation to a uniform heat, its temperature was 65.9. The air in the room 66 nearly.

The charcoal after the combustion weighed 17.8 grains. The loss of weight was, therefore, 56 grains.

EXPERIMENT XIII.

Air in the room	-	61.8
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Barometer	-	30.
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The upper surface of a circular piece of cork, similar to that mentioned in Experiment

ment IX, was covered with a thin plate of tinned iron, to which two small perpendicular plates of the same metal were fixed at a little distance from the centre. Between these plates an oblong piece of charcoal, weighing 91.1 grains, was placed; the diameter of the latter, compared with the distance of the plates from each other, being such, that when it was slipped in between them, it was retained in its proper position by their elasticity.

The charcoal thus connected with the cork was made to swim on the surface of lime water; and the jar, which was used in the former experiments, was inverted over it, the common air being expelled, and dephlogisticated air introduced in the manner described in Experiment IX.

The height of the water in the jar, determined with the precautions mentioned above, was found to be 3.125 inches. The charcoal was then set on fire by a lens, and was suffered to burn until it was extinguished in consequence of the contamination of the air; after which the charcoal was removed by making it to pass under the surface of the water, and was perfectly dried, when it was

found to have lost 14 grains. The air that remained in the jar was introduced into large bottles, and was agitated with lime water; and being again returned into the same jar, the water in the latter was found to stand at 5.75 inches. The diminution of the air was, therefore, 39.375 ounce measures Troy, or 74.65 cubic inches. With a view to avoid the inaccuracy which might arise from a difference in the qualities of different kinds of charcoal, portions of the same piece were used in both these experiments.

The last trial was repeated with the following result.

EXPERIMENT XIV.

Air in the room	-	-	61
Barometer	-	-	29.6

A piece of charcoal, which, when very hot, weighed one dram and $27\frac{1}{4}$ grains, was made to swim upon lime water. The jar being inverted over it as before, and a quantity of dephlogisticated air introduced, the water was found to stand at 3.064 inches. The charcoal was now set on fire by a lens, and was suffered to burn till the air was so much

much vitiated as to be incapable of supporting the inflammation.

The residuum of the coal was then removed by making it to pass under the surface of the water. Being accurately dried and made very hot, it was again weighed, when it was found to have lost $12\frac{1}{2}$ grains.

The air which remained in the jar was agitated as before with lime-water, and was returned into the jar, when the water in the latter stood at 5.625 inches.

Hence it appears, that after the agitation with lime-water, the volume of the air in the jar was reduced 2.561 inches. The quantity of pure air that was altered was, therefore, 38.4 ounce measures troy, or 72.4 cubic inches.

To determine with certainty whether any fixed air was mixed with the air that remained in the jar, a portion of the latter, at the end of the experiment, was agitated with lime-water, when it was found that no precipitation took place.

Reducing the air to the same density in the two preceding experiments, and taking the mean result, we have 41.29 ounce measures, or 78.3 cubic inches for the quantity

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of

of pure air altered by the combustion of 14 grains of charcoal, and hence 107.8 cubic inches would be altered by the combustion of 18 grains.

According to Mr. Lavoisier's experiments, 128.38 French cubic inches were altered by the combustion of an equal quantity of that substance; the barometer standing at 28 inches, and the thermometer at 54.5. If the barometer had stood at 30, which was nearly its mean height in the above experiments, and the thermometer at 61; 121.34 French, or 138.32 English cubic inches would have been altered. There is, therefore, a difference of 30.5 cubic inches between the results of the foregoing trials, and those of Messrs. Lavoisier and De la Place.

It is possible that this variation may have partly arisen from a difference in the qualities of the portions of charcoal that were employed. The coal which I used was charred oak.

From Experiments XIII. and XIV. we learn, that 41.29 ounce measures of pure air were altered by the combustion of 14 grains of charcoal. Consequently, by the combustion of 56 grains, 165.16 ounce measures
will

will be altered. And from Experiment XII. we learn, that the quantity of heat communicated by the burning of 56 grains of charcoal, to 31 lb. 7 oz. of water, was 3.2 degrees. Whence it follows, that, in order to the communication of 2.1 degrees of heat, it would be necessary that 108.3 ounce measures of pure air, should be altered by the combustion of charcoal. And we have seen, that an equal quantity of heat was communicated to the water, when 100 ounce measures of pure air were altered by the burning of wax.

It is proper to observe, that when the wax taper was burned in the above described apparatus, with a view to determine the heat which it produced, a considerable quantity of aqueous vapour was condensed on the inner surface of the interior vessel. But no vapour appeared when the experiment was made with charcoal. It is therefore probable that, in the former instance, a part of the pure air was converted into water; and that in the latter, it was principally, if not entirely, changed into fixed air. This conclusion is, I think, confirmed by the following experiments.

EXPERIMENT XV.

Air in the room	-	-	67
Barometer	-	-	29.5

The jar being inverted as before, over a wax taper, which was made to swim on the surface of pure water, a quantity of dephlogisticated air was introduced, when the water in the jar was found to stand at 3.312 inches.

The taper was now kindled, and suffered to burn till it was extinguished in consequence of the deficiency of pure air, and the jar being speedily cooled to the temperature of the ambient medium, the water, previously to the absorption of the fixed air, was found to stand at 4.187 inches. After the fixed air was absorbed by agitating it with lime-water, and the remaining air was returned into the jar, the water in the latter stood at 6 inches.

The diminution of the air, therefore, by the combustion of the wax, previously to the absorption of the fixed air, was to the entire diminution, after the fixed air was absorbed, as .875 to 2.688, or as 1 to 3.07.

EXPERIMENT XVI.

Air in the room	-	-	61
Barometer	-	-	29.6
A piece			

A piece of charcoal was made to swim on lime-water; the jar was inverted over it; and a quantity of dephlogisticated air was introduced, by which the water in the jar was depressed until it stood at 3.064 inches. The charcoal was now set on fire by a lens, and was suffered to burn till it was extinguished in consequence of the contamination of the air.

The air in the jar being brought to the temperature of the surrounding atmosphere, the water was found to stand at 3.439 inches. The fixed air was then absorbed by agitating it, in bottles, with lime-water; and the remaining air being returned into the jar, the water in the latter stood at 5.625 inches.

Hence the diminution of the air by the combustion of the charcoal, previously to the absorption of the fixed air, was to the entire diminution, as .365 to 2.561, or as 1 to 7.01 nearly.

As I was apprehensive, that in the last experiment, the air in the jar had not been completely cooled to the temperature of the ambient medium, it was repeated with the following result.

EXPERIMENT XVII.

Air in the room	-	-	62
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Barometer	-	-	29.8
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The jar being inverted over a piece of charcoal, which was made to swim on the surface of lime-water, and dephlogisticated air being introduced as before, the water was found to stand at 3.562 inches.

The charcoal was then kindled, and suffered to burn till the pure air was vitiated, and the jar was cooled to the temperature of the surrounding air, by pouring cold water over it. With a view to satisfy myself that it had acquired the exact temperature of the surrounding air, I suffered it to remain in a state of rest during the space of $2\frac{1}{2}$ hours; the air in the room retaining very nearly the same temperature that it had at the beginning of the experiment. The water in the jar was now found to stand at 3.875 inches. After the fixed air was absorbed, it stood at 5.75 inches.

The diminution of the air in this experiment, previously to the absorption of the

fixed air, was to the entire diminution, as .313 to 2.188, or nearly as 1 to 6.99. In the foregoing experiment, it was as 1 to 7.01. And taking the mean result, it will be as 1 to 7 very nearly*.

It was before shewn, that the diminution which was produced in the volume of the pure air, by the combustion of wax, was to

* The mean result of the foregoing experiments, gives 67.3 cubic inches for the volume of the pure air changed by the combustion of the charcoal, and 57.7 for the volume of the fixed air which was produced in that process. The mean height of the barometer was 29.55 inches; and the mean temperature was 61.5. If the barometer had stood at 28, and the thermometer at 54.5, the volumes would have been 70.26 and 59.68. Supposing that when the barometer is at 28, and the thermometer at 54.5, a cubic inch of pure air weighs .32105 of a grain, Troy, and a cubic inch of fixed air .47489 of a grain; the weight of 70.26 cubic inches of the former will be 22.55 grains, and that of 59.68 of the latter, 28.34. It is manifest that the excess of the weight of the fixed, above that of the pure air, must have arisen from its being combined with a principle furnished by the charcoal; and consequently, the quantity of the inflammable principle contained in fixed air will be nearly $\frac{1}{3}$ th of the whole. According to Mr. Lavoisier's experiment, it is only $\frac{1}{9}$ th of the whole. As these proportions vary from each other very much, I repeated my experiments with great care, but I could not find the results to be materially different from those which have been stated above.

the entire diminution after the fixed air was absorbed, as 1 to 3 nearly.

From these experiments, compared with the observations recited in page 339, it follows, that when pure air is phlogistified by the burning of wax, it is partly converted into aqueous vapour, and partly into fixed air; and that when it is phlogistified by the burning of charcoal, it is entirely converted into the latter fluid.

As the vapour which is the result of the former process is suddenly condensed, the air that remains in the jar, previously to the absorption of the fixed air, suffers a much greater diminution of bulk, than it does in those cases in which no aqueous vapour is produced.

I have found that the combustion of tallow and oil, as well as that of wax, is accompanied with the production of aqueous vapour. That a quantity of water is produced by the combustion of oil, is also proved by the experiments of Mr. De Luc.

From these facts it seems extremely probable, that oil, wax and tallow, and most other inflammable substances, contain two kinds of inflammable air, one of which resembles

resembles the light inflammable air of metals, and the other, the heavy inflammable air obtained from vegetables by exposure to heat in close vessels. The first, by its union with pure air, produces water; and the last, fixed air; and hence fixed air and water are the result of the combustion of wax and oil. When those substances are reduced to a coal, by exposure to heat in close vessels, it seems probable that the light inflammable air, combined with the volatile parts, escapes, and that the heavy inflammable air is retained; and, consequently, fixed air alone is produced by the combustion of charcoal.

Having found that the pure air which is altered by the burning of wax, oil, and tallow, is partly converted into water, and partly into fixed air, I next endeavoured to determine whether a similar change is produced by the respiration of an animal.

EXPERIMENT XVIII.

Air in the room	-	-	67
Barometer	-	-	30

A Guinea pig was introduced into a glass jar, containing dephlogisticated air, which was inverted

inverted over water, the mouth of the jar being closed with a cork to prevent the absorption of the fixed air. The animal was suffered to remain in this situation a little more than an hour, when the cork was removed, and the water permitted to enter. The latter now occupied the space of 16 cubic inches in the jar. The Guinea pig was then withdrawn by making it to pass under the surface of the water, and the point at which the water stood in the jar was accurately determined. The experiment being discontinued for a time, the height of the water was again observed at the end of eight hours, when it was found to have remained nearly stationary; from which it appears that fixed air is very slowly absorbed by water when they are both in a state of rest.

The air being now agitated with lime water, the residuum was returned into the jar; and by the ascension of the water it was found that 40.86 cubic inches of fixed air were absorbed.

It was before shown that the diminution of the air produced by the respiration of the animal alone, was 16 cubic inches. It now appears that the diminution produced by the
absorption

absorption of the fixed air was 40.86 cubic inches; the entire diminution, therefore, or the quantity of air altered, was 56.86 cubic inches, or 18.9 grains.

Hence it follows that part of the pure air, changed in the above experiment, was converted into water; for by the experiment with charcoal it is proved that, if the pure air had been entirely converted into fixed air, the diminution which it would have suffered, previously to the agitation with the lime water, would not have been more than the $\frac{1}{7}$ of the whole; but, in the experiment in question, it was nearly $\frac{1}{3.5}$ of the whole. A part of the air, therefore, in the latter instance, must have been converted into aqueous vapour, which was condensed upon the internal surface of the jar. It moreover appears, from the trials with charcoal, that if the pure air had been entirely converted into fixed air, the weight of the latter would have been $\frac{1}{5}$ greater than that of the former; but in the last mentioned experiment, the weight of the pure air altered by the respiration of the animal, was nearly equal to that of the fixed air which was produced by this process; for the weight of the pure air was 18.9 grains, and that

that of the fixed air 20.1 grains. Of the latter quantity $\frac{4}{5}$ consisted of pure air, and $\frac{1}{5}$ of heavy inflammable air; that is the quantity of pure air, changed into fixed air, was 16.08 grains; but the entire quantity of pure air, altered during the process, was 18.9 grains. It follows that 2.82 grains of this fluid were changed into water, and 16.08 grains into fixed air.

From the XVIth Experiment it is manifest, that fixed air consists of pure air, and of a principle furnished by inflammable substances, agreeably to the opinion of Dr. Black, Mr. Kirwan, and Mr. Lavoisier. For the quantity of fixed air produced, in that experiment, was 28.34 grains, (see note, p. 343) the quantity of pure air altered by the process was 22.55 grains, and the quantity of charcoal consumed was 12.5 grains.

Hence it is not possible that the fixed air could have been solely derived from the charcoal, or from the pure air, because its weight exceeded that of either of those substances; and that it could not have been derived from the water, appears from the result of similar experiments which have been made over mercury.

Since,

Since, therefore, during the combustion of charcoal, a quantity of fixed air is produced, greater than the entire quantity, either of coal or of pure air that disappear, we cannot, I think, avoid concluding, that the fixed air is formed by an union between the pure air, and a principle furnished by the charcoal.

It moreover appears from Experiments VIII. and IX. that when wax is burned in pure air, more than the one half of it enters into the composition of the fixed air and aqueous vapour, which are produced during that process. For the volume of the pure air, altered in Experiment VIII. was 184.84 cubic inches; the barometer standing at 30.4, and the thermometer at 70. If the former had stood at 28, and the latter at 54.5, the volume would have been 194.44 cubic inches, or 62.4 grains. The bulk of the pure air, previously to the absorption of the fixed air, was diminished one third in consequence of the combustion of the wax *. The quantity of fixed air produced was, therefore, 129.6 cubic inches, or 61.5 grains.

We have seen that when pure air is converted into fixed air, its bulk is diminish-

* See Experiment XV.

ed $\frac{1}{7}$ of the whole; therefore the volume of the pure air contained in the above-mentioned 129.6 cubic inches of fixed air, must have been 148.2 cubic inches; subtracting this from 194.4, we have 46.2 cubic inches, or 14.8 grains, for the quantity of pure air which was converted into water. The fixed air contained $\frac{1}{5}$ of its weight of inflammable matter, and the water nearly $\frac{1}{6}$; and hence the quantity of inflammable matter, which entered into the composition of the fixed air and water, was 14.78 grains. From which it appears that wax consists principally of heavy and of light inflammable air, in a solid state, since the greater part of it is combined with the pure air which is altered during its combustion.

The following table exhibits the quantities of heat communicated to 31 pounds seven ounces Troy of water, by the combustion of different substances in the apparatus described above *.

* The thermometer employed for measuring the heat expressed in this, and in the subsequent table, had the degrees of Fahrenheit's scale subdivided into decimals; and, consequently, each degree corresponds with the $\frac{1}{10}$ of a degree of Fahrenheit.

Degrees

Degrees.

The heat imparted by the combustion			
of half a dram of wax was	-		24.2
By the combustion of half a dram of			
tallow	-	-	24.
By the combustion of half a dram of			
the best lamp oil	-		22.3
By the combustion of half a dram of			
charcoal	-	-	17.1

When equal quantities of pure air were altered by the combustion of wax and charcoal, and by the respiration of an animal, the quantities of heat communicated to 31 pounds seven ounces of water were as follows.

Degrees.

100 ounce measures of pure air, altered	{	by the combustion of wax, imparted	21.
		by the combustion of charcoal	19.3
		by the respiration of a	
		Guinea pig	17.3

From the foregoing experiments it appears, that the quantities of air altered being equal,

equal, more heat is produced by the combustion of wax than by that of charcoal, and more by the combustion of charcoal than by the respiration of an animal. These differences probably arise from the following causes: in the first instance, a considerable part of the air is converted into water, and more heat is produced by the conversion of pure air into that fluid than into fixed air. In the last instance, a part of the heat is carried off by the insensible perspiration.

The results, however, approach so nearly to each other as to prove that the heat, in these processes, arises principally, if not entirely, from the conversion of pure air into fixed air, or into water.

Upon the whole we have sufficient evidence for concluding, that when an animal is placed in a warm medium, the colour of the venous blood approaches more nearly to that of the arterial than when it is placed in a cold medium; that the quantity of respirable air, which the animal phlogistifies in a given time, in the former instance, is less than that which it phlogistifies, during an equal space of time, in the latter; and that the quantity of heat produced, when a given
portion

portion of pure air is altered by the respiration of an animal, is nearly equal to that which is produced, when the same quantity of air is altered by the combustion of wax or charcoal.

SECTION III.

From 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 received into the lungs of animals, contains more absolute heat than that which is exhaled in expiration. It has been shown, particularly, that nearly $\frac{1}{6}$ of the pure air which is altered by the process of respiration, is converted into aqueous vapour, and the remaining $\frac{5}{6}$ into fixed air; that when a given quantity of pure air is changed into fixed air, in phlogistic processes, the weight of the latter exceeds that of the former by $\frac{1}{5}$ of the whole; and that, when the quantities of matter in these fluids are equal, the comparative quantities of heat are as 4 to 1; from which it appears, that the quantity of matter in the fixed air being to that in the pure air as 5 to 4, the quantity of heat in the latter will

will be to that in the former as 3 to 1 nearly ; and it has been proved, that this is likewise the ratio of the heat of pure air to that of aqueous vapour.

Since, therefore, the fixed air and aqueous vapour, which are exhaled by expiration, are found to contain only the $\frac{1}{3}$ part of the heat which was contained in the purer portion of the atmospherical air, previously to inspiration, it follows that the latter must necessarily give off a considerable proportion of its absolute heat in the lungs.

It has moreover been shown, that the comparative 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 through 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.

The truth of this conclusion will perhaps appear in a clearer light from the following calculation, by which we may form an estimate of the quantity of heat yielded by

the purer part of atmospherical air, when it is converted into fixed air, and aqueous vapour; and also of that which is absorbed during the conversion of venous into arterial blood.

We have seen, that the comparative heat of pure air is to that of the fixed air and aqueous vapour, into which it is changed in the lungs, as 3 to 1. The same heat, therefore, which raises the pure air one degree, will raise the fixed air and aqueous vapour three degrees; and, consequently, the same heat which raises pure air, any given number of degrees, will raise the fixed air and aqueous vapour the same number of degrees multiplied by three.

It has been proved that pure air, when at the common temperature of the atmosphere, contains 1550 degrees of heat. Hence if a certain quantity of pure air, not in contact with any body that would immediately carry off the heat, should suddenly be converted into fixed air and aqueous vapour, the heat which was contained in the former would raise the latter 1550 degrees multiplied by three, or 4650 degrees; and the temperature of red hot iron being 1050, it follows that
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the quantity of heat, which is yielded by pure air, when it is converted into fixed air and aqueous vapour, is such (if it were not dissipated) as would raise the air and vapour, so changed, to more than four times the excess of the heat of red hot iron above the common temperature of the atmosphere.

If, therefore, the absolute heat which is disengaged from the air in respiration, were not absorbed by 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, 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 of 20 to 23; but we know that venous blood contains at least 1580 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 1580 degrees, or 1373 degrees;

and consequently, the sensible heat would suffer a diminution equal to the difference between 1580 and 1373, or nearly 200 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 200 degrees, or it would fall from 96 to 104 below the zero of Fahrenheit's scale.

That animal heat depends upon the separation of elementary fire from the air in the lungs, is moreover supported by the experiments which have been brought in proof of the third and fourth Propositions.

For from the experiments which have been adduced in proof of the third Proposition, it appears, that when bodies are united with the inflammable principle, they part with a portion of their elementary fire; and that when this principle is again disengaged, they re-absorb an equal portion of fire from the surrounding bodies. It moreover appears, from the experiments which have been adduced in support of the fourth Proposition,

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compared with the discoveries of Dr. Priestley and Mr. Cavendish, that when pure air is combined with the inflammable principle, in the state of light inflammable air, it is changed into aqueous vapour; that when it is combined with this principle in the state of heavy inflammable air, it is converted into fixed air; that wax, oil, and tallow consist chiefly of heavy and of light inflammable air in a solid state; that when pure air is altered by the combustion of these substances, fixed air and aqueous vapour are produced, and that the same products are obtained when that fluid is altered by the respiration of an animal. Hence it clearly follows, that, in the respiration of animals, as well as in the combustion of wax, oil, and tallow, the pure air is altered in its properties by its combination with the inflammable principle; and since we know that the union of these elements is universally accompanied with the extrication of heat, and particularly that, by this union, a large quantity of elementary fire is disengaged from the air in the combustion of oleaginous substances, we may conclude that, in the process of respiration, a similar extrication of fire takes place. In

this conclusion we are confirmed by the results of Experiments VIII. IX. X. and XI. Prop. IV. from which it appears, that when equal quantities of air are changed by the respiration of an animal, and by the combustion of a wax taper, the quantities of sensible heat produced are nearly equal.

Again, it is proved by Experiments I. and II. Prop. II. p. 273, compared with the observations in page 84, that the change which the blood undergoes, during its circulation through the lungs, is similar to that which solid bodies undergo when they are melted: that is, the capacities are in both instances increased; and since we know that, when solids are melted, a quantity of heat is absorbed, we may conclude that when venous blood is changed into arterial, a similar absorption takes place.

In the last place, it is evinced by Experiments I. II. III. IV. V. VI. and VII. Prop. IV. that when an animal is placed in a cold medium, the venous blood acquires a deeper hue, and that a greater quantity of air is altered by the process of respiration in a given time, than when it is placed in a warm medium. Now it has been proved by the
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very ingenious experiments of Dr. Priestley, that the livid colour of the blood in the veins, depends upon its union with the inflammable principle; from which it follows, that the blood is more deeply impregnated with this principle, that it furnishes a more copious supply of it to the air in the lungs; that more air is phlogisticated in a given time; and that a greater quantity of elementary fire is absorbed, in proportion as the animal is surrounded by a medium which has a lower temperature.

Hence it appears, that the quantity of heat which is separated from the air, and absorbed by the blood, is, in all cases, proportioned to the necessity. Since, therefore, it has been proved that elementary fire is absorbed from the air in the process of respiration, and since the quantity that is thus absorbed is not only adequate to the effect which we have been endeavouring to explain, but also proportional to it, we may safely conclude that it is the true cause of animal heat.

From the foregoing experiments and observations it follows, that animal heat depends upon a process resembling a chemical elective attraction, The pure air is received into the

6 lungs

lungs containing a great quantity of elementary fire; the blood is returned from the extremities impregnated with the inflammable principle; the attraction of pure air to the latter principle is greater than that of the blood. This principle will therefore leave the blood to combine with the air; by this combination the air is obliged to deposit a part of its elementary fire; and as the capacity of the blood is at the same moment increased, it will instantly absorb that portion of fire which had been detached from the air. The arterial blood, in its passage through the capillary vessels, is again impregnated with the inflammable principle, in consequence of which its capacity for heat is diminished, as appears from the experiments which have been recited in proof of the second Proposition. It will, therefore, in the course of the circulation, gradually give out the 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 the inflammable principle, and absorbing heat; and that in the course of the circulation, it is continually imbibing this principle, and emitting heat.

Though

Though I have endeavoured to show that the process by which heat is separated from the air, and absorbed by the blood in the lungs of animals, is analogous to a chemical elective attraction, yet I do not mean to assert that elementary fire is really capable of being chemically combined with bodies; much less do I affirm, as some have supposed, that it is by means of a double elective attraction, that heat is disengaged from the air in the process of respiration. Before this can be admitted, it must be proved that heat is a substance; and I do not know that any experiments have hitherto been published which demonstrate the materiality of that principle. Indeed this opinion is not at all essential to the truth of the conclusions contained in the preceding pages; for these conclusions have been deduced from facts established by experiment, and which I think must be admitted, whatever be the hypothesis which we adopt concerning the nature of heat.

Thus, whether we consider the sensible heat of bodies as depending upon a peculiar fluid, or whether we consider it as an effect produced by a force inherent in bodies, which
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has by some been called the igneous force, or whether we consider *cold* as arising from the action of an inherent force, and *heat* as depending upon the diminution of this force; still, the general properties of heat recited in the foregoing pages must, I think, be admitted, because they are deduced from experiment, and from the testimony of sense. It must, for example, be granted, that the quantity of the element of fire, or of the igneous force in bodies may be increased or diminished by their communication with neighbouring bodies, which have a different temperature; that unequal quantities of fire are required in order to produce the same degree of sensible heat in equal weights of heterogenous bodies; and that a change of form in the same body will produce a variation in the quantity of fire which is necessary to raise it to a given temperature. It is upon these general properties, and not upon any hypothesis respecting the nature of heat, that the explanation which has been now given of animal heat is founded. By means of those properties the comparative quantities of that principle contained in various bodies, and particularly, in respirable air, and in the blood

blood of animals, have been estimated; and if it be admitted as a certain truth, that a *diminution* is produced in the quantity of heat contained in the air, and an *encrease* in that contained in the blood, by the process of respiration, it will not, I apprehend, be denied that, in this process, heat is continually passing from the air to the blood.

It may be proper to add, that, as the blood by its impregnation with the inflammable principle, has its capacity for heat diminished, so, on the contrary, those parts of the system, from which it receives this principle, will have their capacity for 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 elementary fire separated from the air 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

proved by experiment, that a quantity of elementary fire is communicated to that fluid in the lungs, and is again disengaged from it in its progress through the system. If, therefore, the whole of the fire, which is separated from the blood, were absorbed by those parts of the system from which it receives the inflammable principle, 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 *.

That a part of the fire which is separated from the blood, in the course of the circulation, becomes redundant, is moreover supported by a very extensive analogy. For the process by which the arterial blood is phlogisticated during its passage through the minute vessels, is similar to that by which pure air is phlogisticated in the combustion of oleaginous substances. In the latter process, the inflammable principle is separated from an earthy basis, and combined with the air;

* Vide Newtoni Princip. lib. iii. p. 202.

in the former, it is separated from the putrescent parts of the system, and combined with the blood. In both cases, the capacity of the body which parts with the inflammable principle is increased, and that of the body which receives it is diminished; and since in the combustion of oleaginous substances, the changes which take place in the capacity of the body which receives this principle, and of that which parts with it, bear such a proportion to each other, that a quantity of heat becomes redundant, we may conclude, that in the circulation of the blood through the minute vessels, a similar effect is produced.

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

To this cause it is probably owing, that less heat is produced by an animal, than by the burning of a wax taper, the quantities of air consumed being equal; for it is likely, that the putrescent juices, as soon as they have communicated the inflammable principle to the blood, fly off in the form of insensible

fenfible perfpiration, and carry along with them a part of the fire which is detached in the courfe of the circulation.

From the quantity of heat, however, which becomes fenfible in the animal body, it is manifeft, that the portion of fire which is thus abforbed, is not very confiderable.

It appears, therefore, that the blood, in its progress through the fyftem, gives out the elementary fire which it had received from the air in the lungs; a fmall portion of this fire is abforbed by thofe particles which impart the inflammable principle to the blood; the reft becomes redundant, or is converted into moving and fenfible heat.

Of the Inflammation of combuftible Bodies.

From the above experiments we learn, that atmofpherical air contains much elementary fire or abfolute heat; that when it is converted into fixed air and aqueous vapour, the greater part of this heat is detached; that the capacities of bodies for containing heat are increafed by the changes which they undergo in the procefs of combuftion. Hence we infer, that the heat which is produced by combuftion, is
derived

derived from the air, and not from the inflammable body.

For inflammable bodies contain little absolute heat; atmospherical air, on the contrary, abounds with this principle. In the process of inflammation, the air is converted into fixed air and aqueous vapour, 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 four times the excess of the heat of red hot iron above the common temperature of the atmosphere. 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, 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 through 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 whence does it receive this heat? It cannot receive it from the iron; for the quantity of heat in the calx is more than double of that which was contained in the iron, previously to the calcination.

But in the inflammation of iron, the atmospherical air, with which it is in contact, is altered in its properties, 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

tion, depends upon the separation of absolute heat from the air.

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 the purer part of atmospherical into fixed air, or into aqueous vapour, is invariably accompanied with the production of sensible heat. Thus sensible heat is produced when pure air is exploded with inflammable air, when it is diminished and rendered noxious by putrefaction, by the combustion of oleaginous substances, 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 of the

arterial blood, and thus 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.

It has been supposed by some Philosophers, and particularly by Mr. De Luc and Mr. Lavoisier, that the heat which arises from the inflammation of combustible bodies, does not depend upon a change of capacity in the air, but upon a chemical decomposition.

These Philosophers take it for granted that elementary fire is a substance. They divide it into two kinds, fixed and free fire. The latter is that portion of the element of fire which is known to exist, in all parts of nature, in a moving and sensible state, and which is continually disposed to flow from warmer into colder bodies; and fixed fire is a portion of the same element, which the various classes of natural bodies are conceived to retain in a state of chemical union, according to their several attractions. The fire which is thus chemically combined with bodies, is supposed to lose its distinguishing properties.

properties. Agreeably to this it is conceived that, during the combustion of inflammable and dephlogisticated air, the fire which was chemically united to these substances, and was retained by them in a fixed and insensible state, is, in consequence of their union, extricated by a superior attraction, when it acquires the form of free and moveable fire.

Whether the production of heat, in this process, arises from a change of capacity, or from a chemical decomposition, can only be determined by an appeal to facts.

I do not know that any experiment has hitherto been made, which shows that elementary fire can be chemically combined with bodies, in such a manner as to lose its distinguishing properties. In order to establish this as a fact, it would be necessary to prove that, when bodies produce sensible heat by a change of form, their capacities are either undiminished, or that the diminutions of capacity, are not in proportion to the quantities of heat produced. Messrs. Lavoisier and De la Place have endeavoured to show that, in some instances, the quantities of heat evolved, and the changes of capacity, do not correspond with each other; but they

admit that the apparent deviations from this law, were not greater than such as might have arisen from unavoidable errors in their experiments. I have made many experiments with a view to determine this question, and I have uniformly found that, when bodies produced heat in consequence of a change of form, their capacities were diminished, and, on the contrary, that when they produced cold, their capacities were increased. But I have not yet been enabled to bring my experiments to such a degree of accuracy, as to determine, with certainty, whether the increase and diminution of capacity were precisely proportional to the quantities of heat and cold produced. The results, however, were by no means inconsistent with that law, and did not deviate from it more than might naturally be expected, from the inaccuracies to which such experiments are liable. It is possible, as has been suggested by Mr. De Luc, that when a body suffers an increase of capacity, a quantity of elementary fire may be chemically combined with it, and that when the capacity of the body is again diminished, the heat which is evolved may partly depend upon the diminution of
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its capacity, and partly upon a chemical decomposition.

In order to determine whether any part of the heat which is absorbed by bodies, when their capacities are increased, enters into a chemical union with them, a series of accurate experiments should be instituted, with a view to discover the lowest degree of heat, according to the method which was first proposed by Dr. Irvine, and which has been briefly stated to the public by Mr. Magellan, and Messrs. Lavoisier and De la Place.

If it be found that the lowest degree of heat, as determined by these experiments, is constant, it will follow that heat does not, in any case, enter into a chemical union with bodies; and, consequently, that the sensible heat and cold which they produce, when they undergo a change of form, arise solely from an alteration of capacity; but if the lowest degree of heat be not constant, we may infer that a part of the heat, which is absorbed by bodies, when their capacities are increased, enters into a chemical union with them, and loses its distinguishing properties.

This will be evident if we consider, that the determination of the lowest degree of heat, as calculated according to Dr. Irvin's rule, depends upon the relation which the diminution of the capacity bears to the increase of the sensible heat, or the reverse. Thus, in the foregoing experiments with pure and inflammable air, the diminution of capacity was 6.07, and the increase of the sensible heat was nearly 1300 degrees, whence the lowest degree of heat was found to be 1500 nearly; but if the increase of the sensible heat had been 2600 degrees, the diminution of capacity remaining the same, the lowest degree would have been nearly 3000. Hence it is manifest that, if in *some* bodies, the augmentations of temperature, which are produced by an alteration of form, arise partly from a diminution of capacity, and partly from the extrication of a portion of heat by a chemical decomposition, and if, in *other* bodies, the augmentations of temperature arise solely from a diminution of capacity; the zero or lowest degree of heat, as calculated from the alterations of temperature and of capacity in these bodies, will vary: for, in the former instance, it will appear

appear to be farther removed from the common temperature of the atmosphere, than in the latter. It is, therefore, evident that the question respecting the chemical combination of fire will be resolved, if a series of accurate experiments be instituted, in order to discover the changes of capacity, which arise from alterations of form in different bodies, together with the quantities of heat and cold produced by those alterations *. In the mean time we may observe that the extrication of heat from bodies, when they pass from the state of air or of vapour into a liquid, or a solid state, and the absorption of it when they undergo a contrary change, have been proved by experiment, to depend partly upon an alteration of capacity. It is, therefore, more agreeable to the simplicity of nature to conclude that the phænomena arise solely from that cause.

In the course of the foregoing experiments it has been shown, that given quantities of fire, or of absolute heat, produce unequal alterations of temperature in equal weights of heterogeneous bodies; and that the quan-

* The reader will find this subject more fully discussed in the appendix;

tities of heat required to produce a given alteration of temperature in the same body, while its form remains unchanged, is nearly uniform; from which it follows, that the element of fire must be variously distributed among the different classes of natural bodies, each body requiring more fire or absolute heat, to raise it from the point of total privation to the common temperature of the atmosphere, in proportion as its temperature is less augmented by the addition of a given quantity of that principle.

Dr. Irvine has proved, that when bodies undergo a change of form, their capacities for heat are, in various instances, increased or diminished: in other words, when the form of a body is changed, a greater or less alteration will be produced in its temperature, by a given quantity of fire or of absolute heat, than would have been produced, if the same quantity of fire had been applied to it, previously to the change of form; and hence an alteration in the forms of bodies must necessarily be accompanied with an extrication or absorption of heat.

From the foregoing experiments it appears that, by the union of dephlogisticated
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and inflammable air, the capacities of these fluids are greatly diminished. Whence it necessarily follows, that this process must be accompanied with the production of much sensible heat. For since the fluids have their capacities diminished, at the moment of their union, a greater increase will be produced in their temperature, by the application of a given quantity of fire, than would have been produced, if the same quantity of fire had been applied to them previously to their union; and consequently the same quantity of fire which, previously to their union, was sufficient to raise them, from the point of total privation, to the common temperature of the atmosphere, will now be sufficient to raise them to a much higher temperature.

In like manner as if a cylindrical vessel, containing water, have its width suddenly diminished, the water will necessarily rise to a greater height in the vessel.

In attempting, therefore, to account for the heat produced by the union of inflammable and dephlogisticated air, we have discovered a cause which is proved by experiment to exist, and which is found to operate in all other similar instances, as far as our observations

observations extend. The heat produced by the process in question is, consequently, resolved into a general law of nature; and this law seems to afford an adequate explanation of the phænomena.

Since, therefore, the existence of a cause, sufficient to account for the appearances, has been established by experiment, it seems inconsistent with the simplicity of the operations of nature, to suppose that other causes, for the existence of which there is no experimental proof, concur in the production of the same effects; for nature does nothing in vain, & *frustra fit per plura quod fieri potest per pauciora* *.

Hence we may conclude, in general, that the evolution of heat from the air, in consequence of an alteration in its capacity, is the true cause of animal heat, and of that which is produced by the inflammation of combustible bodies.

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

* Vide Newtoni Princip. lib. iii. p. 357.

SECTION IV.

Of the principal Facts relating to Animal Heat.

1. The above doctrine explains the reason why the breathing animals have a higher temperature than those who 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 a change in its properties.

The quantity of air changed by a man in a minute, is found, by experiment, to be equal to that which is altered 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, 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.

The blood during its passage through the capillary vessels, has its capacity for heat diminished, and by this diminution 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 interesting and decisive experiments which were made in heated rooms, by Dr. Fordyce, it appears, that the animal body has, in certain situations, the power
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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 of Dr. Fordyce prove that the phenomenon does not arise solely from that cause. For the doctor, remained in moist air, heated to 130 during the space of 15 minutes, when the thermometer under his tongue stood at 100; and streams of moisture ran down over his whole body, proceeding from the condensation of vapour, as evidently appeared from a similar condensation on the surface of a Florentine flask that had been filled with water at 100.

He, moreover, found, in the course of his experiments, that a dog could live without much inconvenience for a considerable time in air heated to 260, the body of the animal preserving very nearly its proper temperature, being never raised more than two degrees above the natural standard.

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From the former of these experiments it appears, that the animal body possesses the power of producing cold, in a moist as well as in a dry air; and it is manifest that, in the latter experiment, the refrigeration was much greater than that which would have been produced by an equal mass of inanimate matter, containing an equal quantity of moisture. Whence it follows that, in living animals, the faculty of generating cold does not arise solely from evaporation.

This conclusion is moreover confirmed by the experiments which follow:

A living and dead frog equally moist, and of nearly the same bulk, the former of which was at 67, the latter at 68, were laid upon flannel in air which had been raised to 106.

Two sensible thermometers were placed in contact with the skin of the animals under the axillæ, and the order of cooling was observed during 25 minutes.

	Minute.	Air.	Dead Frog.	Living Frog.
In	1	—	$70\frac{1}{2}$	$67\frac{1}{2}$
	2	102	72	68
	3	100	$72\frac{1}{2}$	$69\frac{1}{2}$
	4	100	$72\frac{1}{2}$	$69\frac{1}{2}$
	25	95	$81\frac{1}{4}$	$78\frac{1}{4}$

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The thermometers being introduced into the stomach, the internal heat was found to be the same with that at the surface.

Hence it appears, that the living frog acquired heat more slowly than the dead one. Its vital powers must, therefore, have been active in the generation of cold.

To discover whether the cold produced in this instance, depended solely upon the evaporation from the surface increased by the energy of the vital principle, a living and dead frog were taken at 75, and were immersed in water at 93, the living frog being placed in such a situation as not to interrupt respiration.

Minute.	Dead Frog.	Living Frog.
In 1	85	81
2	$88\frac{1}{2}$	85
3	$90\frac{1}{2}$	87
5	$91\frac{1}{2}$	89
6	$91\frac{1}{2}$	89
8	$91\frac{1}{2}$	89

Since, therefore, living frogs have the faculty of producing cold in warm water, as well as in warm air; and since the human body has the same power in a moist as well as in a dry air, we may conclude that this

power does not depend solely upon evaporation.

I shall now endeavour, from the preceding facts, to explain what appear to me to be the true causes of the cold produced by animals, when placed in a medium, the temperature of which is above the standard of their natural heat.

From the experiments on venous and arterial blood, and from the calculation in page 357, 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, or from the parts adjacent, it would sink 200 degrees. Hence, if the evaporation from the lungs be so much augmented 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 that are in contact with it, and from the neighbouring parts. The heat which is thus absorbed in the greater vessels will again be extricated in the capillaries, where

where the blood receives a fresh addition of the inflammable principle. If, in these circumstances, the blood during each revolution were to be equally impregnated with this latter principle, it is manifest that the whole effect of the above process would be to cool the system at the centre, and to convey the heat to that part of the body where it is capable of being instantly carried off by evaporation.

But it appears from Experiment VII. Prop. V. that when an animal is placed in a heated medium, the sanguineous mass during each revolution is less impregnated with the inflammable principle; for the venous blood, in these circumstances, becomes gradually paler and paler in its colour, till at length it acquires nearly the appearance of the arterial; and it is proved, by the experiments of Dr. Priestley, that the dark and livid colour of the blood in the veins, depends upon its combination with the inflammable principle in the minute vessels. Since, therefore, in a heated medium, this fluid does not assume the same livid hue, we may conclude that it does not attract an equal quantity of the phlogistic principle;

and in this conclusion we are confirmed by Experiments I. II. III. IV. V. and VI. Prop. V. which prove that the quantity of air which an animal phlogistifies, in a given time, when it is placed in a warm medium, is less than that which it phlogistifies in an equal space of time when it is placed in a cold medium.

It follows, from these facts, that the quantity of heat given off by the blood in the capillaries, will not be equal to that which it had absorbed in the greater vessels, or positive cold will be produced. If the blood, for example, in its passage to the capillaries, absorb from the greater vessels a quantity of heat as 200, and if, in consequence of its receiving a less impregnation of the phlogistic principle than formerly, it give off at the extreme vessels a quantity of heat only as 150, it is manifest that upon the whole a degree of refrigeration will be produced as 50: and this cause of refrigeration will continue to act, while the colour of the venous blood is gradually approaching to that of the arterial, till its approximation becomes the nearest possible in the circumstances in which the animal is placed, after
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which it will cease to operate. Thus it appears that, when animals are placed in a warm medium, the same process which formerly supplied them with heat, becomes for a time the instrument of producing cold, and probably preserves them from such rapid alterations of temperature as might prove fatal to life.

Upon the whole, the increased evaporation from the surface, and the diminution of that power by which the blood, in its natural state, is impregnated with the phlogistic principle, seem to be the great causes on which the refrigeration depends.

By the first of these causes the animal is cooled at the surface, and by the second an accumulation of heat at the centre is obviated.

The power which animals possess of cooling themselves at the centre, when they are placed in a medium above the standard of their natural heat, seems to be peculiarly necessary to the preservation of life. For as the heat which is received at the surface, in those circumstances, is speedily carried towards the internal parts by the reflux of the heated fluids; if it were not absorbed and

rendered insensible by the change which the blood undergoes in the lungs, the central heat would probably soon be increased to such a degree as to destroy the vital principle.

3. We may hence perceive the reason why animals preserve an equal temperature, notwithstanding the great variations that take place in the heat of the atmosphere, arising from the vicissitudes of the weather, and the difference of season and climate.

From the experiments which have been recited in proof of the fourth Proposition, it appears that, in the process of respiration, the difference between the purity of the inhaled and expired air is increased by exposing the animal to cold; and likewise that the difference between the colour of the arterial and venous blood is augmented from the same cause. But the quantity of fire, separated from the air, must necessarily be in proportion to these differences. For as the extrication of heat from that element is produced by its phlogistication, it is manifest that the more compleatly a given portion of air is phlogisticated, the greater will be the quantity of heat detached from it. The same remark will apply to the phlogistication of the blood.

blood. It follows, that as soon as, by exposure to cold, an unusual dissipation of the vital heat is produced, the blood in the course of the circulation will begin to be more deeply impregnated with the phlogistic principle; it will, therefore, furnish a more copious supply of that principle to the air in the lungs, and will imbibe a greater quantity of fire in return. Add to this, that by the tonic and stimulant powers of cold, the vessels on the surface are constricted, the insensible perspiration is diminished, the blood is determined to the lungs, the pulse and respiration are rendered full and frequent, and consequently as the cold advances in winter seasons, and in northern climates, the vigour of the animal powers will be increased, and this will be accompanied with a proportional increase in the faculty of absorbing heat from the air.

In summer, on the contrary, the reverse of this will take place. The powers of life will be relaxed; the air will be less tainted by respiration; a smaller portion of the phlogistic principle will be attracted by the blood in the minute vessels; and the quantity of fire which is absorbed will be propor-

tionally diminished, at the same time that the evaporation from the surface is increased.

It appears, therefore, that the heating and cooling powers in the animal body are adjusted to each other in such a manner as to produce an equal effect; and, consequently, the law by which animals maintain an uniform temperature, notwithstanding the variations in the heat of the surrounding air, is similar to that by which the temperatures of bodies become fixed when they have arrived at the melting or freezing points. When freezing water, for example, is exposed to a cold atmosphere, the quantity of elementary fire carried off by the surrounding air, is precisely balanced by that which is extricated from the water at the moment of its congelation. In like manner, when a living animal is exposed to a cold atmosphere, the quantity of heat carried off by the circumambient medium, is balanced by that which is detached from the blood in the course of the circulation. On the contrary, when ice is placed in a warm atmosphere, the whole of the heat which it receives is rendered insensible; and it has been shown that, when an animal is placed in a warm
medium,

medium, a similar extinction of the heat takes place.

Hence the power which animals possess, of generating heat, is in all cases proportioned to the demand. It is increased by the winter colds, diminished by the summer heats; it is totally suspended, or converted into a contrary power, according as the exigencies of the animal may require.

4. 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
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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 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 through 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 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.

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

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
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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 of the system; in consequence of which, the air inhaled in inspiration, will be more copiously supplied with the inflammable principle, 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 this principle, 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 the inflammable principle. It is capable of being farther diminished by nitrous air, and not more than the eighth part of it
consists

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 the inflammable principle 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 the principle of inflammability 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.

5. 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 putre-

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

faction must be produced by the violent reaction, and by the stagnation of the ferous fluid which is sometimes effused into the adjoining cellular texture. It has been 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
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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.

6. I shall observe, in the last place, that the doctrine contained in the preceding pages, respecting the cause of animal heat, will probably lead to an explanation of the uses of the spleen and of the lymphatic glands.

May not the spleen be intended partly to apply a proper degree of warmth to the left side of the stomach, and partly to supply the liver with phlogisticated blood? As a quantity of fire is extricated during the conversion of arterial into venous blood, in the minute vessels, if the blood which is phlogisticated in the spleen had passed to the liver in the state of arterial blood, and had been changed into venous, in that organ, it seems evident

evident that there would have been a redundancy of heat in the right hypochondriac region, and a deficiency of it in the left; and this effect would have been produced in a much greater degree, if the blood which comes from the intestines had likewise passed to the liver in the state of arterial blood. But in the present situation of things, the blood which flows through the celiac and mesenteric arteries, having communicated the heat which it had absorbed in the lungs to the intestines, and to the left side of the stomach, passes to the liver impregnated with the inflammable principle; in consequence of which it is not any longer in a condition to give off heat, and at the same time, by its union with the principle of inflammability, it is probably brought into that state in which it is best fitted for the secretion of the bile.

As to the lymphatic glands, there is reason to believe that the arterial blood, which is converted into venous, in those glands, attracts from the lymph a quantity of the inflammable principle, and communicates to it a portion of elementary fire. It is probable that the lymph, by this alteration, is rendered

rendered more fit for becoming one of the constituent parts of the blood.

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 that element. Hence it is evident, that when the air, in which an inflamed body is confined, is 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. As the sensible heat, which is produced by combustion, depends upon the separation of absolute heat from pure air, it is manifest that the greater quantity of pure air any species of ærial fluid contains, the longer it must contribute to the support of flame, as well as to the preservation of animal life. Agreeably to this we find that the quantity of pure air, contained in the

best dephlogisticated air, is nearly four times as great as in common air; and Dr. Priestley has shown that five times as much sensible heat is produced by the conversion of it into fixed 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 *.

3. As the sensible heat, in combustion, depends upon a change produced in the air, 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 dephlogisticated into fixed air, a quantity of heat is disengaged, which would be sufficient to raise the air so converted to more than four times the excess of the heat of red hot iron above the common temperature of the

* In phlogistic processes, a small portion of the pure air contained in atmospherical, is protected from the action of the inflammable principle, by the phlogisticated air which forms one of the constituent parts of the atmosphere. See p. 272.

atmosphere. 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. It will be accumulated upon the fuel, absorbed by the vapour, and communicated to the atmosphere. 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. Though it is highly probable that all inflammable bodies have their capacities for containing heat changed by the alterations which they undergo, in the processes of calcination and combustion, 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 1.9 to 1; that the capacity of the calx of antimony is to that of the regulus as 3.5 to 1; that the capacity of pure air is to that of fixed air as 4 to 1; 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 the inflammable principle are required to saturate different bodies. Some of the oleaginous substances abound more with this principle than others; and a greater quantity of it is required to saturate pure air than an equal weight of arterial blood.

From these facts it follows, that when the inflammable principle 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 inflammable principle is separated from the blood and combined 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 inflammable air is required to saturate pure air than an equal weight of arterial blood, the
quantity

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.

The cases in which this will happen may be determined in the following manner.

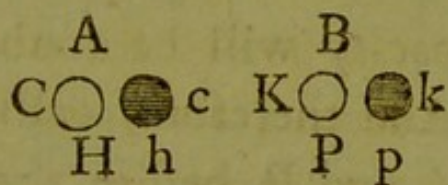
PROPOSITION I.

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

D d 3



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

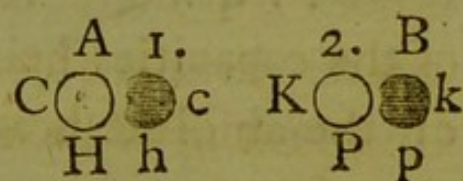
Since therefore $C - c : c : k : K - k$
 as $H - h : h : p : P - p$, by
 equality $C - c : K - k :: H - h : P - p$

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

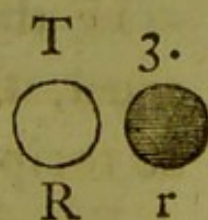
PROPOSITION II.

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

The same things re-
 maining as above, if
 the quantities of mat-
 ter 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 abso-
 lute heats be denoted by R and r, and the



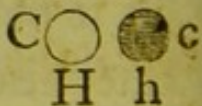
* In this and the following proposition, the bodies are
 supposed to be brought to the same common temperature
 before and after the change.

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 $H : h :: R : r$, and $H - h : h :: R - r : r$, 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 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 quantities of matter being given,  the differences of the absolute heats are directly as the differences of the capacities; and, by the second proposition, the differences

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

is separated from A will not be absorbed 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. The quantities of matter changed being supposed to be equal, since it is required, in order that neither heat nor cold should be produced, that $H-h$ should be equal to $P-p$, it follows that P must be equal to $H + p-h$.

If h be equal to p , that is, if $p-h=0$ then $P=H$. If h be greater than p , $p-h$ will be a negative quantity; on the contrary, if h be less than p , $p-h$ will be positive. In the former case, P will be less than H , and, in the latter, it will be greater than it.

This may be otherwise briefly expressed as follows: the quantities of matter changed being equal, let the capacity of A, after the change, and that of B previously to it, be called the *least* capacities of these substances,

and let the capacity of the former previously to the change, and that of the latter subsequent to it, be called their *greatest* capacities.

Since the diminution in the capacity of A is equal to the increase in that of B, it is manifest, that if the *least* capacities be equal, the *greatest* capacities will likewise be equal. But if the *least* capacities be unequal, this inequality must be balanced by a corresponding inequality in the *greatest* capacities; or in as much as the least capacity of A is greater or less than that of B, in so much must the greatest capacity of the former be greater or less than that of the latter; the difference of the least capacities being, in all cases, equal to the difference of the greatest.

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 in the blood and the air, are such, that the difference

ference 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 inflammable principle,
in

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 applicable to the inflammation of combustible bodies, appears from the following observations and experiments.

In the burning of iron, the metal is converted into a calx, and the purer part of the common air which supports the inflammation is absorbed, either in its simple state, or combined with phlogiston. By this process the capacity of the air for containing heat is diminished, and that of the calx is increased; and, therefore, from the first and third Corollaries, it follows, that if, in the inflammation of iron, equal quantities of air and iron were changed in a given time, and if the difference of the capacities of iron and of its calx, were equal to the difference of the capacities of the air before and after the change which is produced upon it by its union with the metal, the whole heat extricated

cated from the air would be absorbed by the calx.

The alteration of capacity which the air undergoes by its union with the metal may be determined as follows.

It has been proved that the quantity of absolute heat in the calx of iron when saturated with air, is to that contained in the same calx when the air is in a great measure separated by nitrous acid and fire, as 6 to 4. We shall suppose the calx, when saturated with air, to contain $\frac{1}{3}$ of its weight of that fluid. If we conceive the whole heat in the aerated calx to be divided into six equal parts, the absolute heat of that substance, after the separation of the air, will be to its heat, previously to the separation, as 2.6666 to 6. For if the calx, after the separation of the air, were equal in quantity to the aerated calx, the heat of the former would be to that of the latter as 4 to 6; but as it is only equal to two thirds of the aerated calx, it will be as 2.6666 to 6.

Let the difference between the heat of the aerated calx, and of that substance when freed from air, be supposed to give the heat which was contained in the air during its combination

nation with the metal *. It will follow that this heat will be represented by the difference between 2.6666 and 6. That is, the heat contained in the air, will be to that contained in the aerated calx, as 3.3334 to 6, the quantity of matter in the air being one third of that in the calx. And, therefore, taking equal quantities of the air and the calx, the heat of the former will be to that of the latter as 3.3334 multiplied by three, or as 10 to 6 very nearly, or as 1.6666 to 1.

It has been already shown that the heat of the calx is to that of water as 1 to 4; therefore the heat of the combined air is to that of water as 1 to 2.4; but the heat of pure air is to that of water as 4.75 to 1, therefore the heat of the pure air is to that

* In this supposition it is taken for granted that the capacity of the calx is not altered by its combination with the air. It is probable, however, that it is somewhat increased. If this be admitted, it will follow that the quantity of heat contained in the air is less than that which is stated above; but this will not affect the calculation of the heat which becomes sensible during the combustion of iron; because the error in the statement of the heat contained in the combined air will be balanced by a contrary error in the statement of the heat contained in the calx.

of

of the combined air as 11.1 to 1, or as 36.4 to 3.28, and the heat of the combined air is to that of the iron as 3.28 to 1. The capacity of the calx of iron is to that of iron as 2 to 1 nearly.

Calling, therefore, the capacity of pure air in its elastic form 36.4; and that of the combined air 3.28, it appears from Cor. 1. Prop. III. that if the whole heat separated from the air were absorbed by the calx, the quantity of iron calcined would be to the quantity of air which is absorbed during the calcination, as the difference of the capacities of pure and combined air, is to the difference of the capacities of the iron and of the calx.

The quantity of air contained in the calx being $\frac{1}{3}$ of the whole, it follows that the quantity of iron which is altered by the process of calcination is to that of air as 2 to 1; and the difference of the capacities of pure and combined air is $36.4 - 3.28$, or 33.12. Hence, if the whole heat separated from the air were absorbed, it would be as $2 : 1 :: 33.12$ to the difference of the capacities of the iron and the calx. That is, the excess of the capacity of the calx above that of the iron would be 16.56; and the
quantity

quantity of heat in the iron being expressed by unity, the capacity of the calx would be to that of the iron as 17.56 to 1; but it is as 2 to 1. Hence the heat which becomes redundant during the calcination of iron, is to the original heat of the iron as 15.56 to 1; and it is to that of the calx as 15.56 to 2, or as 7.78 to 1.

It was before shown, that bodies, when at the common temperature of the atmosphere, contain at least 1500 degrees of heat. During the inflammation of iron, therefore, a quantity of heat becomes redundant, which would be sufficient to raise the calx 1500 degrees multiplied by 7.78, or 11670 degrees.

Hence we may account for the spark which is produced by the percussion of flint and steel. A particle of the metal is struck off by the force of the flint. This particle is altered in such a manner as to be rendered capable of absorbing a portion of air. The air, at the same moment, parts with a quantity of elementary fire, which raises the particle to a red heat, and thus the spark is produced.

It may be difficult to determine the peculiar mode in which the stroke of the flint operates upon the particle of the metal, so as to render it capable of combining with air. But the fact is certain, for when a quantity of the metal, calcined by this process, is collected, the greater part of it is found to consist of finery cinder, or of *æthiops martialis*, which is known to contain $\frac{1}{3}$ of its weight of air, the remaining portion being apparently converted into glass.

It is manifest, from the foregoing calculation, that the air, which is thus combined with the portion of the metal that is struck off, must, at the moment of its absorption, deposit a quantity of fire, which would be sufficient to raise the calcined particle 11670 degrees.

That the phenomenon derives its origin from this source, is confirmed by experiments which were made on that subject by Mr. Lane; from which it appears that a spark is not produced by the percussion of flint and steel in a vacuum. A very small quantity of pure air is, however, sufficient to produce this effect; for Mr. Kirwan has found that a spark is excited by the percussion

of flint and steel under common spring water.

It appears, upon the whole, that the most important facts relating to the inflammation of combustible bodies, may readily be explained upon the principle that the heat which is excited in combustion is derived from the air.

A part of this heat is absorbed by the elements into which the combustible body is resolved, and the rest becomes redundant, or is converted into sensible heat; it follows, that the quantity of fire which is extricated will be in proportion to the quantity of air which is altered in a given time; and that the degree of sensible heat which is produced will be greater or less, in proportion as a less or a greater quantity of this fire is absorbed by the body which supports the inflammation.

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 vapour; and, therefore, alcohol and sulphur burn with a pale and weak flame. On the other hand, those inflammable bodies which pro-

duce 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 fact that the capacity of pure air for containing heat is diminished by the respiration of animals, and by the action of inflammable bodies.

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

It is probable that the vapour of 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. Hence we may account for the spontaneous accension of a mixture of oil of turpentine and nitrous acid.

When the nitrous acid is mixed with oil of turpentine, the inflammable principle is separated from the oil and combined with the acid; the latter is forced to give out a
portion

portion of its absolute heat, part of which is absorbed by the basis of the oil, and the rest becomes redundant or appears in the form of sensible heat. If the sensible heat be increased to a certain degree, the acid will be converted into vapour, and the latter will suddenly combine with the inflammable principle of the oil, 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. This explosion happens in a vacuum as well as in the open air, because the nitrous acid vapour is capable of supporting flame.

In the deflagration of nitre, the acid is converted into vapour; which being at the same moment united to the inflammable principle of the coal, the fire is instantly disengaged, an elastic fluid is suddenly expanded, and a loud explosion produced.

Upon the same principle we may account for the heat which arises from the mixture of pure and nitrous air; the capacities of these fluids being diminished by their combination.

I endeavoured to determine the quantity of heat produced by mixing nitrous and dephlogisticated air together, in the following

manner. Three pipes were inserted in the brass vessel G R. See plate III. fig. 3. The first of these pipes communicated with an air pump. To the extremity of the second was screwed a bladder containing dephlogisticated air; and to that of the third, a bladder containing nitrous air, the communication between the air in the bladders and that in the brass vessel being interrupted by stop cocks. The brass vessel was then fixed in the tinned vessel A D (see fig. 4,) and was surrounded with water; after which the common air was exhausted by the air pump, and the nitrous and dephlogisticated air were suffered to enter. From several trials it appeared that the heat produced by mixing two parts of nitrous with one of dephlogisticated air, was nearly half as great as that which was produced by burning dephlogisticated and inflammable air together in the same proportions. At the conclusion of the experiments with nitrous and dephlogisticated air, it was found that an elastic fluid remained in the vessel, which issued in the form of smoke, and which smelled strongly of the nitrous acid. It is probable that a considerable quantity of heat was contained in this vapour,
which

which explains the reason why less heat is produced by the union of these fluids, than by that of dephlogistigated and inflammable air.

This doctrine likewise affords an explanation of the heat which is produced by the electric spark. For it has been proved by Dr. Priestley that the electric spark diminishes common air, and renders it incapable of supporting flame; and the air, when it undergoes this change, is found universally to give out a portion of its absolute heat. It is not probable, however, that the heat which is excited by the electric spark entirely originates from this source, for the spark gives a considerable impulse to the body into which it enters, and we know that percussion will produce heat in a vacuum. 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 is thus produced be explained in the following manner?

When iron filings and brimstone are mixed together and moistened with water, the changes produced in these substances are similar to those which take place when they are burned in the open air. The sulphur is converted into an acid, and the iron into a calx. The calx and acid are at the same time combined with a quantity of air or water, by which their weights are considerably augmented. The substance which is thus united to the calx and acid, must either be derived from the common air which is diffused through the pores of the earth, or from the water with which the iron and sulphur were mixed. The latter appears the most probable, for the quantity of common air with which the iron and sulphur are in contact is so small that it does not seem sufficient to supply them with the fluid which they attract during their union.

When, therefore, sulphur and iron are mixed together, and made into a paste with water, it is probable that a part of the water

is combined with these substances *. This combination must necessarily be accompanied with the extrication of a great quantity of sensible heat; for the absolute heat contained in the fluid, which is united to the calx and to the acid, is much less than that contained in water in its uncombined state, as appears from a comparison of the quantity of heat in water, with that which exists in the vitriolic acid, and in the calces of metals. The portion of the water, therefore, which is united to the acid and to the calx, must have its capacity for heat greatly diminished.

Hence a large quantity of elementary fire will be extricated, and at the same moment the remaining part of the water will be converted into vapour, by the expansion of which 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

* See Mr. Kirwan's Essay on Phlogiston, p. 102.

water which is diffused through the earth's substance, or lodged in cavities beneath its surface? May they not suffer the same changes that they are found to undergo 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 much inflammable 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 combustible materials in an ignited state, by the exposure of which, the inflammable principle 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

buſtion 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 by the law of the evolution and abſorption of heat.

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 inflammable air, and are obliged to reſign a portion of their abſolute heat. In combuſtion, the inflammable air is diſjoined from its vegetable baſis, and is combined with the purer part of the common 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 inflammable principle is ſeparated from the blood and diſcharged by reſpiration, in conſequence of which a quantity of fire is abſorbed from the air, and heat is communicated to the animal kingdom.

* 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 action of light upon the stems and leaves of vegetables *; and if this effect be produced by the separation of the inflammable principle, 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 element of fire, by the medium of pure air, will be continually circulating through the animal and vegetable kingdoms.

It was before shown that the quantity of air which an animal phlogistifies in a warm medium is not so great as that which it phlogistifies when it is exposed to a cold medium. Whence it follows, that in the countries between the tropics, all other circumstances being equal, the air will be less contaminated by the respiration of animals, than in the temperate and frigid zones. And the difference between the injury done to the air by respiration in warm and in cold climates, will appear to be very considerable if we recollect, that the phlogistication of a given quantity of air, by an animal

* See Dr. Priestley's Experiments on Air.

at the temperature of 50, was found to be twice as great as that produced by the same animal, in an equal time, at the temperature of 100. In those parts of the earth, therefore, in which the contamination of the air is increased by the influence of heat upon *dead* animal substances, that element sustains less injury from the respiration of *living* animals.

We may add, that in the tropical countries the waste of pure air by putrefaction, is speedily repaired by a more luxuriant vegetation, and a greater exposure to light.

These appear to be the principal agents which the great Author of Nature has employed to maintain an uniform degree of purity in the atmosphere. It is possible, indeed, that many other causes may concur in producing the same effect; but it is evident, from the observations which have been made on the state of the air in different parts of the world, that the causes which purify and contaminate that element, as well as those which preserve the balance of heat in the animal kingdom, are adjusted to each other with admirable precision.

I shall

I shall observe, in the next place, that by the law of the evolution and absorption of heat, the Deity has guarded against sudden vicissitudes of heat and cold upon the surface of the earth.

For if heat were not evolved by the process of congelation, all the waters which were exposed to the influence of the external air, when its temperature was reduced below 32, would speedily become solid; and at the moment of congelation, the progress of cooling would be as rapid as it was before the air had arrived at the freezing point.

This is manifest from what was formerly observed respecting the times of the congelation of different fluids: it was shown, that if the velocities of the separation of heat were equal, the times of the congelation would be in proportion to the quantities of heat which the fluids gave off from an internal source in the freezing process. Whence it follows, that if no heat were evolved, the congelation would be instantaneous.

In the present state of things, as soon as the atmosphere is cooled below 32, the waters begin to freeze, and at the same time to evolve heat; in consequence of which,
 whatever

whatever may be the degree of cold in the external air, the freezing mass remains at 32, until the whole is congealed *. And as the quantity of heat extricated in the freezing of water is considerable, the progress of congelation, in large masses, is very slow.

Hence, in the northern and southern regions, upon the approach of winter, a quantity of elementary fire is extricated from the waters, proportional to the degree of cold that prevails in the atmosphere. Thus the severity of the frost is mitigated and its progress retarded; and it would seem that during this retardation of the cooling process, the various tribes of animals and vegetables which inhabit the circumpolar regions, gradually acquire the power of resisting its influence.

On the contrary, if, in the melting of ice, a quantity of heat were not absorbed and rendered insensible, that substance, when it

* That the absorption and extrication of heat in the melting and freezing of bodies, has a tendency to retard the progress of these processes is remarked by Mr. Wilkie in his Essay on Latent Heat.

The same doctrine is likewise taught by Dr. Black in his lectures.

was exposed to a medium warmer than 32, would speedily become fluid, and the progress of heating would be as rapid as if no alteration in its form had taken place. If things were thus constituted, the vast masses of ice and snow, which are collected in the frigid zones, would, upon the approach of summer, suddenly dissolve, and great inundations would annually overflow the regions near to the poles.

But, by the operation of the law of the absorption of heat, when the ice and snow, upon the return of spring, have arrived at 32, they begin to melt, and at the same time to imbibe heat. During this process a large quantity of elementary fire becomes insensible, in consequence of which the earth is slowly heated, and those gradual changes are produced which are essential to the preservation of the animal and vegetable kingdoms.

We may remark, in the last place, that this law not only resists sudden changes of temperature, but that it likewise contributes to a more equal distribution of the principle of heat, throughout the various parts of the earth, in different seasons and climates. Thus the diurnal heats are moderated by the
evaporation

evaporation of the waters on the earth's surface, a portion of the fire derived from the sun being absorbed and extinguished by the vapours at the moment of their ascent. On the approach of night the vapours are again condensed, and falling in the form of dew, communicate to the air and to the earth, the fire which they had imbibed during the day.

It was before shown, that, in the regions near to the poles, when the vernal and summer heats prevail, provision is made for tempering the severity of the winter colds; a quantity of elementary fire, upon the dissolution of the ice and snow, being absorbed by the waters, and deposited as it were in a great magazine, for the purpose of mitigating the intensity of the cold when the frost returns.

From the Experiments of Hales, Halley, and Watson, it appears, that vast quantities of water are continually converted into vapour, by the action of the solar rays upon that portion of the earth's surface which is exposed to the light. And by the celebrated discovery of Dr. Black, it is proved, that, in the process of evaporation, much elementary fire is absorbed. It is manifest, that this

cause will have a powerful influence in mitigating the intensity of the heat, in the torrid zone, and in promoting a more equal diffusion of it throughout the earth. For a considerable portion of the heat, which is excited by the action of the solar rays upon the earth's surface, within the tropics, is absorbed by the aqueous vapours, which being collected in the form of clouds, are spread like a canopy over the horizon, to defend the subjacent regions from the direct rays of the sun. A great quantity of elementary fire is thus rendered insensible in the torrid zone, and is carried by the dispersion of the vapours to the north and to the south, where it is gradually communicated to the earth when the vapours are condensed.

It was before observed, 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,

degrees, will raise *another*, a greater or a less number, according to the nature of the body to which it is applied. This fact seems to have been sufficiently verified by experiment; and, therefore, the consequences which have been deduced from it, do not appear to be connected with any hypothesis respecting 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 persuaded, that 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 absorbed in the processes of fusion, evaporation, combustion; 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.

Fire will be considered as a principle, which is distributed in various proportions throughout the different kingdoms of nature. The mode of its union with bodies, will resemble that particular species of chemical union, wherein the elements are combined by the joint forces of pressure and of attraction. Of this kind is the combination of fixed air and water; for fixed air is retained in water partly by its attraction to that fluid, and partly by the pressure of the external air; and, if either of these forces be diminished, a portion of the fixed air escapes. In like manner, it may be conceived, that elementary fire is retained in bodies, partly by its attraction to those bodies, and partly by the action of the surrounding heat; and, in that case, a portion of it will be disengaged either by diminishing the attractive force, or by lessening the temperature of the circum-

circumambient medium. If this be admitted, it will follow, that, by the alterations which the air and blood undergo in the process of respiration, the attraction of the former to fire is diminished, and that of the latter increased, in consequence of which the fire passes from the air to the blood; and that, in combustion, the fire is separated from the air by the action of the inflammable principle, in the same manner as the nitrous acid is detached by the vitriolic, from an earth or alkali.

If, however, fire be a substance which is subject to the laws of attraction, the mode of its union with bodies seems to be different from that which takes place in chemical combination. For, in chemical combination, the elements acquire *new* properties, and either wholly, or in part, lose those by which they were formerly characterized. But I have before endeavoured to show, that we have no sufficient evidence for believing that fire, in consequence of its union with bodies, does, in any instance, lose its distinguishing properties.

I may add, in the last place, that a table should be formed, exhibiting the compara-

tive quantities of fire contained in all known bodies.

Inflammable air should be placed at the head of this table, under which should stand, dephlogisticated and atmospherical air—the vapour of the nitrous acid, and probably of some other fluids—arterial blood, water, &c.

Much time, and a series of accurate experiments, are still required, in order to the complete investigation of this subject. There are few subjects, however, which have a better claim to the attention of Philosophers.

The proper adjustment of heat is of the utmost moment to the health and life of animals. An enquiry, therefore, into the causes which maintain the balance of heat in the animal œconomy, will probably give rise to improvements in the prevention and cure of diseases.

Many of the chemical arts wholly depend upon a nice regulation of heat. It is reasonable to believe, that those arts may be improved, and the conveniencies of life multiplied, by an attention to the theory and operations of fire.

The prosecution and advancement of this subject is, moreover, calculated to extend
our

our knowledge of the system of the world; for the operations of the element of fire hold a principal place in the series of causes, by which the adjustment of the universe is maintained. As the law of gravity, combined with the projectile force, regulates the motions of the great bodies which compose the planetary system; so it is the repulsive force of fire, combined with corpuscular attraction, that keeps the smaller particles of matter upon the surface of the earth, and probably throughout the universe, in a state of constant fluctuation; and there is little doubt that the improvements of chemistry will, at some future period, discover the same exquisite contrivance in the arrangement of the more minute parts of Nature, which the improvements of natural philosophy have shown to prevail in the planetary system.

I shall farther observe, that such speculations have a direct tendency to influence the moral character of man. It is this, indeed, which stamps them with their principal value; for all the other improvements, which may be supposed to arise from the cultivation of Nature; if they were un-

accompanied with a corresponding advancement in morals, could scarcely be considered as blessings. It is surely of very little moment to us, that we should procure the means of gratifying our desires, if we do not at the same time acquire the habit of regulating them. Could the increase of power be deemed a benefit, if it were used as a scourge; or of wealth, if it were made the instrument of corruption; or of knowledge, if it were employed to deceive?

The investigation of Nature is, however, peculiarly calculated to instruct us in that most important of all arts, the art of living. To whatever height we may ascend in tracing the causes, which regulate the system of the world, our views must at last terminate in an uncaused Being, in whom all the beauty and order, all the wisdom and power displayed throughout the universe, are centered. Every step which we advance in the cultivation of natural knowledge, contributes more firmly to establish this great truth; because it affords an additional proof of exquisite skill and contrivance in the formation of the world. By thus showing us the Deity in his works, the contemplation of
nature

nature leads to consequences, in the highest degree interesting to intellectual and moral agents. When we look around us, we perceive that every part of the material world is governed by general laws; and when we reflect that, in this vast system of things, a race of Beings exists, to whom the Deity has communicated a portion of his intelligence and activity, we cannot avoid concluding, that laws must have been ordained for the government of such Beings, as well as for that of all other parts of the universe.

Indeed, an attentive observation of human Nature, clearly points out the hand of the Deity, prescribing limits to the intellectual, as well as to the material world; for, in the original feelings and judgments of the human mind, there are evident traces of a moral law, which is antecedent to all positive institutions, and which must consequently have been written on the heart of man by the spirit of the Almighty.

Moreover, in contemplating the plan of the universe, we perceive that the whole is arranged into one great system; that an uninterrupted chain of connection extends throughout the various classes of Beings; that

that nothing in Nature is quiescent; that the past has been subservient to the present, as the present will be to the future; in a word, that all things are in a state of progression, and apparently tend towards the accomplishment of a great design, which seems to have influenced the whole series of events since the creation, and which probably will not be completed, till the present scene of things is closed by the dissolution of the material world.

If we compare the various classes of Beings, which fall within the reach of our observation, we perceive that intelligent and moral agents hold the highest rank; and, consequently, of all the laws which regulate the system of Nature, those that have been established for the purpose of maintaining harmony and order in the intellectual world, must be the most important.

It is thus that a just estimate of the value of the several objects which present themselves to our view, in a general survey of the universe, informs us, that obedience to the moral law constitutes the supreme perfection of the most exalted orders of created Beings; and as consummate moral excellence is the
highest

highest point of eminence, which the imagination can conceive an intellectual nature to attain, we are led to consider this as the first attribute of the Deity.

Hence, in investigating the final causes of the system of the world, there are none which appear more important, or more worthy of an infinitely perfect mind, than that the universe should be considered as formed for the purposes of communicating happiness to sensitive Beings, and of training intellectual and moral agents in the rudiments of wisdom and virtue.

Indeed, the best deductions of human reason concur with revelation in assuring us, that the attainment of moral excellence is the principal object of the creation. This it is which gives unity and perfection to the whole. We see that a great quantity of present happiness is continually sacrificed to it; that it is frequently pursued through danger, and difficulty, and distress; nay, that on some occasions it is sought after in the midst of commotions, which convulse the frame of Nature, and which carry in their train desolation and ruin. In fine, we have reason to believe, that the advancement of moral

excellence is the ultimate point, towards which the movements of this vast system are directed; and, as it seems to have been the great final cause which gave rise to the creation of the material world, so it is probable, that it will be the principal aim of the Deity in its dissolution.

The tendency of such speculations to influence the human character is obvious. In an enlightened age, the state of morals among mankind, principally depends upon the direction which is given to the collective exertions of genius, and of intellectual ability. If the efforts of genius and of intellect be concentrated upon objects which are useful and important, and particularly upon those which enlarge and elevate the mind, the manners of the people will be virtuous; and if the reverse, they will be depraved. In the cultivation of natural knowledge, the intellectual faculties of man are employed in investigating the laws which regulate the course of Nature, and in tracing them to their divine original; and nothing surely can have a greater tendency to elevate and enlarge the mind, and to add weight and dignity to the moral law, than researches
which

which terminate in that august Being, who is the primary cause of existence, and of motion in the material world, and to whom the various orders of intellectual and moral agents, throughout the universe, are related as their Creator, their Lawgiver and Judge.

A P P E N D I X.

AS it may perhaps be imagined that the differences between the heats imparted by pure and common air, in the experiments recited in the foregoing work, were so small that they could not be distinguished with sufficient certainty; I think it proper to observe, that I have repeated those experiments with thermometers that have each degree of Farhenheit's scale, divided into 50 equal parts. With a view to prevent the escape of the heat, each of the tinned vessels G H, I K, (see plate II.) was surrounded with another tinned vessel, which exceeded it in breadth nearly one inch, and the intermediate space was stuffed with fine down. The mouths of the interior vessels were closely covered by lids, which were lined with thick flannel.

The thermometers were inserted in the vessels to such a depth that their bulbs should reach a little lower than the center.

An

An apparatus was contrived for agitating the water when the vessels were covered, and the lids were formed in such a manner that they could be applied, after the cylinders were immersed.

For the purpose of trying the accuracy of the apparatus, equal quantities of common air were introduced into the cylinders described in page 178. The cylinders were heated in the manner formerly explained, (see p. 186.) They were immersed in the tinned vessels, containing equal quantities of water, at the temperature of the atmosphere, and the mouths of the vessels were closed to prevent the access of the external air. Being suffered to remain in this situation during 12 minutes, they were removed; the vessels were again covered; the water was agitated; and the heats communicated were accurately observed.

This experiment was frequently repeated with common air in both cylinders, and it appeared that the deviation, in the results of the different trials, was two degrees, as estimated by the above-mentioned thermometers, or $\frac{2}{50}$ of a degree of Fahrenheit.

Dephlo-

Dephlogisticated air was then introduced into one of the cylinders, and common air into the other; and it was found that when the former was nearly of the same standard with that used in Experiment II. p. 190, the excess of the heat which it imparted, above that communicated by the common air, was eight degrees; and when the purest dephlogisticated air was used, the excess was nearly 13 degrees*.

It is proper to observe, that the agitation of the water was essential to the success of this experiment; for though the bores of the thermometers were very fine, yet, in order to obtain a large scale, it was necessary that the bulbs should be of a considerable size.

And hence when one of these instruments was immersed in water, which had a higher temperature than itself by three or four de-

* The results of these experiments nearly correspond with those which have been recited above. (See p. 190.) For the difference in favour of dephlogisticated air, by my former trials, was $\frac{2}{10}$ of a degree of Fahrenheit; and eight degrees, as measured by the thermometers, which were used in my last experiments, are equal to $\frac{2}{10}$ of a degree of Fahrenheit minus $\frac{1}{25}$.

grees; if the water was kept in a state of rest, an interval of several minutes elapsed before the thermometer arrived at the exact temperature of the fluid with which it was in contact; but if the water was moderately agitated, the thermometer acquired the same temperature with it in less than half a minute.

The scales of these thermometers were divided upon thin brass plates, which were fixed to flat pieces of wood. The degrees were rendered much more distinct by painting black the portion of the tube that was in contact with the scale; an improvement which was first suggested to me by my worthy friend Mr. Benjamin Vaughan.

The proposition recited in page 88; that the comparative quantities of *absolute* heat in bodies, are reciprocally proportional to the changes produced in their *sensible* heats, when they are mixed together at different temperatures, may be proved geometrically as follows. See figures 3 and 4, plate IV. Let $abcd$, $efgb$ represent the quantities of heat contained in equal weights of two heterogeneous bodies A and B, which have

G g

a com-

a common temperature. The temperatures will be expressed by the equal lines dc , ef , and the capacities by ad , eb . Let B be raised to a higher temperature than A, and let its increase of temperature be expressed by the line ek , the augmentation of its absolute heat will be expressed by the figure kl . Let the bodies now be mixed together, and when they have been brought to a common heat, let the diminution of temperature in B be expressed by the line ki , and the increase of temperature in A, by the line do . do and ki will be reciprocally proportional to the quantities of heat in A and B, when they are brought to a common temperature. For km expresses the quantity of absolute heat which is separated from B, and nd that which is added to A; therefore $nd = km$, and consequently $ki : od :: no : im$. But, because the figures nc and ig have the same altitude, it will be as $no : im :: nc : ig$; therefore as $nc : ig :: ki : od$. That is, the quantities of absolute heat in A and B, when they are brought to a common temperature, are reciprocally proportional to the variations in their sensible heats,

heats, when they are mixed together at different temperatures.

The comparative heats express the quantities of absolute heat when the weights and temperatures are equal. See definition, p. 3. And since A and B have equal temperatures and weights, it follows, that nc and ig may be used to express the comparative heats.

Hence, if we call the comparative heats of A and B, C and c ; and if we call the variation of temperature in the former x , and in the latter y , it will be as $C : c :: y : x$; therefore $C = \frac{cy}{x}$. By this equation the comparative heats of bodies may be calculated when the quantities of matter, employed in the experiments, are equal.

If the quantities of matter be unequal, the equation will be as follows: The capacities are proportional to the comparative heats. See definition, p. 7.

Let C express the capacity, or the comparative heat.

T the temperature.

A the absolute heat.

Q the quantity of matter.

It was before shown, page 11, that if Q be given, A is as $C \times T$; and if C and T be given, A will be as Q . That is, if the capacities and temperatures be equal, the quantities of absolute heat will be as the quantities of matter. This appears from the observations in page 17. If, therefore, the capacities, the temperatures, and the quantities of matter vary; that is, if neither C , T , nor Q be given, in that case A will be as $C \times T \times Q$. Consequently C will be as $\frac{A}{T Q}$, and if A be given, C will be reciprocally as $T Q$. That is, if the quantities of absolute heat be equal, the capacities will be reciprocally as the temperatures multiplied into the quantities of matter.

Let C and c be the capacities of two bodies, A and B .

q and Q the quantities of matter.

T and t the temperatures.

It will be $C : c :: Q t : q T$, and therefore

$$C = \frac{c Q t}{q T}.$$

The reader will observe, that as the capacities are supposed to be permanent, t and T express universally the variations of tem-

perature

perature which are produced by the addition or separation of equal quantities of fire; for these variations, multiplied into the quantities of matter, will always be reciprocally proportional to the capacities; and since when two bodies are mixed together at different temperatures, the fire which is separated from the warmer is the same with that which is added to the colder, t and T may be used to express the variations of temperature produced by the separation and addition of this fire. Hence t and T have the same value with y and x in the foregoing equation, and therefore $C = \frac{c Q y}{q x}$. By this equation the comparative heats may be calculated when unequal quantities of matter are employed in the experiments.

An equation for calculating the lowest degree of heat may be obtained as follows: See fig. 5, plate IV. Let $e b$ represent the whole quantity of fire contained in a body A , and let $e g$ represent its capacity, and $e f$ its temperature estimated from the lowest point. Let this body be supposed to have its capacity diminished by a change of form, and after the change let the capacity be re-

presented by the line da , its temperature by dc , and the quantity of fire which it contains by db . If the body be supposed not to be in contact with any other body which can communicate elementary fire to it, or separate that principle from it, the quantities of fire before and after the change will be equal; that is, db will be equal to eb , and $dg = ec$, therefore $eg - gb : gb :: db : bc$; that is, the difference of the capacities before and after the change, is to the capacity, after the change, as the sensible heat produced, is to the whole number of degrees of heat which the body contained, previously to the alteration in its form.

Let therefore C express the capacity before the change.

c the capacity after the change.

l the sensible heat produced.

S the whole number of degrees of heat (previously to the change) estimated from the point of total privation,

Then $C - c : c :: l : S$.

Therefore $S = \frac{cl}{C - c}$.

By

By this equation we may calculate the lowest degree of heat, if the sensible heat, which a body produces, in consequence of a change of form, and the capacities before and after the change, be given.

There is reason to believe, from the experiments of Dr. Irvine, that the sensible heat or cold produced by mixing bodies together at the same temperature, in consequence of a chemical action, arises from a diminution, or an increase, in the sum of the capacities. The lowest degree of heat, as deduced from such experiments, may be calculated in the following manner :

1. Let the quantities of matter mixed together be equal.

Let $C + c$ be the sum of the capacities before the mixture.

$K + k$ the sum of the capacities after the mixture.

Then $C + c - \overline{K + k} : K + k :: l : S$

and $S = \frac{\overline{K + k} \cdot l}{C + c - \overline{K + k}}$.

2. Let the quantities of matter mixed together be unequal, and let Q and q be the quantities of matter.

G g 4

Then

$$\text{Then } C Q + c q - \overline{K Q + k q} : K Q + k q :: L : S \text{ and } S = \frac{\overline{K Q + k q} \cdot L}{C Q + c q - \overline{K Q + k q}}.$$

Hence, if two bodies, which produce sensible heat or cold by chemical action, be mixed together at the same temperature, and if the quantities of matter, the capacities before and after the mixture, and the sensible heat or cold produced, be given by experiment, the lowest degree of heat will be known by calculation.

It was before observed, that the question respecting the chemical combination of fire with bodies would be resolved, if a series of accurate experiments were instituted, in order to discover the changes of capacity which arise from alterations of form in different bodies, together with the quantities of heat and cold produced by those alterations.

If it be found that the lowest degree of heat, as calculated from such experiments is constant, it will follow that heat does not, in any case, enter into a chemical union with bodies, and consequently that the sensible heat or cold which they produce, when they undergo a change of form, arise solely from alterations

alterations of capacity; but if the lowest degree of heat be not constant, we may infer that a part of the heat, which is absorbed by bodies, when their capacities are increased, enters into a chemical union with them, and loses its distinguishing properties.

The following series of well conducted experiments, which were communicated to me by Mr. Gadolin, Professor of Chemistry at Abo, in Sweden, seem to afford a strong presumption that the lowest degree of heat is constant.

Results of Mr. Gadolin's Experiments on the absolute heat of Bodies, published in the Memoirs of Stockholm for 1784.

The capacity of snow is stated by Mr. Magellan, in his Essay on Fire, to be to that of water as 9 to 10. Taking this for granted, I, by direct experiments, found that snow, in melting, deprives an equal weight of water of about 80 degrees of heat according to Celsius's thermometer. This being equal to $\frac{1}{8}$ of the whole quantity of heat contained in water at the melting point, I concluded that this whole quantity is equal to

to 800 degrees according to Celsius, or that the lowest point of the thermometric scale is situated at the 800th degree below 0. This converted into degrees of Fahrenheit, gives 1408 degrees below 0 for the place of the lowest point *.

By direct experiments I also measured the capacity of common salt, as well as its solutions in different proportions of water. Thus I found the comparative heat of cristallised common salt to be - 0,226,
that of a saturated solution (containing 372 parts of salt, united with 1000 parts of water) - 0,793,
that of a solution containing salt and water in the proportion of 3 to 10 0,820

* The experiments made by me, at that time, gave 80,6 degrees for the heat absorbed by snow during its conversion into water, accordingly the number of degrees from 0 to the lowest point should have been 806 according to Celsius, or 1419 of Fahrenheit; however I preferred 800 as a more even number. In repeating those experiments I have afterwards found that the heat absorbed is somewhat greater, viz. 81,2 degrees according to Celsius; whence, supposing the proportion of the capacity to be the same as 9 : 10, it would follow that the absolute degrees are 812 below 0 of Celsius's thermometer, or 1430 below 0 of Fahrenheit.

that

that of a solution	-	2 to 10	0,866
		1,56 to 10	0,905
		1 to 10	0,936
that of water being supposed equal			
to	-	-	1,000

To find out the comparative heats of the saline solutions, I made use of a piece of copper, which I either heated or cooled in those solutions. As for the cristalline common salt, I tried it by mixing it, when finely powdered, with a saturated solution.

To compare the difference of the comparative heats with the colds produced during the solutions, I made the following experiments.

Exp. I. With 10 parts of water, contained in a tinned vessel whose capacity was = 0,6, their common temperature being = 20° (Celsius's,) five parts of common salt reduced to a fine powder, whose temperature was 21,°2, were mixed. After the solution the common temperature was found to be 13,°58.

Here the whole quantity of heat, before the solution, was equal to $10 \times 820,8 + 0,6 \times 820,8 + 5 \times 0,226 \times 821,2$; and the whole quantity of heat after the solution, supposing

supposing the capacity of the saturated solution = X, will be thus expressed *.

$$13,72 \times X \times 818,58 + 1,28 \times 0,226 \times 818,58 + 0,6 \times 818,58.$$

If, during the solution, no part of the heat was chemically bound, this last expression must be equal to the former; which being supposed, the value of X will be found equal to

$$X = \frac{10,6 \times 820,8 + 5 \times 0,226 \times 821,2 - 1,28 \times 0,226 \times 818,58 + 0,6 \times 818,58}{13,72 \times 818,58}$$

= 0,7926; which exactly coincides with the comparative heat found out for the saturated solution, 0,793.

The same experiment was repeated thus :

Exp. II. In a vessel, whose capacity was 0,7; 14,5 parts of water were mixed with 5,5 parts of salt; the common temperature before the mixture being 19°,5. The common temperature after the solution was 16°,95. If the calculation from this experiment be instituted as from the foregoing, we find $X = 0,7927$.

* As 10 parts of water are able to dissolve 3,72 parts of common salt, it follows that the mass, after the mixture, contains 13,72 parts of saturated solution, and 1,28 parts of undissolved common salt.

EXP. III. 3,5 parts of common salt was put into a glass vessel, whose capacity was 1,2, their common temperature being 23° . To this was poured 10 parts of water, heated to $40^{\circ},84$; and the common temperature after the mixture was found to be $35^{\circ},65$.

From this experiment the capacity of a solution of 3,5 parts of salt, in 10 parts of water, may be computed, and it will be found equal to 0,802; that is, less than the capacity of a solution, containing three parts of salt, and 10 of water, but greater than the capacity of a saturated solution.

EXP. IV. In the vessel, whose capacity = 1,2; 4,5 parts of common salt were mixed with 15 parts of water, the common temperature before mixture being = 23° ; that after mixture was 20,54. If the capacity of this solution be computed from this experiment, it will also be found very nearly equal to 0,824.

EXP. V. Two parts of salt and 10 parts of water were mixed together in a vessel, whose capacity = 0,7; the common temperature before mixture was 20,51, and that after solution 18,65; whence the capacity of
the

the solution will, by computation, be found $= 0,873$; the observed was $0,866$.

EXP. VI. Ten parts of water were mixed with one part of salt, in a vessel whose capacity was $= 1,2$. The temperature before mixture was $21,53$, and that after mixture $20,25$. The capacity of the solution calculated from this experiment will also be nearly $= 0,932$.

EXP. VII. To the solution (Experiment VI.) remaining in its vessel one part more of the salt was added, the common temperature being $21^{\circ},52$; the temperature after the mixture was $20^{\circ},99$. Hence, supposing the comparative heat of the weak solution $= 0,932$, that of the new solution will by computation be found $= 0,873$ nearly; which agrees with what was computed from Experiment V.

EXP. VIII. To the solution made in the Experiment VII. one part of salt was added, the temperature before the mixture was $21^{\circ},75$, and after the solution $21^{\circ},45$. Hence the capacity of the weaker solution being supposed $= 0,873$, that of the new made, containing three parts of salt united with 10 of water, will by computation be found

found = 0,824, agreeably to the calculation made from Experiment IV.

To try how the mixture of salt solutions with water would agree with those observations, I made the following experiments.

EXP. IX. Ten parts of saturated salt solution was mixed with 19,7 parts of water, the temperatures of both being = $19^{\circ},6$; the temperature of the mixture was found = $18^{\circ},95$.

If the capacity of the saturated solution is 0,793, that of the diluted in these two experiments, (which consists of one part of salt and 10 parts of water) will by computation be found = 0,932; which coincides with the result of the calculation made from Experiment VI.

EXP. X. Equal parts of saturated solution and water were mixed together at the temperature of $21^{\circ},4$; the temperature of the mixture was $20^{\circ},75$. This mixture contained 1,56 parts of salt dissolved in 10 parts of water, and its capacity calculated from the experiment, supposing the capacity of the saturated solution = 0,793, will be found = 0,896. The capacity of an equally diluted solution

solution was by direct experiments found to be = 0,905.

The following table exhibits the results of the foregoing experiments in one view.

The experiments recited.	Proportion of salt and water.	Computed capacity.	Observed capacity.	Difference.
1, 2,	3,72 : 10	0,793	0,793	0,000
3,	3,5 : 10	0,802	—	—
1, 4, 8,	3 : 10	0,824	0,820 + 0,004	
5, 7,	2 : 10	0,873	0,866 + 0,007	
10	1,56 : 10	0,896	0,905 — 0,009	
6, 9,	1 : 10	0,932	0,936 — 0,004	

In all these experiments the comparative heats of the mixtures, calculated from the comparative heats of the mixed bodies, and their changed temperatures, agree perfectly with one another, and very nearly with the observed comparative heats. It, therefore, seems to be beyond all doubt that the colds produced are intirely owing to the changes of the comparative heats, and that no part of the heat here has been chemically combined.

With a view more fully to investigate this subject, I thought it proper to make similar experiments with oil of vitriol and water, as the heat produced during their mixture is very considerable.

By

By direct experiments I found the capacities of concentrated dephlogisticated oil of vitriol to be - - 0,339,

Of four parts of concentrated oil of vitriol mixed with one part of water - - 0,442,

Of concentrated oil of vitriol and water mixed in the proportion of 2 to 1, - - 0,500,

Of concentrated oil of vitriol and water mixed in proportion of

- -	1 to 1,	0,605,
_____	1 to 2,	0,749,
_____	1 to 5,	0,876,
_____	1 to 10,	0,925.

In these experiments the acid was included in a thin glass bowl, which, being heated in warm water to a certain degree, was cooled in a given quantity of cold water. The capacity of the glass bowl, as well as that of the vessel, was by previous experiments found out, and due corrections accordingly made.

The heats produced during the mixtures were observed in the following experiments.

EXP. I. Four parts of concentrated oil of vitriol were mixed with one part of water,

H h their

their temperature before the mixture being 12° , that after the mixture was $119^{\circ},5$.

Here the heat before the mixture (the capacity of the concentrated acid being supposed $= 0,339$) is $= 4 \times 0,339 + 1 \times 812$; and that after the mixture (the capacity of the mixture being called X) is $= 5 \times X \times 919,5$; therefore, if no alteration in the quantity of the heat took place during the mixture, these expressions must be equal; from thence $X = 0,416$, the observed comparative heat of this diluted acid was $0,442$; and if in the equation $0,442$ be placed instead of X , the value of the capacity of the strong acid will, by calculation, be found equal to $0,372$, instead of $0,339$.

EXP. II. Two parts of concentrated acid were mixed with one part of water at the temperature of $11^{\circ},13$. The quicksilver in the thermometer rose to the 124th degree. Hence, if the capacity of the concentrated acid be supposed $= 0,339$, that of the diluted will, by computation, be found $= 0,491$; the observed was $0,500$. Again, if the capacity of the diluted acid in this experiment be supposed $= 0,500$, that of the concentrated

centrated will, by calculation, be found = 0,354.

Exp. III. Equal parts of concentrated acid and water were mixed together at the temperature of $10^{\circ},5$; the mixture acquired a temperature of 100° . Hence, supposing the capacity of the concentrated acid = 0,339, that of the diluted will, by computation, be found = 0,603. Its observed capacity was 0,605; and if this be supposed to be the true capacity, the computed capacity of the concentrated acid is 0,344: here, therefore, the deviation is very inconsiderable.

Exp. IV. One part of concentrated acid being mixed with two parts of water at the temperature of $10^{\circ},25$, the temperature of the mixture was 70° . By assuming the capacity of the concentrated acid = 0,339, that of the diluted will, by computation, be = 0,726; but by assuming this = 0,749 (as it was observed) that of the concentrated will, by computation, be = 0,413.

Exp. V. One part of concentrated acid being mixed with five parts of water, at the temperature of $10^{\circ},25$, that of the mixture was $38^{\circ},25$. If the capacity of the concentrated acid is supposed = 0,339, that

computed for the diluted is 0,860; and the observed was 0,876, which being substituted, that of the concentrated will be computed to 0,438.

EXP. VI. One part of concentrated acid being mixed with 10 parts of water, at the temperature of $9^{\circ},5$, the temperature of the mixture was $24^{\circ},75$. Hence the computed capacity of the mixture is 0,923, if that of the concentrated acid is supposed = 0,339; but if the former is supposed = 0,925, the computed capacity of the concentrated acid will become = 0,367.

As in these experiments the computed capacity of the mixtures constantly are less than the observed ones, and on the other side the computed capacity of the strong acid constantly greater than the observed, one may be apt to think that here really by the mixtures new quantities of heat are produced, that were not to be found in the mixed liquids, before the mixtures, if not chemically combined therein. But when I consider that this discordance is likewise a consequence of the error that my manner of observing the comparative heats of the acids might have led into, I think it is more likely

ly to attribute the anomaly to this cause. Then as the conducting power of concentrated vitriolic acid is very weak in proportion to that of water or a weaker acid, it may easily be supposed that the capacity observed for the concentrated acid was greater than what was found by direct observation. That the whole deviation of the calculated from the observed capacities is owing to some errors in the latter, is so much the more probable, as in some of the following experiments, (Experiments VII. VIII. IX.) the deviations have fallen out in a different way quite contrary to the opinion of a new produced heat.

EXP. VII. Five parts of diluted vitriolic acid (consisting of four parts of concentrated acid, and one part of water) were mixed with three parts of water, at the temperature of $10^{\circ},5$; the temperature of the mixture was 49° . If now the capacity of the acid employed, be supposed $= 0,442$, that of the mixture will, by computation, be found $= 0,622$; but the observed capacity of this acid, consisting of equal parts of concentrated acid and water was $0,605$; and if this be supposed to be its true value, the capacity of

the acid used, will, by computation, be = 0,414, *viz.* less than what was observed.

EXP. VIII. 0,128 parts of the same acid (used in Experiment VII.) were mixed with one part of water at the temperature of $11^{\circ},15$; the temperature of the mixture was $18,77$. The computation gives 0,928 for the capacity of the mixture, if that of the used acid is supposed = 0,442; but if the former is supposed = 0,925, the latter will, by computation, be found equal to 0,416.

EXP. IX. 0,158 parts of acid (consisting of two parts of concentrated acid, and one part of water) were mixed with one part of water at the temperature of $10^{\circ},5$; that of the mixture was $15^{\circ},5$. Here the capacity of the mixture will be found = 0,928, and that of the used acid 0,492, when calculations are made, supposing in the former case that of the latter acid to be 0,500, and in the latter, the capacity of the mixture to be 0,9 5.

EXP. X. Two parts of acid (consisting of equal parts of concentrated acid and water) being mixed with one part of water, at the temperature of 12° ; that of the mixture was $19^{\circ},43$. Hence the computed heats of the
used

used acid and the mixture are 0,634, 0,730; the observed capacities were 0,605, 0,749.

EXP. XI. One part of the same acid (as that used in Experiment X.) being mixed with two parts of water, at the temperature of $12^{\circ},6$, that of the mixture was $17,6$: here the computed capacities are 0,644, 0,863; the observed were 0,605, 0,876.

EXP. XII. Two parts of the same acid (Experiment X.) being mixed with nine parts of water, at the temperature of $12,4$; that of the mixture was $15^{\circ},15$. Therefore the computed capacities are 0,605, 0,925; which perfectly coincides with the observed ones.

EXP. XIII. One part of diluted vitriolic acid (consisting of one part of concentrated acid, and two parts of water) being mixed with one part of water, at the temperature of $12^{\circ},25$; the temperature of the mixture was 14° . Hence, if the capacity of the used acid is supposed = 0,749; that of the mixture will, by computation, be found = 0,873; and, again, if the latter is supposed = 0,876, the former will be = 0,756.

The differences between the calculated and observed capacities are not greater

than those which may be supposed to have arisen from the errors committed in finding out the latter; nor can they easily be accounted for otherwise, as they are somewhat irregular, the excesses being sometimes on one, sometimes on the other side. However it may be a little more satisfactory to point out the capacities corrected by approximation from the experiments mentioned.

The capacities may be more nearly approximated, by taking a medium between the value of the capacity of each acid, found by computation from the experiments made with it, and its value pointed out by direct observation. Thus I find the corrected capacities to be the following.

The capacity of concentrated acid		0,360
Of concentrated acid and water		
in the proportion of	- 4 : 1	0,429
_____	2 : 1	0,496
_____	1 : 1	0,613
_____	1 : 2	0,743
_____	1 : 5	0,871
_____	1 : 10	0,925

These values agree, I think, very nearly with the true capacities of the acids. I will, therefore, suppose them to be the true values,
and

and, from this supposition, make calculations for the heat that ought to have been produced by the mixtures made in the above-mentioned experiments, if this heat was caused only by the changes of capacities; that is, if the whole quantity of heat was the same before and after the mixture; the lowest point of the thermometer at 800° below 0; and if the capacities of the concentrated and diluted acids exactly agree with the values just exhibited.

Then I find that the temperature of the mixture, in the 1st Experiment, ought to have been, $123^{\circ},7$ instead of $119^{\circ},5$ as observed.

2d. Exp.	137,6	-	124
3d. Exp.	99,1	-	100
4th. Exp.	57,9	-	70
5th. Exp.	31	-	38,25
6th. Exp.	24,22	-	24,75
7th. Exp.	50,33	-	49
8th. Exp.	20,1	-	18,77.

In the other experiments, the heats produced are so small, that they cannot be expected to be determined by calculation, with any degree of precision; for the least deviations in the expressions of capacities, produce very considerable errors in the calculated degrees

degrees of heat. Therefore, however deviating the above-mentioned computed sensible heats are from the observed ones, still I look upon them as very nearly answering the expectation. Then very small corrections of the capacities may make them perfectly agree with the observations.

Having found that the doctrine of capacities so well agreed with the phenomena of cold produced by solutions of common salt in water, I was desirous to know how far the cold, produced by mixing salt and snow, could be accounted for, from the same principles.

As it is well known, that when cold arises in a saline mixture, this, from a state of dryness, grows humid, or is entirely converted into a liquid solution; I had no doubt but that this conversion into a liquid state, and the increase of capacity arising from it, was the cause of the cold produced. I was confirmed in this opinion by the following observations. In every mixture of salt and snow, there is a maximum of producible cold, or there is a certain degree determinable on the thermometric scale, beyond which no farther cold, by a given mixture of salt
 4 and

and snow, can be produced. If the salt as well as the snow, before the mixture, are cooled below that point, there is no cold produced, and the particles of the mixed bodies continue unaltered in their dry state. If the same mixture, by means of heat, is converted into a liquid solution, and afterwards exposed to a degree of cold below that point, it will loose its liquidity, and be converted into solid ice. From thence it is evident, that the lowest point of cold, producible by a saline mixture, is the freezing point of that mixture; and as, at that point, no liquefaction can take place, the cause of the production of cold is removed. So I found that common salt and snow mixed together produced a cold of -21° ; (or $-5^{\circ},8$ according to Fahrenheit's thermometer) that a saturated solution of this salt and water, when exposed to the free air in a lower temperature than -21° , was partly frozen, and made a thermometer, immersed in it, rise to -21° , and that the salt and snow mixed in any temperature below that degree, continued unaltered, as to their external appearance, and produced no cold at all.

Although

Although these general facts perfectly agreed with the theory, yet I did not look upon them as fully convincing, till I, from more determined phenomena, was able to make computations for the cold produced, as well as for the variation of the capacities. Accordingly I mixed 1,75 parts of snow, and 0,625 parts of common salt, their common temperature before the mixture being $-7^{\circ},5$, in a thin globe of glass, whose capacity was 0,35. This globe was immediately put into a vessel filled with 34 parts of water, (where to avoid prolixity I have included the capacity of the vessel) heated to a temperature of $+21^{\circ}$. When the saline mixture was melted, and after it had ceased to communicate any farther cold to the water, the common temperature was observed to be $+15^{\circ},25$. Now the capacity of the water being 1, that of the snow 0,9, that of the common salt 0,226, and that of the solution (as it contained 2,8 parts of water to 1 part of salt) 0,8; and the lowest point of the thermometric scale being supposed to be situated at -800° , I had the whole quantity of heat contained in the materials before the mixture, equal to $(1,75 \times 0,9$

$\times 0,9 + 0,625 \times 0,226 + 0,35) \times (800 - 7,5) + 34 \times 821 = 29551,5$; but the sum of the capacities, after the mixture, are equal to $34 + 2,375 \times 0,8 + 0,35 = 36,25$; consequently the common temperature after the mixture ought to have been $= \frac{29551,5}{36,25} = 815^{\circ},21$, or $15^{\circ},21$ above the freezing point, which does not perceptibly deviate from the observed temperature of $15^{\circ},25$.

The reader will perceive that the lowest degree of heat, as determined by these experiments of Mr. Gadolin's, is only 1400 below the beginning of Fahrenheit's scale. From the experiments with pure and inflammable air recited above, the point of total privation appeared to be nearly 1500 degrees below the zero of Fahrenheit.

It is proper, however, to remark that the capacity of a mixture of oil of vitriol and water, is somewhat increased by an augmentation of temperature; and if allowance be made for this circumstance, in Mr. Gadolin's experiments, the results will nearly coincide with those of the experiments on the combustion of pure and inflammable air.

That

That the capacity of a mixture of oil of vitriol and water is increased by an augmentation of temperature, is proved as follows.

1. More sensible heat is produced by mixing oil of vitriol and water, or strong and weak spirit of vitriol at a low temperature, than by mixing them at a high temperature. Thus I have found that when strong and weak spirit of vitriol were mixed successively at the temperatures of 50, of 110, and of 212, the quantities of sensible heat produced were uniformly diminished, in proportion as the temperatures were increased. Strong and weak spirit of vitriol are better adapted for these experiments, than oil of vitriol and water, because when the latter fluids are mixed, a considerable evaporation is produced.

Mr. Cavendish was, I believe, the first who discovered that spirit of wine and water excite less sensible heat, when mixed at a high, than at a low temperature. I have found that the same thing happens when spirit of wine and oil of vitriol, and likewise when caustic alkali and water, are mixed at different temperatures. These facts render it probable, that the compounds, which are produced

produced by the chemical union of those substances with each other, have their capacities increased by augmenting their sensible heats; for if, in such experiments, the capacity of the compound be increased by augmenting its sensible heat, it will be less heated by the application of a given quantity of fire, at a high, than at a low temperature.

2. When substances which produce heat by chemical action are combined together, the density of the compound is greater than the mean density of the ingredients. This has been called by Mr. Kirwan the accrued density. Now it appears by experiment that the accrued density of spirit of wine and water, and of oil of vitriol and water, is greater when they are mixed at a low than at a high temperature. The truth of this conclusion, at least as far as relates to spirit of wine and water, is clearly proved by the experiments of Mr. De Luc. From those experiments it appears, that the expansions of a mixture of water and alcohol, have an increasing progress compared with the mean expansions of these fluids in their separate state; and consequently the accrued density must diminish, or the density of the compound

pound must approach more nearly to the mean density of the ingredients, in proportion as the temperature is increased.

I have also found, that when oil of vitriol and water are mixed at a high temperature, the increase in the density of the compound is less than when they are mixed at a low temperature.

Since, therefore, the accrued density of weak spirit of vitriol, as well as that of a mixture of alcohol and water, is diminished by an augmentation of temperature; and since the accrued density, and the diminution of capacity, are effects which proceed from the same cause, that is from chemical action, there is reason to believe that those effects will, in some measure, correspond with each other; and consequently, in proportion as there is less accrued density, there will be a less diminution of capacity. If this be admitted, it will follow that the compounds, which are produced by the chemical union of alcohol and water, and by that of spirit of vitriol and water, have their capacities somewhat increased by an augmentation of temperature.

3. That weak spirit of vitriol has its capacity increased by augmenting its sensible heat, is confirmed by direct experiment; for when equal parts of diluted spirit of vitriol are mixed together at different temperatures, the temperature of the mixture is greater than the arithmetical mean; and consequently, the capacity of this fluid must be increased by augmenting its temperature, as appears from Proposition II. page 59.

If allowance be made for this circumstance, the results of Mr. Gadolin's Experiments will be found nearly to coincide with those which have been deduced from the experiments on the combustion of pure and inflammable air. There is, therefore, the utmost reason to believe that the lowest degree of heat, or the point of total privation, as calculated from trials upon different bodies, according to the rule proposed by Dr. Irvine, is constant; and it is probable that this point is nearly 1500° below the beginning of Fahrenheit's scale.

It is proper, however, to observe, that we cannot as yet be assured that the lowest degree of heat is determined with perfect precision; for though, from similar trials which

I have myself made, I am convinced that Mr. Gadolin's experiments are nearly accurate; and though these experiments do not materially differ in their results from the trials with pure and inflammable air related in the former part of this work; yet, in order to arrive at a sufficient degree of precision in a subject of such delicacy, it is necessary that we should take the mean result of an extensive series of experiments made with a view to determine the changes of capacity which arise from alterations of form in a *variety* of different bodies, together with the quantities of heat or cold produced by such alterations.

As the method of investigating the beginning of the scale of heat was originally proposed by Dr. Irvine, I was unwilling to interfere with him in the solution of this curious problem. I have, therefore, hitherto avoided entering into this subject more minutely than was necessary for the illustration of the principles contained in the foregoing pages. But as that Gentleman is lately deceased, and as the question is not yet determined in a manner that is altogether decisive, I mean in future, if it please God to grant
me

me life and health, to make it the object of my particular attention.

I have endeavoured, in the course of the foregoing work, to mark with as much fidelity and accuracy as possible, the improvements which were made by Dr. Black and Dr. Irvine, in the doctrine of heat, before I began to pay attention to this subject. The following is, I believe, a correct statement of the progress of these improvements, as far as they relate to the investigation of the comparative heats of bodies.

Dr. Black was the first who concluded from Fahrenheit's experiment, recited in page 86, that mercury and water have different capacities for heat. In pursuance of this idea he made experiments to determine the comparative heats of various substances. It is, therefore, to that Philosopher we are indebted for our knowledge of the general fact, that the capacities of bodies for heat are different.

Dr. Irvine inferred from this fact, compared with the discovery of latent heat, which had previously been established by Dr. Black, that the capacities of fluid bodies are greater than those of the same substances when they

are changed into solids. This inference, which he afterwards confirmed by experiment, led him to the discovery of the rule for ascertaining the lowest degree of heat; and upon the same principles he explained the cause of the heat which is produced by chemical mixture.

It has been insinuated that I published, in a former edition of this work, a part of the discoveries made by Dr. Irvine, without acknowledging the author. The following letter, which I received from that Philosopher, in the year 1780, will prove the falshood of such insinuations.

“ S I R,

“ As from some cause or another, probably from the mistakes and misapprehensions of people ill-informed in the subject of our researches, I have been represented as the author of the doctrines concerning heat in your late publication; I take this opportunity of signifying, that I lay no claim to the discovery that atmospherical air contains *more* absolute heat than fixed air, upon which the doctrines of animal heat and inflammation, explained in your book, so im-

mediately depend. I likewise lay no claim to the *general fact* concerning the increase or diminution of the absolute heat of bodies, in consequence of the separation or addition of phlogiston, which is contained in your book; and in general there is nothing in your publication on Animal Heat, and the Inflammation of Combustible Bodies, that I lay any claim to, which you have not in that work already ascribed to me.

Glasgow College,

“ I am,

Jan. 27, 1780. “ Your humble servant,

(Signed) W. IRVINE.”

The Proposition enunciated in page 59, is strictly deduced from the definition which has been given of a permanent capacity; see p. 53. In that definition are included all those cases, in which equal variations of temperature are produced by the addition or separation of equal quantities of absolute heat. If, however, a permanent capacity should be differently defined; if we should restrict this expression to those cases only in which no heat is absorbed or evolved by a variation

variation of temperature, or in which the ordinates, representing the capacities in different parts of the scale, are equal to each other, the above-mentioned Proposition would not be universally true; for we can readily suppose that when heat is communicated to a body, a part of it may be absorbed, and a part may become sensible; and the proportion between the part which is absorbed, and that which becomes sensible, may be conceived to be so adjusted, in different temperatures, that an equal effect shall be produced, or that the same quantity of absolute heat, which raises the body one or two degrees, at a given temperature, shall raise it an equal number of degrees at all other temperatures.

In this case the capacity would be permanent, according to the first definition which I have given of that quality in bodies; but, by the second definition, it would be an increasing capacity.

It is plain, however, from the results of the foregoing experiments on the lowest degree of heat, that if, in particular instances, the capacities vary according to the law which has been now stated, the effects produced

duced are nearly the same as if no such variation took place.

I have observed in page 33, that if, in the experiments with warm and cold water, the mercurial thermometer had indicated the exact arithmetical mean, we could not from thence infer, with certainty, that the dilatations of this instrument are proportional to the increments of heat; for that if unequal augmentations of capacity were produced in water by equal increments of heat, and if unequal contractions were produced in mercury by equal diminutions of heat, these contrary irregularities might be conceived to be so adjusted, as precisely to balance each other. It is proper to observe that, in this case, the mercurial thermometer would be an accurate measure of the quantities of heat applied to water, but would not be an accurate measure of the quantities applied to itself.

In the calculations of the experiments which were made by mixing warm and cold water together, (see p. 20) the capacity of the vessel was compared with that of the cold water, whereas it should have been compared with that of the mixture. If allowance

lowance be made for this error, it will appear that the deviation of the mercurial thermometer from the arithmetical mean, in those experiments, was a little less than I supposed it to have been.

Thus it has been supposed that the deviation, in Experiment III. p. 21, was .55. When the above-mentioned mistake is corrected, it will be found to have been only .25 of a degree.

I am indeed convinced, by a careful repetition of my experiments, that, in such trials, the temperature of the mixture, as indicated by the mercurial thermometer, corrected according to Mr. Cavendish's method, corresponds very nearly with the arithmetical mean.

It is observed, in page 294, that heavy inflammable air consists partly of light inflammable air, and partly either of fixed air, or of a substance that may be considered as its basis. This conclusion was drawn from Dr. Priestley's experiments, which prove that a large quantity of fixed air is produced by the combustion of dephlogisticated and heavy inflammable air; but that the weight of the fixed air, which is the result of that process,

is

is not altogether equal to the sum of the two airs used in the experiment.

The reader will perceive that I had reason to be convinced, from subsequent trials, that when heavy inflammable air is in its purest state, it consists entirely of a substance which forms the basis of fixed air *.

As it may perhaps be useful to the reader to have a general view of the results of the experiments recited in the foregoing pages, have comprized them in the following table.

Table of the comparative heats of different bodies, as determined by the preceding experiments.

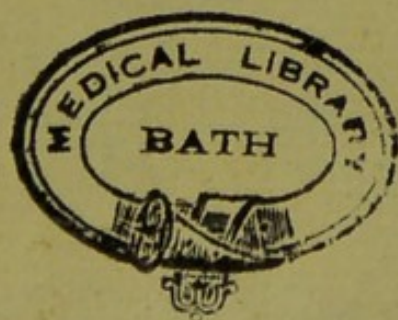
Inflammable air	—	21,4000
Dephlogistified air	—	4.7490
Atmospherical air	—	1.7900
Aqueous vapour	—	1.5500
Fixed air	— —	1.0454
Arterial blood	—	1.0300
Water	— —	1.0000
Fresh milk of a cow	—	.9999
Venous blood	—	.8928
Phlogistified air	—	.7936
Hide of an ox, with the hair	-	.7870

* See page 344.

Lungs of a sheep	—	.7690
Lean of the beef of an ox	—	.7400
Alcohol	—	.6021
Rice	—	.5060
Horse beans	—	.5020
Spermaceti oil	—	.5000
Dust of the pine tree	—	.5000
Peas	—	.4920
Wheat	—	.4770
Barley	—	.4210
Oats	—	.4160
Vitriolic acid	—	.4290
Pit-coal	—	.2777
Charcoal	—	.2631
Chalk	—	.2564
Rust of iron	—	.2500
Washed diaphoretic antimony	—	.2272
Calx of copper nearly freed from air	—	.2272
Quicklime	—	.2229
Cinders	—	.1923
Ashes of cinders	—	.1855
Rust of iron nearly freed from air	—	.1666
Washed diaphoretic antimony nearly freed from air	—	.1666
Ashes of the elm tree	—	.1402
Calx of zinc nearly freed from air	—	.1369
Iron	—	.1269

Brass	—	—	.1123
Copper	—	—	.1111
White calx of tin nearly freed from air	—	—	.0990
Regulus of zinc	—	—	.0943
Ashes of charcoal	—	—	.0909
Tin	—	—	.0704
Yellow calx of lead nearly freed from air	—	—	.0680
Regulus of antimony	—	—	.0645
Lead	—	—	.0352

F I N I S.



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FIG. 1.

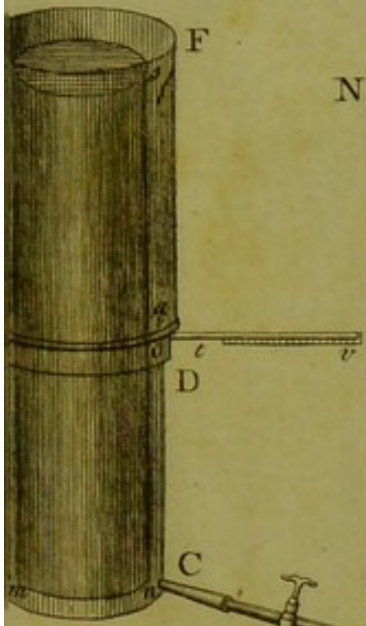


FIG. 3.

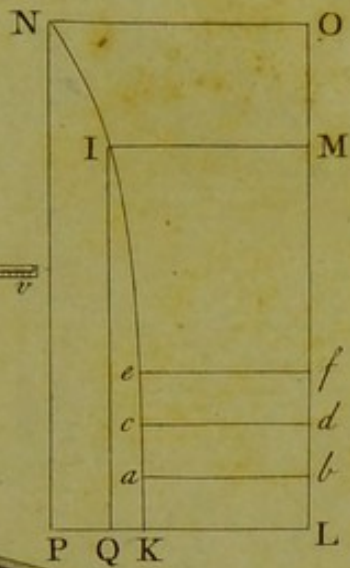


FIG. 2.

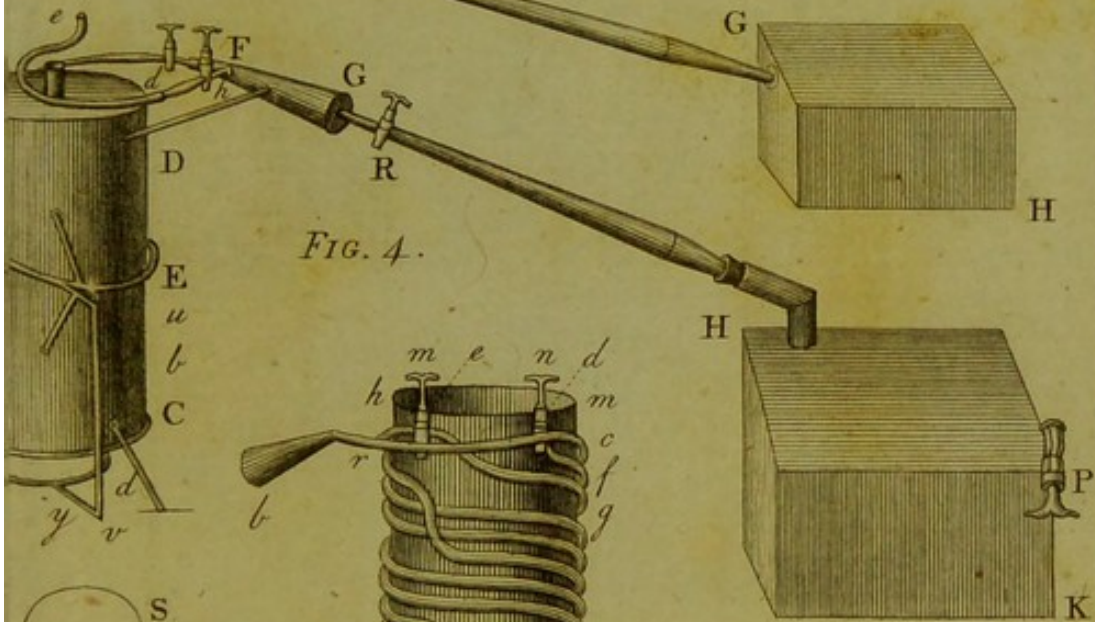
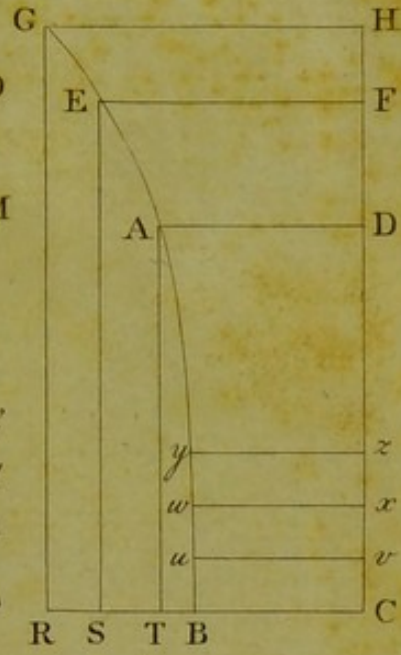


FIG. 4.

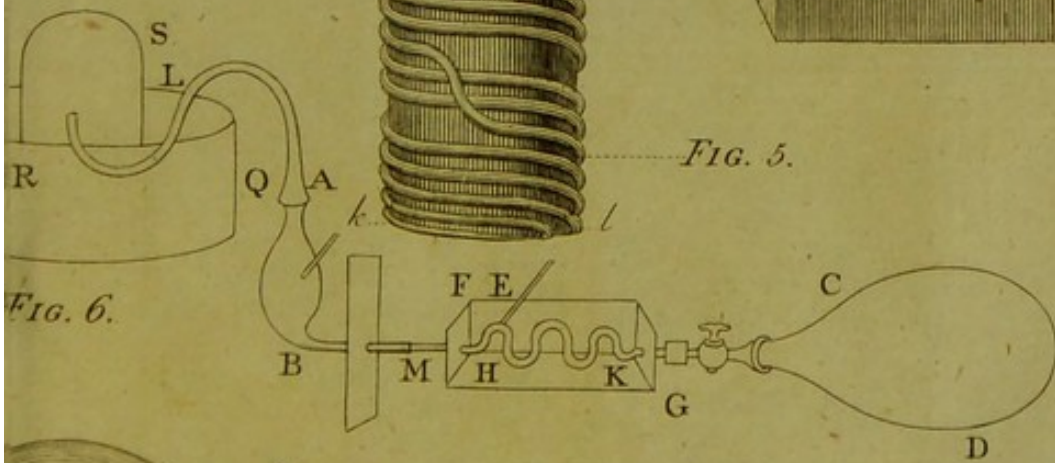


FIG. 5.

FIG. 6.

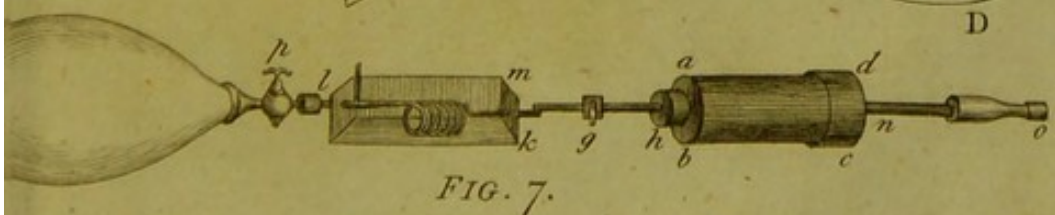
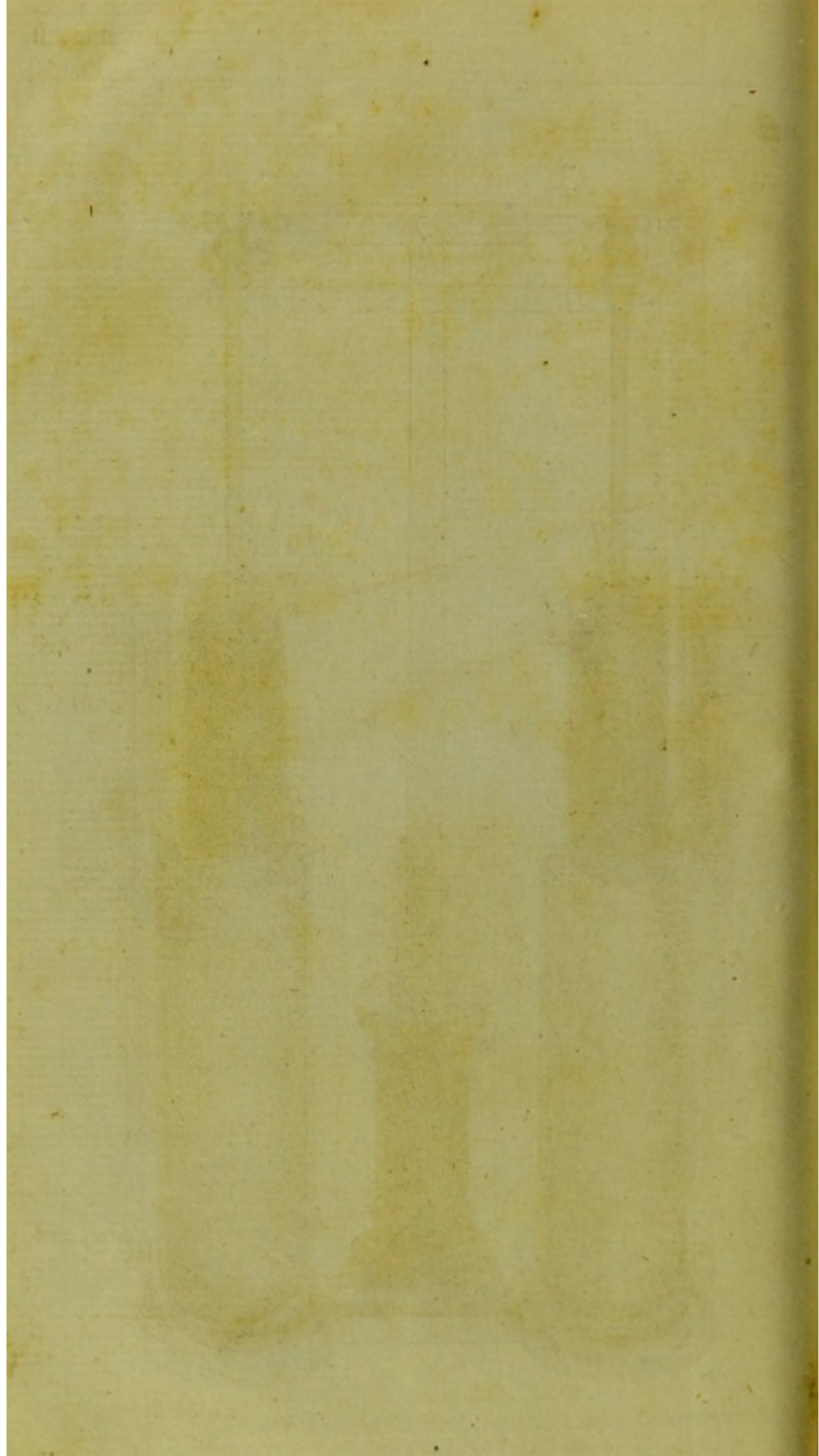
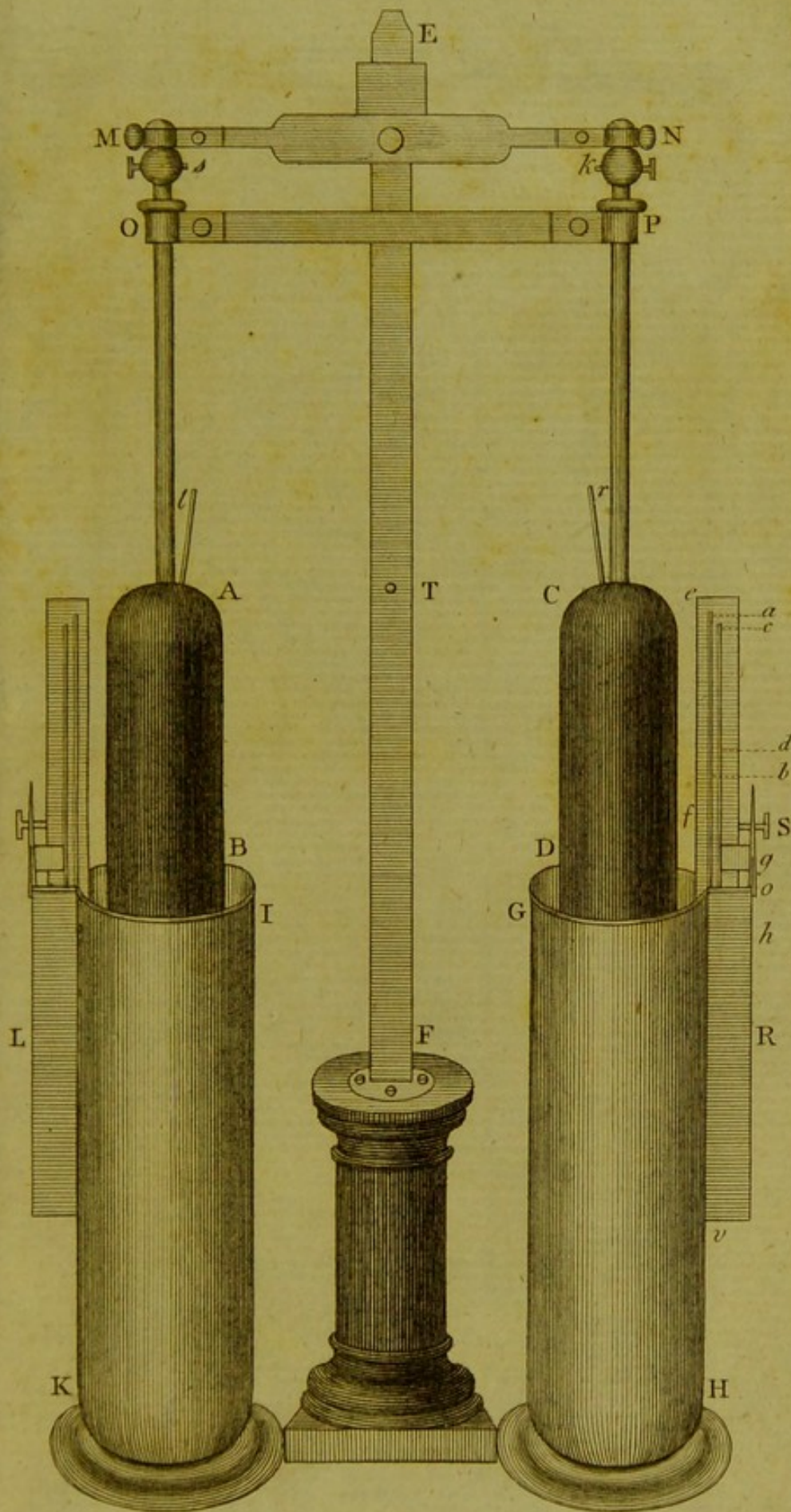


FIG. 7.





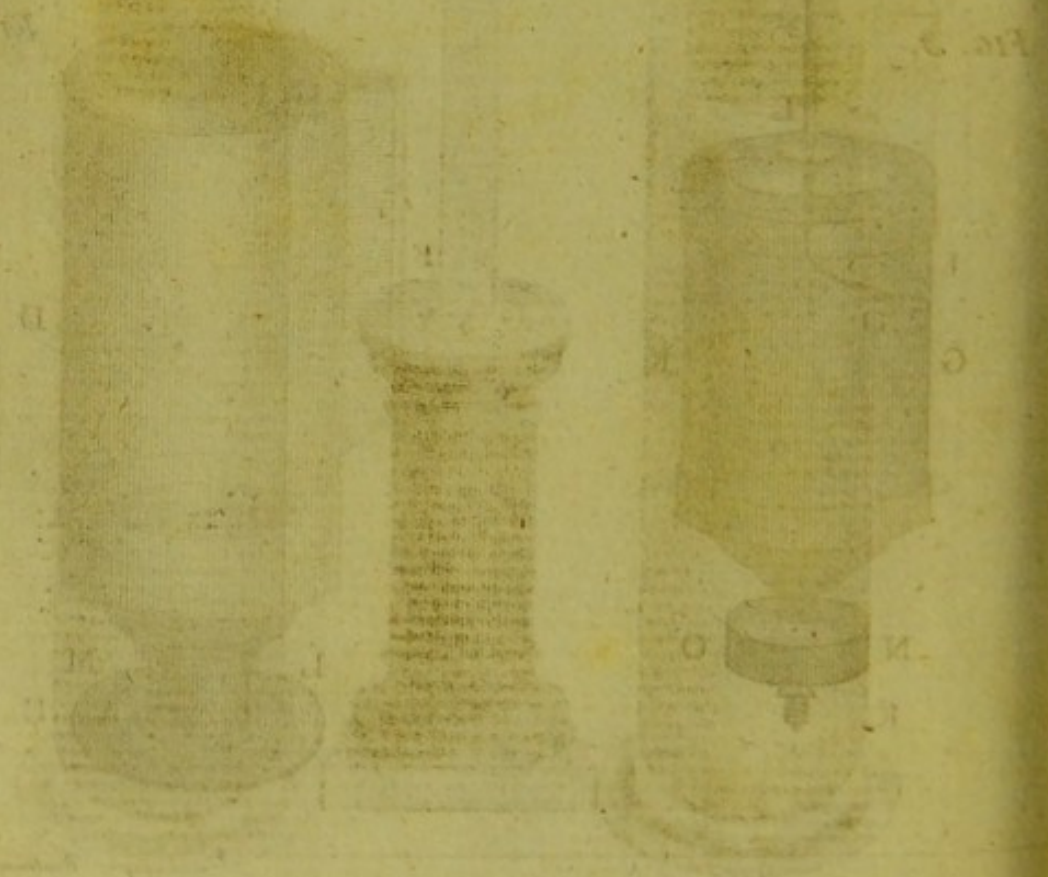


FIG. 1.

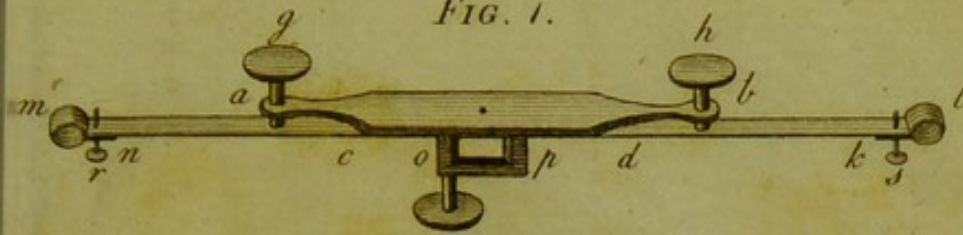


FIG. 2.

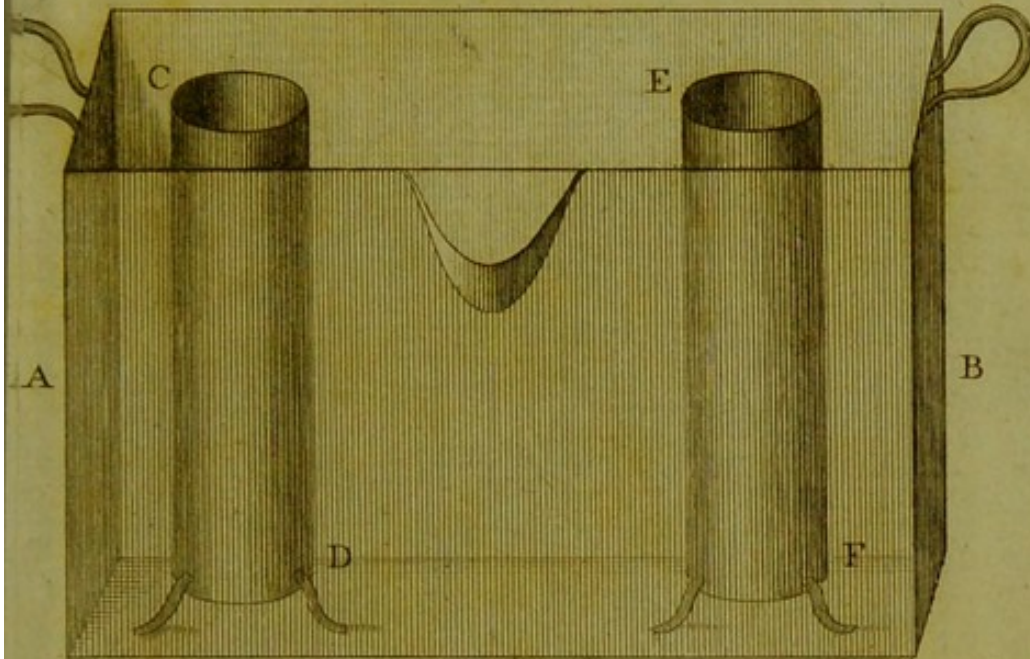


FIG. 3.

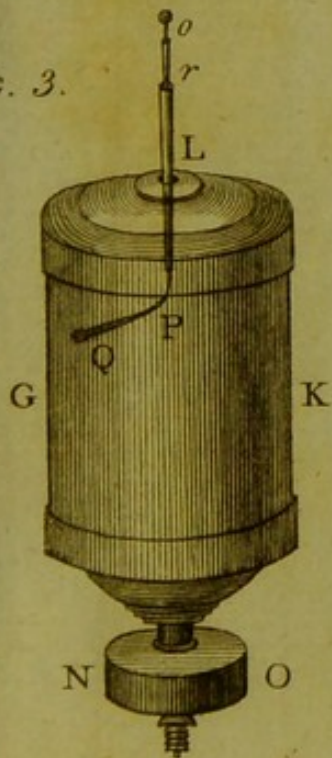


FIG. 4.

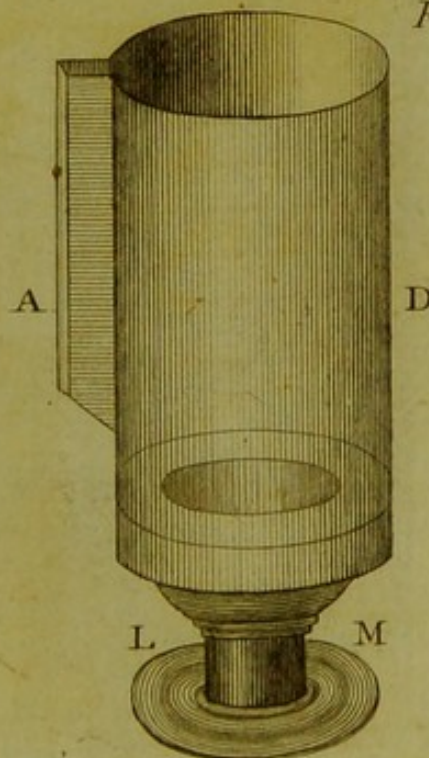




FIG. 5.

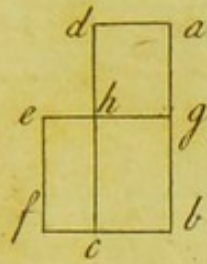


FIG. 3.

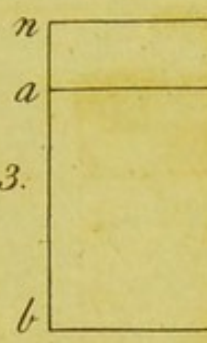


FIG. 4.

