COUNT *RUMFORD's*

EXPERIMENTAL ESSAYS, POLITICAL, ECONOMICAL, AND PHILOSOPHICAL.

ESSAY VIII.

Of the Propagation of Heat in various Subſtances.

ESSAY IX.

An Inquiry concerning the Source of the Heat excited by Friction.

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POLITICAL, ECONOMICAL,

AND

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VOL. II.

ESSAYS,

POLITICAL, ECONOMICAL,

AND

PHILOSOPHICAL.

By BENJAMIN Count of RUMFORD,

KNIGHT OF THE ORDERS OF THE WHITE EAGLE, AND ST. STANISLAUS; *Chamberlain, Privy Counsellor of State, and Lieutenant-General in the Service of his Most Serene Highness the* ELECTOR PALATINE, *Reigning* DUKE *of* BAVARIA; *Colonel of his Regiment of Artillery, and Commander in Chief of the General Staff of his Army*; F. R. S. Acad. R. Hiber. Berol. Elec. Boicœ. Palat. et Amer. Soc.

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OF THE

EIGHTH ESSAY.

CONTENTS.

ESSAY VIII.

OF THE PROPAGATION OF HEAT IN VARIOUS SUBSTANCES:

BEING

An Account of a Number of New Experiments made with a View to the Investigation of the Causes of the Warmth of Natural and Artificial Clothing.

First published in the Philosophical Transactions.

INTRODUCTION.

THIS Eſſay contains nothing that will be new to philoſophical readers; for it is little more than the ſubſtance of two Papers which have already appeared in the Philoſophical Tranſactions of the Royal Society of London; one in the year 1786; and the other (for which the Author had the honour to receive from the Society the Copleian Annual Medal) in the year 1792.

As reference has frequently been made to theſe Papers in ſeveral of the preceding Eſſays; and as many of the Experiments of which an account is given in them are not only intereſting in themſelves, but are neceſſary to be known in all their details in order to judge of ſeveral important concluſions that have been founded on their reſults, the Author has thought that it would not be improper to republiſh them under the preſent form. He was alſo deſirous of adding the ſubſtance of thoſe Papers to his Sixth and Seventh Eſſays, in order that all that he has written on the *Science* of *Heat* might be brought together in one volume.

The Eſſays which are deſtined to compoſe the next volume (many of which are already in great forwardneſs) are all on practical ſubjects of a popular nature, and of general utility; and on that account it was judged beſt to keep them ſeparate from thoſe contained in this volume, which partake more of the nature of abſtruſe philoſophical inveſtigations.

Various unforeſeen events have contributed to retard the publication of the promiſed Eſſays on Kitchen Fire-places—on Cottage Fire-places—and on Clothing; but the Author has well-founded hopes of being able to bring them forward in the courſe of a few months.

ESSAY VIII.

Of the PROPAGATION of HEAT in various Subſtances.

CHAP. I.

An Account of the Inſtruments that were prepared for making the propoſed Experiments.—A Thermometer is conſtructed whoſe Bulb is ſurrounded by a TORRICELLIAN VACUUM.*—Heat is found to paſs in a Torricellian Vacuum with greater Difficulty than in Air.—Relative conducting Powers of a Torricellian Vacuum and of Air with regard to Heat determined by Experiment.—Relative conducting Powers of dry Air and of moiſt Air.—Relative conducting Powers of Air of different Degrees of Denſity.—Relative conducting Powers of* MERCURY; WATER; AIR; *and a* TORRICELLIAN VACUUM.

[Read before the ROYAL SOCIETY, March 9, 1786.]

EXAMINING the conducting power of air, and of various other fluid and ſolid bodies, with regard to Heat, I was led to examine the conducting power of the *Torricellian vacuum.* From the ſtriking analogy between the electric fluid and Heat reſpecting their conductors and non-conductors, (having

(having found that bodies, in general, which are conductors of the electric fluid, are likewife good conductors of Heat, and, on the contrary, that electric bodies, or fuch as are bad conductors of the electric fluid, are likewife bad conductors of Heat,) I was led to imagine that the Torricellian vacuum, which is known to afford fo ready a paffage to the electric fluid, would alfo have afforded a ready paffage to Heat.

The common experiments of heating and cooling bodies under the receiver of an air-pump I conceived to be inadequate to determining this queftion; not only on account of the impoffibility of making a perfect void of air by means of the pump; but alfo on account of the moift vapour, which exhaling from the wet leather and the oil ufed in the machine, expands under the receiver, and fills it with a watery fluid, which, though extremely rare, is yet capable of conducting a great deal of Heat: I had recourfe therefore to other contrivances.

I took a thermometer, unfilled, the diameter of whofe bulb (which was globular) was juft half an inch, Paris meafure, and fixed it in the centre of a hollow glafs ball of the diameter of $1\frac{3}{4}$ Paris inch, in fuch a manner, that the fhort neck or opening of the ball being foldered faft to the tube of the thermometer $7\frac{1}{2}$ lines above its bulb, the bulb of the thermometer remained fixed in the centre of the ball, and confequently was cut off from all communication with the external air. In the bottom of the glafs ball was fixed a fmall hollow tube or point, which projecting outwards was foldered to the end of a common barometer tube about 32 inches in length, and by means of this opening the ſpace between the internal ſurface of the glaſs ball and the bulb of the thermometer was filled with hot mercury, which had been previouſly freed of air and moiſture by boiling. The ball, and alſo the barometrical tube attached to it, being filled with mercury, the tube was carefully inverted, and its open end placed in a bowl in which there was a quantity of mercury. The inſtrument now became a barometer, and the mercury deſcending from the ball (which was now uppermoſt) left the ſpace ſurrounding the bulb of the thermometer free of air. The mercury having totally quitted the glaſs ball, and having ſunk in the tube to the height of 28 inches, (being the height of the mercury in the common barometer at that time) with a lamp and a blow-pipe I melted the tube together, or ſealed it hermetically, about three-quarters of an inch below the ball, and cutting it at this place with a fine file, I ſeparated the ball from the long barometrical tube. The thermometer being afterwards filled with mercury in the common way, I now poſſeſſed a thermometer whoſe bulb was confined in the centre of a *Torricellian vacuum*, and which ſerved at the ſame time as the body to be heated, and as the inſtrument for meaſuring the Heat communicated.

Experiment, N° 1.

With this inſtrument (ſee Fig. 1.) I made the following Experiment. Having plunged it into a veffel filled with water, warm to the 18th degree of REAUMUR'S fcale, and fuffered it to remain there till it had acquired the temperature of the water, that is to fay, till the mercury in the inclofed thermometer ftood at 18°, I took it out of this veffel and plunged it fuddenly into a veffel of boiling water, and holding it in the water (which was kept conftantly boiling) by the end of the tube, in fuch a manner that the glafs ball, in the centre of which was the bulb of the thermometer, was juft fubmerged, I obferved the number of degrees to which the mercury in the thermometer had arifen at different periods of time, counted from the moment of its immerfion. Thus, after it had remained in the boiling water 1 min. 30 fec. I found the mercury had rifen from 18° to 27°. After 4 minutes had elapfed, it had rifen to $44^{\circ}\frac{9}{10}$; and at the end of 5 minutes it had rifen to $48^{\circ}\frac{2}{10}$.

Experiment, N° 2.

Taking it now out of the boiling water I fuffered it to cool gradually in the air, and after it had acquired the temperature of the atmofphere, which was that of 15° R. (the weather being perfectly fine) I broke off a little piece from the point of the fmall tube which remained at the bottom of the glafs ball, where it had been hermetically fealed, and of courfe the atmofpheric air rufhed immediately into the ball. The ball furrounding the bulb of the thermometer being now filled with air, (inftead of being emptied of air, as it was in the before-mentioned Experiment,) I refealed the end of of the ſmall tube at the bottom of the glaſs ball hermetically, and by that means cut off all communication between the air confined in the ball and the external air; and with the inſtrument ſo prepared I repeated the Experiment before-mentioned; that is to ſay, I put it into wáter warmed to 18°, and when it had acquired the temperature of the water, I plunged it into boiling water, and obſerved the times of the aſcent of the mercury in the thermometer. They were as follows:

	Time elapſed.	Heat acquired.
Heat at the moment of being plunged into the boiling water, - - - -		18° R.
	M. S.	°
After having remained in the boiling water	0 45	27
	1 0	$34\frac{1}{10}$
	2 10	$44\frac{9}{10}$
	2 40	$48\frac{2}{10}$
	4 0	$56\frac{2}{10}$
	5 0	$60\frac{0}{10}$

From the rufult of theſe Experiments it appears evidently, that the Torricellian vacuum, which affords ſo ready a paſſage to the electric fluid, ſo far from being a good conductor of Heat, is a much worſe conductor of it than common air, which of itſelf is reckoned among the worſt: for in the laſt Experiment, when the bulb of the thermometer was ſurrounded with air, and the inſtrument was plunged into boiling water, the mercury roſe from 18° to 27° in 45 ſeconds; but in the former Experiment, when it was ſurrounded by a Torricellian vacuum, it required to remain in the boiling water 1 minute

minute 30 ſeconds = 90 ſeconds, to acquire that degree of heat. In the vacuum it required 5 minutes to riſe to $48°\frac{2}{10}$; but in air it roſe to that height in 2 minutes 40 ſeconds; and the proportion of the times in the other obſervations is nearly the ſame, as will appear by the following Table.

	The bulb of the thermometer placed in the centre of the glaſs ball, and			
	ſurrounded by a Torricellian vacuum. (Exp. N° 1.)		ſurrounded by air. (Exp. N° 2.)	
	Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
Upon being plunged into boiling water		18°		18°
	M. S.	°	M. S.	°
After remaining in it	1 30	27	0 45	27
	——	——	1 0	$30\frac{4}{10}$
	4 0	$44\frac{9}{10}$	2 10	$44\frac{9}{10}$
	5 0	$48\frac{2}{10}$	2 40	$48\frac{2}{10}$
	——	——	4 0	$56\frac{2}{10}$
	——	——	5 0	$60\frac{0}{10}$

Theſe Experiments were made at Manheim, upon the firſt day of July 1785, in the preſence of Profeſſor Hemmer, of the Electoral Academy of Sciences of Manheim, and Charles Artaria, meteorological inſtrument maker to the academy, by whom I was aſſiſted in making them.

Finding the conſtruction of the inſtrument made uſe of in theſe Experiments attended with much trouble and riſk, on account of the difficulty of ſoldering the glaſs ball to the tube of the thermometer without at the ſame time either cloſing up, or otherwiſe injuring, the bore of the tube, I had

had recourſe to another contrivance much more commodious, and much eaſier in the execution.

At the end of a glaſs tube or cylinder about eleven inches in length, and near three quarters of an inch in diameter internally, I cauſed a hollow globe to be blown $1\frac{1}{2}$ inch in diameter, with an opening in the bottom of it correſponding with the bore of the tube, and equal to it in diameter, leaving to the opening a neck or ſhort tube, about an inch in length. Having a thermometer prepared, whoſe bulb was juſt half an inch in diameter, and whoſe freezing point fell at about $2\frac{3}{4}$ inches above its bulb, I graduated its tube according to Reaumur's ſcale, beginning at 0°, and marking that point, and alſo every tenth degree above it to 80°, with threads of fine ſilk bound round it, which being moiſtened with lac varniſh adhered firmly to the tube. This thermometer I introduced into the glaſs cylinder and globe juſt deſcribed, by the opening in the bottom of the globe, having firſt choaked the cylinder at about 2 inches from its junction with the globe by heating it, and crowding its ſides inwards towards its axis, leaving only an opening ſufficient to admit the tube of the thermometer. The thermometer being introduced into the cylinder in ſuch a manner that the centre of its bulb coincided with the centre of the globe, I marked a place in the cylinder, about three-quarters of an inch above the 80th degree or boiling point upon the tube of the incloſed thermometer, and taking out the thermometer, I choaked the cylinder again in this place.

Intro-

Introducing now the thermometer for the laſt time, I cloſed the opening at the bottom of the globe at the lamp, taking care before I brought it to the fire, to turn the cylinder upſide down, and to let the bulb of the thermometer fall into the cylinder till it reſted upon the lower choak in the cylinder. By this means the bulb of the thermometer was removed more than 3 inches from the flame of the lamp. The opening at the bottom of the globe being now cloſed, and the bulb of the thermometer being ſuffered to return into the globe, the end of the cylinder was cut off to within about half an inch of the upper choak. This being done, it is plain, that the tube of the thermometer projected beyond the end of the cylinder. Taking hold of the end of the tube, I placed the bulb of the thermometer as nearly as poſſible in the centre of the globe, and obſerving and marking a point in the tube immediately above the upper choak of the cylinder, I turned the cylinder upſide down, and ſuffering the bulb of the thermometer to enter the cylinder, and reſt upon the firſt or lower choak, (by which means the end of the tube of the thermometer came further out of the cylinder) the end of the tube was cut off at the mark juſt mentioned, (care having firſt been taken to melt the internal cavity or bore of the tube together at that place) and a ſmall ſolid ball of glaſs, a little larger than the internal diameter or opening of the choak, was ſoldered to the end of the tube, forming a little button or knob, which reſting upon the upper choak of the cylinder ſerved to ſuſpend the thermometer

meter in ſuch a manner that the centre of its bulb coincided with the centre of the globe in which it was ſhut up. The end of the cylinder above the upper choak being now heated and drawn out to a point, or rather being formed into the figure of the fruſtum of a hollow cone, the end of it was ſoldered to the end of a barometrical tube, by the help of which the cavity of the cylinder and globe containing the thermometer was completely voided of air with mercury; when, the end of the cylinder being hermetically ſealed, the barometrical tube was detached from it with a file, and the thermometer was left completely ſhut up in a Torricellian vacuum, the centre of the bulb of the thermometer being confined in the centre of the glaſs globe, without touching it in any part, by means of the two choaks in the cylinder, and the button upon the end of the tube. (See Fig. 2.)

Of theſe inſtruments I provided myſelf with two, as nearly as poſſible of the ſame dimenſions; the one, which I ſhall call N° 1. being voided of air, in the manner above deſcribed; the other, N° 2. being filled with air, and hermetically ſealed.

With theſe two inſtruments (ſee Fig. 2.) I made the following Experiments upon the 11th of July laſt at Manheim, between the hours of ten and twelve, the weather being very fine and clear, the mercury in the barometer ſtanding at 27 inches 11 lines, Reaumur's thermometer at 15°, and the quill hygrometer of the academy of Manheim at 47°.

Experiments,

Experiments, N° 3, 4, 5, and 6.

Putting both the inſtruments into a mixture of pounded ice and water, I let them remain there till the mercury in the incloſed thermometers reſted at the point o°, that is to ſay, till they had acquired exactly the temperature of the cold mixture; and then taking them out of it I plunged them ſuddenly into a large veſſel of boiling water, and obſerved the time required for the mercury to riſe in the thermometers from ten degrees to ten degrees, from o° to 80°, taking care to keep the water conſtantly boiling during the whole of this time, and taking care alſo to keep the inſtruments immerſed to the ſame depth, that is to ſay, juſt ſo deep that the point o° of the incloſed thermometer was even with the ſurface of the water.

Theſe Experiments I repeated twice with the utmoſt care; and the following Table gives the reſult of them.

Thermometer N° 1.

Its bulb half an inch in diameter, ſhut up in the centre of a hollow glaſs globe, $1\frac{1}{2}$ inch in diameter, *void of air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed. Exp. N° 3.	Time elapſed. Exp. N° 4.	Heat acquired.
		0°
M. S.	M. S.	0
0 51	0 51	10
0 59	0 59	20
1 1	1 2	30
1 18	1 22	40
1 24	1 23	50
2 0	1 51	60
3 30	3 6	70
11 41	10 27	80
22 44	21 1	= total time of heating from 0° to 80°.

Total time from 0° to 70°:

	M. S.
In Exp. N° 3.=	11 3
In Exp. N° 4.=	10 34
Medium=	10 $48\frac{1}{2}$.

Thermometer N° 2.

Its bulb half an inch in diameter, ſhut up in the centre of a hollow glaſs globe, $1\frac{1}{2}$ inch in diameter, *filled with air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed. Exp. N° 5.	Time elapſed. Exp. N° 6.	Heat acquired.
		0°
M. S.	M. S.	0
0 30	0 30	10
0 35	0 37	20
0 41	0 41	30
0 49	0 53	40
1 1	0 59	50
1 24	1 20	60
2 45	2 25	70
9 10	9 38	80
16 55	17 3	= total time of heating from 0° to 80°.

Total time from 0° to 70°:

	M. S.
In Exp. N° 5.=	7 45
In Exp. N° 6.=	7 25
Medium=	7 35

It appears from theſe Experiments that the conducting power of air to that of the Torricellian vacuum, under the circumſtances deſcribed, is as $7\frac{35}{60}$ to $10\frac{48\frac{1}{2}}{60}$ inverſely, or as 1000 to 702 nearly; for the quantities of Heat communicated being equal, the intenſity of the communication is as the times inverſely.

In theſe Experiments the Heat paſſed through the ſurrounding medium *into* the bulb of the thermometer:

meter: in order to reverſe the Experiment, and make the Heat paſs *out of* the thermometer, I put the inſtruments into boiling water, and let them remain therein till they had acquired the temperature of the water; that is to ſay, till the mercury in the incloſed thermometers ſtood at 80°; and then, taking them out of the boiling water, I plunged them ſuddenly into a mixture of water and pounded ice, and moving them about continually in this mixture, I obſerved the times employed in cooling as follows:

Thermometer N° 1. Surrounded by a *Torricellian vacuum.* *Taken out of boiling water, and plunged into freezing water.*			*Thermometer* N° 2. Surrounded by *air.* *Taken out of boiling water, and plunged into freezing water.*		
Time elapſed. Exp. N° 7.	Time elapſed. Exp. N° 8.	Heat loſt.	Time elapſed. Exp. N° 9.	Time elapſed. Exp. N° 10.	Heat loſt.
		80°			80°
M. S.	M. S.	o	M. S.	M. S.	o
1 2	0 54	70	0 33	0 33	70
0 58	1 2	60	0 39	0 34	60
1 17	1 18	50	0 44	0 44	50
1 46	1 37	40	0 55	0 55	40
2 5	2 16	30	1 17	1 18	30
3 14	3 10	20	1 57	1 57	20
5 42	5 59	10	3 44	3 40	10
Not obſerved.	Not obſerved.	0	40 10	Not obſerved.	0
Total time of cooling from 80° to 10°. M. S. In Exp. N° 7.=16 4 In Exp. N° 8.=16 16 Medium=16 10			Total time of cooling from 80° to 10°. M. S. In Exp. N° 9.=9 49 In Exp. N° 10.=9 41 Medium=9 45		

By

By theſe Experiments it appears, that the conducting power of air is to that of the Torricellian vacuum as $9\frac{45}{60}$ to $16\frac{10}{60}$ inverſely, or as 1000 to 603.

To determine whether the ſame law would hold good when the heated thermometers, inſtead of being plunged into freezing water, were ſuffered to cool in the open air, I made the following Experiments. The thermometers N° 1 and N° 2 being again heated in boiling water, as in the laſt Experiments, I took them out of the water, and ſuſpended them in the middle of a large room, where the air (which appeared to be perfectly at reſt, the windows and doors being all ſhut) was warm to the 16th degree of Reaumur's thermometer, and the times of cooling were obſerved as follows:

(Exp. N° 11.) *Thermometer* N° 1. Surrounded by a Torricellian vacuum. *Heated to* 80°, *and ſuſpended in the open air warm to* 16°.		(Exp. N° 12.) *Thermometer* N° 2. Surrounded by air. *Heated to* 80°, *and ſuſpended in the open air warm to* 16°.	
Time elapſed.	Heat loſt.	Time elapſed.	Heat loſt.
——	80°	——	80°
M. S.	°	M. S.	°
Not obſerved.	70	Not obſerved.	70
1 24	60	0 51	60
1 44	50	1 5	50
2 28	40	1 34	40
4 16	30	2 41	30
10 12 = total time employed in cooling from 70° to 30°.		6 11 = total time employed in cooling from 70° to 30°.	

Here the difference in the conducting powers of air and of the Torricellian vacuum appears to be

 nearly

nearly the ſame as in the foregoing Experiments, being as $6\frac{11}{60}$ to $10\frac{12}{60}$ inverſely, or as 1000 to 605. I could not obſerve the time of cooling from 80° to 70°, being at that time buſied in ſuſpending the inſtruments.

As it might poſſibly be objected to the concluſions drawn from theſe Experiments that, notwithſtanding all the care that was taken in the conſtructing of the two inſtruments made uſe of that they ſhould be perfectly alike, yet they might in reality be ſo far different either in ſhape or ſize, as to occaſion a very ſenſible error in the reſult of the Experiments; to remove theſe doubts I made the following Experiments:

In the morning towards eleven o'clock, the weather being remarkably fine, the mercury in the barometer ſtanding at 27 inches 11 lines, Reaumur's thermometer at 15°, and the hygrometer at 47°, I repeated the Experiment N° 3. (of heating the thermometer N° 1 in boiling water, &c.) and immediately afterwards opened the cylinder containing the thermometer at its upper end, where it had been ſealed, and letting the air into it, I re-ſealed it hermetically, and repeated the Experiment again with the ſame inſtrument, the thermometer being now ſurrounded with air, like the thermometer N° 2.

The reſult of theſe Experiments, which may be ſeen in the following Table, ſhews evidently, that the error ariſing from the difference of the ſhapes or dimenſions of the two inſtruments in queſtion was inconſiderable, if not totally imperceptible.

(Exp. N° 13.) *Thermometer* N° 1. Its bulb half an inch in diameter ſhut up in the centre of a glaſs globe 1½ inch in diameter, *voided of air*, and hermetically ſealed. *Taken out of freezing water, and plunged into boiling water.*		(Exp. N° 14.) *The ſame Thermometer* (N° 1.) The glaſs globe, containing the bulb of the thermometer, being now *filled with air*, and hermetically ſealed. *Taken out of freezing water, and plunged into boiling water.*	
Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
	0°		0°
M. S.	0	M. S.	0
0 55	10	0 32	10
0 55	20	0 32	20
1 7	30	0 43	30
1 15	40	0 50	40
1 29	50	1 1	50
2 2	60	1 24	60
3 21	70	2 38	70
13 44	80	10 25	80
24 48 = total time of heating from 0° to 80°. Total time from 0° to 70° = 11′ 4″.		18 5 = total time of heating from 0° to 80°. Total time from 0° to 70° = 7′ 40″.	

It appears, therefore, from theſe Experiments, that the conducting power of common atmoſpheric air is to that of the Torricellian vacuum as $7\frac{40}{60}$ to $11\frac{4}{60}$ inverſely, or as 1000 to 602; which differs but very little from the reſult of all the foregoing Experiments.

Notwithſtanding that it appeared, from the reſult of theſe laſt Experiments, that any difference there might poſſibly have been in the forms or dimenſions of the inſtruments N° 1 and N° 2 could hardly have produced any ſenſible error in the reſult of the Experiments in queſtion; I was willing, however, to ſee how far any con-

 ſiderable

fiderable alterations of fize in the inftrument would affect the Experiment: I therefore provided myfelf with another inftrument which I fhall call *Thermometer* N° 3. different from thofe already defcribed in fize, and a little different in its conftruction.

The bulb of the thermometer was of the fame form and fize as in the inftruments N° 1 and N° 2. that is to fay, it was globular, and half an inch in diameter; but the glafs globe, in the centre of which it was confined, was much larger, being 3 inches $7\frac{1}{2}$ lines in diameter; and the bore of the tube of the thermometer was much finer, and confequently its length, and the divifions of its fcale, were greater. The divifions were marked upon the tube with threads of filk of different colours at every tenth degree, from 0° to 80°, as in the before-mentioned inftruments. The tube or cylinder belonging to the glafs globe was 8 lines in diameter, a little longer than the tube of the thermometer, and perfectly cylindrical from its upper end to its junction with the globe, being without any choak; the thermometer being confined in the centre of the globe by a different contrivance, which was as follows. To the opening of the cylinder was fitted a ftopple of dry wood, covered with a coating of hard varnifh, through the centre or axis of which paffed the end of the tube of the thermometer: this ftopple confined the tube in the axis of the cylinder at its upper end. To confine it at its lower end, there was fitted to it a fmall fteel fpring, a little below the point 0°; which, being faftened to the tube of the thermometer,

meter, had three elaſtic points projecting outwards, which, preſſing againſt the inſide of the cylinder, confined the thermometer in its place. The total length of this inſtrument, from the bottom of the globe to the upper end of the cylinder, was 18 inches, and the freezing point upon the thermometer fell about 3 inches above the bulb; conſequently this point lay about 1½ inch above the junction of the cylinder with the globe, when the thermometer was confined in its place, the centre of its bulb coinciding with the centre of the globe. Through the ſtopple which cloſed the end of the cylinder paſſed two ſmall glaſs tubes, about a line in diameter, which being about a line longer than the ſtopple were cloſed occaſionally with ſmall ſtopples fitted to their bores. Theſe tubes (which were fitted exactly in the holes bored in the great ſtopple of the cylinder to receive them, and fixed in their places with cement) ſerved to convey air, or any other fluid, into the glaſs ball, without its being neceſſary to remove the ſtopple cloſing the end of the cylinder; which ſtopple, in order to prevent the poſition of the thermometer from being eaſily deranged, was cemented in its place.

I have been the more particular in the deſcription of theſe inſtruments, as I conceive it to be abſolutely neceſſary to have a perfect idea of them in order to judge of the Experiments made with them, and of their reſults.

With the inſtrument laſt deſcribed (which I have called *Thermometer* N° 3.) I made the following

Experiment. It was upon the 18th of July 1785, in the afternoon, the weather variable, alternate clouds and fun-fhine; wind ftrong at S. E. with now and then a fprinkling of rain; barometer at 27 inches 10½ lines, thermometer at 18°¼, and hygrometer variable from 44° to extreme moifture.

In order to compare the refult of the Experiment made with this inftrument with thofe made with the thermometer N° 2. I have placed together in the fame Table the different Experiments made with them.

(Exp. N° 15.) *Thermometer* N° 3. Its bulb half an inch in diameter, fhut up in the centre of a glafs tube, 3 inches 7½ lines in diameter, and furrounded by *air*. *Taken out of freezing water, and plunged into boiling water.*		(Exp. N° 4 and N° 5.) *Thermometer* N° 2. Its bulb half an inch in diameter, fhut up in the centre of a glafs globe, 1½ inch in diameter, and furrounded by *air*. *Taken out of freezing water, and plunged into boiling water.*			
		Time elapfed.			Heat acquired.
Time elapfed.	Heat acquired.	Exp. N° 4.	Exp. N° 5.	Medium.	
	0°				0°
M. S.	0	M. S.	M. S.	M. S.	0
0 33	10	0 30	0 30	0 30	10
0 38	20	0 35	0 37	0 36	20
0 54	30	0 41	0 41	0 41	30
0 51	40	0 49	0 53	0 51	40
1 7	50	1 1	0 59	1 0	50
1 28	60	1 24	1 20	1 22	60
2 28	70	2 45	2 25	2 35	70
9 0	80	9 10	9 38	9 24	80
16 59 = total time of heating from 0° to 80°. Time from 0° to 70° = 7′ 59″.		16 55	17 3	16 59 = total time of heating from 0° to 80°. Time from 0° to 70° = 7′ 35″.	

If the agreement of thefe Experiments with the thermometers N° 2 and N° 3 furprifed me, I was not lefs furprifed with their difagreement in the Experiment which follows:

Experi-

Experiment, N° 16.

Taking the thermometer N° 3. out of the boiling water, I immediately ſuſpended it in the middle of a large room, where the air, which was quiet, was at the temperature of 18°¼ R. and obſerved the times of cooling as follows:

Time elapſed.	Heat loſt.
——	80°
M. S.	°
1 55	70
0 12	60
0 33	50
2 15	40
4 0	30
9 55 = total time of cooling from 80° to 30°.	

Time from 70° to 30° = 8′ 0″; but in the Experiment No. 12. with the thermometer N° 2, the time employed in cooling from 70° to 30° was only 6′ 11″. In this Experiment, with the thermometer N° 3. the time employed in cooling from 60° to 30° was 7′ 48″; but in the above-mentioned Experiment, with the thermometer N° 2. it was only 5′ 20″. It is true, the air of the room was ſomewhat cooler when the former Experiment was made, than when this latter was made, with the thermometer N° 3; but this difference of temperature, which was only 2°¼, (in the former caſe the thermometer in the room ſtanding at 16°, and in the latter at 18°¼,) certainly could not have occaſioned the whole of the apparent difference in the reſults of the Experiments.

Does air receive Heat more readily than it parts with it? This is a queſtion highly deſerving of further inveſtigation, and I hope to be able to give it a full examination in the courſe of my projected inquiries; but leaving it for the preſent, I ſhall proceed to give an account of the Experiments which I have already made. Conceiving it to be a ſtep of conſiderable importance towards coming at a further knowledge of the nature of Heat, to aſcertain, by indiſputable evidence, its paſſage through the Torricellian vacuum, and to determine, with as much preciſion as poſſible, the law of its motions in that medium; and being apprehenſive that doubts might ariſe with reſpect to the Experiments before deſcribed, on account of the contact of the tubes of the incloſed thermometers in the inſtruments made uſe of with the containing glaſs globes, or rather with their cylinders: by means of which (it might be ſuſpected) that a certain quantity, if not all the Heat acquired, might poſſibly be communicated; to put this matter beyond all doubt, I made the following Experiment.

In the middle of a glaſs body, of a pear-like form, about 8 inches long, and 2½ inches in its greateſt diameter, I ſuſpended a ſmall mercurial thermometer, 5½ inches long, by a fine thread of ſilk, in ſuch a manner that neither the bulb of the thermometer, nor its tube, touched the containing glaſs body in any part. The tube of the thermometer

mometer was graduated, and marked with fine threads of ſilk of different colours, bound round it, as in the thermometers belonging to the other inſtruments already deſcribed; and the thermometer was ſuſpended in its place by means of a ſmall ſteel ſpring, to which the end of the thread of ſilk which held the thermometer being attached, it (the ſpring) was forced into a ſmall globular protuberance or cavity, blown in the upper extremity of the glaſs body, about half an inch in diameter, where the ſpring remaining, the thermometer neceſſarily remained ſuſpended in the axis of the glaſs body. There was an opening at the bottom of the glaſs body, through which the thermometer was introduced; and a barometrical tube being ſoldered to this opening, the inſide of the glaſs body was voided of air by means of mercury; and this opening being afterwards ſealed hermetically, and the barometrical tube being taken away, the thermometer was left ſuſpended in a Torricellian vacuum.

In this inſtrument, as the incloſed thermometer did not touch the containing glaſs body in any part, on the contrary, being diſtant from its internal ſurface an inch or more in every part, it is clear, that whatever Heat paſſed *into* or *out of* the thermometer muſt have paſſed *through* the ſurrounding Torricellian vacuum: for it cannot be ſuppoſed, that the fine thread of ſilk, by which the thermometer was ſuſpended, was capable of conducting any Heat at all, or at leaſt any ſenſible quantity. I therefore flattered myſelf with hopes of being able,

able, with the affiftance of this inftrument, to determine pofitively with regard to the paffage of Heat in the Torricellian vacuum: and this, I think, I have done, notwithftanding an unfortunate accident that put it out of my power to purfue the Experiments fo far as I intended.

This inftrument being fitted to a fmall ftand or foot of wood, in fuch a manner that the glafs body remained in a perpendicular fituation, I placed it in my room, by the fide of another inclofed thermometer (N° 2.), which was furrounded by air, and obferved the effects produced on it by the variation of Heat in the atmofphere. I foon difcovered, by the motion of the mercury in the inclofed thermometer, that the Heat paffed through the Torricellian vacuum; but it appeared plainly from the fluggifhnefs, or great infenfibility of the thermometer, that the Heat paffed with much greater difficulty in this medium than in common air. I now plunged both the thermometers into a bucket of cold water; and I obferved that the mercury in the thermometer furrounded by air defcended much fafter than that in the thermometer furrounded by the Torricellian vacuum. I took them out of the cold water, and plunged them into a veffel of hot water (having no conveniencies at hand to repeat the Experiment in due form with the freezing and with the boiling water); and the thermometer furrounded by the Torricellian vacuum appeared ftill to be much more infenfible or fluggifh than that furrounded by air.

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Theſe trials were quite ſufficient to convince me of the paſſage of Heat in the Torricellian vacuum, and alſo of the greater difficulty of its paſſage in that medium than in common air; but, not ſatisfied to reſt my inquiries here, I took the firſt opportunity that offered, and ſet myſelf to repeat the Experiments which I had before made with the inſtruments N° 1 and N° 2. I plunged this inſtrument into a mixture of pounded ice and water, where I let it remain till the mercury in the incloſed thermometer had deſcended to 0°; when, taking it out of this cold mixture, I plunged it ſuddenly into a veſſel of boiling water, and prepared myſelf to obſerve the aſcent of the mercury in the incloſed thermometer, as in the foregoing Experiments; but unfortunately the moment the end of the glaſs body touched the boiling water, it cracked with the Heat at the point where it had been hermetically ſealed, and the water ruſhing into the body, ſpoiled the Experiment: and I have not ſince had an opportunity of providing myſelf with another inſtrument to repeat it.

It having been my intention from the beginning to examine the conducting powers of the artificial airs or gaſſes, the thermometer N° 3. was conſtructed with a view to thoſe Experiments; and having now provided myſelf with a ſtock of thoſe different kinds of airs, I began with *fixed air*, with which, by means of water, I filled the globe and cylinder containing the thermometer; and ſtopping up the two holes in the great ſtopple cloſing the end of the cylinder, I expoſed the inſtrument in freezing

freezing water till the mercury in the incloſed thermometer had deſcended to 0°; when, taking it out of the freezing water, I plunged it into a large veſſel of boiling water, and prepared myſelf to obſerve the times of heating, as in the former caſes; but an accident happened, which ſuddenly put a ſtop to the Experiment. Immediately upon plunging the inſtrument into the boiling water, the mercury began to riſe in the thermometer with ſuch uncommon celerity, that it had paſſed the firſt diviſion upon the tube (which marked the 10th degree, according to REAUMUR's ſcale) before I was aware of its being yet in motion; and having thus miſſed the opportunity of obſerving the time elapſed when the mercury arrived at that point, I was preparing to obſerve its paſſage of the next, when all of a ſudden the ſtopple cloſing the end of the cylinder was blown up the chimney with a great exploſion, and the thermometer, which, being cemented to it by its tube, was taken along with it, and was broken to pieces, and deſtroyed in its fall.

This unfortunate Experiment, though it put a ſtop for the time to the inquiries propoſed, opened the way to other reſearches not leſs intereſting. Suſpecting that the exploſion was occaſioned by the rarefaction of the water which remained attached to the inſide of the globe and cylinder after the operation of filling them with fixed air; and thinking it more than probable, that the uncommon celerity with which the mercury roſe in the thermometer was principally owing to the ſame cauſe,

cauſe, I was led to examine the conducting power of *moiſt air*, or air ſaturated with water.

For this Experiment I provided myſelf with a new thermometer N° 4. the bulb of which, being of the ſame form as thoſe already deſcribed (*viz.* globular) was alſo of the ſame ſize, or half an inch in diameter. To receive this thermometer a glaſs cylinder was provided, 8 lines in diameter, and about 14 inches long, and terminated at one end by a globe 1½ inch in diameter. In the centre of this globe the bulb of the thermometer was confined, by means of the ſtopple which cloſed the end of the cylinder; which ſtopple, being near 2 inches long, received the end of the tube of the thermometer into a hole bored through its centre or axis, and confined the thermometer in its place, without the aſſiſtance of any other apparatus. Through this ſtopple two other ſmall holes were bored, and lined with thin glaſs tubes, as in the thermometer N° 3. opening a paſſage into the cylinder, which holes were occaſionally ſtopped up with ſtopples of cork; but to prevent accidents, ſuch as I have before experienced from an exploſion, great care was taken not to preſs theſe ſtopples into their places with any conſiderable force, that they might the more eaſily be blown out by any conſiderable effort of the confined air, or vapour.

Though in this inſtrument the thermometer was not altogether ſo ſteady in its place as in the thermometers N° 1, N° 2, and N° 3. the elaſticity of the tube, and the weight of the mercury in the bulb of the thermometer, occaſioning a ſmall vibration

vibration or trembling of the thermometer upon any ſudden motion or jar; yet I preferred this method to the others, on account of the lower part of this thermometer being entirely free, or ſuſpended in ſuch a manner as not to touch, or have any communication with, the lower part of the cylinder or the globe: for though the quantity of Heat received by the tube of the thermometer at its contact with the cylinder at its choaks, in the inſtruments N° 1 and N° 2. or with the branches of the ſteel ſpring in N° 3. and from thence communicated to the bulb, muſt have been exceedingly ſmall; yet I was deſirous to prevent even that, and every other poſſible cauſe of error or inaccuracy.

Does humidity augment the conducting power of air?

To determine this queſtion I made the following Experiments, the weather being clear and fine, the mercury in the barometer ſtanding at 27 inches 8 lines, the thermometer at 19°, and the hygrometer at 44°.

(Exp. N° 17.) *Thermometer* N° 4. Surrounded by air *dry* to the 44th degree of the quill hygrometer of the Manheim Academy. *Taken out of freezing water, and plunged into boiling water.*		(Exp. N° 18.) *The same Thermometer* (N° 4.) Surrounded by air rendered as *moist* as possible by wetting the inside of the cylinder and globe with water. *Taken out of freezing water, and plunged into boiling water.*	
Time elapsed.	Heat acquired.	Time elapsed.	Heat acquired.
	80°		0°
M. S.	°	M. S.	°
0 34	10	0 6	10
0 39	20	0 4	20
0 44	30	0 5	30
0 51	40	0 9	40
1 6	50	0 18	50
1 35	60	0 26	60
2 40	70	0 43	70
not observed.	80	7 45	80
8 9 = total time of heating from 0° to 70°.		1 51 = total time of heating from 0° to 70°.	

From these Experiments it appears, that the conducting power of air is very much increased by humidity. To see if the same result would obtain when the Experiment was reversed, I now took the thermometer with the *moist air* out of the boiling water, and plunged it into freezing water; and moving it about continually from place to place in the freezing water, I observed the times of cooling, as set down in the following Table. N. B. To compare the result of this Experiment with those made with *dry air*, I have placed on one side in the following Table the Experiment in question, and on the other side the Experiment N° 19. made with the thermometer N° 2.

(Exp. N° 19.) *Thermometer* N° 4. Surrounded by *moiſt air.* *Taken out of boiling water, and plunged into freezing water.*		(Exp. N° 10.) *Thermometer* N° 2. Surrounded by *dry air.* *Taken out of boiling water, and plunged into freezing water.*	
Time elapſed.	Heat loſt.	Time elapſed.	Heat loſt.
	80°		80°
M. S.	°	M. S.	°
0 4	70	0 33	70
0 14	60	0 34	60
0 31	50	0 44	50
0 52	40	0 55	40
1 22	30	1 18	30
2 3	20	1 57	20
4 2	10	3 40	10
9 8 = total time of cooling from 80° to 10°.		9 12 = total time of cooling from 80° to 10°.	

Though the difference of the whole times of cooling from 80° to 10° in theſe two Experiments appears to have been very ſmall, yet the difference of the times taken up by the firſt twenty or thirty degrees from the boiling point is very remarkable, and ſhows with how much greater facility Heat paſſes in moiſt air than in dry air. Even the ſlowneſs with which the mercury in the thermometer N° 4. deſcended in this Experiment from the 30th to the 20th, and from the 20th to the 10th degree, I attribute in ſome meaſure to the great conducting power of the moiſt air with which it was ſurrounded; for the cylinder containing the thermometer and the moiſt air, being not wholly ſubmerged in the freezing water, that part of it which remained out of the water was neceſſarily ſurrounded by the air

air of the atmoſphere; which being much warmer than the water, communicated of its Heat to the glaſs; which, paſſing from thence into the contained moiſt air as ſoon as that air became colder than the external air, was, through that medium, communicated to the bulb of the incloſed thermometer, which prevented its cooling ſo faſt as it would otherwiſe have done. But when the weather becomes cold, I propoſe to repeat this Experiment with variations, in ſuch a manner as to put the matter beyond all doubt. In the mean time I cannot help obſerving, with what infinite wiſdom and goodneſs Divine Providence appears to have guarded us againſt the evil effects of exceſſive Heat and Cold in the atmoſphere; for if it were poſſible for the air to be equally damp during the ſevere cold of the winter months as it ſometimes is in ſummer, its conducting power, and conſequently its apparent coldneſs, when applied to our bodies, would be ſo much increaſed, by ſuch an additional degree of moiſture, that it would become quite intolerable; but, happily for us, its power to hold water in ſolution is diminiſhed, and with it its power to rob us of our animal heat, in proportion as its coldneſs is increaſed. Every body knows how very diſagreeable a very moderate degree of cold is when the air is very damp; and from hence it appears, why the thermometer is not always a juſt meaſure of the apparent or ſenſible Heat of the atmoſphere. If colds or catarrhs are occaſioned by our bodies being robbed of our animal heat, the reaſon is plain why thoſe diſorders prevail moſt during the

 cold

cold autumnal rains, and upon the breaking up of the froſt in the ſpring. It is likewiſe plain from whence it is that ſleeping in damp beds, and inhabiting damp houſes, is ſo very dangerous; and why the evening air is ſo pernicious in ſummer and in autumn, and why it is not ſo during the hard froſts of winter. It has puzzled many very able philoſophers and phyſicians to account for the manner in which the extraordinary degree or rather *quantity* of Heat is generated which an animal body is ſuppoſed to loſe, when expoſed to the cold of winter, above what it communicates to the ſurrounding atmoſphere in warm ſummer weather; but is it not more than probable, that the difference of the quantities of Heat, actually loſt or communicated, is infinitely leſs than what they have imagined? Theſe inquiries are certainly very intereſting; and they are undoubtedly within the reach of well contrived and well conducted Experiments. But taking my leave for the preſent of this curious ſubject of inveſtigation, I haſten to the ſequel of my Experiments.

Finding ſo great a difference in the conducting powers of common air and of the Torricellian vacuum, I was led to examine the conducting powers of common air of different degrees of denſity. For this Experiment I prepared the thermometer N° 4. by ſtopping up one of the ſmall glaſs tubes paſſing through the ſtopple, and opening a paſſage into the cylinder, and by fitting a valve to the external overture of the other. The inſtrument, thus prepared, being put under the receiver of an

air-

air-pump, the air paſſed freely out of the globe and cylinder upon working the machine, but the valve above deſcribed prevented its return upon letting air into the receiver. The gage of the air-pump ſhowed the degree of rarity of the air under the receiver, and conſequently of that filling the globe and cylinder, and immediately ſurrounding the thermometer.

With this inſtrument, the weather being clear and fine, the mercury in the barometer ſtanding at 27 inches 9 lines, the thermometer at 15°, and the hygrometer at 47°, I made the following Experiments.

(Exp. N° 20.) *Thermometer* N° 4. Surrounded by common air, barometer ſtanding at 27 inches 9 lines. *Taken out of freezing water, and plunged into boiling water.*		(Exp. N° 21.) *Thermometer* N° 4. Surrounded by air rarefied by pumping till the barometer-gage ſtood at 6 inches $11\frac{1}{2}$ lines. *Taken out of freezing water, and plunged into boiling water.*		(Exp. N° 22.) *Thermometer* N° 4. Surrounded by air rarefied by pumping till the barometer-gage ſtood at 1 inch 2 lines. *Taken out of freezing water, and plunged into boiling water.*	
Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
	0°		0°		0°
M. S.	0	M. S.	0	M. S.	0
0 31	10	0 31	10	0 29	10
0 40	20	0 38	20	0 36	20
0 41	30	0 44	30	0 49	30
0 47	40	0 51	40	1 1	40
1 4	50	1 7	50	1 1	50
1 25	60	1 19	60	1 24	60
2 28	70	2 27	70	2 31	70
10 17	80	10 21	80	not obſerved.	80
7 36 = total time of heating from 0° to 70°.		7 37 = total time of heating from 0° to 70°.		7 51 = total time of heating from 0° to 70°.	

The reſult of theſe Experiments, I confeſs, ſurpriſed me not a little; but the diſcovery of truth

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being the ſole object of my inquiries (having no favourite theory to defend) it brings no diſappointment along with it, under whatever unexpected ſhape it may appear. I hope that further Experiments may lead to the diſcovery of the cauſe why there is ſo little difference in the conducting powers of air of ſuch very different degrees of rarity, while there is ſo great a difference in the conducting powers of air, and of the Torricellian vacuum. At preſent, I ſhall not venture any conjectures upon the ſubject; but in the mean time I dare to aſſert, that the Experiments I have made may be depended on.

The time of my ſtay at Manheim being expired (having had the honour to attend thither his moſt Serene Highneſs the Elector Palatine, reigning Duke of Bavaria, in his late journey), I was prevented from purſuing theſe inquiries further at that time; but I ſhall not fail to recommence them the firſt leiſure moment I can find, which I fancy will be about the beginning of the month of November. In the mean time, to enable myſelf to purſue them with effect, I am ſparing neither labour nor expence to provide a complete apparatus neceſſary for my purpoſe; and his Electoral Highneſs has been graciouſly pleaſed to order M. Artaria (who is in his ſervice) to come to Munich to aſſiſt me. With ſuch a Patron as his moſt Serene Highneſs, and with ſuch an aſſiſtant as Artaria, I ſhall go on in my purſuits with chearfulneſs. Would to God that my labours might be

as

as uſeful to others as they will be pleaſant to me!

I ſhall conclude this chapter with a ſhort account of ſome Experiments I have made to determine the conducting powers of water and of mercury; and with a table, ſhowing at one view the conducting powers of all the different mediums which I have examined.

Having filled the glaſs globe incloſing the bulb of the thermometer N° 4, firſt with water, and then with mercury, I made the following Experiments, to aſcertain the conducting powers of thoſe two Fluids.

(Exp. N° 23.) *Thermometer* N° 4. Surrounded by *water*. *Taken out of freezing water, and plunged into boiling water.*		(Exp. N° 24, 25, and 26.) *Thermometer* N° 4. Surrounded by *mercury*. *Taken out of freezing water, and plunged into boiling water.*			
Time elapſed.	Heat acquired.	Time elapſed. Ex. N° 24.	Ex. N° 25.	Ex. N° 26.	Heat acquired.
	0°				0°
M. S.	0	M. S.	M. S.	M. S.	
0 19	10	0 5	0 5	0 5	10
0 8	20	0 4	0 2	0 5	20
0 9	30	0 2	0 2	0 4	30
0 11	40	0 4	0 5	0 5	40
0 15	50	0 4	0 4	0 7	50
0 21	60	0 7	0 4	0 8	60
0 34	70	0 15	0 9	0 14	70
2 13	80	Not obſerved.	0 58	Not obſerved.	80
1 57 = total time of heating from 0° to 70°.		0 41	0 31	0 48 = total times of heating from 0° to 70°.	

The total times of heating from 0° to 70° in the three Experiments with mercury being 41 ſeconds,

 31 ſeconds,

31 feconds, and 48 feconds, the mean of thefe times is $36\frac{2}{3}$ feconds; and as in the Experiment with water the time employed in acquiring the fame degree of Heat was 1′ 57″ = 117 feconds, it appears from thefe Experiments, that the conducting power of mercury to that of water, under the circumftances defcribed, is as $36\frac{2}{3}$ to 117 inverfely, or as 1000 to 313. And hence it is plain, why mercury *appears* fo much hotter, and fo much colder, to the touch than water, when in fact it is of the fame temperature: for the force or violence of the fenfation of what appears *hot* or *cold* depends not entirely upon the temperature of the body exciting in us thofe fenfations, or upon the degree of Heat it actually poffeffes, but upon the *quantity* of Heat it is capable of communicating to us, or receiving from us, in any given fhort period of time, or as the intenfity of the communication; and this depends in a great meafure upon the conducting powers of the bodies in queftion.

The fenfation excited in us when we touch any thing that appears to us to be *hot* is the entrance of Heat into our bodies; that of *cold* is its exit; and whatever contributes to facilitate or accelerate this communication adds to the violence of the fenfation. And this is another proof that the thermometer cannot be a juft meafure of the intenfity of the *fenfible* Heat, or Cold, exifting in bodies; or rather, that the touch does not afford us a juft indication of their *real* temperatures.

A Table

A Table *of the* conducting Powers *of the under-mentioned* Mediums *as determined by the fore-going Experiments.*

Therm. Nº 1.	Thermometer Nº 4.						
Taken out of freezing water, and plunged into boiling water.							
Time elapſed.							
Torricellian Vacuum (Exp. Nº 3, 4. and 13.)	Common air, denſity=1, (Exp. Nº 20.)	Rarefied air, denſity=$\frac{1}{4}$ (Exp. Nº 21.)	Rarefied air, denſity=$\frac{1}{24}$ (Exp. Nº 22.)	Moiſt air (Exp. Nº 18.)	Water (Exp. Nº 23.)	Mercury (Exp. Nº 24, 25, and 26.)	Heat acquired.
							0°
M. S.	M. S.	M. S.	M. S.	M. S.	M. S.	M. S.	0
0 52	0 31	0 31	0 29	0 6	0 19	0 5	10
0 58	0 40	0 38	0 36	0 4	0 8	0 $3\frac{2}{3}$	20
1 3	0 41	0 44	0 49	0 5	0 9	0 $2\frac{2}{3}$	30
1 18	0 47	0 51	1 1	0 9	0 11	0 $4\frac{2}{3}$	40
1 25	1 4	1 7	1 1	0 18	0 15	0 . 5	50
1 58	1 25	1 19	1 24	0 26	0 21	0 $6\frac{1}{3}$	60
3 19	2 28	2 27	2 31	0 43	0 34	0 $12\frac{2}{3}$	70
11 57	10 17	10 21	——	7 45	2 13	0 58	80
10 53	7 36	7 37	7 51	1 51	1 57	0 $36\frac{2}{3}$ = total	
times of heating from 0° to 70°.							

In determining the relative conducting powers of theſe mediums, I have compared the times of the heating of the thermometers from 0° to 70° inſtead of taking the whole times from 0° to 80°, and this I have done on account of the ſmall variation in the Heat of the boiling water ariſing from the variation of the weight of the atmoſphere, and alſo on account of the very ſlow motion of the mercury between the 70th and the 80th degrees, and the difficulty of determining the preciſe mo-

ment

ment when the mercury arrives at the 80th degree.

Taking now the conducting power of mercury =1000, the conducting powers of the other mediums, as determined by these Experiments, will be as follows, *viz.*

Mercury - - -	1000
Moist air - - -	330
Water - - -	313
Common air, density=1	$80\frac{41}{100}$
Rarefied air, density=$\frac{1}{4}$	$80\frac{23}{100}$
Rarefied air, density=$\frac{1}{24}$	78
The Torricellian vacuum	55

And in these proportions are the quantities of Heat which these different mediums are capable of transmitting in any given time; and consequently these numbers express the relative *sensible* temperatures of the mediums, as well as their conducting powers. How far these decisions will hold good under a variation of circumstances experiment only can determine. This is certainly a subject of investigation not less curious in itself than it is interesting to mankind; and I wish that what I have done may induce others to turn their attention to this long neglected field of experimental inquiry. For my own part, I am determined not to quit it.

In the future prosecution of these inquiries, I do not mean to confine myself solely to the determining

ing of the conducting powers of Fluids; on the contrary, ſolids, and particularly ſuch bodies as are made uſe of for cloathing, will be principal ſubjects of my future Experiments. I have indeed already begun theſe reſearches, and have made ſome progreſs in them; but I forbear to anticipate a matter which will be the ſubject of a future communication.

CHAP. II.

The relative Warmth of various Substances used in making artificial Cloathing, determined by Experiment.—Relative Warmth of Coverings of the same Thickness, and formed of the same Substance, but of different Densities.—Relative Warmth of Coverings formed of equal Quantities of the same Substance, disposed in different Ways.—Experiments made with a View to determining how far the Power which certain Bodies possess of confining Heat depends on their chemical Properties.—Experiments with Charcoal—*with* Lampblack—*with* Woodashes—*Striking Experiments with* Semen ...podii.—*All these Experiments indicate that the ... occupies the Interstices of Substances ... Coverings for confining Heat, acts a ... that Operation.—Those Sub- ... Air from conducting ... Manner in ... to a* refearches chiefly to thofe points, conceiving that very great advantages to mankind could not fail to be derived from the difcovery of any new facts relative to thefe operations.

If the laws of the communication of Heat from one body to another were known, meafures might be taken with certainty, in all cafes, for confining it, and directing its operations, and this would not only be productive of great œconomy in the articles of fuel and clothing, but would likewife greatly increafe the comforts and conveniencies of life; objects of which the philofopher fhould never lofe fight.

The route which I have followed in this inquiry is that which I thought bid faireft to lead to ufeful difcoveries. Without embarraffing myfelf with any particular theory, I have formed to myfelf a plan of experimental inveftigation, which I conceived would conduct me to the knowledge of *certain facts*, of which we are now ignorant, or very imperfectly informed, and with which it is of confequence that we fhould be made acquainted.

The firft great object which I had in view in this enquiry was to afcertain, if poffible, the caufe of the warmth of certain bodies; or the circumftances upon which their power of confining Heat depends. This, in other words, is no other than to determine the caufe of the conducting and non-conducting power of bodies, with regard to Heat.

To this end I began by determining by actual experiment the relative conducting powers of various

rious bodies of very different natures, both fluids and ſolids, of ſome of which Experiments I have already given an account in the Paper above mentioned, which is publiſhed in the Tranſactions of the Royal Society for the year 1786; I ſhall now, taking up the matter where I left it, give the continuation of the hiſtory of my reſearches.

Having diſcovered that the Torricellian vacuum is a much worſe conductor of Heat than common air, and having aſcertained the relative conducting powers of air, of water, and of mercury, under different circumſtances, I proceeded to examine the conducting powers of various *ſolid bodies*, and particularly of ſuch ſubſtances as are commonly made uſe of for clothing.

The method of making theſe Experiments was as follows: a mercurial thermometer, (ſee Fig. 4,) whoſe bulb was about $\frac{55}{100}$ of an inch in diameter, and its tube, about 10 inches in length, was ſuſpended in the axis of a cylindrical glaſs tube, about $\frac{3}{4}$ of an inch in diameter, ending with a globe $1\frac{6}{10}$ inch in diameter, in ſuch a manner that the centre of the bulb of the thermometer occupied the centre of the globe; and the ſpace between the internal ſurface of the globe and the ſurface of the bulb of the thermometer being filled with the ſubſtance whoſe conducting power was to be determined, the inſtrument was heated in boiling water, and afterwards being plunged into a freezing mixture of pounded ice and water, the times of cooling were obſerved, and noted down.

The

The tube of the thermometer was divided at every tenth degree from 0°, or the point of freezing, to 80°, that of boiling water, and theſe diviſions being marked upon the tube with the point of a diamond, and the cylindrical tube being left empty, the height of the mercury in the tube of the thermometer was ſeen through it.

The thermometer was confined in its place by means of a ſtopple of cork, about $1\frac{1}{2}$ inch long, fitted to the mouth of the cylindrical tube, through the centre of which ſtopple the end of the tube of the thermometer paſſed, and in which it was cemented.

The operation of introducing into the globe the ſubſtances whoſe conducting powers are to be determined, is performed in the following manner; the thermometer being taken out of the cylindrical tube, about two-thirds of the ſubſtance which is to be the ſubject of the Experiment are introduced into the globe; after which, the bulb of the thermometer is introduced a few inches into the cylinder; and, after it, the remainder of the ſubſtance being placed round about the tube of the thermometer; and laſtly, the thermometer being introduced farther into the tube, and being brought into its proper place, that part of the ſubſtance which, being introduced laſt, remains in the cylindrical tube above the bulb of the thermometer, is puſhed down into the globe, and placed equally round the bulb of the thermometer by means of a braſs wire which is paſſed through holes made for that purpoſe in the ſtopple cloſing the end of the cylindrical tube.

As this inſtrument is calculated merely for meaſuring the paſſage of Heat in the ſubſtance whoſe conducting power is examined, I ſhall give it the name of *paſſage-thermometer*, and I ſhall apply the ſame appellation to all other inſtruments conſtructed upon the ſame principles, and for the ſame uſe, which I may in future have occaſion to mention; and as this inſtrument has been ſo particularly deſcribed, both here, and in my former Paper upon the ſubject of Heat, in ſpeaking of any others of the ſame kind in future it will not be neceſſary to enter into ſuch minute details. I ſhall, therefore, only mention their *ſizes*, or the diameters of their bulbs, the diameters of their globes, the diameters of their cylinders, and the lengths and diviſions of their tubes, taking it for granted that this will be quite ſufficient to give a clear idea of the inſtrument.

In moſt of my former Experiments, in order to aſcertain the conducting power of any body, the body being introduced into the globe of the paſſage-thermometer, the inſtrument was cooled to the temperature of freezing water, after which, being taken out of the ice water, it was plunged ſuddenly into boiling water, and the times of heating from ten to ten degrees were obſerved and noted; and I ſaid that theſe times were as the conducting power of the body inverſely; but in the Experiments of which I am now about to give an account, I have in general reverſed the operation; that is to ſay, inſtead

inſtead of obſerving the times of heating, I have firſt heated the body in boiling water, and then plunging it into a mixture of pounded ice and ice-cold water, I have noted the times taken up in cooling.

I have preferred this laſt method to the former, not only on account of the greater eaſe and convenience with which a thermometer, plunged into a mixture of water, may be obſerved, than when placed in a veſſel of boiling water, and ſurrounded by hot ſteam, but alſo on account of the greater accuracy of the Experiment, the heat of boiling water varying with the variations of the preſſure of the atmoſphere: conſequently the Experiments made upon different days will have different reſults, and of courſe, ſtrictly ſpeaking, cannot be compared together; but the temperature of pounded ice and water is ever the ſame, and of courſe the reſults of the Experiments are uniform.

In heating the thermometer, I did not in general bring it to the temperature of the boiling water, as this temperature, as I have juſt obſerved, is variable; but when the mercury had attained the 75° of its ſcale, I immediately took it out of the boiling water, and plunged it into the ice and water; or, which I take to be ſtill more accurate, ſuffering the mercury to riſe a degree or two above 75°, and then taking it out of the boiling water, I held it over the veſſel containing the pounded ice and water, ready to plunge it into that mixture the moment the mercury, deſcending, paſſes the 75°.

Having a watch at my ear which beat half ſeconds (which I counted), I noted the time of the paſſage

paſſage of the mercury over the diviſions of the thermometer, marking 70° and every tenth degree from it, deſcending to 10° of the ſcale. I continued the cooling to 0°, or the temperature of the ice and water, in very few inſtances, as this took up much time, and was attended with no particular advantage, the determination of the times taken up in cooling 60 degrees of Reaumur's ſcale, that is to ſay, from 70° to 10°, being quite ſufficient to aſcertain the conducting power of any body whatever.

During the time of cooling in ice and water, the thermometer was conſtantly moved about in this mixture from one place to another; and there was always ſo much pounded ice mixed with the water, that the ice appeared above the ſurface of the water; the veſſel, which was a large earthen jar, being firſt quite filled with pounded ice, and the water being afterwards poured upon it, and freſh quantities of pounded ice being added as the occaſion required.

Having deſcribed the apparatus made uſe of in theſe Experiments, and the manner of performing the different operations, I ſhall now proceed to give an account of the Experiments themſelves.

My firſt attempt was to diſcover the relative conducting powers of ſuch ſubſtances as are commonly made uſe for clothing; accordingly, having procured a quantity of *raw ſilk*, as ſpun by the worm, *ſheep's wool*, *cotton wool*, *linen* in the form of the fineſt lint, being the ſcrapings of very fine Iriſh linen, the fineſt part of the *fur of the beaver* ſeparated from the ſkin, and from the long hair, the fineſt part of the *fur of a white Ruſſian hare*, and

Eider

Eider down; I introduced ſucceſſively 16 grains in weight of each of theſe ſubſtances into the globe of the paſſage-thermometer, and placing it carefully and equally round the bulb of the thermometer, I heated the thermometer in boiling water, as before deſcribed, and taking it out of the boiling water, plunged it into pounded ice and water, and obſerved the times of cooling.

But as the interſtices of theſe bodies thus placed in the globe were filled with air, I firſt made the Experiment with air alone, and took the reſult of that Experiment, as a ſtandard by which to compare all the others; the reſults of three Experiments with air were as follows:

The bulb of the thermometer ſurrounded by air.				
Heat loſt.	Exp. No. 1. Time elapſed.	Exp. No. 2. Time elapſed.	Heat acquired.	Exp. No. 3. Time elapſed.
70°	—	—	10°	—
60°	38″	38″	20°	39″
50°	46	46	30°	43
40°	59	59	40°	53
30°	80	79	50°	67
20°	122	122	60°	96
10°	231	230	70°	175
Total times.	576	574	—	473

 The

The following Table shows the results of the Experiments, with the various substances therein mentioned:

Heat lost.	Air.	Raw silk, 16 grs.	Sheep's wool, 16 grs.	Cotton wool, 16 grs.	Fine lint, 16 grs.	Beavers fur, 16 grs.	Hares fur, 16 grs.	Eider down, 16 grs.
	Exp. 1.	Exp. 4.	Exp. 5.	Exp. 6.	Exp. 7.	Exp. 8.	Exp. 9.	Ex. 10.
70°	—	—	—	—	—	—	—	—
60°	38″	94″	79″	83″	80″	99″	97″	98″
50°	46	110	95	95	93	116	117	116
40°	59	133	118	117	115	153	144	146
30°	80	185	162	152	150	185	193	192
20°	122	273	238	221	218	265	270	268
10°	231	489	426	378	376	478	494	485
Total times.	576	1284	1118	1046	1032	1296	1315	1305

Now the *warmth* of a body, or its power to confine Heat, being as its power of resisting the passage of Heat through it, (which I shall call its *non-conducting power*,) and the time taken up by any body in cooling, which is surrounded by any medium through which the Heat is obliged to pass, being, *cæteris paribus*, as the resistance which the medium opposes to the passage of the Heat, it appears that the *warmth* of the bodies mentioned in the foregoing Table are as the times of cooling; the *conducting powers* being inversely as those times, as I have formerly shown.

From the results of the foregoing Experiments it appears, that of the seven different substances made use of, hares fur and Eider down were the warmest;

warmeſt; after theſe came beavers fur; raw ſilk; ſheep's wool; cotton wool; and laſtly, lint, or the ſcrapings of fine linen; but I acknowledge that the differences in the warmth of theſe ſubſtances were much leſs than I expected to have found them.

Suſpecting that this might ariſe from the volumes or ſolid contents of the ſubſtances being different, (though their weights were the ſame,) ariſing from the difference of their ſpecific gravities; and as it was not eaſy to determine the ſpecific gravities of theſe ſubſtances with accuracy, in order to ſee how far any known difference in the volume or quantity of the ſame ſubſtance, confined always in the ſame ſpace, would add to or diminiſh the time of cooling, or the apparent warmth of the covering, I made the three following Experiments.

In the firſt, the bulb of the thermometer was ſurrounded by 16 grains of Eider down; in the ſecond by 32 grains; and in the third by 64 grains; and in all theſe Experiments the ſubſtance was made to occupy exactly the ſame ſpace, viz. the whole internal capacity of the glaſs globe, in the centre of which the bulb of the thermometer was placed; conſequently the thickneſs of the covering of the thermometer remained the ſame, while its denſity was varied in proportion to the numbers 1, 2, and 4.

The reſults of theſe Experiments were as follow:

The bulb of the thermometer being ſurrounded by Eider down.			
Heat loſt.	16 grains.	32 grains.	64 grains.
	(Exp. No. 11.)	(Exp. No. 12.)	(Exp. No. 13.)
70°	—	—	—
60°	97″	111″	112″
50°	117	128	130
40°	145	157	165
30°	192	207	224
20°	267	304	326
10°	486	565	658
Total times.	1304	1472	1615

Without ſtopping at preſent to draw any particular concluſions from the reſults of theſe Experiments, I ſhall proceed to give an account of ſome others, which will afford us a little further inſight into the nature of ſome of the circumſtances upon which the warmth of covering depends.

Finding, by the laſt Experiments, that the denſity of the covering added ſo conſiderably to the warmth of it, its thickneſs remaining the ſame, I was now deſirous of diſcovering how far the internal ſtructure of it contributed to render it more or leſs pervious to Heat, its thickneſs and quantity of matter remaining the ſame. By internal ſtructure, I mean the diſpoſition of the parts of the ſubſtance which forms the covering; thus they may be extremely divided, or very fine, as raw ſilk

ſilk as ſpun by the worms, and they may be equally diſtributed through the whole ſpace they occupy; or they may be coarſer, or in larger maſſes, with larger interſtices, as the ravelings of cloth, or cuttings of threads.

If Heat paſſed *through* the ſubſtances made uſe of for covering, and if the warmth of the covering depended ſolely upon the difficulty which the Heat meets with in its paſſage through the ſubſtances, *or ſolid parts*, of which they are compoſed; in that caſe, the warmth of covering would be always, *cæteris paribus*, as the quantity of materials of of which it is compoſed; but that this is not the caſe, the following, as well as the foregoing Experiments clearly evince.

Having, in the Experiment N° 4, aſcertained the warmth of 16 grains of raw ſilk, I now repeated the Experiment with the ſame quantity, or weight, of the ravelings of white taffety, and afterwards with a like quantity of common ſewing ſilk, cut into lengths of about two inches.

The following Table ſhows the reſults of theſe three Experiments:

Heat loſt.	Raw ſilk, 16 grs.	Ravelins of taffety, 16 grs.	Sewing ſilk cut into lenghts, 16 grs.
	Exp. 4.	Exp. 14.	Exp. 15.
70°	—	—	—
60°	94″	90″	67″
50°	110	106	79
40°	133	128	99
30°	185	172	135
20°	273	246	195
10°	489	427	342
Total times.	1284	1169	917

Here, notwithſtanding that the quantities of the ſilk were the ſame in the three Experiments, and though in each of them it was made to occupy the ſame ſpace, yet the warmth of the coverings which were formed were very different, owing to the different diſpoſition of the material.

The raw ſilk was very fine, and was very equally diſtributed through the ſpace it occupied, and it formed a warm covering.

The ravelings of taffety were alſo fine, but not ſo fine as the raw ſilk, and of courſe the interſtices between its threads were greater, and it was leſs warm; but the cuttings of ſewing ſilk were very coarſe, and conſequently it was very unequally diſtributed in the ſpace in which it was confined; and it made a very bad covering for confining Heat.

It

It is clear from the reſults of the five laſt Experiments, that the air which occupies the interſtices of bodies, made uſe of for covering, acts a very important part in the operation of confining Heat; yet I ſhall poſtpone the examination of that circumſtance till I ſhall have given an account of ſeveral other Experiments, which, I think, will throw ſtill more light upon that ſubject.

But, before I go any further, I will give an account of three Experiments which I made, or rather the ſame Experiment which I repeated three times the ſame day, in order to ſee how far they may be depended on, as being regular in their reſults.

The glaſs globe of the paſſage-thermometer being filled with 16 grains of cotton-wool, the inſtrument was heated and cooled three times ſucceſſively, when the times of cooling were obſerved as follows:

Heat loſt.	Exp. 16.	Exp. 17.	Exp. 18.
70°	—	—	—
60°	82″	84″	83″
50°	96	95	95
40°	118	117	116
30°	152	153	151
20°	221	221	220
10°	380	377	377
Total times.	1049	1047	1042

The difference of the times of cooling in theſe three Experiments were extremely ſmall; but regular as these Experiments appear to have been in their results, they were not more so than the other Experiments made in the same way, many of which were repeated two or three times, though, for the sake of brevity, I have put them down as single Experiments.

But to proceed in the account of my investigations relative to the causes of the warmth of warm clothing. Having found that the fineness and equal distribution of a body or substance made use of to form a covering to confine Heat, contributes so much to the warmth of the covering, I was desirous, in the next place to see the effect of condensing the covering, its quantity of matter remaining the same, but its thickness being diminished in proportion to the increase of its density.

The Experiment I made for this purpose was as follows:—I took 16 grains of common sewing silk, neither very fine nor very coarse, and winding it about the bulb of the thermometer in such a manner that it entirely covered it, and was as nearly as possible of the same thickness in every part, I replaced the thermometer in its cylinder and globe, and heating it in boiling water, cooled it in ice and water, as in the foregoing Experiments. The results of the Experiment were as may be seen in the following Table; and in order that it may be compared with those made with the same quantity of silk differently disposed of, I have placed those Experiments by the side of it:

Heat loſt.	Raw ſilk, 16 grs.	Fine ravelings of taffety, 16 grs.	Sewing ſilk cut into lengths, 16 grs.	Sewing ſilk, 16 grs. wound round the bulb of the thermometer.
	Exp. No. 4.	Exp. No. 14.	Exp. No. 15.	Exp. No. 19.
70°	—	—	—	—
60°	94″	90″	67″	46″
50°	110	106	79	62
40°	133	128	99	85
30°	185	172	135	121
20°	273	246	195	191
10°	489	427	342	399
Total times.	1214	1169	917	904

It is not a little remarkable, that, though the covering formed of ſewing ſilk wound round the bulb of the thermometer in the 19th Experiment, appeared to have ſo little power of confining the Heat when the inſtrument was very hot, or when it was firſt plunged into the ice and water, yet afterwards, when the Heat of the thermometer approached much nearer to that of the ſurrounding medium, its power of confining the Heat which remained in the bulb of the thermometer appeared to be even greater than that of the ſilk in the Experiment N° 15, the time of cooling from 20° to 10° being in the one 399″, and in the other 342″. The ſame appearance was obſerved in the following Experiments, in which the bulb of the thermometer was ſurrounded by threads of *wool*, of *cotton*, and of *linen*, or *flax*, wound round it, in the like manner

manner as the ſewing ſilk was wound round it in the laſt Experiment.

The following Table ſhows the reſults of theſe Experiments, with the threads of various kinds; and that they may the more eaſily be compared with thoſe made with the ſame quantity of the ſame ſubſtances in a different form, I have placed the accounts of theſe Experiments by the ſide of each other. I have alſo added the account of an Experiment, in which 16 grains of fine linen cloth were wrapped round the bulb of the thermometer, going roung it nine times, and being bound together at the top and bottom of it, ſo as completely to cover it.

Heat loſt.	*Sheep's wool*, 16 grains, ſurrounding the bulb of the thermometer.	*Woollen thread*, 16 grains, wound round the bulb of the thermometer.	*Cotton wool*, 16 grains, ſurrounding the bulb of the thermometer.	*Cotton thread*, 16 grains, wound round the bulb of the thermometer.	*Lint*, 16 grains, ſurrounding the bulb of the thermometer.	*Linen thread*, 16 grains, wound round the bulb of the thermometer.	*Linen cloth*, 16 grains, wrapped round the bulb of the thermometer.
	Exp. 5.	Ex. 20.	Exp. 6.	Ex. 21.	Exp. 7.	Ex. 22.	Exp. 23.
70°	—	—	—	—	—	—	—
60°	79″	46″	83″	45″	80″	46″	42″
50°	95	63	95	60	93	62	56
40°	118	89	117	83	115	83	74
30°	162	126	152	115	150	117	108
20°	238	200	221	179	218	180	168
10°	426	410	378	370	376	385	338
Total times.	1118	934	1046	852	1032	873	783

That thread wound light round the bulb of the thermometer ſhould form a covering leſs warm than the ſame quantity of wool, or other raw materials

terials of which the thread is made, ſurrounding the bulb of the thermometer in a more looſe manner, and conſequently occupying a greater ſpace, is no more than what I expected, from the idea I had formed of the cauſes of the warmth of covering; but I confeſs I was much ſurpriſed to find that there is ſo great a difference in the relative warmth of theſe two coverings, when they are employed to confine great degrees of Heat, and when the Heat they confine is much leſs in proportion to the temperature of the ſurrounding medium. This difference was very remarkable; in the Experiments with ſheep's wool, and with woollen thread, the warmth of the covering formed of 16 grains of the former, was to that formed of 16 grains of the latter, when the bulb of the thermometer was heated to 70° and cooled to 60°, as 79 to 46 (the ſurrounding medium being at 0°); but afterwards, when the thermometer had only fallen from 20° to 10° of Heat, the warmth of the wool was to that of the woollen thread only as 426 to 410; and in the Experiments with lint, and with linen thread, when the Heat was much abated, the covering of the thread appeared to be even warmer than that of the lint, though in the beginning of the Experiments, when the Heat was much greater, the lint was warmer than the thread, in the proportion of 80 to 46.

From hence it ſhould ſeem that a covering may, under certain circumſtances, be very good for confining ſmall degrees of warmth, which would be but

but very indifferent when made uſe of for confining a more intenſe Heat, and *vice verſa*. This, I believe, is a new fact; and, I think the knowledge of it may lead to further diſcoveries relative to the cauſes of the warmth of coverings, or the manner in which Heat makes its paſſage through them. But I forbear to enlarge upon this ſubject, till I ſhall have given an account of ſeveral other Experiments, which I think throw more light upon it, and which will conſequently render the inveſtigation eaſier and more ſatisfactory.

With a view to determine how far the power which certain bodies appear to poſſeſs of confining Heat, when made uſe of as covering, depends upon the natures of thoſe bodies, conſidered as chymical ſubſtances, or upon the chymical principles of which they are compoſed, I made the following Experiments.

As charcoal is ſuppoſed to be compoſed almoſt entirely of phlogiſton, I thought that, if that principle was the cauſe either of the conducting power, or the non-conducting power of the bodies which contain it, I ſhould diſcover it by making the Experiment with charcoal, as I had done with various other bodies. Accordingly, having filled the globe of the paſſage-thermometer with 176 grains of that ſubſtance in very fine powder, (it having been pounded in a mortar, and ſifted through a fine ſieve,) the bulb of the thermometer being ſurrounded by this powder, the inſtrument was heated in boiling water, and being afterwards plunged

into

into a mixture of pounded ice and water, the times of cooling were obſerved as mentioned in the following Table. I afterwards repeated the Experiment with lampblack, and with very pure and very dry wood aſhes; the reſults of which Experiments were as under-mentioned:

The bulb of the thermometer ſurrounded by				
Heat loſt.	176 grains of fine powder of charcoal.	176 grains of fine powder of charcoal.	195 grains of lampblack.	307 grains of pure dry wood aſhes.
	Exp. No. 24.	Exp. No. 25.	Exp. No. 26.	Exp. No. 27.
70°	—	—	—	—
60°	79″	91″	124″	96″
50°	95	91	118	92
40°	100	109	134	107
30°	139	133	164	136
20°	196	192	237	185
10°	331	321	394	311
Total times.	940	937	1171	927

The Experiment No. 25 was ſimply a repetition of that numbered 24, and was made immediately after it; but, in moving the thermometer about in the former Experiment, the powder of charcoal which filled the globe was ſhaken a little together, and to this circumſtance I attribute the difference in the reſults of the two Experiments.

In the Experiments with lampblack and with wood aſhes, the times taken up in cooling from 70° to 60° were greater than thoſe employed in cooling from 60° to 50°; this moſt probably aroſe from

from the confiderable quantity of Heat contained by thefe fubftances, which was firft to be difpofed of, before they could receive and communicate to the furrounding medium that which was contained by the bulb of the thermometer.

The next Experiment I made was with *femen lycopodii*, commonly called witch-meal, a fubftance which poffeffes very extraordinary properties. It is almoft impoffible to wet it; a quantity of it ftrewed upon the furface of a bafin of water, not only fwims upon the water without being wet, but it prevents other bodies from being wet which are plunged into the water through it; fo that a piece of money, or other folid body, may be taken from the bottom of the bafin by the naked hand, without wetting the hand; which is one of the tricks commonly fhown by the jugglers in the country: this meal covers the hand, and defcending along with it to the bottom of the bafin, defends it from the water. This fubftance has the appearance of an exceeding fine, light, and very moveable yellow powder, and it is very inflammable; fo much fo, that being blown out of a quill into the flame of a candle, it flafhes like gunpowder, and it is made ufe of in this manner in our theatres for imitating lightning.

Conceiving that there muft have been a ftrong attraction between this fubftance and air, and fufpecting, from fome circumftances attending fome of the foregoing Experiments, that the warmth of a covering depends not merely upon the finenefs of the

the ſubſtance of which the covering is formed, and the diſpoſition of its parts, but that it ariſes in ſome meaſure from a certain attraction between the ſubſtance and the air which fills its interſtices, I thought that an Experiment with *ſemen lycopodii* might poſſibly throw ſome light upon this matter; and in this opinion I was not altogether miſtaken, as will appear by the reſults of the three following Experiments.

The bulb of the thermometer ſurrounded by 256 grs. of *ſemen lycopodii.*

Heat loſt.	Cooled.	Cooled.	Heat acquired.	Heated.
	Exp. No. 28.	Exp. No. 29.		Exp. No. 30.
70°	—	—	0°	—
60°	146″	157″	10°	230″
50°	162	160	20°	68
40°	175	170	30°	63
30°	209	203	40°	76
20°	284	288	50°	121
10°	502	513	60°	316
—	—	—	70°	1585
Total times.	1478	1491	—	2459

In the laſt Experiment (N° 30) the reſult of which was ſo very extraordinary, the inſtrument was cooled to 0° in thawing ice, after which it was plunged ſuddenly into boiling water, where it remained till the incloſed thermometer had acquired the Heat of 70°, which took up no leſs than 2456 ſeconds, or above 40 minutes; and it had remained in the boiling water full a minute and an half before the mercury in the thermometer ſhowed the leaſt ſign of riſing. Having at length been put into motion, it roſe very rapidly 40 or 50 degrees, after which its motion gradually abating became ſo ſlow, that it took up 1585 ſeconds, or ſomething more than 26 minutes, in riſing from 60° to 70°, though the temperature of the medium in which it was placed during the whole of this time was very nearly 80°; the mercury in the barometer ſtanding but little ſhort of 27 Paris inches.

All the different ſubſtances which I had yet made uſe of in theſe Experiments for ſurrounding or covering the bulb of the thermometer, fluids excepted, had, in a greater, or in a leſs degree confined the Heat, or prevented its paſſing into or out of the thermometer ſo rapidly as it would have done, had there been nothing but air in the glaſs globe, in the centre of which the bulb of the thermometer was ſuſpended. But the great queſtion is, how, or in what manner, they produced this effect?

And firſt, it was not in conſequence of their own non-conducting powers, ſimply conſidered; for, if inſtead of being only bad conductors of Heat, we ſuppoſe them to have been totally impervious to Heat, their volumes or ſolid contents were ſo exceedingly ſmall in proportion to the capacity of the globe in which they were placed, that, had they had no effect whatever upon the air filling their interſtices, that air would have been ſufficient to have conducted all the Heat communicated, in leſs time than was actually taken up in the Experiment.

The

The diameter of the globe being 1,6 inches, its contents amounted to 2,14466 cubic inches; and the contents of the bulb of the thermometer being only 0,08711 of a cubic inch, (its diameter being 0,55 of an inch,) the ſpace between the bulb of the thermometer and the internal ſurface of the globe amounted to $2,14466-0,08711=2,05755$ cubic inches; the whole of which ſpace was occupied by the ſubſtances by which the bulb of the thermometer was ſurrounded in the Experiments in queſtion.

But though theſe ſubſtances occupied this ſpace, they were far from *filling it;* by much the greater part of it being filled by the air which occupied the interſtices of the ſubſtances in queſtion. In the Experiment N 4, this ſpace was occupied by 16 grains of raw ſilk; and as the ſpecific gravity of raw ſilk is to that of water as 1734 to 1000, the volume of this ſilk was equal to the volume of 9,4422 grains of water; and as 1 cubic inch of water weighs 253,185 grains, its volume was equal to $\frac{9,4422}{253,1850}=0,037294$ of a cubic inch; and, as the ſpace it occupied amounted to 2,05755 cubic inches, it appears that the ſilk filled no more than about $\frac{1}{55}$ part of the ſpace in which it was confined, the reſt of that ſpace being filled with air.

In the Experiment N° 1, when the ſpace between the bulb of the thermometer and the glaſs globe, in the centre of which it was confined, was filled with nothing but air, the time taken up by the thermometer in cooling from 70° to 10° was

576 feconds; but in the Experiment N° 4, when this fame fpace was filled with 54 parts air, and 1 part raw filk, the time of cooling was 1284 feconds.

Now, fuppofing that the filk had been totally incapable of conducting any Heat at all, if we fuppofe, at the fame time, that it had no power to prevent the air remaining in the globe from conducting it, in that cafe its prefence in the globe could only have prolonged the time of cooling in proportion to the quantity of the air it had difplaced to the quantity remaining, that is to fay, as 1 is to 54, or a little more than 10 feconds. But the time of cooling was actually prolonged 708 feconds (for in the Experiment N° 1, it was 576 feconds, and in the Experiment N° 4, it was 1284 feconds, as has juft been obferved); and this fhows, that the filk not only did not conduct the Heat itfelf, but that it prevented the air by which its interftices were filled from conducting it; or, at leaft, it greatly weakened its power of conducting it.

The next queftion which arifes is, how air can be prevented from conducting Heat? and this neceffarily involves another, which is, how does air conduct Heat?

If air conducted Heat, as it is probable that the metals and water, and all other folid bodies and unelaftic fluids conduct it, that is to fay, if its particles remaining in their places, the Heat paffed from one particle to another, through the whole mafs, maſs, as there is no reaſon to ſuppoſe that the propagation of Heat is neceſſarily in right lines, I cannot conceive how the interpoſition of ſo ſmall a quantity of any ſolid body as $\frac{1}{55}$ part of the volume of the air could have effected ſo remarkable a diminution of the conducting power of the air, as appeared in the Experiment (N° 4) with raw ſilk, above mentioned.

If air and water conducted Heat in the ſame *manner*, it is more than probable that their conducting powers might be impaired by the ſame means; but when I made the Experiment with water, by filling the glaſs globe, in the centre of which the bulb of the thermometer was ſuſpended, with that fluid, and afterwards varied the Experiment, by adding 16 grains of raw ſilk to the water, I did not find that the conducting power of the water was ſenſibly impaired by the preſence of the ſilk *.

But we have juſt ſeen that the ſame ſilk, mixed with an equal volume of air, diminiſhed its conducting power in a very remarkable degree; conſequently, there is great reaſon to conclude that water and air conduct Heat in a *different manner*.

But the following Experiment, I think, puts the matter beyond all doubt.

* The Experiment here mentioned was made in the year 1787; but the reſult of a more careful inveſtigation of the ſubject has ſince ſhown that Heat is not propagated in water in the manner here ſuppoſed. (See Eſſay VII.)

 It

It is well known, that the power which air poſſeſſes of holding water in ſolution is augmented by Heat, and diminiſhed by cold, and that, if hot air is ſaturated with water, and if this air is afterwards cooled, a part of its water is neceſſarily depoſed.

I took a cylindrical bottle of very clear tranſparent glaſs, about 8 inches in diameter, and 12 inches high, with a ſhort and narrow neck, and ſuſpending a ſmall piece of linen rag, moderately wet, in the middle of it, I plunged it into a large veſſel of water, warmed to about 100° of Fahrenheit's thermometer, where I ſuffered it to remain till the contained air was not only warm, but thoroughly ſaturated with the moiſture which it attracted from the linen rag, the mouth of the bottle being well ſtopped up during this time with a good cork; this being done, I removed the cork for a moment, to take away the linen rag, and ſtopping up the bottle again immediately, I took it out of the warm water, and plunged it into a large cylindrical jar, about 12 inches in diameter, and 16 inches high, containing juſt ſo much ice-cold water, that, when the bottle was plunged into it, and quite covered by it, the jar was quite full.

As the jar was of very fine tranſparent glaſs, as well as the bottle, and as the cold water contained in the jar was perfectly clear, I could ſee what paſſed in the bottle moſt diſtinctly; and having taken care to place the jar upon a table near the window,

window, in a very favourable light, I ſet myſelf to obſerve the appearances which ſhould take place, with all that anxious expectation which a conviction that the reſult of the Experiment muſt be deciſive, naturally inſpired.

I was certain, that the air contained in the bottle could not part with its Heat, without at the ſame time, that is to ſay, *at the ſame moment*, and *in the ſame place*, parting with a portion of its water; if, therefore, the Heat penetrated the maſs of air from the centre to the ſurface, or *paſſed through it* from particle to particle, in the ſame manner as it is probable that it paſſes through water, and all other unelaſtic fluids *, by far the greateſt part of the air contained in the bottle would part with its Heat, when *not actually in contact with the glaſs*, and a proportional part of its water being let fall at the ſame time, and in the *ſame place*, would neceſſarily deſcend in the form of rain; and, though this rain might be too fine to be viſible in its deſcent, yet I was ſure I ſhould find it at the bottom of the bottle, if not in viſible drops of water, yet in that kind of cloudy covering which cold glaſs acquires from a contact with hot ſteam or watery vapour.

But if the particles of air, inſtead of communicating their Heat from one to another, from the centre to the ſurface of the bottle, each in its turn, and for itſelf, came to the ſurface of the bottle, and

* This opinion reſpecting the manner in which Heat is propagated in water, and other unelaſtic fluids, was afterwards found to be erroneous, as has been ſhown in the preceding Eſſay.

there depofited its Heat and its water, I concluded that the cloudinefs occafioned by this depofit of water would appear all over the bottle, or, at leaft, not more of it at the bottom than at the fides, but rather lefs; and this I found to be the cafe in fact.

The cloudinefs firft made its appearance upon the fides of the bottle, near the top of it; and from thence it gradually fpread itfelf downwards, till, growing fainter as it defcended lower, it was hardly vifible at the diftance of half an inch from the bottom of the bottle; and upon the bottom itfelf, which was nearly flat, there was fcarcely the fmalleft appearance of cloudinefs.

Thefe appearances, I think, are eafy to be accounted for. The air immediately in contact with the glafs being cooled, and having depofited a part of its water upon the furface of the glafs, at the fame time that it communicates to it its Heat, flides downwards by the fides of the bottle in confequence of its increafed fpecific gravity, and, taking its place at the bottom of the bottle, forces the whole mafs of hot air upwards; which, in its turn coming to the fides of the bottle, *there* depofits its Heat and its water, and afterwards bending its courfe downwards, this circulation is continued till all the air in the bottle has acquired the exact temperature of the water in the jar.

From hence it is clear why the firft appearance of condenfed vapour is near the top of the bottle, as alfo why the greateft collection of vapour is in that

that part, and that ſo very ſmall a quantity of it is found nearer the bottom of the bottle.

This Experiment confirmed me in an opinion which I had for ſome time entertained, that, though the particles of air individually, or each for itſelf, are capable of receiving and *tranſporting* Heat, yet air in a quieſcent ſtate, or as a fluid whoſe parts are at reſt with reſpect to each other, is not capable of conducting it, or giving it a paſſage; in ſhort, that Heat is incapable of *paſſing through a maſs of air*, penetrating from one particle of it to another, and that it is to this circumſtance that its non-conducting power is principally owing.

It is alſo to this circumſtance, in a great meaſure, that it is owing that its non-conducting power, or its apparent warmth when employed as a covering for confining Heat, is ſo remarkably increaſed upon its being mixed with a ſmall quantity of any very fine, light, ſolid ſubſtance, ſuch as the raw ſilk, fur, Eider down, &c. in the foregoing Experiments: for as I have already obſerved, though theſe ſubſtances, in the very ſmall quantities in which they were made uſe of, could hardly have prevented, in any conſiderable degree, the air from conducting, or giving a *paſſage* to the Heat, had it been capable of paſſing through it, yet they might very much impede it in the operation of tranſporting it.

But there is another circumſtance which it is neceſſary to take into the account, and that is the attraction which ſubſiſts between air and the bodies above-mentioned, and other like ſubſtances, conſtituting natural and artificial clothing. For, though the incapacity of air to give a paſſage to Heat in the manner ſolid bodies permit it to paſs through them, may enable us to account for its warmth under certain circumſtances, yet the bare admiſſion of this principle does not ſeem to be ſufficient to account for the very extraordinary degrees of warmth which we find in furs and in feathers, and in various other kinds of natural and artificial clothing; nor even that which we find in ſnow; for if we ſuppoſe the particles of air to be at liberty to *carry off* the Heat which theſe bodies are meant to confine, without any other obſtruction or hindrance than that ariſing from their *vis inertiæ*, or the force neceſſary to put them in motion, it ſeems probable that the ſucceſſion of freſh particles of cold air, and the conſequent loſs of Heat, would be much more rapid than we find it to be in fact.

That an attraction, and a very ſtrong one, actually ſubſiſts between the particles of air, and the fine hair or furs of beaſts, the feathers of birds, wool, &c. appears by the obſtinacy with which theſe ſubſtances retain the air which adheres to them, even when immerſed in water, and put under the receiver of an air-pump; and that this attraction is eſſential to the warmth of theſe bodies, I think is very eaſy to be demonſtrated.

In furs, for inſtance, the attraction between the particles of air, and the fine hairs in which it is concealed, being greater than the increaſed elaſ-

ticity,

ticity, or repulſion of thoſe particles with regard to each other, ariſing from the Heat communicated to them by the animal body, the air in the fur, though heated, is not eaſily diſplaced; and this coat of confined air is the real barrier which defends the animal body from the external cold. This air cannot *carry off* the Heat of the animal, becauſe it is itſelf confined, by its attraction to the hair or fur; and it tranſmits it with great difficulty, if it tranſmits at all, as has been abundantly ſhewn by the foregoing Experiments.

Hence it appears why thoſe furs which are the fineſt, longeſt, and thickeſt, are likewiſe the warmeſt; and how the furs of the beaver, of the otter, and of other like quadrupeds which live much in water, and the feathers of water-fowls, are able to confine the Heat of thoſe animals in winter, notwithſtanding the extreme coldneſs and great conducting power of the water in which they ſwim. The attraction between theſe ſubſtances, and the air which occupies their interſtices, is ſo great, that this air is not diſlodged even by the contact of water, but remaining in its place, it defends the body of the animal at the ſame time from being wet, and from being robbed of its Heat by the ſurrounding cold fluid; and it is poſſible that the preſſure of this fluid upon the covering of air confined in the interſtices of the fur, or feathers, may at the ſame time increaſe its warmth, or non-conducting power, in ſuch a manner that the animal may not, in fact, loſe more Heat when in water, than

when

when in air: for we have ſeen by the foregoing Experiments, that, under certain circumſtances, the warmth of a covering is increaſed, by bringing its component parts nearer together, or by increaſing its denſity even at the expence of its thickneſs. But this point will be further inveſtigated hereafter.

Bears, wolves, foxes, hares, and other like quadrupeds, inhabitants of cold countries, which do not often take the water, have their fur much thicker upon their backs than upon their bellies. The heated air occupying the interſtices of the hairs of the animal tending naturally to riſe upwards, in conſequence of its increaſed elaſticity, would eſcape with much greater eaſe from the backs of quadrupeds than from their bellies, had not Providence wiſely guarded againſt this evil by increaſing the obſtructions in thoſe parts, which entangle it and confine it to the body of the animal. And this, I think, amounts almoſt to a proof of the principles aſſumed relative to the manner in which Heat is carried off by air, and the cauſes of the non-conducting power of air, or its apparent warmth, when, being combined with other bodies, it acts as a covering for confining Heat.

The ſnows which cover the ſurface of the earth in winter, in high latitudes, are doubtleſs deſigned by an all-provident Creator as a garment to defend it againſt the piercing winds from the polar regions, which prevail during the cold ſeaſon.

These

Theſe winds, notwithſtanding the vaſt tracts of continent over which they blow, retain their ſharpneſs as long as the ground they paſs over is covered with ſnow; and it is not till meeting with the ocean they acquire, from a contact with its waters, the Heat which the ſnows prevent their acquiring from the earth, that the edge of their coldneſs is taken off, and they gradually die away and are loſt.

The winds are always found to be much colder when the ground is covered with ſnow than when it is bare, and this extraordinary coldneſs is vulgarly ſuppoſed to be communicated to the air by the ſnow; but this is an erroneous opinion; for theſe winds are in general much colder than the ſnow itſelf.

They retain their coldneſs, becauſe the ſnow prevents them from being warmed at the expence of the earth; and this is a ſtriking proof of the uſe of the ſnows in preſerving the Heat of the earth during the winter in cold latitudes.

It is remarkable that theſe winds ſeldom blow from the poles directly towards the equator, but from the land towards the ſea. Upon the eaſtern coaſt of North America the cold winds come from the north-weſt; but upon the weſtern coaſt of Europe they blow from the north-eaſt.

That they ſhould blow towards thoſe parts where they can moſt eaſily acquire the Heat they are in ſearch of, is not extraordinary; and that they ſhould gradually ceaſe and die away, upon being warmed

warmed by a contact with the waters of the ocean, is likewise agreeable to the nature and causes of their motion; and if I might be allowed a conjecture respecting the principal use of the seas, or the reason why the proportion of water upon the surface of our globe is so great, compared to that of the land, it is to maintain a more equal temperature in the different climates, by heating or cooling the winds which at certain periods blow from the great continents.

That cold winds actually grow much milder upon passing over the sea, and that hot winds are refreshed by a contact with its waters, is very certain; and it is equally certain that the winds from the ocean are, in all climates, much more temperate than those which blow from the land.

In the islands of Great Britain and Ireland, there is not the least doubt but the great mildness of the climate is entirely owing to their separation from the neighbouring continent by so large a tract of sea; and in all similar situations, in every part of the globe, similar causes are found to produce similar effects.

The cold north-west winds, which prevail upon the coast of North America during the winter, seldom extend above 100 leagues from the shore, and they are always found to be less violent, and less piercing, as they are further from the land.

These periodical winds from the continents of Europe and North America prevail most towards the end of the month of February, and in the month of of March; and I conceive that they contribute very eſſentially towards bringing on an early ſpring, and a fruitful ſummer, particularly when they are very violent in the month of March, and if at that time the ground is well covered with ſnow. The whole atmoſphere of the polar regions being, as it were, tranſported into the ocean by theſe winds, is there warmed and ſaturated with water: and, a great accumulation of air upon the ſea being the neceſſary conſequence of the long continuance of theſe cold winds from the ſhore, upon their ceaſing the warm breezes from the ſea neceſſarily commence, and, ſpreading themſelves upon the land far and wide, aſſiſt the returning ſun in diſmantling the earth of the remains of her winter garment, and in bringing forward into life all the manifold beauties of the new-born year.

This warmed air which comes in from the ſea, having acquired its Heat from a contact with the ocean, is, of courſe, ſaturated with water; and hence the warm ſhowers of April and May, ſo neceſſary to a fruitful ſeaſon.

The ocean may be conſidered as the great reſervoir and equalizer of Heat; and its benign influences in preſerving a proper temperature in the atmoſphere operate in all ſeaſons and in all climates.

The parching winds from the land under the torrid zone are cooled by a contact with its waters, and, in return, the breezes from the ſea, which at certain hours of the day come in to the ſhores in almoſt all hot countries, bring with them refreſhment, and, as it were, new life and vigour both to the animal and vegetable creation, fainting and melting under the exceſſive Heats of a burning ſun. What a vaſt tract of country, now the moſt fertile upon the face of the globe, would be abſolutely barren and uninhabitable on account of the exceſſive Heat, were it not for theſe refreſhing ſea-breezes! And is it not more than probable, that the extremes of heat and of cold in the different ſeaſons in the temperate and frigid zones would be quite intolerable, were it not for the influence of the ocean in preſerving an equability of temperature?

And to theſe purpoſes the ocean is wonderfully well adapted not only on account of the great power of water to abſorb Heat, and the vaſt depth and extent of the different ſeas (which are ſuch that one ſummer or one winter could hardly be ſuppoſed to have any ſenſible effect in heating or cooling this enormous maſs); but alſo on account of the continual circulation which is carried on in the ocean itſelf, by means of the currents which prevail in it. The waters under the torrid zone being carried by theſe currents towards the polar regions, are there cooled by a contact with the cold winds, and, having thus communicated their Heat to theſe inhoſpitable regions, return towards the equator, carrying with them refreſhment for thoſe parching climates.

The wiſdom and goodneſs of Providence have often been called in queſtion with regard to the diſtribution of land and water upon the ſurface of our

our globe, the vaſt extent of the ocean having been conſidered as a proof of the little regard that has been paid to man in this diſtribution. But, the more light we acquire reſpecting the real conſtitution of things, and the various uſes of the different parts of the viſible creation, the leſs we ſhall be diſpoſed to indulge ourſelves in ſuch frivolous criticiſms.

END OF THE EIGHTH ESSAY.

ESSAY IX.

AN

EXPERIMENTAL INQUIRY

CONCERNING

THE SOURCE OF THE HEAT WHICH IS EXCITED BY FRICTION.

[Read before the ROYAL SOCIETY, January 25, 1798.]

ESSAY IX.

An INQUIRY concerning the SOURCE of the HEAT which is EXCITED by FRICTION.

[Read before the ROYAL SOCIETY, January 25, 1798.]

IT frequently happens, that in the ordinary affairs and occupations of life, opportunities present themselves of contemplating some of the most curious operations of Nature; and very interesting philosophical experiments might often be made, almost without trouble or expence, by means of machinery contrived for the mere mechanical purposes of the arts and manufactures.

I have frequently had occasion to make this observation; and am persuaded, that a habit of keeping the eyes open to every thing that is going on in the ordinary course of the business of life has oftener led, as it were by accident, or in the playful excursions of the imagination, put into action by contemplating the most common appearances, to useful doubts, and sensible schemes for investigation and

and improvement, than all the more intenſe meditations of philoſophers, in the hours expreſsly ſet apart for ſtudy.

It was by accident that I was led to make the Experiments of which I am about to give an account; and, though they are not perhaps of ſufficient importance to merit ſo formal an introduction, I cannot help flattering myſelf that they will be thought curious in ſeveral reſpects, and worthy of the honour of being made known to the Royal Society.

Being engaged, lately, in ſuperintending the boring of cannon, in the workſhops of the military arſenal at Munich, I was ſtruck with the very conſiderable degree of Heat which a braſs gun acquires, in a ſhort time, in being bored; and with the ſtill more intenſe Heat (much greater than that of boiling water, as I found by experiment) of the metallic chips ſeparated from it by the borer.

The more I meditated on theſe phænomena, the more they appeared to me to be curious and intereſting. A thorough inveſtigation of them ſeemed even to bid fair to give a farther inſight into the hidden nature of Heat; and to enable us to form ſome reaſonable conjectures reſpecting the exiſtence, or non-exiſtence, of an *igneous fluid:* a ſubject on which the opinions of philoſophers have, in all ages, been much divided.

In order that the Society may have clear and diſtinct ideas of the ſpeculations and reaſonings to which theſe appearances gave riſe in my mind, and alſo of the ſpecific objects of philoſophical inveſtigation

gation they fuggefted to me, I muft beg leave to ftate them at fome length, and in fuch manner as I fhall think beft fuited to anfwer this purpofe.

From *whence comes* the Heat actually produced in the mechanical operation above mentioned ?

Is it furnifhed by the metallic chips which are feparated by the borer from the folid mafs of metal ?

If this were the cafe, then, according to the modern doctrines of latent Heat, and of caloric, the *capacity for Heat* of the parts of the metal, fo reduced to chips, ought not only to be changed, but the change undergone by them fhould be fufficiently great to account for *all* the Heat produced.

But no fuch change had taken place; for I found, upon taking equal quantities, by weight, of thefe chips, and of thin flips of the fame block of metal feparated by means of a fine faw, and putting them, at the fame temperature, (that of boiling water,) into equal quantities of cold water, (that is to fay, at the temperature of $59°\frac{1}{2}$ F.) the portion of water into which the chips were put was not, to all appearance, heated either lefs or more than the other portion, in which the flips of metal were put.

This Experiment being repeated feveral times, the refults were always fo nearly the fame, that I could not determine whether any, or what change, had been produced in the metal, *in regard to its* *capacity for Heat*, by being reduced to chips by the borer *.

From hence it is evident, that the Heat produced could not poſſibly have been furniſhed at the expence of the latent Heat of the metallic chips. But, not being willing to reſt ſatisfied with theſe trials, however concluſive they appeared to me to be, I had recourſe to the following ſtill more deciſive Experiment:

Taking a cannon, (a braſs ſix-pounder,) caſt ſolid, and rough as it came from the foundry, (ſee Fig. 1. Tab. IV.) and fixing it (horizontally) in

* As theſe Experiments are important, it may perhaps be agreeable to the Society to be made acquainted with them in their details.

One of them was as follows:

To 4590 grains of water, at the temperature of 59°½ F. (an allowance as compenſation, reckoned in water, for the capacity for Heat of the containing cylindrical tin veſſel, being included,) were added 1016⅛ grains of gun-metal in thin ſlips, ſeparated from the gun by means of a fine ſaw, being at the temperature of 210° F. When they had remained together 1 minute, and had been well ſtirred about, by means of a ſmall rod of light wood, the Heat of the mixture was found to be = 63°.

From this Experiment, the *ſpecific Heat* of the metal, calculated according to the rule given by Dr. CRAWFORD, turns out to be = 0.1100, that of water being = 1.0000.

An Experiment was afterwards made with the metallic chips, as follows:

To the ſame quantity of water as was uſed in the Experiment above mentioned, at the ſame temperature, (*viz.* 59°½,) and in the ſame cylindrical tin veſſel, were now put 1016⅛ grains of metallic chips of gun-metal, bored out of the ſame gun from which the ſlips uſed in the foregoing Experiment were taken, and at the ſame temperature (210°). The Heat of the mixture, at the end of 1 minute, was juſt 63°, as before; conſequently the ſpecific Heat of theſe metallic chips was = 0.1100. Each of the above Experiments was repeated three times, and always with nearly the ſame reſults.

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the machine uſed for boring, and at the ſame time finiſhing the outſide of the cannon by turning, (ſee Fig. 2.) I cauſed its extremity to be cut off; and, by turning down the metal in that part, a ſolid cylinder was formed, $7\frac{3}{4}$ inches in diameter, and $9\frac{8}{10}$ inches long; which, when finiſhed, remained joined to the reſt of the metal (that which, properly ſpeaking, conſtituted the cannon) by a ſmall cylindrical neck, only $2\frac{1}{5}$ inches in diameter, and $3\frac{8}{10}$ inches long.

This ſhort cylinder, which was ſupported in its horizontal poſition, and turned round its axis, by means of the neck by which it remained united to the cannon, was now bored with the horizontal borer uſed in boring cannon; but its bore, which was 3.7 inches in diameter, inſtead of being continued through its whole length (9.8 inches) was only 7.2 inches in length; ſo that a ſolid bottom was left to this hollow cylinder, which bottom was 2.6 inches in thickneſs.

This cavity is repreſented by dotted lines in Fig. 2; as alſo in Fig. 3. where the cylinder is repreſented on an enlarged ſcale.

This cylinder being deſigned for the expreſs purpoſe of generating Heat *by friction*, by having a blunt borer forced againſt its ſolid bottom at the ſame time that it ſhould be turned round its axis by the force of horſes, in order that the Heat accumulated in the cylinder might from time to time be meaſured, a ſmall round hole, (ſee *d*, *e*, Fig. 3.) 0.37 of an inch only in diameter, and 4.2 inches in

 depth,

depth, for the purpose of introducing a small cylindrical mercurial thermometer, was made in it, on one side, in a direction perpendicular to the axis of the cylinder, and ending in the middle of the solid part of the metal which formed the bottom of its bore.

The solid contents of this hollow cylinder, exclusive of the cylindrical neck by which it remained united to the cannon, were $385\frac{3}{4}$ cubic inches, English measure; and it weighed 113.13lb. Avoirdupois: as I found, on weighing it at the end of the course of Experiments made with it, and after it had been separated from the cannon with which, during the Experiments, it remained connected*.

Experiment, N° 1.

This Experiment was made in order to ascertain how much Heat was actually generated by friction, when a blunt steel borer being so forcibly shoved (by means of a strong screw) against the bottom of

* For fear I should be suspected of prodigality in the prosecution of my philosophical researches, I think it necessary to inform the Society, that the cannon I made use of in this Experiment was not sacrificed to it. The short hollow cylinder which was formed at the end of it, was turned out of a cylindrical mass of metal, about 2 feet in length, projecting beyond the muzzle of the gun, called in the German language the *verlorner kopf*, (the head of the cannon to be thrown away,) and which is represented in Fig. 1.

This original projection, which is cut off before the gun is bored, is always cast with it, in order that, by means of the pressure of its weight on the metal in the lower part of the mould, during the time it is cooling, the gun may be the more compact in the neighbourhood of the muzzle; where, without this precaution, the metal would be apt to be porous, or full of honeycombs.

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the bore of the cylinder, that the preſſure againſt it was equal to the weight of about 10000 lb. Avoirdupois, the cylinder was turned round on its axis (by the force of horſes) at the rate of about 32 times in a minute.

This machinery, as it was put together for the Experiment, is repreſented by Fig. 2. W is a ſtrong horizontal iron bar, connected with proper machinery carried round by horſes, by means of which the cannon was made to turn round it axis.

To prevent, as far as poſſible, the loſs of any part of the Heat that was generated in the Experiment, the cylinder was well covered up with a fit coating of thick and warm flannel, which was carefully wrapped round it, and defended it on every ſide from the cold air of the atmoſphere. This covering is not repreſented in the drawing of the apparatus, Fig. 2.

I ought to mention, that the borer was a flat piece of hardened ſteel, 0.63 of an inch thick, 4 inches long, and nearly as wide as the cavity of the bore of the cylinder, namely, $3\frac{1}{2}$ inches. Its corners were rounded off at its end, ſo as to make it fit the hollow bottom of the bore; and it was firmly faſtened to the iron bar (*m*) which kept it in its place. The area of the ſurface by which its end was in contact with the bottom of the bore of the cylinder was nearly $2\frac{1}{3}$ inches. This borer, which is diſtinguiſhed by the letter *n*, is repreſented in moſt of the figures.

At

At the beginning of the Experiment, the temperature of the air in the ſhade, as alſo that of the cylinder, was juſt 60° F.

At the end of 30 minutes, when the cylinder had made 960 revolutions about its axis, the horſes being ſtopped, a cylindrical mercurial thermometer, whoſe bulb was $\frac{32}{100}$ of an inch in diameter, and $3\frac{1}{4}$ inches in length, was introduced into the hole made to receive it, in the ſide of the cylinder, when the mercury roſe almoſt inſtantly to 130°.

Though the Heat could not be ſuppoſed to be quite equally diſtributed in every part of the cylinder, yet, as the length of the bulb of the thermometer was ſuch that it extended from the axis of the cylinder to near its ſurface, the Heat indicated by it could not be very different from that of the *mean temperature* of the cylinder; and it was on this account that a thermometer of that particular form was choſen for this Experiment.

To ſee how faſt the Heat eſcaped out of the cylinder, (in order to be able to make a probable conjecture reſpecting the quantity given off by it, during the time the Heat generated by the friction was accumulating,) the machinery ſtanding ſtill, I ſuffered the thermometer to remain in its place near three quarters of an hour, obſerving and noting down, at ſmall intervals of time, the height of the temperature indicated by it.

Thus,

Thus, at the end of	The Heat, as ſhown by the thermometer, was
4 minutes - - - - - -	126°
after 5 minutes, always reckoning from the firſt obſervation, - - - - -	125°
at the end of 7 minutes - - - - - -	123°
12 ——— - - - - - -	120°
14 ——— - - - - - -	119°
16 ——— - - - - - -	118°
20 ——— - - - - - -	116°
24 ——— - - - - - -	115°
28 ——— - - - - - -	114°
31 ——— - - - - - -	113°
34 ——— - - - - - -	112°
37½ ——— - - - - - -	111°
and when 41 minutes had elapſed -	110°

Having taken away the borer, I now removed the metallic duſt, or rather ſcaly matter, which had been detached from the bottom of the cylinder by the blunt ſteel borer, in this Experiment; and, having carefully weighed it, I found its weight to be 837 grains Troy.

Is it poſſible that the very conſiderable quantity of Heat that was produced in this Experiment (a quantity which actually raiſed the temperature of above 113lb. of gun-metal at leaſt 70 degrees of Fahrenheit's thermometer, and which, of courſe, would have been capable of melting 6½lb. of ice, or of cauſing near 5lb. of ice-cold water to boil) could

could have been furniſhed by ſo inconſiderable a quantity of metallic duſt? and this merely in conſequence of *a change* of its capacity for Heat?

As the weight of this duſt (837 grains Troy) amounted to no more than $\frac{1}{948}$th part of that of the cylinder, it muſt have loſt no leſs than 948 degrees of Heat, to have been able to have raiſed the temperature of the cylinder 1 degree; and conſequently it muſt have given off 66360 degrees of Heat, to have produced the effects which were actually found to have been produced in the Experiment!

But, without inſiſting on the improbability of this ſuppoſition, we have only to recollect, that from the reſults of actual and deciſive Experiments, made for the expreſs purpoſe of aſcertaining that fact, the capacity for Heat, of the metal of which great guns are caſt, *is not ſenſibly changed* by being reduced to the form of metallic chips, in the operation of boring cannon; and there does not ſeem to be any reaſon to think that it can be much changed, if it be changed at all, in being reduced to much ſmaller pieces, by means of a borer that is leſs ſharp.

If the Heat, or any conſiderable part of it, were produced in conſequence of a change in the capacity for Heat of a part of the metal of the cylinder, as ſuch change could only be *ſuperficial*, the cylinder would by degrees be *exhauſted;* or the quantities of Heat produced, in any given ſhort ſpace of time, would be found to diminiſh gradually, in

ſucceſſive

ſucceſſive Experiments. To find out if this really happened or not, I repeated the laſt-mentioned Experiment ſeveral times, with the utmoſt care; but I did not diſcover the ſmalleſt ſign of exhauſtion in the metal, notwithſtanding the large quantities of Heat actually given off.

Finding ſo much reaſon to conclude, that the Heat generated in theſe Experiments, or *excited*, as I would rather chooſe to expreſs it, was not furniſhed *at the expence of the latent Heat* or *combined caloric* of the metal, I puſhed my inquiries a ſtep farther, and endeavoured to find out whether the air did, or did not, contribute any thing in the generation of it.

Experiment, N° 2.

As the bore of the cylinder was cylindrical, and as the iron bar (*m*), to the end of which the blunt ſteel borer was fixed, was ſquare, the air had free acceſs to the inſide of the bore, and even to the bottom of it, where the friction took place by which the Heat was excited.

As neither the metallic chips produced in the ordinary courſe of the operation of boring braſs cannon, nor the finer ſcaly particles produced in the laſt-mentioned Experiments by the friction of the blunt borer, ſhowed any ſigns of calcination, I did not ſee how the air could poſſibly have been the cauſe of the Heat that was produced; but, in an inveſtigation of this kind, I thought that no pains ſhould be ſpared to clear away the rubbiſh, and leave the ſubject as naked and open to inſpection as poſſible.

In order, by one deciſive Experiment, to determine whether the air of the atmoſphere had any part, or not, in the generation of the Heat, I contrived to repeat the Experiment, under circumſtances in which *it was evidently impoſſible for it to produce any effect whatever.* By means of a piſton exactly fitted to the mouth of the bore of the cylinder, through the middle of which piſton the ſquare iron bar, to the end of which the blunt ſteel borer was fixed, paſſed in a ſquare hole made perfectly air-tight, the acceſs of the external air, to the inſide of the bore of the cylinder, was effectually prevented. (In Fig. 3. this piſton (*p*) is ſeen in its place; it is likewiſe ſhown in Fig. 7 and 8.).

I did not find, however, by this Experiment, that the excluſion of the air diminiſhed, in the ſmalleſt degree, the quantity of Heat excited by the friction.

There ſtill remained one doubt, which, though it appeared to me to be ſo ſlight as hardly to deſerve any attention, I was however deſirous to remove. The piſton which cloſed the mouth of the bore of the cylinder, in order that it might be air-tight, was fitted into it with ſo much nicety, by means of its collars of leather, and preſſed againſt it with ſo much force, that, notwithſtanding its being oiled, it occaſioned a conſiderable degree of friction, when the hollow cylinder was turned round

round its axis. Was not the Heat produced, or at leaft fome part of it, occafioned by this friction of the pifton? and, as the external air had free accefs to the extremity of the bore, where it came in contact with the pifton, is it not poffible that this air may have had fome fhare in the generation of the Heat produced?

Experiment, N° 3.

A quadrangular oblong deal box, (fee Fig. 4.) water-tight, $11\frac{1}{2}$ Englifh inches long, $9\frac{4}{10}$ inches wide, and $9\frac{6}{10}$ inches deep, (meafured in the clear,) being provided, with holes or flits in the middle of each of its ends, juft large enough to receive, the one, the fquare iron rod to the end of which the blunt fteel borer was faftened, the other, the fmall cylindrical neck which joined the hollow cylinder to the cannon; when this box (which was occafionally clofed above, by a wooden cover or lid moving on hinges) was put into its place; that is to fay, when, by means of the two vertical openings or flits in its two ends, (the upper parts of which openings were occafionally clofed, by means of narrow pieces of wood fliding in vertical grooves,) the box (*g*, *h*, *i*, *k*, Fig. 3.) was fixed to the machinery, in fuch a manner that its bottom (*i*, *k*,) being in the plane of the horizon, its axis coincided with the axis of the hollow metallic cylinder; it is evident, from the defcription, that the hollow metallic cylinder would occupy the middle of the box, without touching it on either fide (as it is repre-

fented

ſented in Fig. 3.); and that, on pouring water into the box, and filling it to the brim, the cylinder would be completely covered, and ſurrounded on every ſide, by that fluid. And farther, as the box was held faſt by the ſtrong ſquare iron rod (*m*), which paſſed, in a *ſquare hole*, in the centre of one of its ends, (*a*, Fig. 4.) while the round or cylindrical neck, which joined the hollow cylinder to the end of the cannon, could turn round freely on its axis in the *round hole* in the centre of the other end of it, it is evident that the machinery could be put in motion, without the leaſt danger of forcing the box out of its place, throwing the water out of it, or deranging any part of the apparatus.

Every thing being ready, I proceeded to make the Experiment I had projected, in the following manner:

The hollow cylinder having been previouſly cleaned out, and the inſide of its bore wiped with a clean towel till it was quite dry, the ſquare iron bar, with the blunt ſteel borer fixed to the end of it, was put into its place; the mouth of the bore of the cylinder being cloſed at the ſame time, by means of the circular piſton, through the centre of which the iron bar paſſed.

This being done, the box was put in its place, and the joinings of the iron rod, and of the neck of the cylinder, with the two ends of the box, having been made water-tight, by means of collars of oiled leather, the box was filled with cold water, (*viz.* at the temperature of 60°,) and the machine was put in motion.

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The refult of this beautiful Experiment was very ftriking, and the pleafure it afforded me amply repaid me for all the trouble I had had, in contriving and arranging the complicated machinery ufed in making it.

The cylinder, revolving at the rate of about 32 times in a minute, had been in motion but a fhort time, when I perceived, by putting my hand into the water, and touching the outfide of the cylinder, that Heat was generated; and it was not long before the water which furrounded the cylinder began to be fenfibly warm.

At the end of 1 hour I found, by plunging a thermometer into the water in the box, (the quantity of which fluid amounted to 18.77 lb. Avoirdupois, or 2¼ wine gallons,) that its temperature had been raifed no lefs than 47 degrees; being now 107° of FAHRENHEIT'S fcale.

When 30 minutes more had elapfed, or 1 hour and 30 minutes after the machinery had been put in motion, the Heat of the water in the box was 142°.

At the end of 2 hours, reckoning from the beginning of the Experiment, the temperature of the water was found to be raifed to 178°.

At 2 hours 20 minutes it was at 200°; and at 2 hours 30 minutes it ACTUALLY BOILED!

It would be difficult to defcribe the furprife and aftonifhment expreffed in the countenances of

 the

the by-ſtanders, on ſeeing ſo large a quantity of cold water heated, and actually made to boil, without any fire.

Though there was, in fact, nothing that could juſtly be conſidered as ſurpriſing in this event, yet I acknowledge fairly that it afforded me a degree of childiſh pleaſure, which, were I ambitious of the reputation of a *grave philoſopher*, I ought moſt certainly rather to hide than to diſcover.

The quantity of Heat excited and accumulated in this Experiment was very conſiderable; for, not only the water in the box, but *alſo* the box itſelf, (which weighed 15¼ lb.) and the hollow metallic cylinder, and that part of the iron bar which, being ſituated within the cavity of the box, was immerſed in the water, were heated 150 degrees of FAHRENHEIT'S ſcale; *viz.* from 60° (which was the temperature of the water, and of the machinery, at the beginning of the Experiment) to 210°, the Heat of boiling water at Munich.

The total quantity of Heat generated may be eſtimated with ſome conſiderable degree of preciſion, as follows:

Of the Heat excited there appears to have been actually accumulated,	Quantity of ice-cold water which, with the given quantity of Heat, might have been heated 180 degrees, or made to boil. In Avoirdupois weight.
In the water contained in the wooden box, 18$\frac{3}{4}$ lb. Avoirdupois, heated 150 degrees, namely, from 60° to 210° F. - - - - - -	lb. 15.2
In 113.13 lb. of gun-metal, (the hollow cylinder,) heated 150 degrees; and, as the capacity for Heat of this metal is to that of water as 0.1100 to 1.0000, this quantity of Heat would have heated 12$\frac{1}{2}$ lb. of water the same number of degrees -	10.37
In 36.75 cubic inches of iron, (being that part of the iron bar to which the borer was fixed which entered the box,) heated 150 degrees; which may be reckoned equal in capacity for Heat to 1.21 lb. of water - - - -	1.01

N. B. No estimate is here made of the Heat accumulated in the wooden box, nor of that dispersed during the Experiment.

Total quantity of ice-cold water which, with the Heat actually generated by friction, and accumulated in 2 hours and 30 minutes, might have been heated 180 degrees, or made to boil - - -	26.58

From the knowledge of the *quantity* of Heat actually produced in the foregoing Experiment, and of the *time* in which it was generated, we are enabled to ascertain *the velocity of its production*, and to determine how large a fire must have been, or how much fuel must have been consumed, in order that, in burning equably, it should have produced by combustion the same quantity of Heat in the same time.

In one of Dr. CRAWFORD's Experiments, (see his Treatise on Heat, p. 321,) 37 lb. 7 oz. Troy, = 181920 grains, of water, were heated $2\frac{1}{10}$ degrees of FAHRENHEIT's thermometer, with the Heat generated in the combustion of 26 grains of wax. This gives 382032 grains of water heated 1 degree with 26 grains of wax; or $14693\frac{14}{26}$ grains of water heated 1 degree, or $\frac{14693}{180} = 81.631$ grains heated 180 degrees, with the Heat generated in the combustion of 1 grain of wax.

The quantity of ice-cold water which might have been heated 180 degrees, with the Heat generated by friction in the before-mentioned Experiment, was found to be 26.58 lb. Avoirdupois, = 188060 grains; and, as 81.631 grains of ice-cold water require the Heat generated in the combustion of 1 grain of wax, to heat it 180 degrees, the former quantity of ice-cold water, namely 188060 grains, would require the combustion of no less than 2303.8 grains ($=4\frac{8}{10}$oz. Troy) of wax, to heat it 180 degrees.

As

As the Experiment (N° 3) in which the given quantity of Heat was generated by friction, lafted 2 hours and 30 minutes, = 150 minutes, it is neceffary, for the purpofe of afcertaining how many wax-candles of any given fize muft burn together, in order that in the combuftion of them the given quantity of Heat may be generated in the given time, and confequently *with the fame celerity* as that with which the Heat was generated by friction in the Experiment, that the fize of the candles fhould be determined, and the quantity of wax confumed in a given time by each candle, in burning equably, fhould be known.

Now I found by an Experiment, made on purpofe to finifh thefe computations, that when a good wax-candle, of a moderate fize, $\frac{3}{4}$ of an inch in diameter, burns with a clear flame, juft 49 grains of wax are confumed in 30 minutes. Hence it appears, that 245 grains of wax would be confumed by fuch a candle in 150 minutes; and that, to burn the quantity of wax (= 2303.8 grains) neceffary to produce the quantity of Heat actually obtained by friction in the Experiment in queftion, and in the given time, (150 minutes,) *nine candles*, burning at once, would not be fufficient; for 9 multiplied into 245 (the number of grains confumed by each candle in 150 minutes) amounts to no more than 2205 grains; whereas the quantity of wax neceffary to be burnt, in order to produce the given quantity of Heat, was found to be 2303.8 grains.

 From

From the refult of thefe computations it appears, that the quantity of Heat produced equably, or in a continual ftream, (if I may ufe that expreffion,) by the friction of the blunt fteel borer againft the bottom of the hollow metallic cylinder, in the Experiment under confideration, was *greater* than that produced equably in the combuftion of *nine wax-candles*, each $\frac{3}{4}$ of an inch in diameter, all burning together, or at the fame time, with clear bright flames.

As the machinery ufed in this Experiment could eafily be carried round by the force of one horfe, (though, to render the work lighter, two horfes were actually employed in doing it,) thefe computations fhow further how large a quantity of Heat might be produced, by proper mechanical contrivance, merely by the ftrength of a horfe, without either fire, light, combuftion, or chemical decompofition; and, in a cafe of neceffity, the Heat thus produced might be ufed in cooking victuals.

But no circumftances can be imagined, in which this method of procuring Heat would not be difadvantageous; for, more Heat might be obtained by ufing the fodder neceffary for the fupport of a horfe, as fuel.

As foon as the laft-mentioned Experiment (N° 3.) was finifhed, the water in the wooden box was let off, and the box removed; and the borer being taken out of the cylinder, the fcaly metallic powder, which had been produced by the friction of the borer againft the bottom of the

the cylinder, was collected, and, being carefully weighed, was found to weigh 4145 grains, or about $8\frac{2}{3}$ oz. Troy.

As this quantity was produced in $2\frac{1}{2}$ hours, this gives 824 grains for the quantity produced *in half an hour.*

In the first Experiment, which lasted only *half an hour*, the quantity produced was 837 grains.

In the Experiment N° 1. the quantity of Heat generated, in *half an hour*, was found to be equal to that which would be required to heat 5 lb. Avoirdupois of ice-cold water 180 degrees, or cause it to boil.

According to the result of the Experiment N° 3. the Heat generated in *half an hour* would have caused 5.31 lb. of ice-cold water to boil. But, in this last-mentioned Experiment, the Heat generated being more effectually confined, less of it was lost; which accounts for the difference of the results of the two Experiments.

It remains for me to give an account of one Experiment more, which was made with this apparatus. I found by the Experiment N° 1. how much Heat was generated when the air had free access to the metallic surfaces which were rubbed together. By the Experiment N° 2. I found that the quantity of Heat generated was not sensibly diminished when the free access of the air was prevented; and by the result of N° 3. it appeared that the generation of the Heat was not prevented, or retarded, by keeping the apparatus

 immersed

immerſed in water. But as, in this laſt-mentioned Experiment, the water, though it ſurrounded the hollow metallic cylinder on every ſide, externally, was not ſuffered to enter the cavity of its bore; (being prevented by the piſton,) and conſequently did not come into contact with the metallic ſurfaces where the Heat was generated; to ſee what effects would be produced by giving the water free acceſs to theſe ſurfaces, I now made the

Experiment, N° 4.

The piſton which cloſed the end of the bore of the cylinder being removed, the blunt borer and the cylinder were once more put together; and the box being fixed in its place, and filled with water, the machinery was again put in motion.

There was nothing in the reſult of this Experiment that renders it neceſſary for me to be very particular in my account of it. Heat was generated, as in the former Experiments, and, to all appearance, quite as rapidly; and I have no doubt but the water in the box would have been brought to boil, had the Experiment been continued as long as the laſt. The only circumſtance that ſurpriſed me was, to find how little difference was occaſioned in the noiſe made by the borer in rubbing againſt the bottom of the bore of the cylinder, by filling the bore with water. This noiſe, which was very grating to the ear, and ſometimes almoſt inſupportable, was, as nearly as I could judge of it, quite as loud, and as diſagreeable, when the ſur-

 faces

faces rubbed together were wet with water, as when they were in contact with air.

By meditating on the results of all these Experiments, we are naturally brought to that great question which has so often been the subject of speculation among philosophers; namely,

What is Heat?—Is there any such thing as an *igneous fluid?*—Is there any thing that can with propriety be called *caloric?*

We have seen that a very considerable quantity of Heat may be excited in the Friction of two metallic surfaces, and given off in a constant stream or flux, *in all directions*, without interruption or intermission, and without any signs of diminution or exhaustion.

From whence came the Heat which was continually given off in this manner, in the foregoing Experiments? Was it furnished by the small particles of metal, detached from the larger solid masses, on their being rubbed together? This, as we have already seen, could not possibly have been the case.

Was it furnished by the air? This could not have been the case; for, in three of the Experiments, the machinery being kept immersed in water, the access of the air of the atmosphere was completely prevented.

Was it furnished by the water which surrounded the machinery? That this could not have been the case is evident: *first*, because this water was continually *receiving Heat* from the machinery, and could

could not, at the ſame time, be *giving to*, and *receiving Heat from*, the ſame body; and *ſecondly*, becauſe there was no chemical decompoſition of any part of this water. Had any ſuch decompoſition taken place, (which indeed could not reaſonably have been expected,) one of its component elaſtic fluids (moſt probably inflammable air) muſt, at the ſame time, have been ſet at liberty, and, in making its eſcape into the atmoſphere, would have been detected; but though I frequently examined the water to ſee if any air bubbles roſe up through it, and had even made preparations for catching them, in order to examine them, if any ſhould appear, I could perceive none; nor was there any ſign of decompoſition of any kind whatever, or other chemical proceſs, going on in the water.

Is it poſſible that the Heat could have been ſupplied by means of the iron bar to the end of which the blunt ſteel borer was fixed? or by the ſmall neck of gun-metal by which the hollow cylinder was united to the cannon? Theſe ſuppoſitions appear more improbable even than either of thoſe before mentioned; for Heat was continually going off, or *out of the machinery*, by both theſe paſſages, during the whole time the Experiment laſted.

And, in reaſoning on this ſubject, we muſt not forget to conſider that moſt remarkable circumſtance, that the ſource of the Heat generated by friction, in theſe Experiments, appeared evidently to be *inexhauſtible*.

It

It is hardly neceſſary to add, that any thing which any *inſulated* body, or ſyſtem of bodies, can continue to furniſh *without limitation*, cannot poſſibly be *a material ſubſtance:* and it appears to me to be extremely difficult, if not quite impoſſible, to form any diſtinct idea of any thing, capable of being excited and communicated, in the manner the Heat was excited and communicated in theſe Experiments, except it be MOTION.

I am very far from pretending to know how, or by what means, or mechanical contrivance, that particular kind of motion in bodies, which has been ſuppoſed to conſtitute Heat, is excited, continued, and propagated, and I ſhall not preſume to trouble the Society with mere conjectures; particularly on a ſubject which, during ſo many thouſand years, the moſt enlightened philoſophers have endeavoured, but in vain, to comprehend.

But, although the mechaniſm of Heat ſhould, in fact, be one of thoſe myſteries of nature which are beyond the reach of human intelligence, this ought by no means to diſcourage us, or even leſſen our ardour, in our attempts to inveſtigate the laws of its operations. How far can we advance in any of the paths which ſcience has opened to us, before we find ourſelves enveloped in thoſe thick miſts which, on every ſide, bound the horizon of the human intellect? But how ample, and how intereſting, is the field that is given us to explore!

Nobody,

Nobody, ſurely, in his ſober ſenſes, has ever pretended to underſtand the mechaniſm of gravitation; and yet what ſublime diſcoveries was our immortal NEWTON enabled to make, merely by the inveſtigation of the laws of its action!

The effects produced in the world by the agency of Heat are probably *juſt as extenſive*, and quite as important, as thoſe which are owing to the tendency of the particles of matter towards each other; and there is no doubt but its operations are, in all caſes, determined by laws equally immutable.

Before I finiſh this Eſſay, I would beg leave to obſerve, that although, in treating the ſubject I have endeavoured to inveſtigate, I have made no mention of the names of thoſe who have gone over the ſame ground before me, nor of the ſucceſs of their labours; this omiſſion has not been owing to any want of reſpect for my predeceſſors, but was merely to avoid prolixity, and to be more at liberty to purſue, without interruption, the natural train of my own ideas.

DESCRIP-

DESCRIPTION *of the* FIGURES.

Fig. 1 ſhows the cannon uſed in the foregoing Experiments, in the ſtate it was in when it came from the foundry.

Fig. 2 ſhows the machinery uſed in the Experiments N° 1 and N° 2. The cannon is ſeen fixed in the machine uſed for boring cannon. W is a ſtrong iron bar, (which, to ſave room in the drawing, is repreſented as broken off,) which bar, being united with machinery (not expreſſed in the figure) that is carried round by horſes, cauſes the cannon to turn round its axis.

m is a ſtrong iron bar, to the end of which the blunt borer is fixed; which, by being forced againſt the bottom of the bore of the ſhort hollow cylinder that remains connected by a ſmall cylindrical neck to the end of the cannon, is uſed in generating Heat by friction.

Fig. 3 ſhows, on an enlarged ſcale, the ſame hollow cylinder that is repreſented on a ſmaller ſcale in the foregoing Figure. It is here ſeen connected with the wooden box (*g*, *h*, *i*, *k*,) uſed in the Experiments N° 3 and N° 4. when this hollow cylinder was immerſed in water.

p, which is marked by dotted lines, is the piſton which cloſed the end of the bore of the cylinder.

n is the blunt borer ſeen ſidewiſe.

d, *e*, is the ſmall hole by which the thermometer was introduced, that was uſed for aſcertaining

ing the Heat of the cylinder. To ſave room in the drawing, the cannon is repreſented broken off near its muzzle; and the iron bar, to which the blunt borer is fixed, is repreſented broken off at *m*.

Fig. 4, is a perſpective view of the wooden box, a ſection of which is ſeen in the foregoing Figure. (See *g*, *h*, *i*, *k*, Fig. 3.)

Fig. 5 and 6, repreſent the blunt borer *n*, joined to the iron bar *m*, to which it was faſtened.

Fig. 7 and 8, repreſent the ſame borer, with its iron bar, together with the piſton which, in the Experiments N° 2 and N° 3. was uſed to cloſe the mouth of the hollow cylinder.

END OF THE SECOND VOLUME.