II. *Of the Heat*, &c. *of Animals and Vegetables*. *By Mr*. John Hunter, *F. R. S.*

Read June 19, and Nov. 13, 1777.

IN the courfe of a variety of experiments on animals and vegetables, I have frequently obferved that the refult of experiments in the one has explained the œconomy of the other, and pointed out fome principle common to both; I have therefore collected fome experiments which relate to the heat and cold of thofe fubftances. Having found variations in the degree of heat and cold in the fame experiment, for which I could not account; I fufpected that this might arife from fome imperfection in the conftruction of the thermometer. I mentioned to Mr. RAMSDEN my objection to the common conftruction of that inftrument, and my ideas of one more perfect in its nature, and better adapted to the experiments in which I was engaged. He accordingly made me fome very fmall thermometers, fix or feven inches long, not above $\frac{2}{12}$ths of an inch thick in the ftem; having the external diameter of the ball very little larger than that of the

the ſtem, on which was marked the freezing point. The ſtem was embraced by a ſmall ivory ſcale ſo as to ſlide upon it eaſily, and retain any poſition. Upon the hollow ſurface of this ſcale were marked the degrees which were ſeen through the ſtem. By theſe means the ſize of the thermometer was very much reduced, and it could be applied to ſoft bodies with much more eaſe and certainty, and in many caſes in which the former ones could not be conveniently applied: I therefore repeated with it ſuch of my former experiments as were not originally ſatisfactory, and found the degrees of heat very different, not only from what I generally imagined, but alſo from what I had found in my former experiments with the thermometers of the common conſtruction.

I have obſerved in a former paper[a], and find it ſupported by every experiment I have made on the heat and cold of animals, that the more perfect have the greateſt power of retaining a certain degree of heat, which may be called their ſtandard heat, and allow of much leſs variation than the more imperfect animals: however, it will appear from the firſt, ſecond, and third experiments, that many, if not all of them, are not capable of keeping conſtantly to one ſtandard; but vary from their ſtandard

(a) Vide Philoſophical Tranſactions for the year 1775, vol. LXV. part II. p. 446.

heat,

heat, either by external applications, or diſeaſe. However, theſe variations are much greater below the ſtandard heat than above it; the perfect animals having a greater power of reſiſting heat than cold, ſo that they are commonly near their ultimate heat. Indeed we do not want any other proof of this variation than our own feelings: we are all ſenſible of heat and of cold, which ſenſations could not be produced without an alteration really taking place in the parts affected; which alteration in the parts could not take place, if they did not become actually warmer or colder. I have often cooled my hands to ſuch a degree, that I have put them into pump-water, immediately pumped, to warm them; therefore, my hands were really colder than the pump-water.

Real increaſe of heat muſt alter the texture or poſition of the parts, ſo as to produce the ſenſation called heat: and as this heat is diminiſhed, the texture or poſition of the parts is altered in a contrary way; which, when carried to a certain degree, becomes the cauſe of the ſenſation of cold. Now theſe effects could not take place in either caſe without a real increaſe or decreaſe of heat in the part; heat, therefore, in its different degrees, muſt be preſent. When heat is applied to the ſkin, it becomes hot, in ſome degree, according to the application; and this may be carried ſo far as actually to burn the living parts:

parts: on the contrary, in a cold atmoſphere, a man's hand ſhall become ſo cold as to loſe the ſenſation of cold altogether, and change it for that of pain. Real heat and cold can be carried ſo far, as even to alter the ſtructure of the parts upon which the actions of life depend.

As animals are ſubject to variations in their degrees of heat and cold from external applications, they are of courſe, in this reſpect, affected in ſome meaſure like inanimate matter; and therefore, as parts are elongated or recede from the common maſs, theſe effects more readily take place: for inſtance, all projecting parts and extremities, more eſpecially toes, fingers, noſe, ears, combs of fowls, particularly of the cock, are more readily cooled, and are therefore moſt ſubject to be affected by cold. Animals are not only ſubject to increaſe and decreaſe of heat, ſimilar to inanimate matter; but the tranſition from the one to the other (as far as they allow themſelves to go) is nearly as quick. However, I ſhall not confine myſelf to ſenſation alone, for it is in ſome degree ruled by habit: the habit of uniformity in the degree of the one or the other, will be the cauſe of a conſiderable increaſe of ſenſation from the ſmalleſt variation; while the habit of variation in the degree of heat and cold, will, in a proportional degree, prevent the ſenſation ariſing from either: but we ſhall be guided by actual experiment.

The parts above mentioned (*viz.* projecting parts and extremities) are ſuch as will admit of the greateſt change in their degrees of heat and cold, without materially affecting the animal. I find that they will raiſe or ſink the thermometer, in ſome degree, according to the external heat or cold applied; although not in a proportional degree to this application, as would be the caſe in inanimate matter. Nor are the living parts cooled or heated in the ſame degrees, which appears from the application of the thermometer to the ſkin; for the cuticle may be conſidered as a dead covering, capable of taking greater degrees of heat or cold, than the living parts underneath can do; and it might be ſuſpected, that the whole of the variation was in the covering. To remove this doubt I made the following experiments.

EXP. I. I ſunk the ball of my thermometer under my tongue, which lay perfectly covered by all the ſurrounding parts, kept it there ſome minutes, and found that it roſe to 97°; having continued it ſome time longer there, I found it roſe no higher. I then took ſeveral pieces of ice, about the ſize of walnuts, and put them in the ſame ſituation, allowing them to melt in part, but not wholly, that the application of cold might be better kept up, occaſionally ſpitting out the water ariſing from the ſolution: this I continued for ten minutes, and found, on introducing my thermometer, that it fell to 77°; ſo that the mouth at this part had loſt 20° of heat. It gradually roſe to 97° again; but the thermometer in this experiment did not ſink ſo low as it would have done in the hand, if a piece of ice had been held in it ſo long. Perhaps one reaſon may be aſſigned: the ſurface under the tongue being ſurrounded with warm parts, renders it next to an impoſſibility to cool it to any greater degree: but I ſuſpect ſtill another reaſon, *viz.* parts which have been in a habit of conſiderably varying in this reſpect, as the hand, will allow of greater latitude, being as it were inſenſibly drawn into cold, nor ſo ſuſceptible of it, as has been already obſerved.

As a further proof, that the more perfect animals are capable of varying their heat, in ſome degree, according to the external heat applied, I ſhall adduce the following experiments made on the human ſubject.

The mouth being a part ſo frequently in contact with the external atmoſphere in the action of breathing, whatever is put into it will be ſuppoſed to be influenced by that atmoſphere; this will always render an experiment made in the mouth, relative to heat and cold, in ſome degree doubtful. I imagined that the urethra would anſwer better, becauſe it is an internal cavity, and can be only influenced by heat and cold applied to the

external

external ſkin of the parts. I imagined alſo, that whatever effects heat or cold might have, when applied, would ſooner take place in the urethra than in any other part of the body, as it is a projecting part; and therefore, if living animal matter was in any degree ſubject to the common laws of matter in this reſpect, the urethra would be readily affected: for this purpoſe I got a perſon, who allowed me to make ſuch experiments as I thought neceſſary.

EXP. II. I introduced the ball of my thermometer into the urethra about an inch; after it had remained there a minute, the quickſilver roſe only to 92°; at two inches, it roſe to 93°; at four inches, the quickſilver roſe to 94°; and when the ball had got as far as the bulb of the urethra, where it is ſurrounded by warm parts, the quickſilver roſe to 97°.

EXP. III. Theſe parts being immerſed in water heated only to 65° for one minute, and the thermometer introduced about an inch and a half into the urethra, the quickſilver roſe to 79°: this was repeated ſeveral times with the ſame ſucceſs. To find if there was any difference in the quickneſs of the tranſition of heat and cold in living and dead parts, and alſo if the latitudes to which each would go were alſo different, I made the following experiments.. As this (*viz.* the urethra) ſtill appeared

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appeared to me to be the very beſt part of any animal body for experiments of this kind, I had recourſe to it; and as all comparative experiments ſhould be as ſimilar to one another as poſſible, excepting in thoſe points where the difference (if there is any) makes the eſſential part of the experiment, I procured a dead penis.

EXP. IV. The heat of the penis of a living perſon, an inch and a half in the urethra, was 92° exactly. I firſt heated the dead one to the ſame degree; and then had the living one immerſed in water at 50°, at the ſame time immerſing the dead one in the ſame water; when, introducing the thermometer at different times, I obſerved their comparative quickneſs in cooling from 92°. The dead one cooled faſter; but only by two or three degrees. The living came down to 58°, and the dead to 55°. After having continued the thermometer there ſome time longer, it fell no lower. I repeated the ſame experiment ſeveral times, with the ſame ſucceſs; although ſometimes there was a ſmall difference in the degrees of heat from thoſe of others, the heat of the water alſo differing; but the difference in the reſult was nearly in proportion, in all the three different trials, therefore the ſame concluſions are to be drawn from them. In theſe laſt experiments we find very little difference between the cooling of a part of a dead body, and that of the living; but we cannot ſuppoſe that this can take

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place through the whole body, as in this caſe a living man ſhould always be of the ſame degree of heat with the atmoſphere in which he lives. The man not chooſing to be cooled lower than 53° or 54°, put it out of my power to ſee if the powers of generating heat were exerted in a higher degree, when the heat was brought ſo low as to threaten deſtruction; but from ſome experiments on mice, which will be related hereafter, it will appear, that the animal powers are called upon to exert themſelves in this, when neceſſary.

From the experiments related I found, that parts of an animal were capable of becoming much colder than the common or natural heat: I therefore made farther experiments, with a view to ſee whether the ſame parts were capable of becoming much hotter than the ſtandard heat of animals. The experiments were made in the ſame manner as the former, only the water was now hotter than the natural heat of the animal.

EXP. V. The natural heat of the parts being 92°, they were now immerſed in water heated to 113° for two minutes, and the thermometer being introduced as before, the quickſilver roſe to 100°½. This experiment I alſo repeated ſeveral times, but could not raiſe the heat of the penis beyond 100°½: this was probably owing to the perſon not being able at this time to bear the application of water warmer than 113°. As theſe were only ſingle

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experiments, I chofe to make a comparative one with the dead part.

EXP. VI. Both the living and dead part being immerfed in water, gradually made warmer and warmer from 100° to 118°, and continued in this heat for fome minutes, the dead part raifed the thermometer to 114°, while the living could not raife it higher than 102°$\frac{1}{4}$. It was obferved, by the perfon on whom the experiment was made, that, after the parts had been in the water about a minute, the water did not feel hot; but, on its being agitated, it felt fo hot that he could hardly bear it. Upon applying the thermometer to the fides of the living gland, the quickfilver immediately fell from 118° to about 104°, while it did not fall above a degree when put clofe to the dead; fo that the living gland produced a cold fpace of water around it[(b)].

EXP. VII. The heat of the rectum in the fame man was 98°$\frac{1}{2}$ exactly.

In the fecond, third, fourth, fifth, and fixth experiments, we had an internal cavity, which is both very vafcular and fenfible, evidently influenced by external heat and cold, though only applied to the fkin of the part;

(*b*) This might furnifh an ufeful hint refpecting bathing in water, whether colder or warmer than the heat of the body: for if intended to be either colder or hotter, it will foon be of the fame temperature with that of the body; therefore in a large bath, the patient fhould move from place to place: and in a fmall one, there fhould be a conftant fucceffion of water of the intended heat.

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while, in the ſeventh experiment, another part of the ſame body, where external heat and cold can make little or no impreſſion, was of the ſtandard heat. Although we ſhall find hereafter, from experiment, that the rectum is not the warmeſt part of an animal; yet, in order to determine how far the heat could be increaſed by ſtimulating the conſtitution to a degree ſufficient to quicken the pulſe, I repeated the ſeventh experiment after the man had eaten a hearty ſupper, and drank a bottle of wine, which increaſed the pulſe from 73° to 87°, and yet the thermometer only roſe to $98°\frac{1}{2}$.

Having formerly made experiments upon dormice in the ſleeping ſeaſon, with a view to ſee if there was any alteration in the animal œconomy at that time, I find amongſt theſe experiments the following which appear to be to our preſent purpoſe: but, that I might be more certain of the accuracy of my former experiments, I repeated them with my new thermometer.

EXP. VIII. In a room, in which the air was at between 50° and 60° of temperature, a ſmall opening was made in the belly of a dormouſe, of a ſufficient ſize to admit the ball of my thermometer, which, being introduced into the belly at about the middle of that cavity, roſe to 80°, and no higher.

EXP. IX. The moufe was put into a cold atmofphere of 15° above 0, and left there for fifteen minutes; after which, the thermometer being introduced a fecond time, it rofe to 85°.

EXP. X. The moufe was again put into a cold atmofphere for fifteen minutes more; and the thermometer being then introduced, the quickfilver rofe to 72° only, but gradually came up to 83°, 84°, and 85°.

EXP. XI. It was put a third time into the cold atmofphere, and allowed to ftay there for thirty minutes; the lower part of the moufe was at the bottom of the difh, and almoft frozen; the whole of the animal was a little numbed, and a good deal weakened. When the thermometer was introduced, it varied according to the different parts of the belly; in the pelvis, near the parts moft expofed to the cold, it was as low as 62°; in the middle, among the inteftines, about 70°; but near the diaphragm it rofe to 80°, 82°, 84°, and 85°; fo that in the middle of the body the heat had decreafed 10°. Finding a variation in different parts of the fame cavity in the fame animal, I repeated the fame experiments upon another dormoufe.

EXP. XII. I took a healthy dormoufe, which had been afleep in a room in which there was a fire (the atmofphere at 64°); I put the thermometer into its belly, nearly at the middle,

middle, between the thorax and pubis, and the quickſilver roſe to 74° or 75°; when I turned the ball towards the diaphragm, it roſe to 80°; and when I applied it to the liver, it roſe to 81°½.

EXP. XIII. The mouſe was put into an atmoſphere at 20°, and left there half an hour; when taken out, it was very lively, much more ſo than when put in. I introduced the thermometer into the lower part of the belly, and it roſe to 91°; and upon turning it up to the liver, to 93°.

EXP. XIV. The animal was put back into the cold atmoſphere at 30° for an hour, when the thermometer was again introduced into the belly; at the liver it roſe to 93°; in the pelvis, to 92°: it was ſtill very lively.

EXP. XV. It was again put back into the cold atmoſphere at 19°, and left there an hour; the thermometer at the diaphragm was 87°; in the pelvis, 83°; but the animal was now leſs lively.

EXP. XVI. It was put into its cage, and two hours after the thermometer, placed at the diaphragm, was at 93°.

From theſe experiments we have actual heat increaſed and decreaſed by the application of external cold; and likewiſe the heat varied according to the powers of life, as well in the ſame parts, as alſo in the different parts, of the ſame animal; for at firſt the natural heat of the animal was much below the common ſtandard, and, by the application of cold, and the powers of reſiſtance to the cold being thus increaſed, the heat was conſiderably augmented; but when the animal was weakened by thoſe exertions, it fell off with reſpect to the power of producing heat, and this in proportion to the diſtance from the heart.

Why the heat of this animal ſhould be ſo low as 80° in an atmoſphere of between 50° and 60°, is not eaſily accounted for, except upon the principle of ſleep. But I ſhould very much ſuſpect, that the ſimple principle of ſleep is out of the queſtion, as ſleep is an effect that takes place in all degrees of heat and cold. In thoſe animals where the voluntary actions are ſuſpended, it appears to be an effect ariſing from a certain degree of cold acting as a ſedative, under which the animal faculties are proportionably weakened, but ſtill retain the power of carrying on all the functions of life under ſuch circumſtances; but beyond this degree cold ſeems to act as a ſtimulant, and the animal powers are rouſed to action for ſelf-preſervation. It is more than probable, that moſt animals are under this predicament; and that every order has its degree of cold, in which the voluntary actions can be ſuſpended.

When man is aſleep, he is colder than when awake; and I find, in general, that the difference is about one de-

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gree and a half, ſometimes leſs. But this difference in the degree of cold between ſleeping and waking is not a cauſe of ſleep, but an effect; for many diſeaſes produce a much greater degree of cold in the animal, without giving the leaſt tendency to ſleep; therefore the inactivity of animals from cold is different from ſleep. Beſides, all the operations of perfect life are going on in the time of natural ſleep, at leaſt in the perfect animals, ſuch as digeſtion, ſenſations, &c.; but none of theſe operations are performed in the latter tribe.

To ſee how far the reſult of theſe experiments upon dormice was peculiar to them, I repeated the ſame expements upon common mice. I procured two; one ſtrong and vigorous, the other weakened by faſting.

EXP. XVII. The common atmoſphere being at 60°, I introduced the thermometer into the abdomen of the ſtrong mouſe: the ball being at the diaphragm, the quickſilver was raiſed to 99°, but at the pelvis only to 96°$\frac{3}{4}$.

Here there was a real difference of about 9° in two animals of the ſame ſize, in ſome degree of the ſame genus, and at the ſame ſeaſon of the year, and the atmoſphere of nearly the ſame temperature.

EXP. XVIII. The ſame mouſe was put into a cold atmoſphere of 13°, for an hour, and then the thermometer

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was introduced as before; the quickfilver at the diaphragm was raifed to 83°, in the pelvis only to 78°.

Here the real heat of the animal was diminifhed 16° at the diaphragm, and 18° in the pelvis.

EXP. XIX. In order to determine whether an animal that is weakened, has the fame powers, with refpect to preferving heat and cold, as one that is vigorous and ftrong, I introduced the ball of the thermometer into the belly of the weak moufe; the ball being at the diaphragm, the quickfilver rofe to 97°; in the pelvis to 95°: the moufe being put into the cold atmofphere as the other, and the thermometer again introduced, the quickfilver ftood at 79° at the diaphragm, and at 74° in the pelvis.

In this experiment the heat at the diaphragm was diminifhed 18°, in the pelvis 21°.

Here was a diminution of heat in the fecond greater than in the firft, we may fuppofe proportional to the decreafed power of the animal arifing from want of food.

To determine how far different parts of other animals than thofe mentioned were of different degrees of heat; I made the following experiments upon a healthy dog.

EXP. XX. The ball of the thermometer was introduced two inches within the rectum, the quickfilver rofe to $100°\frac{1}{2}$ exactly. The cheft of the dog was opened, and

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a wound made into the right ventricle of the heart, and the ball immediately introduced; the quickſilver roſe to 101° exactly. A wound was next made ſome way into the ſubſtance of the liver; and the ball being introduced, the quickſilver roſe to 100°$\frac{3}{4}$. It was next introduced into the cavity of the ſtomach, where it ſtood exactly at 101°. All theſe experiments were made in a few minutes.

EXP. XXI. The ſame experiments were made upon oxen; the quickſilver roſe exactly to 99°$\frac{1}{2}$.

EXP. XXII. The ſame were alſo made upon a rabbit, and the quickſilver roſe to 99°$\frac{1}{2}$.

From the experiments on mice, and thoſe upon the dog, it plainly appears, that every part of an animal is not of the ſame degree of heat; and hence we may reaſonably infer, that the heat of the vital parts of man is greater than what it is found to be either in the mouth, the rectum, or the urethra.

To determine how far my idea, that animals could have their heat varied in proportion to their imperfections, is juſt, I made the following experiments upon fowls, which I conſider to be one remove below what are commonly called quadrupeds.

EXP. XXIII. I introduced the ball of the thermometer ſucceſſively into the *inteſtinum rectum* of ſeveral hens, and

and found that the quickfilver rofe as high as 103°, 103°$\frac{1}{2}$, and in one of them to 104°.

EXP. XXIV. I made the fame experiments on feveral cocks, and the refult was the fame.

EXP. XXV. To determine if the heat of the hen was increafed when fhe was prepared for incubation, I repeated the twenty-third experiment upon feveral fitting or clocking hens; in one the quickfilver rofe to 104°; in the others, to 103$\frac{1}{2}$, 103°, as in the twenty-third experiment.

EXP. XXVI. Under the hen, who raifed the quickfilver to 104°, I placed the ball of the thermometer, and found the heat there as great as in the rectum.

EXP. XXVII. I took fome of the eggs from under the fame hen, where the chick was about three parts formed, broke a hole in the fhell, &c. and introduced the ball of the thermometer, and found that the quickfilver rofe to 99°$\frac{1}{2}$. In fome that were addled, I found their heat not fo high by two degrees; fo that the life in the living egg affifted in fome degree to fupport its own heat.

It may be afked, whether thofe three or four degrees of heat, which are found in the fowl more than in the quadruped, are for the purpofe of incubation? We found that the heat of the eggs, which was caufed and fupported by this heat, was not above the ftandard of the quadrupeds;

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and that it muſt probably have been leſs, if the heat of the hen had not been ſo great.

Finding from the above experiments, that fowls were ſome degrees warmer than that claſs commonly called quadrupeds (although certainly not ſo perfect animals) I choſe to continue the experiments upon the ſame principles, and made the following upon thoſe of a ſtill inferior order. The next remove from the fowl are thoſe commonly called amphibious.

EXP. XXVIII. I took a healthy viper, and introduced the thermometer into its ſtomach and anus; the quickſilver roſe from 58° (the heat of the atmoſphere in which it was) to 68°; ſo that in a common atmoſphere it is 10° warmer.

EXP. XXIX. The viper was put into a pan, and the pan into a cold mixture of about 10°; after being there about ten minutes, its heat was reduced to 37°. It was allowed to ſtay ten minutes longer, the mixture being at 13°, and its heat was reduced to 35°. It was allowed to ſtay ten minutes more, the mixture at 20°, its heat at 31°, and it did not become lower; its tail was beginning to freeze; and it was now very weak. It may be remarked, that it became cold much ſlower than many of the following animals.

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The frog being, in its ftructure, more fimilar to the viper than to either fowl or fifh, I made the following experiments on that animal.

EXP. XXX. I introduced the ball of the thermometer into its ftomach, and the quickfilver ftood at 44°. I then put it into a cold mixture, and the quickfilver funk to 31°; the animal appeared almoft dead, but recovered very foon: beyond this point it was not poffible to leffen the heat, without deftroying the animal. But its decreafe of heat was quicker than in the viper, although the mixture was nearly the fame.

The next order of animals were fifh.

EXP. XXXI. I afcertained the heat of water in a pond, where there were carp, and found it 65°$\frac{1}{2}$. I then took a carp out of the fame water, and introduced the thermometer into the ftomach; the quickfilver rofe to 69°; fo that the difference between the water and the fifh was only 3°$\frac{1}{2}$.

EXP. XXXII. In an eel, the heat in the ftomach, which at firft was at 37°, funk, after it had been fome time in the cold mixture, to 31°. The animal at that time appeared dead, but was alive the next day.

EXP. XXXIII. In a fnail, whofe heat was at 44°, it funk, after it had been put into the cold mixture, to 31°, and then the animal froze.

EXP. XXXIV. Several leaches having been put into a bottle, and that bottle immerfed in the cold mixture, the ball of the thermometer being placed in the middle of them, the quickfilver funk to 31°; and by continuing the immerfion for a fufficient time to deftroy life, the quickfilver rofe to 32°, and then the leaches froze. In all thefe experiments none of the animals returned to life when they became thawed.

Finding that thefe imperfect claffes of animals are capable of varying their heat to that ftandard which can freeze the folids or fluids when dead, and not much farther before death enfues, I wifhed to determine to what degree of heat the animal could be brought.

EXP. XXXV. A healthy viper was put into an atmofphere of 108°, and allowed to ftay feven minutes, when the heat of the animal in the ftomach and anus was found to be 92°½, beyond which it would not rife in the above heat. The fame experiment was made upon frogs with nearly the fame fuccefs.

EXP. XXXVI. An eel very weak, its heat at 44°, which was nearly that of the atmofphere, was put into water at 65°, for fifteen minutes; and, upon examination, it was of the fame degree of heat with the water.

EXP. XXXVII. A tench, whofe heat was 41°, was put into water at 65°, and left there ten minutes; the

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ball of the thermometer being introduced both into the ftomach and rectum, the quickfilver rofe to 55°. Thefe experiments were repeated with nearly the fame fuccefs.

To determine whether life had any power of refifting heat and cold in thefe claffes of animals, I made comparative trials between living and dead ones.

EXP. XXXVIII. I took a living and a dead tench, and a living and a dead eel, and put them into warm water; they all received heat equally faft; and when they were put into the cold, both the living and the dead received it equally.

I long fufpected, that the principle of life was not wholly confined to animals, or animal fubftance endowed with vifible organization and fpontaneous motion; but I conceived, that the fame principle exifted in animal fubftances, devoid of apparent organization and motion, where the power of prefervation fimply was required.

I was led to this notion twenty years ago, when I was making drawings of the growth of the chick in the procefs of incubation. I then obferved, that whenever an egg was hatched, the yolk (which is not diminifhed in the time of incubation) was always perfectly fweet to the very laft; and that part of the albumen, which is not expended on the growth of the animal, fome days before hatching, was alſo perfectly ſweet; although both were kept in a heat of 103°, in the hen's egg for three weeks, and in the duck's for four; but I obſerved, that if an egg was not hatched, that egg became putrid in nearly the ſame time with any other dead animal matter.

To determine how far eggs would ſtand other teſts of a living principle, I made the following experiments.

EXP. XXXIX. I put an egg into cold at about 0, and froze it, then allowed it to thaw; from this proceſs I conceived, that the preſerving powers of the egg muſt be loſt. I then put this egg into the cold mixture, and with it one newly laid; and the difference in freezing was ſeven minutes and a half, the freſh one taking ſo much longer time in freezing.

EXP. XL. A new laid egg was put into a cold atmoſphere, fluctuating between 17° and 15°; it took above half an hour to freeze; but, when thawed and put into an atmoſphere at 25°, it froze in half the time. This experiment was repeated ſeveral times, with nearly the ſame ſucceſs.

To determine the comparative heat between a living and a dead egg, and alſo to determine whether a living egg be ſubject to the ſame laws with the more imperfect animals, I made the following experiments.

EXP. XLI. A freſh egg, and one which had been frozen and thawed, were put into the cold mixture at 15°; the thawed one ſoon came to 32°, and began to ſwell and congeal; the freſh one ſunk to $29°\frac{1}{2}$, and in twenty-five minutes after the dead one, it roſe to 32°, and began to ſwell and freeze.

The reſult of this experiment upon the freſh egg was ſimilar to the above experiments upon the frog, eel, ſnail, &c. where life allowed the heat to be diminiſhed 2° or 3° below the freezing point, and then reſiſted all further decreaſe; but the powers of life were expended by this exertion, and then the parts froze like any other dead animal matter.

From theſe experiments in general it muſt appear, that a freſh egg has the power of reſiſting heat, cold, and putrefaction, equal to many of the more imperfect animals; and it is more than poſſible, that this power ariſes from the ſame principle in both.

From ſome of theſe experiments it appears, that the more imperfect animals are capable of having their heat and cold varied very conſiderably, not according to the extent of the heat or cold of the ſurrounding medium in which they can live, but according to the degree of cold which is capable of altering the parts in a dead ſtate, below which the living power will not go far; for

for whenever the ſurrounding cold brings them to that degree, the power of generating heat takes place till life is gone, then the animal freezes, and is immediately capable of admitting any degree of cold.

From theſe circumſtances of thoſe imperfect animals (upon which I made my experiments) varying their heat ſo readily, we may conclude, that heat is not ſo very eſſential to life in them as in the more perfect; although it be eſſential to many of the operations, or what may be called the ſecondary actions of life, ſuch as digeſting food[b], and the propagation of their ſpecies, which requires the greateſt power an animal can exert, more eſpecially the laſt; and, as moſt of the more perfect of theſe imperfect animals are commonly employed in the firſt, we may ſuppoſe their heat to be ſuch as this action of life requires, although in them it be never eſſentially neceſſary to be ſo high as to produce propagation[c]. Therefore

(*b*) How far this idea holds good with fiſh I am not certain.

(*c*) How far the animal heat is lowered in the more perfect animals, when theſe ſecondary actions are not neceſſary, as in the bat, hedge-hog, bear, &c. I have not been able to determine, not having opportunities of examining theſe animals in their involuntary ſtate. Dormice are in a mixed ſtate between the voluntary and involuntary, and we find the heat diminiſhed when the actions are not vigorous; and from a general review of this whole ſubject it would appear, that a certain degree of heat in the animal is neceſſary for digeſtion, and that neceſſary heat will be according to the nature of the animal. A frog will digeſt food when its heat is at 60°, but not when at 35° or 40°; and it is very

Therefore, whenever theſe imperfect animals are in a cold ſo low as to weaken their powers, and diſable them from performing the firſt of theſe ſecondary actions, they become in ſome meaſure involuntary, and remain in a torpid ſtate during the degree of cold which will always occur in ſome part of the winter in ſuch countries as they inhabit; and the food of ſuch animals in general not being produced in the cold ſeaſon, affords another reaſon for their torpidity.

From the circumſtances of their heat being allowed to ſink to the freezing point, or ſomewhat lower, and then becoming ſtationary; and of the animal not being able to ſupport life in a much greater degree of cold for a conſiderable time, we ſee a reaſon why thoſe animals always endeavour to procure ſuch places of abode in the winter as ſeldom arrive at that point. Thus we have toads burrowing, frogs living under large ſtones, ſnails protected under the ſhelter of ſtones and in holes, fiſh hav-

very probable that, when the heat of the bear, hedge-hog, dormouſe, bat, &c. is reduced to 70°, 75°, or 80°, they loſe their power of digeſtion; or rather that the body, in ſuch a degree of cold, has no call upon the ſtomach. That animals, in a certain degree of heat, muſt always have food, is further illuſtrated by the inſtance of bees. The conſtruction of a bee is very ſimilar to a fly, a waſp, &c. A fly and a waſp can allow their heat to diminiſh as in the fiſh, ſnake, &c. without loſing life, but a bee cannot; therefore a bee is obliged to keep up its heat as high as what we may call its digeſtive heat, but not its propagating; for which purpoſe they provide againſt ſuch cold as would deprive them even of their digeſtive heat, if they had not food to preſerve it.

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ing recourſe to deep water, all which places are generally above the freezing point in our hardeſt froſts: however, our froſts are ſometimes ſo ſevere as to kill many whoſe habitations are not very ſecure.

When the froſt is more intenſe and of longer ſtanding than common, or in countries where the winters are always ſevere, there is generally ſnow, and the water freezes: the advantage ariſing from theſe two circumſtances are great; the ſnow ſerving as a blanket to the earth, and the ice to the water[(e)].

(e) Snow and ice are perhaps the worſt conductors of heat of any ſubſtance yet known. In the firſt place, they never allow their own heat to riſe above the freezing point, ſo that no heat can paſs through ice or ſnow when at 32°, by which means they become an abſolute barrier to all heat that is at or above that degree; ſo that the heat of the earth, or whatever ſubſtance they cover, is retained: but they are conductors of heat below 32°. Perhaps that power decreaſes in proportion as the heat decreaſes under that part.

In the winter 1776 a froſt came on, the ſurface of the ground was frozen; but a conſiderable fall of ſnow alſo came on, and continued ſeveral weeks; the atmoſphere at this time was often at 15°, but it was not allowed to affect the ſurface of the earth conſiderably, ſo that the ſurface of the ground thawed, and the earth retained the heat of 34°, in which beans and peas grow.

The ſame thing took place in water, in a pond where the water was frozen on the ſurface to a conſiderable thickneſs; a large quantity of ſnow fell and covered the ice; the heat of the water was preſerved and thawed the ice, and the ſnow at its under ſurface was found mixed with the water.

The heat of the water under the ſnow was at 35°, in which the fiſh lived very well.

It would be worthy of the attention of the philoſopher, to inveſtigate the cauſe of the heat of the earth, upon what principle it is preſerved, &c.

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As all the experiments I ever made upon the freezing of animals, with a view to ſee if it were poſſible to reſtore the actions of life when thawed, were made upon whole ones, and as I never ſaw life return by thawing[(f)]; I wiſhed to ſee how far parts were ſimilar to the whole in this reſpect; eſpecially as we have it aſſerted, and with ſome authority, that parts of a man may be frozen, and afterwards recover: for this purpoſe I made the following experiments upon an animal of the ſame order as ourſelves.

In January 1777, I mixed ſalt and ice till the cold was about 0; on the ſide of the veſſel was a hole, through which I introduced the ear of a rabbit. To carry off the heat as faſt as poſſible, it was held between two flat pieces of iron that went farther into the mixture. That part of the ear projecting into the veſſel became ſtiff, and when cut did not bleed; and the part cut off by a pair of ſciſſars, flew from between the blades like a hard chip.

The ear remained in the mixture nearly an hour: when taken out it ſoon thawed, and began to bleed; it became very flaccid, ſo as to double upon itſelf, having loſt its natural elaſticity. When out of the mixture nearly an hour, it became warm, and this warmth in-

(f) Vide Phil. Tranſ. for the year 1775, vol. LXV. part. II. p. 446.

creaſed

creaſed to a conſiderable degree; while the other ear continued in its uſual cold, and alſo began to thicken. The day following the frozen ear was ſtill warm; and two days after it ſtill retained its heat and thickneſs, which continued for many days after.

About a week after this, the mixture being the ſame as the former, I introduced both ears of the ſame rabbit through the hole, and froze them both: the ſound one, however, froze firſt, probably from its being conſiderably colder at the beginning. When withdrawn, they ſoon thawed, and ſoon both became warm, and the freſh ear thickened as the other had done before.

Feb. 23, 1777, I repeated theſe experiments. I froze the ear of a white rabbit till it became as hard as a board. It was longer in thawing than in the former experiment, and much longer before it became warm; however, in about two hours it became a little warm, and the day following it was very warm and thickened.

In the ſpring 1776, I obſerved that the cocks I had in the country had their combs ſmooth with an even edge, and not ſo broad as formerly, appearing as if near one half of them had been cut off. Having inquired into the cauſe of this, my ſervant told me, that it had been common in that winter during the hard froſt. He obſerved, that they had become in part dead, and at laſt dropped off:

alſo, that the comb of another cock had dropped intirely off, which I did not ſee, as by accident he burnt himſelf to death. I naturally imputed this effect to thoſe combs having been frozen in the time of the ſevere froſt; and having, conſequently, loſt the life of that part by this operation. I endeavoured to try the ſolidity of this reaſoning by experiment.

I attempted to freeze the comb of a very large young cock (which was of a conſiderable breadth) but could only freeze the ſerrated edges (which proceſſes were full half an inch long); the comb itſelf being very thick and warm reſiſted the cold. The frozen parts became white and hard; and, when I cut off a little bit, it did not bleed, nor did the animal ſhew any ſigns of pain. I next introduced into the cold mixture one of his wattles, which was very broad and thin; it froze very readily: upon thawing both the comb and wattle, they became warm, but were of a purple colour, having loſt that tranſparency which the other parts of the comb and the other wattle had. The wound in the comb now bled freely.

Both comb and wattle recovered perfectly in about a month. The natural colour returned firſt neareſt to the ſound parts, increaſing gradually till the whole was become perfectly ſound.

There was a very material difference in the effect between thofe fowls, the ferrated edges of whofe combs I fufpected to have been frozen in the winter of $176\frac{5}{6}$, for they muft have dropped off. The only way in which I can account for this difference is, that in thofe fowls the parts were kept fo long frozen, that the unfrozen or active parts had time to inflame, and had brought about a feparation of the frozen parts, treating them exactly as dead, fimilar to a mortified part; and that before they thawed, the feparation was fo far compleated as to deprive them of farther fupport.

As it is confidently afferted, that fifh are often frozen and come to life again, and as I had never fucceeded in any of my experiments of this kind upon whole fifh; I made fome partial experiments upon this clafs of animals, being led to it by having found a material difference in my experiments upon whole individuals and only parts of the more perfect order of animals.

I froze the tail of a tench (as high as the anus) which became as hard as a board; when it thawed, that part was whiter than common; and when it moved, the whole tail moved as one piece, and the termination of the frozen part appeared like the joint on which it moved.

On the fame day I froze the tails of two gold fifh till they became as folid as a piece of wood. They were put

into

into cold water to thaw: they appeared at firft, for fome days, to be very well; but that part of the tail which had been frozen had not the natural colour, and the fin of the tail became ragged. About three weeks after a furr came all over the frozen part; the tail became lighter, fo that the fifh was fufpended in the water perpendicularly, and they had almoft loft the power of motion; at laft they died. The water in which they were kept was New River water, fhifted every day, and about ten gallons in quantity.

I made fimilar experiments upon an order of animals ftill inferior, *viz.* common earth worms.

I firft froze the whole of an earth worm as a ftandard; when thawed it was perfectly dead.

I then froze the anterior half of another earth worm; but the whole died.

I next froze the pofterior half of an earth worm; the anterior half lived, and feparated itfelf from the dead part.

As I had formerly in making my experiments upon animals, relative to heat and cold, made fimilar ones on vegetables, and had generally found a great fimilarity between them in thefe refpects, I was led to purfue the fubject upon the fame plan; but I was ftill farther induced to continue my experiments upon vegetables, as

I ima-

I imagined I ſaw a material difference between them in their power of ſupporting cold.

From obſervations and the foregoing experiments it plainly appears, that the living principle will not allow the heat of ſuch animals to ſink much lower than the freezing point, although the ſurrounding atmoſphere be much colder, and that in ſuch a ſtate they cannot ſupport life long; but it may be obſerved, that moſt vegetables of every country can ſuſtain the cold of their climate. In very cold regions, as in the more Northern parts of America, where the thermometer is often 50° below 0, where peoples feet are known to freeze and their noſes to drop off if great care be not taken, yet the ſpruce-fir, birch, juniper, &c. are not affected.

Yet that vegetables can be affected by cold, daily experience evinces; for the vegetables of every country are affected if the ſeaſon be more than ordinarily cold for that country, and ſome more than others; for in the cold climates abovementioned, the life of the vegetable is often obliged to give way to the cold of the country: a tree ſhall die by the cold, then freeze and ſplit into a great number of pieces, and in ſo doing produce conſiderable noiſe, giving loud cracks which are often heard at a great diſtance.

In this country the ſame thing ſometimes happens to exotics from warmer climates: a remarkable inſtance of this

this kind happened this winter in his Majeſty's garden at Kew. The *Erica arborea* or Tree-heath, a native of Spain and Portugal, which had kept its health extremely well againſt a garden-wall for four or five years, though covered with a mat, was killed by the cold, and then being frozen ſplit into innumerable pieces(g). But the queſtion is, is every tree dead that is frozen? I can only ſay, that in all the experiments I ever made upon trees and ſhrubs, whether in the growing or active ſtate, or in the paſſive, that whole or part which was frozen, was dead when thawed.

The winter $177\frac{5}{6}$ afforded a very favourable opportunity for making experiments relative to cold, which I carefully availed myſelf of. However, previous to that winter, I had made many experiments upon vegetables reſpecting their temperature comparatively with that of the atmoſphere, and when they were in their different ſtates of activity: I therefore examined them in different ſeaſons, with a

(g) This muſt be owing to the ſap in the tree freezing, and occupying a larger ſpace when frozen than in a fluid ſtate, ſimilar to water; and that there is a ſufficient quantity of ſap in a tree newly killed is proved by the vaſt quantity which flows out upon wounding a tree. But what appeared moſt remarkable to me was, that in a walnut-tree, on which I made many of my experiments, I obſerved that more ſap iſſued out in the winter than in the ſummer. In the ſummer, a hole being bored, ſcarcely any came out; but in the winter it flowed out abundantly.

view

view to ſee what power vegetables have. I ſhall relate theſe experiments in the order in which they were made.

They were begun in the ſpring, the actions of life upon which growth depends being then upon the increaſe; and they were continued till thoſe actions were upon the decline, and alſo when all actions were at an end, but whilſt the paſſive powers of life were ſtill retained.

The firſt were made on a walnut tree, nine feet high in the ſtem, and ſeven feet in circumference in the middle.

A hole was bored into it on the North ſide, five feet above the ſurface of the ground, eleven inches deep towards the centre of the tree, but obliquely upwards, to allow any ſap, which might ooze through the wounded ſurface, to run out.

I then fitted to this part a box about eight inches wide and five deep, and faſtened it to the tree: the bottom of the box opened like a door with a hinge. I ſtuffed the box with wool, excepting the middle, oppoſite to the hole in the tree: for this part I had a plug of wool to ſtuff in, which, when the door was ſhut, incloſed the whole. The intention of this was to keep off as much as poſſible all immediate external influence either of heat or cold.

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The ſame thermometer with which I made my former experiments, ſeven inches and a half long, was ſunk into a long feather of a peacock's tail, with a ſlit upon one ſide to ſhow the degrees; by this means the ball of the thermometer could be introduced into the bottom of the hole.

EXP. I. March 29th, I began my experiments at ſix in the morning, the atmoſphere at 57°½, the thermometer in the tree at 55°; when it was withdrawn the quickſilver ſunk to 53°, but ſoon roſe to 57°½ (b).

This experiment was repeated three times with the ſame ſucceſs. Here the tree was cooler than the atmoſphere; when one ſhould rather have expected to have found it warmer, ſince it could not be ſuppoſed to have as yet loſt its former day's heat.

EXP. II. April 4th, half paſt five in the evening, the tree at 56°, the atmoſphere at 62°; the tree therefore ſtill cooler than the atmoſphere.

EXP. III. April 5th, wind in the North, a coldiſh day, ſix o'clock in the evening, the thermometer in the tree was at 55°, the atmoſphere at 47°; the tree warmer than the atmoſphere.

(b) The ſinking of the quickſilver upon being withdrawn I imputed to the evaporating of the moiſture of the fluid upon the ball.

EXP.

EXP. IV. April 7th, a cold day, wind in the North, cloudy, at three o'clock in the afternoon, the thermometer in the tree was at 42°, the atmoſphere at 42° alſo.

EXP. V. April 9th, a cold day, with ſnow, hail, and wind, in the North-eaſt; at ſix in the evening the thermometer in the tree at 45°, the atmoſphere at 39°.

Here the tree was warmer than the atmoſphere, juſt as might have been expected. If theſe experiments prove any thing, it is that there is no ſtandard; and probably theſe variations aroſe from ſome circumſtance which had no immediate connection with the internal powers of the tree; but it may alſo be ſuppoſed to have ariſen from a power in the tree to produce or diminiſh heat, as ſome of them were in oppoſition to the atmoſphere.

After having endeavoured to find out the comparative heat between vegetables and the atmoſphere, when the vegetables were in action; I next made my experiments upon them when they were in the paſſive life.

As the difference was very little when in their moſt active ſtate, I could expect but very little when the powers of the plant were at reſt.

From experiment upon the more imperfect claſſes of animals it plainly appears, that although they do not reſiſt the effects of extreme cold till they are brought to the freezing point, they then appear to have the power of refifting it, and of not allowing their cold to be brought much lower.

To fee how far vegetables are fimilar to thofe animals in this refpect, I made feveral experiments: I however fufpected them not to be fimilar, becaufe fuch animals will die in a cold in which vegetables do live; I therefore fuppofed that there is fome other principle.

I did not confine thefe experiments to the walnut tree, but made fimilar ones on feveral trees of different kinds, as pines, yews, poplars, &c. to fee what was the difference in different kinds of trees. The difference proved not to be great, not above a degree or two: however, this difference, although fmall, fhews a principle in life, all other things being equal; for as the fame experiments were made on a dead tree, which ftood with its roots in the ground, fimilar to the living ones, they became more conclufive.

In October I began the experiments upon the walnut tree, when its powers of action were upon the decline, and when it was going into its paffive life.

EXP. VI. October 18th, at half paft fix in the morning, the atmofphere at $51^\circ\frac{1}{2}$, the thermometer in the tree was at $55^\circ\frac{1}{2}$; but, on withdrawing and expofing i for a few minutes in the common atmofphere, it fell to $50^\circ\frac{1}{2}$.

EXP.

EXP. VII. October 21ſt, ſeven o'clock in the morning, the atmoſphere at 41°, the tree at 47°.

EXP. VIII. October 21ſt, in the evening at five o'clock, the atmoſphere at 51°½, the tree at 57°.

EXP. IX. October 22d, at ſeven in the morning, the atmoſphere at 42°, the tree at 48°.

EXP. X. October 22d, one o'clock after noon, the atmoſphere at 51°, the tree at 53°.

EXP. XI. October 23d, in the evening of a wet day, the atmoſphere at 46°, the tree at 48°.

EXP. XII. October 28, a dry day, the atmoſphere at 45°, the tree at 46°.

EXP. XIII. October 29th, a fine day, the atmoſphere at 45°, the tree at 49°.

EXP. XIV. November 2d, wind Eaſt, the atmoſphere at 43°, the tree at 43°.

EXP. XV. November 5th, wet day, the atmoſphere at 43°, the tree at 45°.

EXP. XVI. Nov. 10th, atmoſphere at 49°, the tree at 55°.

EXP. XVII. November 18th, atmoſphere at 42°, the tree at 44°.

EXP. XVIII. November 20th, fine day, the atmoſphere at 40°, the tree at 42°.

EXP. XIX. December 2d, the atmofphere at 54°, the tree at 54°.

In all thefe experiments, which were made at very different times in the day, *viz.* in the morning, at noon, and in the evening, the tree was in fome degree warmer than the atmofphere, excepting in one, when their temperatures were equal. For the fake of brevity I have drawn up my other experiments (which were made on different trees) into four tables, as they were made at four different degrees of heat of the atmofphere, including thofe made in the time of the very hard froft in the winter of 177$\frac{5}{6}$. They were as follows.

1ft.

Atmofphere.	Names.	Height. Ft. In.	Diameter. Ft. In.	Heat. °
29 deg.	Carol. poplar,	2	2	29$\frac{1}{2}$
	Engl. poplar,	4	2$\frac{1}{4}$	29$\frac{1}{2}$
	Orien. plane,	3	1$\frac{1}{4}$	30
	Occid. plane,	3.6	2	30
	Carol. plane,	1	1$\frac{3}{4}$	30
	Birch,	3.6	2$\frac{1}{2}$	29$\frac{1}{2}$
	Scotch fir,	3.6	4	28$\frac{1}{2}$
	Cedar libanon,	2.2	4$\frac{1}{2}$	28$\frac{1}{2}$
	Arbutus,	2.6	3$\frac{1}{2}$	30
	Arbor vitæ,	2.8	3$\frac{1}{2}$	29
	Diffid. cyprus,	3	2$\frac{1}{2}$	30
	Lacker varnifh,	3.6	2	30
	Walnut tree,	5	2.4	31

The

The old hole in the walnut tree being full of ſap was frozen up, but a freſh one was made.

2d.

Atmoſphere.	Names.	Height. Ft. In.	Diameter. In.	Heat. °
27 deg.	Spruce fir,	4	2½	32
	Scotch fir,	1.5½	1½	28
	Silver fir,	3.11	2½	30
	Weymouth fir,	4.6	2½	30
	Yew,	3.7	3	30
	Holly,	2.6	2	30
	Plumb tree,	4.6	3	31½
	Dead cedar,	3.11	3	29
	Ground under ſnow,	3 deep	—	34

3d.

Atmoſphere.	Names.	Heat.
24 deg.	Spruce fir,	23°
	Scotch fir,	23
	Silver fir,	23
	Weymouth fir,	23
	Yew,	22
	Holly,	23
	Dead cedar,	24

The ſame trees we mentioned when the thermometer was at 29°, in new holes made at the ſame height, and left ſome time pegged up till the heat produced by the gimlet was gone off; but in which, as they were moiſt from

from the ſap, the heat could be very little, eſpecially as the gimlet was not in the leaſt heated by the operation.

4th.

16 deg.	Car. poplar,	17°
	Eng. poplar,	17
	Ori. plane,	17
	Occ. plane,	17
	Carol. plane,	17
	Birch,	17
	Scotch fir,	$16\frac{1}{2}$

It will be neceſſary to obſerve, that the ſap of the walnut tree, which flowed out in great quantity, froze at 32°. I did not try to freeze the ſap of the others.

Now, ſince the ſap of a tree, when taken out, freezes at 32°; alſo, ſince the ſap of the tree, when taken out of its proper canals, freezes when the heat of the tree is at 31°; and ſince the heat of the tree can be ſo low as 17° without freezing; by what power are the juices of the tree, when in their proper canals, kept fluid in ſuch a cold? Is it the principle of vegetation? Or is the ſap incloſed in ſuch a way as that the proceſs of freezing cannot take place, which we find to be the caſe when water is confined in globular veſſels? If ſo, its confinement muſt be very different from the confinement of the moiſture in dead vegetables; but the circumſtance of

vege-

vegetables dying with the cold, and then freezing, appears to anſwer the laſt queſtion. Theſe, however, are queſtions which at preſent I ſhall not endeavour to ſolve.

I have made ſeveral experiments upon the ſeeds of vegetables ſimilar to thoſe on the eggs of animals; but, as inſerting them would draw out this paper to too great a length, I will reſerve them for another.