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Feb. 21

PHYSIOLOGICAL RESEARCHES.

PHYSIOLOGICAL RESEARCHES.

BY

JOHN DAVY, M.D., F.R.S.,

LONDON AND EDINBURGH, ETC.; INSPECTOR GENERAL OF ARMY HOSPITALS, H.P.

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ADVERTISEMENT.

This work, like the two volumes bearing nearly the same title which preceded it, in 1838, is composed for most part of papers which, since that time, have been published in scientific journals, and in the Transactions and Proceedings of the Royal and other Societies.

The author has availed himself of this opportunity to correct some errors in the originals, chiefly typographical, owing to his having been abroad at the time of their printing, and to make some additions in the form of notes.

In the arrangement of the materials, those papers on the same or nearly allied subjects are placed as much as possible in sequence, irrespective of their dates.

In the table of contents the time of publication of each paper is given, and the volume of the Transactions or Journal in which it is to be found. Those not so marked, it is right to mention, have never before been published.

J. D.

Lesketh How, near Ambleside,
May 1, 1863.

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PHYSIOLOGICAL RESEARCHES.

I. MISCELLANEOUS OBSERVATIONS ON ANIMAL HEAT.

1. *On the Temperature of the Pelamides* (*Auxis vulgaris*).

Fishes generally are commonly considered as cold-blooded. In a work published in 1839, I have stated particulars tending to show, that this commonly received opinion is not universally correct, and that fishes of the genus Thynnus, with some others of the Scomber family, may be inferred to be an exception.*

As this inference was founded chiefly on the reports of fishermen, it appeared very desirable to determine by actual thermometrical measurement what is the exact temperature of fishes of this family.

Hitherto, although watching for opportunities, and promised the aid of friends favourably situated, I have not been able to make any observations of the kind required, excepting on one species of these fishes, the Pelamides. Like most of its congeners, this fish is migratory in its habits. In the early part of summer it appears in the Sea of Marmora and the Bosphorus, and in August in the Black Sea, from whence, after spawning, it returns in September and October, on its passage to the Mediterranean. It is caught in the same manner as the Tunny.

In June, 1841, whilst at Constantinople, I visited a fishing-station for this fish, in an inlet of the Sea of Marmora, and was present when a small capture was made, enabling me to ascertain the temperature of four specimens. This was done the instant they were taken out of the water, being in a boat alongside the net, by introducing a thermometer with a projecting bulb through

* Researches, Physiological and Anatomical, vol. i., p. 218.

a small incision, into the muscle of the back, about an inch and a half, and immediately after into the cavity of the abdomen. In three instances, the thermometer in the back rose to 75° Fahr.; in one to 74°; in all, in the abdomen it rose to 73°. The Pelamides were of moderate size, between two and three feet long. The air at the time was 71°; the sea at the surface 68°; but probably at the depth from which the fishes were taken it was a few degrees lower, the descending current of the Bosphorus then being, where coldest, at 62°.

Supposing that the water from which they were taken was 62°—and it might have been lower, as the Pelamides swim in deep water—the temperature of this fish would appear to be about 12° above the medium in which it swims, and at least 7° above that of the surface.

This result seems in accordance with the inference that all fishes are not cold-blooded. In the work already referred to, reasoning from the smaller size of the respiratory nerves of the Pelamides compared with those of the Tunny, I offered the conjecture that its temperature would be found less than that of the Tunny, and somewhat higher than that of fishes of other orders with still smaller respiratory nerves, a conjecture which the observations described may be adduced as confirming.

In connexion with their temperature, my attention was directed to the blood of these fishes. I have been able to examine it only in three instances, and that partially, viz. the Sword-fish, the Pelamides, and the common Tunny. Considering the great difficulty there is in obtaining the subjects for experiment under favourable circumstances for examination, imperfect as were my results, I am induced to offer them now.

The Sword-fish appears to abound less in blood than the Pelamides, and the Pelamides less than the common Tunny; and accordingly the muscles of the former two are of a much lighter colour than those of the latter.

The blood of the Tunny is very rich in red particles: this is indicated not only by its appearance, but also by its specific gravity, which I have found as high as 1,070. The blood tried was taken from a fish, caught in the Sea of Marmora, that weighed between two and three hundred pounds.

The blood of the Pelamides appears to be less rich in red particles than that of the Tunny, but more than that of the Sword-fish: I have not ascertained its specific gravity. The specific gravity of the blood of the Sword-fish I have found to be 1,051; the fish from which the blood was taken was caught in the Bosphorus, in the month of December, and was of large size.

Under the microscope the appearance of the red particles of the blood of these three fishes is very similar. They are commonly thin oval discs (very soft), containing oval nuclei: a few circular discs are intermixed with them. The medium dimensions of those of the Pelamides were about $\frac{1}{2000}$th of an inch by $\frac{1}{3000}$th; of the Sword-fish, about $\frac{1}{3000}$th by $\frac{1}{4000}$th; and of the Tunny, about $\frac{1}{1600}$th by $\frac{1}{2000}$th.

That the red particles constitute that portion of the blood which is chiefly concerned in the production of animal heat, is now generally admitted. What a contrast appears, in comparing the blood of the fishes under consideration with that of some of the colder, especially of the cartilaginous kind, in which it is very small in quantity, accompanied by a proportionally diminutive heart, and poor in red particles! The blood of the *Squalus Acanthias* I have found to exceed in density only a little its serum, one being of the specific gravity 1,030, the other of the specific gravity 1,027.

Whether the peculiar constitution of the red particles operates in any way in promoting their union with oxygen, seems to be deserving of consideration. It may be thrown out as a conjecture, that the circumstance of their possessing nuclei may have an effect of the kind, supposing, which is possible, the blood-corpuscle and nucleus, or containing and contained part, to be in the electrical relation to each other of positive and negative. If it be objected to this, that as regards nuclei as well as size, there is an analogy between the blood-corpuscle of fishes, birds, and reptiles, the temperature of which commonly is so very different, it may be answered, that in all these classes such a constitution of blood-corpuscle may be designed for the same end, and that birds partly owe their high temperature to it; and that in reptiles and fishes, in most of which the proportion of red particles is small, were the constitution of blood-corpuscle different it would be inadequate to perform the part required of it.*

* Is not the electrical condition of the egg of the common fowl—the white and yolk in opposite electrical states—in favor of this conjecture?—See further on the paper on this subject.

2. *On the Temperature of Man in advanced age.*

Not aware of any observations having been published on the temperature of man in advanced old age, I have been induced to institute some trials, the results of which I shall now briefly describe.

1. 91 years of age; feeble on his legs, but in pretty good health; a native of Grasmere in Westmoreland, where he has always resided, in easy circumstances, cultivating his own land. In June, when the temperature of the air was 60°, a thermometer placed under the tongue rose to 99·5°; his hands were warm; his pulse at the wrist 48, strong, intermitting. The observation was made at 2 p.m.; he had dined at noon. On the 28th of the October following, his temperature was again tried, about the same time of day, when the open air was 42°, the air of his room 52°: now, under the tongue, the thermometer was 98·5°; the pulse 56; his state of health much the same as before.

2. 88 years of age, also a native of Grasmere, where he has mostly resided, as a day labourer; is pretty firm on his feet, but troubled with chronic cough and difficulty of breathing. In June, when the temperature of the air was 60°, a thermometer placed under the tongue rose to 99·5°; his pulse was 56, and rather feeble; he had dined three hours previously. On the 28th of October, an hour after dinner, when his pulse was 70, the thermometer under the tongue was 98°; the air of the room 55°. In February, about three hours after dinner, when his pulse was 44 and feeble, the temperature under the tongue was 96°. This was on the 27th: the air then of his room was 44°; the open air about 32°, after a heavy fall of snow, and a sharp frost of several days' duration. The old man was feebler than in the summer and autumn; and though he did not complain of cold, his hand felt cold.

3. The wife of the preceding, the mother of several children, 76 years of age; hale for her years, but blind from cataract complicated with amaurosis. Her temperature, tried at the same time as her husband's, in June, was found under the tongue to be 98·5°, her pulse 78, and pretty strong. Tried again in October, it was found to be 98°, with a pulse of 70; and again in February, on the 27th, it was found to be 99°; her pulse being 80.

4. 87 years of age, a native of Ambleside, where she has commonly resided; feeble, but, excepting chronic cough, in tolerable health. On the 26th of October, at 3 p.m., the temperature under the tongue was found to be 98·5°; her pulse 84, and pretty strong; the air of the room then was 57°; the open air about 42°.

5. On the same day, and in the same village, tried the temperature of another old inhabitant, 92 years of age. The thermometer under her tongue stood about 98°; it could not be determined with perfect exactness, on account of the tremulous motion of her head, which also affected the limbs, preventing the counting of her pulse; her general health was pretty good.

6. An inhabitant of Ambleside, by trade a hatter, 89 years of age, hale, able to walk to church. On the 27th of October, when the air of his room was 56°, the outer air 42°, his pulse 64, strong and regular; the thermometer under his tongue stood at 98°. Observed again on the 27th of February, at 1 p.m., just after dinner, when the outer air was 32°, the air of his room 54°, the temperature under his tongue was found to be 99·5°; his pulse 70.

7. The temperature of his wife, two years younger, taken on the 27th of October, was 98·5°; her pulse was 88, irregular; she was very infirm, and suffering from asthma.

8. A native of Scotland, 95 years of age, now residing in Ambleside, where he has been many years, always in good health, still tolerably strong and active. On the 28th of October found his temperature under the tongue 98·5°; his pulse 56, intermitting; the air of his room 57°. The old people in all the preceding instances, at the time the observations were made, were sitting by their fireside, as is their usage in the cool climate of Westmoreland the greater part of the year, and all of them, with one exception, seemed to be comfortably warm; the poorest of them were not in want.

9. A man, a blacksmith, a native and resident of Ambleside, 91 years of age, in good health and remarkable for activity, having during the summer served as a guide to Helvellyn. On the 3rd October, the temperature under his tongue was 98·8°; his pulse 74, of moderate strength; his respirations 24 in the sitting posture; he had breakfasted about an hour before.

Old age is commonly represented as cold, and the temperature of the body is commonly supposed to diminish with advancing age. The results of the preceding observations generally are not in accordance with this opinion; they seem on the contrary to show, that the temperature of old people, at least as regards the deep-seated parts, of which the tongue at its base may be considered as some indication, is rather above than below the average temperature of middle age, taking that to be about 98° of Fahr. Nor, perhaps, is this surprising, when we reflect, that most of the food consumed by old persons—and their appetite generally is good—is probably chiefly employed in administering to the function of respiration, being very partially expended in meeting the waste of the body.

Probably in very advanced old age, as in very early infancy, the power of resistance to cold is feeble, and the temperature of the body is easily reduced on exposure. An observation which I made many years ago in Ceylon would seem to be confirmatory of this. At 7 o'clock in the morning, when the air was 72°, I tried the temperature of an old man, almost a century old, and of a boy about twelve years old, both cool, being thinly clad and out of doors; the temperature of the old man under the tongue was 95°; in the axilla 93°; that of the boy under the former 98°, and in the latter 96·5°. The observation too on the old man in Grasmere, made in February in cold weather, is also favourable to this conclusion: whilst those made at the same time, on the other two old persons in stronger health, seem to show that, provided there is a vigorous action of the heart and free circulation of the blood, the temperature of the body is easily maintained.*

* The following observation, made at Oxford in the last week of May, 1861, on a man (John Pratt) of the very advanced age of 106 years, may be considered confirmatory of the above; and also in favor of the inference, which I am disposed to make, that if the function of the lungs be materially impaired by chronic disease, the temperature will be below rather than above par. The thermometer under his tongue was 96° (not a good observation, owing to his impatience); in the axilla, 97·5°. His pulse at the time was 80, and slightly intermitting; his respirations 36. He was a herbalist by trade, had lived an active life and very much in the open air, and had been temperate in all things. He had been twice married: the last time about twenty years ago. He had had, he said, uninterrupted good health up to his 101st year, and without any decline of vigor—not excluding the sexual. Since, he had been ailing; and at the present time he had to complain of "four evils: cough, cramp, convulsive movements of the limbs, and scalding and difficulty in making water." His veins were large, especially in the extremities; his hands and feet warm, and he said he seldom felt cold. He was cheerful and intelligent and tolerably active, and that morning had walked at least half a mile.

He died a year after. His autopsy, conducted by Professor Rolleston, who has given an interesting account of it in the number of the British and Foreign Medico-

3. *On the Effect of Air of different Temperatures on Animal Heat.*

As from observations made on man on entering the tropics, and within the tropics on descending from a cool mountainous district to a hot low country, it would appear that his temperature, as measured by a thermometer placed under the tongue, is liable to fluctuate—rising one or two degrees in a warm atmosphere, and falling as much on entering a cool one,*—it seemed probable that like differences of effect might be produced by air kept at different degrees of temperature in buildings in this country.

In the autumn of last year (1842), when going through the cotton manufactory of Deanstone, in the neighbourhood of Doune in Stirlingshire—an establishment admirably conducted and in the highest order—I availed myself of the opportunity to try the temperature of a few individuals in relation to this question. In the room called the "piecing-room," where a high temperature is always required on account of the kind of work—a temperature kept up by warm air and steam—when at 92°, I found the thermometer placed under the tongue of one man who had been at work there about six hours, rise to 100·5°; and of another, who had been there the same time, to 100°: the former was 52 years of age, healthy, his pulse 64; the other 33 years of age, in pretty good health, but liable to acidity of stomach; his pulse 78.

In an adjoining room, where the temperature of the air was 73°, the thermometer placed under the tongue of a young woman rose to 99°; and in a large room, where 300 persons were employed in weaving, and where the temperature of the air was 60°, the thermometer placed under the tongue of another healthy young woman rose only to 97·5°.

Few as are these observations, they seem to warrant the conclusion that a high temperature of even a few hours in the heated air of a room is capable of raising the temperature of the body above its usual standard, in accordance with what had been anti-

Chirurgical Review for January, 1862, displayed partial induration of lungs, the result of chronic pneumonia, and a very thickened state of the urinary bladder from protracted chronic inflammation. The heart was sound; its valves healthy and unusually large; the aorta but little enlarged; brain little apparently changed: its exact weight was 43oz. avoirdupois.

* Op. cit., vol. i., p. 169.

cipated from the effect of different degrees of atmospheric temperature.

In further confirmation of the same, I may briefly state the results of multiplied observations made on the temperature of the same individual. The subject of them was of middle age, in good health, under whose tongue the thermometer commonly was stationary at 98°, when neither suffering from heat nor cold. The place where they were made was Constantinople — the climate of which capital, it may be observed, is exceedingly variable, often cold in winter and the early spring, and commonly very hot in summer, liable to great vicissitudes from its situation on the confines of two seas, in regard to warmth, very different in character during a great part of the year. The observations were begun early in March, and were continued at intervals till the latter end of July. During this time the thermometer in the open air ranged from 31° to 94°, and the temperature observed under the tongue from 97° to 99·5°. It may not be amiss to mention some particular instances.

On the 5th of March, after having been exposed several hours in an open boat on the Bosphorus, with a strong wind at 43°, the thermometer placed under the tongue stood at 97°.

On the 11th of the same month, when the ground was covered deeply with snow, and the thermometer in the open air at 7 a.m. was 31°, and in a bed-room 45°, the temperature under the tongue was found to be 97·5°.

On the 3rd of April, when the thermometer in the room, with the window open, was 66°, under the tongue it was 98·5°.

On the 17th of July, when the thermometer was 87°, under the tongue it rose to 99·5°. On the 21st of the same month, when the air was 87°, the temperature under the tongue was 99·5°; and on the 28th, when the former was 94°, the latter was 99°.

During the hot weather of July, it may be deserving of remark, that the pulse was less affected than the respiration, which, habitually about sixteen in the minute, was now commonly fourteen, and one day did not exceed twelve.

It may also be mentioned that attention was paid to the temperature of the extremities, and also to that of the urine, and that commonly it was found of highest temperature when the tongue and extremities were of lowest temperature: thus, on the

5th of March, when the thermometer under the tongue was 97°, the feet and hands cold, in the urine it rose to 101°; and on the 28th of July, when under the former it was 99·5°, in the latter it was the same.

Do not these observations, besides tending to confirm the preceding conclusion for which they were brought forward, viz., that the temperature of the body rises and falls in a perceptible manner with the temperature of the air, lead also to the further conclusion, that the tendency of a high temperature of atmosphere is to raise the temperature of the surface and of the parts adjoining the surface in a somewhat higher ratio than the deep-seated organs; and of a low temperature of atmosphere to raise the temperature of the deep-seated parts, whilst that of the surface is subjected to undue reduction from the cooling agencies to which it is exposed, directed, as it were, in both instances, for a beneficial result, on the principle of compensation?

4. *On the Effect of Exercise on the Temperature of the Body.*

This subject of inquiry, notwithstanding its manifest importance, has been much neglected; indeed, I do not know of any work in which any precise information is to be obtained respecting it.

The observations which I have to offer are fewer than I could wish and more limited; they were made at Constantinople in 1841, at intervals between February and August, and had for their object mainly to endeavour to determine the effect of moderate exercise in walking on the temperature of the body. The individual on whom they were made was the same as was mentioned in the last section. The particular observations were the following:—

February 19th, at 1½ p.m., air of room 60°; before walking, feet cold, temperature between the toes 66°; under the tongue 98°; urine 100°. At 5 p.m., open air 40°; just returned from a walk, gently warmed by the exercise; feet and hands warm; the former 96·5°, the latter 97°; under the tongue 98°; urine 101°.

March 2nd, at 4½ p.m., open air 50; air of room 66°; feet and hands moderately warm; the former 75°, the latter 81°; under the tongue 98°; urine 100°. At 5½ p.m., after having walked pretty quickly an hour, a gentle perspiration produced, the hands and feet hot, found the latter 99°, the former 98°; under the tongue 98°; the urine 101·5°.

March 20th, at 5½ p.m., open air 42°; returned warm after a walk of three hours: the hands, which had worn warm gloves, were 99°; feet 97°; under the tongue 98°; the urine 101·5°.

April 7th, after a walk of three hours in the open air, between 60° and 70°, returned at 5 p.m., gently perspiring: the hands were 94°; the feet 96·5°; under the tongue 98 5°; the urine 100·5°.

May 27th, at 6½ p.m. after a walk of an hour and a half, the air 68°, returned slightly perspiring: the hands were 95°; the feet hot; under the tongue 99·5°; the urine 101·5°.

May 28th, air 65°; under the tongue before taking exercise 98·5°: after a walk of four hours and a half, gently perspiring, under the tongue 98°; hands 93°; feet 97·5°; urine 100·5°.

September 13th, at 4 p.m., the open air on the shore of the Bosphorus 76°: ascended in about twenty minutes, without stopping, the steep side of the hill, called the Giant's Mountain; on reaching its summit, when profusely perspiring, the pulse was 102, usually about 52; the hand 98°; under the tongue 98°. The pulse of another individual in company, of about the same age, also profusely perspiring, was 138; thermometer under his tongue 98°; and in the hand the same. After descent, the pulse of the former was 94; thermometer under his tongue and in the hand 98·5°; the pulse of the latter was 112; the thermometer under his tongue 98·5°; both only gently perspiring.*

* I shall give in this note a few additional observations illustrating the effects of exercise in ascending mountains on the heart's action and on the temperature:—

On the 24th of August, 1843, in company with a gentleman ætat about 30, and in vigorous health, ascended Helvellyn, 3,070 feet above the level of the sea. We rode as far as Wyeburn Chapel. After walking up the steep side of the mountain about five minutes, the temperature under the tongue of my friend was found to be 99°; his pulse 130; my pulse was 112; temperature 98° Just after reaching the summit—the latter part of the ascent was less steep—my friend's pulse was 116; his temperature 98°; my pulse was 110; temperature 98°. The thermometer below was 52° in the air, above, 46°.

On the 14th of July, 1851, in company of a youth of 17, accustomed to active exercise, ascended Goatfield, 2,900 feet above the level of the sea, in the Isle of Arran. No part of the way was very steep; the distance walked about two miles and a half, and this without stopping. On reaching the summit, my respirations, somewhat laborious, were 32 in the minute; pulse 120; my young friend's the same. After resting ten minutes, my respirations had fallen to 16, the pulse to 90; ten minutes later, the one was 14, the other 84.

On April the 20th, 1852, after walking up the steepest part of Kirkstone (the pass is about 1,575 feet above the sea-level) from Paterdale, the pulse on reaching its summit was 102 and throbbing; respirations 22. At midnight, after several hours' rest, the pulse had fallen to 54; the respiration to 14.

On the 3rd of August, 1862, after ascending a steep part of Blackcombe, a moun-

What is the inference from these observations? Do they not seem to indicate that whilst moderate exercise promotes the diffusion of temperature and its exaltation in the extremities, it augments very little, if at all, the heat of the deep-seated parts? And considering the blood as the heating medium, warmed itself chiefly by respiration, is not this what might be expected, reasoning on the subject? By active exercise, the pulse and the respiration are both accelerated; more oxygen, it may be presumed, is consumed, more heat is generated; the blood is made to circulate more rapidly, and is sent in larger quantity into the extremities, and where, in consequence, the excess of heat is conveyed and expended, and its accumulation in the central and deep-seated organs prevented, affording another striking example of harmonious adaptation.*

The same thermometer was employed in making all the observations described in this paper; and in every instance, in stating the results, allowance has been made for error in its graduation, carefully determined by comparison with a standard instrument, one belonging to Professor Forbes of Edinburgh, now Principal of the University of St. Andrew's, and for the use of which I have been indebted to his kindness.

tain of Cumberland, in some spots so steep that it was necessary to climb, laying hold of tufts of grass; though the exertion was only for a few minutes, the effect on the heart and the respiratory muscles was great. On stopping, the pulse was 154; the heart throbbing; the respirations 42; the temperature of the palm of the hand closed 96·5°. On descending, about half an hour later, on reaching the foot of the hill, the pulse was 92; the respirations 20; the temperature of the hand the same as before; the air about 60°.

It need hardly be pointed out how dangerous such exercise as that just described must be in the instances of persons with any apoplectic tendency or tendency to cardiac disease; and the risk that must be incurred in making such strains on the organization by those in advanced life or with ailing health.

* The effect of walking exercise in warming the extremities without materially increasing the heat of the deep-seated parts and of the brain, may account in part for its beneficial influence on health, especially in the instance of persons of sedentary and studious habits, those habits conducing, it may be presumed, to an undue heat of the intellectual organ.

II. ON THE TEMPERATURE OF MAN.

It has been too generally taken for granted that the temperature of man in health, as measured by a thermometer placed under the tongue, is a constant one. I have endeavoured to prove from the results of observations, that this is not strictly correct; that when not disturbed by disease it is subject to variation, to rise and fall under certain influences, especially of heat and cold, rest and exercise.*

In the present communication I propose to submit to the Royal Society some further observations on the same subject, made with an instrument better adapted for the inquiry than the medical thermometer commonly used, and which has afforded results of a precise and satisfactory kind.

The thermometer I have employed is a bent one, about twelve inches and a half long, its bulb about an inch long, and, where widest, half an inch thick; its curvature about three and a half inches from the bulb, and its stem, to which the scale is attached, nearly at right angles to the bulb, so that when inserted under the tongue, the observer has no difficulty in distinguishing accurately the degrees himself, whether near-sighted or the contrary; in the latter instance using merely a common magnifying glass. Each degree of the scale is little more than half an inch (·6 inch), and is divided into ten parts; and each of these parts is sufficiently large to admit of subdivision by the eye.

It may be right to premise a few words regarding the manner of observing with this instrument; and to notice some precautions which it is necessary to take to avoid error.

First, as to the placing of the thermometer: it is requisite that the bulb should be introduced under the tongue, and as far back as possible; and that, whilst in the mouth, respiration should be carried on entirely through the nostrils. If the thermometer is placed in the side of the mouth, between the teeth and the cheek, the temperature indicated is from three-tenths to one-tenth of a degree less, according to the degree of coldness of the atmosphere.

* Researches, Physiological and Anatomical, vol. i., p. 162; and Philosophical Transactions for 1844, p. 61 (the preceding paper).

Next, as to time: it is necessary that the thermometer remain in the mouth many minutes, till the observer is sure that the maximum height is attained. If the mouth has been kept closed for a quarter of an hour previously, a shorter time is required than if allowed to be open and the passage of respiration. This is well shown by trials with the thermometer raised a few degrees above the temperature of the mouth before introduction. In the one case, the thermometer slowly falls to the temperature of the mouth, and is stationary; in the other case, after having fallen it again rises, continuing to rise till the maximum temperature of the closed mouth is acquired.

The observations which I have made with this thermometer have been altogether on myself; it would have been difficult indeed to have made them on another, with the requisite degree of accuracy, as they are tedious, demanding so much time and care. They were begun in August last, and have been continued almost daily up to the present time, with the exception of the greater part of the month of October, when they were interrupted until a second thermometer could be procured to supply the place of the first, which was then broken, and which was even more delicate than the second. It was my intention to have extended them to a period of twelve months before collecting the results; but this I am not able to accomplish, having received an order to prepare and hold myself in readiness for foreign service. Abroad I hope to be able to continue them, and as that will be in a tropical climate, I am the more desirous of communicating now the information I have already obtained; the comparison of the two sets may prove interesting.

In conjunction with the temperature under the tongue, I have in most instances noticed the pulse and respiration, considering it a desideratum so to do, and with the hope that the observations on the latter may be useful data, and may in some measure tend to throw light on the former, there being such an intimate connexion between them. The posture in which the pulse and respiration have been counted has always been a sitting one.

Of the many problems which might be proposed regarding the temperature of the body, I shall now only touch on a small number; and I shall be well-pleased if the information I have to give be considered merely as a contribution towards their solution, a beginning of an inquiry to be extended.

1. *Of the Variation of Temperature during the twenty-four hours.*

To endeavour to determine what is the extent of this variation, I have made on several occasions observations every second or third hour, from the time of rising to that of going to rest, confining myself to the house during the whole time, and to rooms of nearly the same temperature the greater part of it, and varying but little my occupation. The following for a single day will give a pretty accurate idea of the result:—

	Temperature under tongue.	Pulse.	Respirations.	Temperature of air of room.	
	Deg.			Deg.	
April 13. 7 a.m.	98·5	54	15	49	Just after rising.
9 a.m.	98·4	74	16	53	Just after breakfast.
11 a.m.	98·4	60	15	53	
2 p.m.	98·7	54	15	55	
4 p.m.	98·9	54	15	55	
5 p.m.	98·7	54	15	55	
6½ p.m.	98·3	62	16	57	Shortly after dinner.
7½ p.m.	97·7	66	15	56	Before drinking tea.
11 p.m.	98·1	52	15	64	
14. 1 a.m.	97·6	54	15	60	

During the whole period, I have almost constantly tried the temperature under the tongue on rising and before going to rest, and in many instances in the middle of the day, between 2 and 4 p.m., when the circumstances were favourable. These observations I shall give in detail in tables appended; here it may be sufficient to notice the mean of each month's observations.

	Temperature under tongue.			Pulse.			Respirations.			Temperature of air of room.		
	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
Aug ..	98·7	98·5	98·0	56·9	52·5	53·1	15·3	15·1	15	61	63	67
Sept...	98·8	98·9	98·0	59·3	55·2	55·8	15·5	15	15·4	66	63	65
Nov...	98·9	98·6	97·9	57·8	57·6	54·5	16·7	17·1	15.8	51	48	64
Dec. ..	98·7	98·2	97·9	58·9	55·2	56·5	15·7	16	15·4	42	47	60
Jan. .	98·8	98·07	97·9	58 7	59·3	57·9	15·5	15·3	15·1	45	55	60
Feb. ..	98.6	98·6	97·9	55·5	54·4	52·3	15·5	15	15·1	42	48	61
Mar...	98·74	98·59	97·93	57	53	54	15·1	15·1	15	46	54	60
April .	98·66	98·57	97·88	56·5	54·8	53·6	15·4	15	14·8	54 6	59·8	62·4
	98·74	98·52	97·92	57·6	55·2	54·7	15·6	15·4	15·2	50·9	54·7	62

During this period, comprising eight months, the health of the observer (aged fifty-five) was pretty good, almost uninterruptedly so, excepting in December and January, when he experienced slight lumbago, not preventing the taking of exercise; and for a few days in November and January an attack of catarrh in a mild form.

As I wish to be as concise as possible, I shall comment very little on the results of the summary of observations. They seem to prove in a decided manner that the temperature under the tongue, when under no disturbing influence, is about its maximum on waking after the repose of the night; that it continues high, but fluctuating more or less (probably owing to disturbing circumstances) till towards nightfall; and that it is lowest about midnight. Its lowness at the last-mentioned time is the more remarkable, as the temperature of the room in which the observer sat at night was almost uniformly higher than of that which he occupied during the day.

2. *Of the Variation of Temperature during different seasons of the year.*

The following Table exhibits the mean results of the observations made during the eight months, at the different periods of the day, both of the temperature under the tongue and of the air of the room.

	Mean temperature under the tongue.	Air of room.		Mean temperature under the tongue.	Air of room.
	Deg.	Deg.		Deg.	Deg.
August ...	98·4	63·7	January ...	98·36	53·3
September	98·57	64·7	February ..	98·37	50·3
November	98·47	54·3	March......	98·42	53·3
December	98·27	49·7	April	98·37	55·5

These results give an average temperature of 98·4°, that of the air being about 55·5°. They show a slight relation between the temperature of the body and of the air, but less perhaps than might be expected, and less unquestionably than would have been exhibited under circumstances not equally favourable for the preservation of an equable warmth, especially at night, in the uniform temperature of the sitting-room; and when at rest, from warm bed-clothes, and during the day from sitting in cold weather near a fire, and from the clothing then, as well as at night, being varied with the degree of cold to be resisted, having in view the preserving of an agreeable feeling—the effect and perhaps best sign of the happy temperate mean. Some facts which I shall have presently to bring forward, may aid in illustrating the remark just made.

3. *Of the Effect of Active Exercise on the Temperature.*

By active exercise, I mean that which occasions acceleration of the heart's action, and of respiration, and commonly a feeling of increased warmth, such as fast walking and riding, in contradistinction to the passive kind, as that which is taken in an easy carriage.

The following detail exhibits the results of observations made immediately after active exercise, in different months and under various circumstances:—

		Tongue.	Pulse.	Respirations.	Air.
		Deg.			Deg.
Aug. 15. 5 p.m.	After fly-fishing by the river side, and riding about seven hours; feet and hands warm	99·4	80	18	63
,, 17. 3 p.m.	After a walk of three miles; gently perspiring	99.0	70	16	56
,, 20. 5 p.m.	After fishing five hours; gently perspiring	99·3	80	20	62
,, 27. 2 p.m.	After a ride (pretty fast) of five miles; feet and hands warm	98·7	58	16	64
,, 29. 5 p.m.	After a ride (pretty fast) of about fourteen miles; sun powerful; perspiring	99·5	84	18	64
,, 31. 12 m.	After a ride of ten miles; perspiring ...	99·1	60	16	65
,, 31. 4 p.m.	After an hour's walk; sun powerful; perspiring	99·3	64	18	70
Sept. 2. 12 m.	After a walk of two hours; the sun powerful; perspiring	99·2	64	18	72
Oct. 30. 5 p.m.	After a ride of ten miles (pretty fast); feet and hands warm	99·3	78	16	49
Nov. 16. 4 p.m.	After fishing two hours; slightly perspiring	98·9	62	18	55
Dec. 31. 3 p.m.	After riding and walking several hours; feet and hands warm	99·1	74	16	34
Feb. 3.	After a walk of seventeen miles; moderately warm	99·1	98	22	32
Mar. 7. 5 p.m.	After a mountain excursion on foot for several hours, and riding ten miles ...	99·2	90	17	33
,, 20. 3 p.m.	After a ride of ten miles; feet and hands warm	98·9	56	16	37
,, 31. 1 p.m.	After two hours' fishing; pleasantly warm	98·9	62	16	54
April 2. 1 p.m.	After riding ten miles; moderately warm	98·9	62	16	54
,, 11. 4 p.m.	After five hours' fishing; not heated ...	99·0	70	16	40
,, 17. 2 p.m.	After four hours' fishing; slightly perspiring	99·2	84	18	55

These observations, selected from a large number of similar bearing, show in a decided manner, that active exercise, not carried to the extent of exhausting fatigue, raises the temperature of the body; and that the increase is, at least within a certain limit, proportional to the degree of muscular exertion made.

4. *Of the Effect of Carriage Exercise on the Temperature.*

The observations which follow were made immediately after getting out of the carriage, which was a close one, and its windows commonly closed; and the dress worn, at the time of being out, was warm.

		Tongue.	Pulse.	Respiration.	Air.
		Deg.			Deg.
Nov. 17. 1 p.m.	After a drive of 8 miles	97·7	52	18	53
„ 19. 3 p.m.	„ „ 10 „	97·7	48	16	48
„ 25. 2 p.m.	„ „ 10 „	97·0	56	16	44
„ 27. 12 m.	„ „ 8 „	97·5	56	18	42
„ 30. 12 m.	„ „ 8 „	97·4	56	16	44
Jan. 5. 5 p.m.	„ „ 7 „	97·7	50	17	32

Feet and hands cool, almost cold, as was experienced in all the preceding instances.

These results are strongly contrasted with those given in the preceding section, showing the exalting effect of active exercise on the temperature. I have other results, equally proving how gentle exercise, in a cold atmosphere, has a depressing effect, whether taken in a carriage, on horseback, or on foot, walking slowly.

5. *Of the Effect of Exposure to Cold Air without Exercise.*

The few observations I have collected on this point, have been made the instant after returning from an adjoining church, the temperature of which in the cold weather of winter is little above the freezing-point, no attempt being made to warm it, and the congregation which assembles in it at that season being small.

	Tongue.	Pulse.	Respirations.	Air.
	Deg.			Deg.
Nov. 24. 1 p.m.	97·0	52	16	42
Jan. 12. 1 p.m.	97·1	50	15	40
Feb. 9. 1 p.m.	96·7	48	15	33
Mar. 16. 1 p.m.	95·9	44	16	32

In each of the above instances, in spite of warm clothing, the sensation experienced by the observer was that of disagreeable chilliness, and in the feet and hands, of coldness; a feeling of drowsiness was also perceived, as if the condition induced were an approach to the state of temperature of a hybernating animal, or to that which is probably the prelude to the sleep in the human being resulting from long exposure to severe cold without exercise.

6. *Of the Effect of Excited and Sustained attention on the Temperature.*

The state of mind referred to is that accompanied with exertion, such as is experienced in composition, or in reading a work of exciting interest.

The observations which follow have been made entirely at night, after from two to five hours of sustained attention. Many more were made by day; but these are not given, as they are not so well fitted for comparison.

	Tongue.	Pulse.	Respirations.	Air.		Tongue.	Pulse.	Respirations.	Air.
	Deg.			Deg.		Deg.			Deg.
Aug. 19. 12 p.m.	98·45	58	15	68	Feb. 12. 12 p.m.	98·2	58	16	60
„ 29. 11 p.m.	98·5	62	16	62	„ 21. 1 a.m.	98·4	54	15	60
Sept. 23. 1 a.m.	98·5	54	16	65	„ 24. 2 a.m.	98·4	58	14	60
Nov. 26. 12 p.m.	98·4	56	16	60	„ 26. 2 a.m.	98·0	56	15	60
„ 28. 12 p.m.	98·7	60	16	62	Mar. 4. 1 a.m.	98·5	56	15	60
Dec. 14. 1 a.m.	98·5	56	15	64	„ 11. 12 p.m.	98·5	52	14	60
„ 20. 1 a.m.	98·7	58	16	60	„ 14. 1 a.m.	98·2	54	15	61
„ 30. 1 a.m.	98·0	56	16	55	Apr. 3. 12 p.m.	98·4	58	16	68
Jan. 23. 12 p.m.	98·35	60	14	61					
Feb. 3. 1 a.m.	98·4	60	17	60		98·4			

These observations show an increase of temperature after sustained exertion of mind. Though the increase is slight, yet I think it must be admitted to be decided, comparing the mean (98·4°) with the average result of the observations (97·92°) made at the same period of the twenty-four hours, when the attention was not roused, when it was rather in a passive indolent state, as in reading merely for amusement, or in the mechanical process of copying writing, both which seem to have, as is indeed generally believed, rather a sedative influence than an exciting one; and are to the former very like what passive bodily exercise is to active muscular exertion.

7. *Of the Effect of Taking Food on the Temperature.*

The following observations were made after rising from the dinner table, at which the observer commonly sat down at five o'clock, and partook pretty fully, using a mixed diet—never taking anything between the breakfast and dinner hour—and using wine commonly at the latter meal, to the extent of three or four glasses, to the exclusion of malt liquor.

	Tongue.	Pulse.	Respirations.	Air.		Tongue.	Pulse.	Respirations.	Air.
	Deg.			Deg.		Deg.			Deg.
Aug. 15. 7 p.m.	98·2				Sept. 29. 6 p.m.	98·5	68	16	62
,, 22. 6½ p.m.	97·9	60	16	60	Nov. 16. 7 p.m.	97·9	62	15	60
,, 25. 6½ p m.	98 1	62	15	59	,, 23. 7½ p m.	98·1	70	18	54
,, 27. 6½ p.m.	98·4	58	16	62	Dec. 21. 7 p.m.	97·9	70	14	63
,, 28. 7 p.m.	98·6	76	16	68	,, 28. 7 p.m.	97·7	64	15	58
,, 29. 7 p.m.	98·3	82	16	63	,, 29. 8 p.m.	98·0	70	15	55
Sept. 2. 6 p.m.	98·5	68	18	71	Jan. 2. 6½ p.m.	97·9	68	15	55
,, 3. 6 p.m.	98·3	60	15	70	Mar. 24. 6½ p.m.	98·5	66	15	52
,, 8. 8 p.m.	97·8	60	15	65					
,, 22. 6½ p.m.	98·4	70	15	55		98·1			

The majority of these results (the mean temperature of the whole being 98·1°) seem to prove that the amount of heat is reduced by a full meal. In the observer's case, as in most others, drowsiness followed this meal, thus approximating the condition of the animal system to that which precedes sleep. On particular occasions, when a larger quantity of wine than usual was taken, the reduction of temperature was commonly most strongly marked. A light meal, such as that of breakfast, consisting of tea, with a portion of toasted bread with butter, and often an egg, has had little effect in depressing or altering materially the temperature. It may be noticed, as regards the habits of the observer, in connection with the observations on temperature made at a late hour, that after dinner he never took solid food, only two or three cups of tea, and this about 8 p.m.

The preceding observations, generally considered, appear to indicate clearly that the temperature of man, as determined in the manner described, is like the animal functions and secretions, constantly fluctuating within certain limits; and, like them, observing in its fluctuation a certain order, constituting as it were two series: one regular, as the diurnal, connected with rest and refreshment from rest; the other, casual or accidental, depending on varying circumstances of irregular occurrence, as exercise, mental exertion, exposure to heat, and the contrary.

As the observations brought forward have been made on one individual, the inferences from them as regards extended application, can be held to be only probable, but probably, I cannot but think, in a high degree, the average temperature of the observer being nowise peculiar; and the results moreover being what might be expected reasoning on the subject, taking for data the proportions of oxygen which have been ascertained to be consumed, and of carbonic acid evolved in respiration, at different periods of the twenty-four hours, and under different circumstances.

Should observations similarly made on others present the like results (and I cannot but be confident that they will), more particular inferences may be drawn from them, especially in conjunction with respiration and the heart's action, not without interest to physiology; and they may admit of important practical application to the regulation of clothing, the taking of exercise, the warming of dwelling-rooms, in brief, to various measures conducive to comfort, the prevention of disease, and its cure. A step in advance is made, if it is only determined, as I believe it to be, that in the healthiest condition of the system, there is danger attending either extreme, either of low uniform temperature, or of a high uniform temperature, and that the circumstances which are proper to regulate variability within certain limits, not prevent it, are those which conduce most to health, as well as to agreeable sensation, enjoyment, and length of life.

The Tables which follow, containing the monthly observations, require little additional explanation. It may be right to state that they do not include the observations made under the influence of accidental disturbing circumstances, as active exercise, etc., the most distinct of which have been given apart in a section appropriated to them. The observations in these Tables, made under ordinary circumstances, or nearly such, will, I believe, be useful for comparison with the former, and I would hope, for reference in progress of inquiry. In most instances, it will be found on comparison, that an unusual elevation of temperature has been followed by unwonted depression, and *vice versâ*.

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
Aug. 6	98·3	...	97·9									
„ 7	98·8	...	98·2	56	...	54	13	...	15	63	...	68
„ 8	98·6	98·5	97·8	56	52	52	13	14	16	63	60	66
„ 9	98·8	...	98·2	60	...	62	14	...	16	63	...	70
„ 10	98·8	...	97·8	...	...	70	...	...	14	...	...	70
„ 11	98·5	98·6	98·0	56	50	58	14	15	16	62	59	67
„ 12	98·6	...	98·0	...	...	58	...	...	14	...	...	68
„ 13	98·7	98·5	97·7	52	52	58	14	15	16	61	62	68
„ 14	98·8	98·7	98·1	56	60	52	15	16	15	62	64	61
„ 15	98·7	...	...	58	...	...	16	...	...	64	...	...
„ 16	98·5	98·5	97·6	56	50	50	16	14	15	63	63	63
„ 17	98·8	98·5	98·1	54	50	50	15	15	14	62	62	68
„ 18	98·7	98·4	98·2	62	54	56	16	15	16	60	68	68
„ 19	98·8	98·4	98·4	66	50	58	18	14	15	60	63	68
„ 20	98·7	...	97·9	54	...	58	16	...	15	62	...	68
„ 21	98·6	...	98·0	58	...	56	16	...	15	62	...	66
„ 22	99·0	...	98·0	56	...	58	16	...	15	60	...	68
„ 23	98·6	...	97·7	52	...	52	16	...	16	60	...	68
„ 24	98·5	98·5	97·8	52	50	48	16	16	14	60	61	68
„ 25	98·7	98·8	98·4	56	54	60	15	15	16	60	54	66
„ 26	99·0	...	98·1	58	...	50	15	...	15	60	...	65
„ 27	98·7	...	98·2	54	...	52	15	...	15	60	...	62
„ 28	98·7	98·8	98·1	60	54	54	15	17	17	62	72	68
„ 29	98·8	...	98·5	54	...	62	16	...	16	61	...	62
„ 30	98·9	98·8	97·7	62	54	50	16	15	16	60	63	63
„ 31	98·7	...	97·8	62	...	58	16	...	16	65	...	68
	98·7	98·5	98·0	56·9	52·5	53·1	15·3	15·1	15	61	63	67

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
Sept. 1	98·7	98·7	97·8	62	56	56	17	16	15	67	67	67
,, 2	98·8	98·7	98·1	54	54	54	16	16	16	68	71	68
,, 3	98·9	98·7	98·3	58	54	54	16	15	16	70	71	68
,, 4	99·0	...	98.1	60	...	48	16	...	14	72	...	70
,, 5	98·8	98·6	98·1	62	54	60	16	16	15	70	68	68
,, 6	98·8	...	...	60	...	...	15	...	...	69	...	...
,, 7	98·7	98·7	98·2	60	60	56	15	16	16	68	68	68
,, 8	98·8	98·9	97·8	54	60	52	15	15	16	62	65	64
,, 9	99·0	99·0	98·1	58	...	50	15	...	15	64	...	64
,, 10	99·0	...	...	60	...	...	15	...	...	64	...	...
,, 11	...	...	97·6	...	...	50	...	...	...	...	...	67
,, 12	98·7	...	98·2	56	...	50	15	...	15	62	...	64
,, 13	98·8	98·8	98·1	66	52	54	16	17	16	62	60	68
,, 14	99·0	98·7	98·0	62	52	50	16	16	15	61	60	60
,, 15	99·0	99·0	98·2	66	58	54	16	18	17	63	60	69
,, 16	98·8	...	97·9	66	...	52	16	...	15	62	...	68
,, 17	98·6	...	97·7	64	...	64	15	...	16	62	...	68
,, 18	99·0	...	98·4	62	...	50	17	...	14	62	...	61
,, 19	99·0	99·0	97·9	64	60	54	15	16	16	60	62	63
,, 20	99·0	...	98·3	58	...	52	15	...	16	60	...	58
,, 21	99·0	...	98·1	60	...	54	16	...	15	58	...	58
,, 22	99·0	99·0	98·5	54	48	54	15	16	16	58	55	65
,, 23	99·0	...	97·7	56	...	52	16	...	14	65	...	68
,, 24	98·7	...	98·3	54	...	56	14	...	15	58	...	63
,, 25	99·2	...	97·2	58	...	48	16	...	16	63	...	66
,, 26	98·3	99·0	98·3	58	56	54	16	16	16	58	58	67
,, 27	98·9	...	98·0	58	...	50	16	...	15	60	...	68
,, 28	98·6	...	97·7	60	...	62	16	...	14	62	...	69
,, 29	98·6	98·9	98·3	54	56	52	15	16	16	59	63	68
,, 30	99·0	...	...	56	...	...	16	...	...	58	...	...
	98·8	98·9	98·0	59·3	55·2	55·8	15·5	16	15·4	66	63	65

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
Nov. 1	98·75	98·8	97·6	54	58	...	16	17	16	53	54	62
„ 2	98·8	98·5	97·7	60	58	54	17	18	16	52	44	60
„ 3	98·6	98·5	97·9	52	48	48	16	17	17	50	55	65
„ 4	98·7	...	97·9	60	...	70	16	...	16	51	...	66
„ 5	98·9	...	97·6	64	...	52	16	...	16	52	...	68
„ 6	99·2	..	98·0	66	...	58	17	...	16	50	...	63
„ 7	98·7	98·7	98·3	58	58	58	16	16	16	51	46	62
„ 8	99·5	...	97·8	63	...	54	17	...	17	52	...	68
„ 9	98·8	...	...	62	...	...	16	...	...	51	...	...
„ 11	99·3	98·3	97·8	62	61	50	17	17	15	52	54	68
„ 12	98·6	98·7	98·2	60	56	52	17	17	15	47	40	65
„ 13	98·8	98·7	98·1	56	62	54	16	17	16	52	50	62
„ 14	98·5	...	98·0	58	...	58	16	...	16	51	...	64
„ 15	98·8	98·7	97·9	56	56	60	16	17	17	54	55	63
„ 16	98·9	98·9	97·7	56	62	58	15	18	15	55	55	68
„ 17	93·7	...	97·5	52	...	52	18	...	15	53	...	65
„ 18	98·6	...	...	54	...	...	15	...	...	55	...	...
„ 19	99·1	...	98·2	60	...	56	17	...	16	55	...	66
„ 20	99·1	...	98·1	56	...	56	17	...	15	55	...	65
„ 21	98·9	...	98·0	60	...	58	17	...	15	55	...	62
„ 22	99·0	...	98·0	60	...	56	16	...	14	55	...	66
„ 23	99·0	...	98·2	60	...	62	16	...	16	53	...	65
„ 24	99·0	...	98·2	58	...	62	16	...	16	52	...	64
„ 25	99·2	...	98·1	56	...	56	17	...	16	48	...	60
„ 26	98·8	...	98·4	54	...	56	15	...	15	44	...	60
„ 27	98·7	...	97·4	60	...	50	16	...	16	43	...	62
„ 28	98·6	...	...	54	...	...	17	...	...	48	...	...
„ 29	99·2	...	98·2	54	...	60	16	...	17	49	...	64
„ 30	99 0	...	97·4	54	...	56	17	...	16	46	...	62
	98·9	98·6	97·9	57·8	57·6	51·3	16·7	17·1	15·8	51	48	64

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
Dec. 1	98·7	98·6	98·3	62	58	50	17	16	16	50	54	64
„ 2	98·8	...	...	54	...	...	16	...	...	48	...	...
„ 3	...	...	97·0	...	...	56	...	...	15	...	...	62
„ 4	98·7	98·7	97·7	56	60	50	16	19	15	44	32	58
„ 5	98·7	...	98·2	58	...	58	16	...	15	40	...	62
„ 6	98·9	97·8	98·3	56	56	60	16	17	16	38	43	60
„ 7	98·6	98·0	98·2	56	54	60	15	16	14	36	47	62
„ 8	98·5	98·4	98·1	56	50	54	16	16	16	38	43	60
„ 10	98·4	98·2	97·9	56	54	52	16	16	16	40	44	62
„ 11	98·5	...	...	5	...	...	16	...	...	40	...	...
„ 13	98·6	...	98·5	58	...	56	17	...	15	38	...	64
„ 14	98·4	...	97·9	60	...	58	16	...	16	40	...	58
„ 15	99·0	98·6	98·3	63	54	56	15	15	16	42	48	62
„ 16	98·4	97·9	98·1	56	56	66	16	15	16	42	58	60
„ 17	99·0	97·7	98·0	60	50	58	15	17	14	45	50	64
„ 18	98·7	...	98·2	60	...	58	15	...	16	45	...	62
„ 19	98·8	...	...	58	...	...	15	...	...	45	...	...
„ 20	98·8	98·0	97·5	64	56	54	15	16	15	43	43	65
„ 21	98·6	...	98·2	58	...	50	15	...	15	43	...	58
„ 22	98·8	98·4	97·8	52	52	62	16	15	14	43	50	66
„ 23	98·9	...	97·9	60	...	54	15	...	16	43	...	62
„ 24	98·6	...	98·2	60	...	54	15	...	15	...	...	65
„ 25	99·2	...	...	60	...	...	15	...	...	43	...	...
„ 26	98·8	98·4	98·3	58	64	52	16	17	15	41	34	50
„ 27	98·5	...	97·7	58	...	66	16	...	16	43	...	...
„ 28	99·3	98·7	97·5	66	58	52	16	16	15	41	53	55
„ 29	98·2	98·6	98·0	50	52	56	16	15	16	45	58	55
„ 30	98·8	98·9	97·3	60	62	66	16	15	16	43	58	58
„ 31	88·5	...	98·0	58	...	...	16	...	...	43	..	55
	98·7	98·2	97·9	58·9	55·2	56·5	15·7	16	15·4	42	47	60

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
Jan. 1	98·5	...	97·5	58	...	58	16	...	15	40	...	...
,, 2	98·4	...	...	54	...	...	16	...	...	42	...	...
,, 3	99·0	...	97·9	64	...	58	16	...	17	42	...	55
,, 4	98·8	...	...	66	...	...	17	...	16	48	...	...
,, 5	99·1	...	98·7	64	...	60	18	...	15	46	...	56
,, 6	99·7	...	98·1	70	...	66	18	...	...	48	...	66
,, 7	100·0	98·9	98·1	72	60	52	18	17	16	50	58	65
,, 8	99·1	...	98·1	62	...	56	16	...	15	49	...	68
,, 9	99·0	...	98·3	60	...	60	16	...	16	46	...	61
,, 10	99·1	98·6	98·3	60	58	54	16	15	16	46	53	60
,, 11	98·7	...	97·4	56	...	58	14	...	14	47	...	60
,, 12	98·7	...	98·0	54	...	60	14	...	16	47	...	64
,, 13	98·9	98·7	98·1	52	60	62	16	14	16	46	53	61
,, 14	99·2	...	97·9	56	...	60	14	...	15	46	...	65
,, 15	98·8	...	97·7	58	...	56	14	...	16	47	...	64
,, 16	98·7	...	97·7	56	...	60	16	...	16	47	...	62
,, 17	98·9	...	98·1	54	...	60	15	...	14	47	...	62
,, 18	98·9	...	98·2	60	...	58	15	...	14	47	...	55
,, 19	98·8	...	97·8	60	...	56	14	...	15	47	...	60
,, 20	98·8	...	98·0	56	...	54	15	...	14	44	...	60
,, 21	98·7	...	97·9	58	...	62	16	...	16	44	...	63
,, 22	98·8	...	97·9	60	...	50	16	...	14	47	...	63
,, 23	98·8	...	...	56	...	...	16	...	...	47	...	...
,, 24	98·9	...	97·3	58	...	54	15	...	15	49	...	68
,, 25	98·6	...	...	54	...	...	15	...	...	47	...	...
,, 26	98·6	...	98·1	58	...	62	15	...	16	48	...	66
,, 27	98·7	...	97·9	58	...	54	15	...	14	45	...	62
,, 28	99·0	...	97·6	62	...	58	15	...	15	43	...	62
,, 29	98·5	...	97·9	56	...	56	15	...	15	42	...	60
,, 30	98·5	...	98·2	56	...	58	14	...	15	41	...	60
,, 31	98·8	...	97·9	60	...	62	16	...	16	38	...	62
	98·8	98·07	97·9	58·7	59·3	57·9	15·5	15 3	15·1	45	55	60

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
Feb. 1	98·8	...	97·5	60	...	52	16	...	15	39	...	62
„ 2	98·5	98·7	...	60	52	...	16	14	...	38	48	...
„ 3	98·7	...	...	60	...	...	16	...	...	41	...	...
„ 5	98·6	...	...	64	...	...	18	...	...	44	...	...
„ 6	98·8	...	97·9	56	...	54	15	...	15	43	...	60
„ 7	98·5	...	98·1	56	...	62	14	...	15	39	...	60
„ 8	98·9	...	98·0	52	...	54	16	...	16	40	...	60
„ 9	98·1	98·5	97·9	52	56	54	14	15	15	38	50	61
„ 10	98·7	98·9	97·7	54	58	52	16	15	15	40	48	60
„ 11	98·7	...	97·7	56	...	54	16	...	15	40	...	60
„ 12	98·7	...	98·2	50	...	58	14	...	16	40	...	60
„ 13	98·4	...	98·2	50	...	54	15	...	15	40	...	60
„ 14	98·2	98·4	97·8	50	54	54	17	15	17	41	48	63
„ 15	98·8	...	97·6	54	...	54	17	...	15	43	...	60
„ 16	98·6	98·7	98·1	54	52	52	15	...	16	43	48	62
„ 17	98·5	...	98·2	54	...	50	16	16	15	44	...	60
„ 18	99·0	...	98·2	58	...	56	16	...	15	45	...	60
„ 19	98·7	...	97·7	56	...	48	15	...	15	44	...	60
„ 20	98·7	...	...	52	...	...	15	...	...	45	...	...
„ 21	98·8	...	98·1	52	...	58	14	...	15	45	...	60
„ 22	99·0	...	97·4	60	...	60	15	...	15	44	...	62
„ 23	98·6	...	98·3	58	...	52	15	...	15	43	...	62
„ 24	98·3	...	97·8	58	...	54	16	...	16	42	...	62
„ 25	99·0	...	97·9	56	...	50	15	...	15	43	...	62
„ 26	98·3	...	98·0	56	...	60	16	...	14	46	...	62
„ 27	99·0	...	97·8	58	...	52	16	...	14	47	...	62
„ 28	98·7	...	98·5	54	...	60	15	...	15	47	...	60
	98·6	98·6	97·9	55·5	54·4	52·3	15·5	15	15·1	42	48	61

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
Mar. 1	99·0	98·6	97·6	58	56	58	16	16	16	45	52	60
„ 2	99·1	98·7	97·9	58	52	46	16	15	16	44	55	60
„ 3	98·5	...	98·5	52	...	56	15	...	15	47	...	60
„ 4	99·0	98·3	98·0	60	54	54	15	16	15	45	52	60
„ 5	98·6	...	98·0	58	...	66	17	...	15	43	...	60
„ 6	99·0	...	98·2	60	...	54	16	...	15	43	...	60
„ 7	99·0	...	98·0	60	...	64	14	...	15	44	...	62
„ 8	99·2	...	98·1	60	...	60	16	...	15	46	...	60
„ 9	99·1	98·7	93·0	60	48	54	15	15	15	48	54	58
„ 10	99·0	...	97·5	60	...	54	16	...	15	48	...	60
„ 11	98·8	...	97·9	56	...	48	15	...	14	47	...	58
„ 12	98·4	...	97·7	54	...	56	16	...	14	47	...	61
„ 13	98·8	...	98·2	60	...	54	15	...	15	43	...	61
„ 14	98·6	...	97·9	54	...	46	14	...	14	44	...	58
„ 15	98·2	...	97·5	54	...	60	14	...	14	42	...	62
„ 16	98·7	...	98·1	58	...	62	15	...	14	43	...	58
„ 17	98·6	...	98·0	60	...	52	15	...	15	40	...	58
„ 18	98·5	98·7	98·0	62	58	48	15	15	15	43	53	60
„ 19	98·5	...	98·4	56	...	54	14	...	15	43	...	58
„ 20	98·6	...	97·5	54	...	54	15	...	15	42	...	60
„ 21	98·8	98·6	98·1	54	50	50	14	14	14	44	53	60
„ 22	99·0	98.5	97·8	56	54	54	15	15	13	48	58	60
„ 23	99·0	...	97·8	58	...	52	15	...	14	50	...	61
„ 24	98·7	...	98·0	58	...	48	15	...	13	53	...	58
„ 25	98·4	...	98·0	54	...	48	14	...	14	52	...	60
„ 26	98·4	...	98·2	54	...	60	15	...	15	51	...	62
„ 27	98·8	...	97·8	60	...	52	15	...	16	52	...	64
„ 28	98·7	...	98·3	54	...	54	16	...	15	50	...	64
„ 29	98·7	...	97·3	56	...	54	14	...	14	50	...	65
„ 30	98·6	98·6	97·8	55	52	48	15	15	15	51	54	62
„ 31	98·6	...	97·5	54	...	54	15	...	15	51	...	63
	98·74	98·59	97·93	57	53	54	15·1	15·1	15	46	54	60

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
Apr. 1	98·3	...	98·0	58	...	58	16	...	15	53	...	62
,, 2	98·7	98·8	97·7	58	58	52	15	16	15	53	62	64
,, 3	98·8	...	97·7	56	...	48	15	...	14	55	...	62
,, 4	98·8	...	97·8	54	...	60	15	...	14	58	...	64
,, 5	98·9	...	98·1	60	...	64	15	...	16	58	...	64
,, 6	99·3	98·8	97·9	62	60	50	15	15	15	55	61	62
,, 7	98·8	98·8	98·2	54	56	56	14	15	16	58	61	63
,, 8	98·5	...	97·9	56	...	56	15	...	15	58	...	61
,, 9	98·6	...	98·1	60	...	58	14	...	15	54	...	62
,, 10	98·5	98·3	98·0	62	52	62	14	14	15	52	52	63
,, 11	98·7	...	97·6	60	...	54	14	...	15	51	...	63
,, 12	98·4	...	97·9	56	...	50	15	...	15	49	...	59
,, 13	98·5	98·7	97·6	54	54	54	15	15	15	49	55	60
,, 14	98·7	...	98·0	56	...	54	15	...	14	52	...	60
,, 15	98·6	98·8	...	56	60	...	15	14	...	49	58	...
,, 16	98·4	...	97·7	58	...	52	15	...	15	52	...	62
,, 17	98·8	...	97·8	56	...	50	15	...	14	56	...	58
,, 18	98·4	98·8	98·1	60	54	54	15	15	15	60	60	66
,, 19	98·7	98·6	97·6	60	52	50	15	16	15	58	60	62
,, 20	98·7	...	98·3	58	...	54	15	...	15	60	...	64
,, 21	98·7	...	97·3	58	...	56	16	...	13	60	...	62
,, 22	98·4	98·1	98·0	60	56	50	16	15	15	62	65	64
,, 23	98·5	...	97·7	56	...	54	15	...	15	62	...	62
,, 24	98·7	98·2	97·7	58	52	52	16	16	15	64	64	65
,, 25	98·6	98·4	97·9	64	52	52	16	15	16	65	63	62
,, 26	99·0	...	98·0	58	...	50	15	...	14	62	...	62
,, 27	98·8	98·8	97·9	54	52	50	15	14	15	60	57	63
,, 28	98·6	...	...	58	...	...	16	...	...	58	...	...
,, 29	...	98·3	93·2	...	54	52	...	15	15	...	60	64
,, 30	98·9	98·7	98·2	58	56	48	14	15	14	58	58	65
	98·66	98·57	97·88	56·5	54·8	53·6	15·4	15	14·8	54·6	59·8	62·4

III. MISCELLANEOUS OBSERVATIONS MADE DURING A VOYAGE FROM ENGLAND TO BARBADOS IN 1845.

Publishing observations shortly after they have been made, whilst all the circumstances are fresh in recollection, ought to insure the greatest degree of accuracy that is attainable ; and, it may be said, further, in favour of speedy publication, that there will be less chance of the observations being in any wise altered to make them harmonize, as it may be supposed, with others, or to suit any particular theory.

The observations I have to offer will be given in accordance with the feeling just expressed, and I shall be well pleased if they are received as a small contribution to the branches of science to which they pertain, and if the remarks offered in conclusion on the ventilation, conjointly with the temperature of steam-packets, at present so defective, excite attention, with a view to improvement.

1. *Of the Temperature of the Sea at the Surface, and of the Specific Gravity of the Surface Water.*

The voyage was made in the "Clyde," one of the West Indian Royal mail steamers, in the space of twenty days, having got under way at Southampton in the afternoon of the 17th of June, and cast anchor in the road-stead of Barbados at noon on the 7th of July. All the way the wind was fair, chiefly north-easterly, and the greater part of the way the weather was agreeable. We touched nowhere, except at Madeira, where we stopt about four hours.

Every day at noon a portion of the surface water was taken up and bottled, and at the same time the temperature of the air was observed. The samples of water so obtained I have carefully weighed, using a very delicate balance, since my arrival here. The results, with the daily latitude and longitude, determined by the officers of the vessel, and also the temperature, taken chiefly at 8 a.m., are given in the following table :—

	Latitude, north.		Longitude, west.		Temp. of surface water.	Sp. gr. of surface water.	Temperature of air.	
	deg.	min.	deg.	min.	deg.			deg.
June 18	49	23	4	7	60	10,254	11 a.m.	58
„ 19	49	4	6	45	62	10,263	8 „	61
„ 20	44	19	8	41	62	10,264	„	62
„ 21	41	7	10	58	62	10,264	„	62
„ 22	37	52	12	56	64	10,264	„	63
„ 23	34	44	14	52	66	10,270	„	65
„ 24	Madeira		Madeira		70	10,272	„	65
„ 25	31	5	20	24	70	10,272	„	67
„ 26	29	32	23	54	71	10,274	„	70
„ 27	27	51	27	16	73	10,274	12 noon	73
„ 28	26	9	30	37	74	10,275	8 a.m.	72
„ 29	24	33	33	53	76	10,277	„	72
„ 30	23	2*	36	38	76	10,279	„	75
July 1	21	25	40	13	78	10,272	„	76
„ 2	19	58	43	32	78	10,273	6 „	75
„ 3	18	37	46	42	79	10,273	8 „	78
„ 4	17	17	49	48	80	10,267	6 „	77
„ 5	15	55	52	59	80	10,252	8 „	80
„ 6	14	31	56	30	81	10,246†	„	81
„ 7	About ¼ mile off Bridge town, Barbados				81	10,233†	„	81

The observations on the temperature of the sea were, I believe, pretty accurate ; but I am not sure of their perfect accuracy, as they were made on water pumped into a bucket in the engine-room (the first bucket or two rejected), and with a thermometer belonging to the engineer, the scale of which had not been compared with a standard one.

The very slight differences of the specific gravity of the surface water of the greater part of the ocean traversed, extending to 36° of latitude, is a circumstance worthy of notice ; and, further, that the slight variations observed were chiefly at no great distances from land, and on entering a part of the ocean subject at this season of the year to heavy rains : thus, in accordance with the latter remark, the water taken up at noon on the fourth was a little lighter than the samples of many preceding days—heavy rain fell during the preceding night ; and the water taken up daily till we reached Barbados was still a little lighter.

The thermometer has been usefully applied to determine currents at sea ; and it has been proposed to employ it to determine

* Sun's altitude 89° 40′.

† Sea slightly greenish, not the pure blue of mid-ocean ; a bank extends about 300 miles from Barbados.

the vicinity of land. May not the specific gravity of sea-water be turned to a similar account? Any well marked change should indicate some powerful modifying influence, such, in the instance of diminution, as the approach to some great river, or to a region of storm and rain; or, on the contrary, of increase, the approach to a shore without rivers, or to a region suffering from drought—indications which, to the exploring navigator, might not be destitute of utility.

Besides noticing the temperature of the air at 8 a.m., I often observed it at other times, about noon especially, and at sunset. It was commonly highest at noon, in accordance with what I had observed many years ago on a voyage to Ceylon, when I found that, whilst the temperature of the air was at its maximum when the sun was highest, that of the surface water did not attain its maximum till two or three hours later.

The low temperature of the sea and air between England and Madeira in the last week of June is worthy of remark; and the latter especially, compared with the heated state of the atmosphere in England during the preceding week, when, in most parts of the country as well as in London, the temperature by day was often about 70° and 75°. The marked difference is suggestive of a probable advantage to be derived by invalids, who may have the means at their command, in sailing at this season on the Atlantic as far as the latitude of Madeira, or a degree or two farther south. The agreeable temperature we experienced, it may be inferred from several circumstances, was not singular, but of regular occurrence, depending on the temperature of the sea and the direction of the winds, which are commonly, ou this portion of the sea, from cool regions—circumstances to which Madeira, no doubt, is mainly indebted for the coolness of its summer's climate. Another recommendation of the ocean climate is, that besides its agreeable coolness and little variation by day and night, no dew ever appearing, the degree of dryness of the air is favorable for most pulmonary complaints. A thermometer used as a hygrometer generally fell 5° compared with a thermometer with a dry bulb.

2. *Of the Effect of Sea-sickness as a Remedial Means.*

It has for a long while been supposed that sailing is beneficial in pulmonary complaints, and especially in pulmonary consumption; and that it has proved so in many instances can hardly be doubted. The effect has been referred by different writers to various circumstances—partly to mildness and equability of climate—partly to the gentle exercise, unavoidable and constant, connected with the motion of a ship—partly to the sickness or feeling of nausea more or less commonly produced; and by some, perhaps fancifully, to a something unknown, some peculiar vapour exhaled from the sea, and floating over it, in its atmosphere.*

The few following observations made on myself tend to support the generally-received opinion referred to, and to shew that the effect belongs to the class of the lowering and sedative, that is, the reverse of the stimulating or exciting.

I may premise that I am very subject to sea-sickness; but that on this voyage I suffered less from it than on any preceding, owing to the comparatively little motion, from the great size of the steamer, and the fineness of the weather: only twice or thrice, I may add, did the sickness amount to vomiting, and but little nausea even was experienced after leaving Madeira.

The following table shews the temperature of the body determined by a delicate thermometer placed under the tongue, the state of the pulse and of the respiration as regards frequency, carefully counted in the sitting posture, and at the earlier hours in bed in the same posture.

	Temperature under tongue.	Pulse.	Respirations.	Temperature of cabin.
	deg.			deg.
June 18 7 a.m.	98·25	52	14	64
„ 18 1 p.m.	97·9	54	13	66
„ 19 7 a.m.	98·55	60	13	65
„ 20 6 „	98·3	54	13	66
„ 21 6 „	98·3	58	13	64
„ 22 6 „	98·2	54	13	68
„ 23 6 „	98·3	54	13	70
„ 24 6 „	98·2	60	14	73
	98·25	55·57	13·25	

These observations, to have weight, must be compared with others made under different circumstances, which I am enabled

* Whether any effect can be produced by the minute quantity of saline matter, raised as spray and mechanically suspended in the air over the sea, is open to question—as also the influence of ozone.

to do, having for many months previously made similar ones on myself, the results of which are contained in the preceding paper. It may be sufficient here to mention, that the mean of seven months' observations, viz., from the first of August to the last day of April, interrupted only in November, is, for the morning temperature, 98·74°; the morning pulse, 57·6; the morning respirations, 15·6.

Comparing these mean numbers with those for the eight days on the voyage to Madeira, do they not seem to warrant the conclusion before mentioned, that the influence of sailing is rather lowering or sedative than stimulating or exciting, and especially as regards the function of respiration.

It is reasonable to suppose that this effect may diminish as the individual becomes accustomed to the motion of the ship, and loses all sense of nausea. If so, short voyages with intervals of a week or two might be preferable for the invalid to voyages of long duration. I continued my observations to the end of the voyage, and their results, I think, are in favor of this conclusion; but they are far from decisive, as a disturbing cause soon interposed, viz., the higher and exciting temperature of the intertropical region, which we so soon entered after leaving Madeira. Probably were minute inquiry instituted, besides the effects on the system that I have hinted at, others might be detected. It is not unlikely that most of the secreting organs are influenced, and, with their action, their secretions modified. Whilst living on shore in England a deposit of lithate of ammonia often appeared in my urine. During the voyage, though using a good deal of animal food, I never witnessed it; but, on the contrary, there was almost constantly a formation of ammoniaco-magnesian phosphate, which presented itself in its characteristic form of an iridescent pellicle. Gout is, I believe, unknown amongst sailors, and calculous complaints far from common. May not this be owing in part to an influence such as that alluded to, tending to check the lithic acid diathesis? I may add another observation. It appeared to me that at sea, whilst no lithic acid was deposited in the urine, but an unusual proportion of the double phosphate, less tartar was deposited on the teeth,—a matter, as it is well known, which consists chiefly of phosphate of lime. Admitting the fact, and I do not think I could have been mistaken, having on shore been in the habit of removing the tartar from the inner surface of my lower front teeth from its collecting rapidly,—the excess of phosphates in the urine might be connected with, and occasion, a diminution of them in the salivary secretion.

3. *Of the Effect of Increased Atmospheric Temperature on the Temperature of Man.*

In a voyage to Ceylon already adverted to, which I made in 1816, a pretty extensive series of observations then instituted tended to prove, that the temperature of man, as determined by a thermometer placed under the tongue, is not a constant one even in health, and that it is elevated by one or two degrees by passing from temperate into tropical regions. Provided now with a more delicate thermometer than I then used, I took advantage of a rapid transition from a comparatively cool to a warm climate to repeat the observations. This was done on the 2nd of July, when the temperature of the air between two o'clock in the afternoon, and half-past two, under the awning on deck, was about 78°. The subjects of the observations were a certain number of the passengers, then apparently in good health, who were sitting at the time, and who, after luncheon at noon, had taken little or no exercise.

	Temp. under tongue.	Temp. of closed hand.	Pulse.	Age, about.	
	Deg.	Deg.			
1	99·4	97·1	86	30	A German of large and robust frame.
2	99·2	...	106	18	A Creole, rather tall and slender, returning to the West Indies after having been several years in England.
3	100·2	98.5	82	22	Tall and robust, also a Creole, returning after having been educated in England.
4	99·8	96	60	52	Stout, returning to the West Indies, where, for many years, he had resided.
5	99·4	98	72	22	An undergraduate of Cambridge, well made and vigorous, visiting the West Indies for his amusement during the long vacation.
6	99	98·2	78	24	Rather stout, never before in a hot climate; a native of Ireland.
7	99·2	...	78	24	Rather short and stout.
8	99·8	...	78	18	A young lady, a Creole, well made, of rather large frame, returning to the West Indies, after having been several months in England.

Observations on my own temperature were in accordance with the preceding. I may mention a few made on the following day, at different times, when in my cabin, where the temperature of the air was uniformly 80°.

	Temp. under tongue.	Temp. of hand.	Pulse.	Respirations.	
	Deg.	Deg.			
6 a.m.	98·7	98	64	14	Skin moist; sitting up in bed before rising.
8 ,,	99	98	62	16	Just after washing from head to foot.
10 ,,	99·1	98	64	15	After walking on deck half an hour; skin moist with perspiration.
11½ ,,	99·2	98	60	15	After reclining an hour.
1 p.m.	99	98	60	16	After a light fruit luncheon and a glass of water.

These observations, and many more which I made during the voyage, and since landing in Barbados, clearly shew that the temperature of the body rises with the temperature of the air, and that on sudden transition from a mild or cool climate to a hot climate, there is a tendency to a state approaching to the feverish, marked not only by increase of temperature, but also by accelerated action of the heart, and quickened respiration, especially on making bodily exertion.

Such effects obviously suggest to the newly arrived in a tropical climate, great temperance in all things; the using less wine and animal food than they had been accustomed to in a cooler atmosphere; dressing coolly, and avoiding as much as possible, for a time, all fatiguing exercise and exposure to the sun. Unfortunately such care is seldom observed, and many have to regret the neglect of it; indeed, on landing it is often difficult to observe the precautions which health requires, there being commonly duties to be performed demanding immediate exertion and exposure; not to mention the temptations to do too much, arising out of curiosity on visiting a new country.

4. *Of the Temperature of Different Parts of the Body on entering a Warm Climate.*

Common observation shews that on entering a warm climate, the extremities especially become warm; coldness of the hands and feet is no longer experienced, but rather an unpleasant sensation of heat in them with a disposition generally in the skin to be moist with perspiration. It may not be amiss here to give a few precise observations on the temperature of different parts of the body, ascertained with as much accuracy as possible. When the situation permitted, the bulb of the thermometer was completely covered; when otherwise, the bulb was gently pressed on the surface, and barely covered with a small pledget of cotton-wool. The following observations of the kind alluded to were made at sea, on the morning of the 6th of July, between six and seven, a.m., before rising, when the temperature of my cabin was 82°. On commencing them it may be premised, that the temperature under the tongue was 98·7°; of the closed hand 98°; the pulse 60; the respirations 14; and just after finishing them, the temperature under the tongue was 98·9°; of the closed hand 98·4°; the pulse 64; the respirations 15; an augmentation probably owing more to the excited attention than to bodily exertion, which was intentionally as little as possible, keeping chiefly in the horizontal posture.

	Deg.
Closed hand	98
Back of hand covered	94·45
Back of hand not covered	94·4
Axilla in different parts	98·9 98·4 98
Lower part of groin	98·9
Groin over femoral artery	98·4
Region of stomach, drawn in by stooping forward so as to cover well the bulb	99·2
Right hypochondrium, the bulb in like manner covered	99·25
Umbilical region	99·2
Ham	96

Two mornings previously, viz., on the 4th of July, between six and seven a.m., when the air of my cabin was also 82°, the temperature under the tongue 98·7°, the pulse 60, the respirations 13, the observations which follow were made:—

	Deg.
Between great and second toe of foot	97
Ham	95
Over femoral artery in thigh, about midway; the bulb covered	96·7
Ditto, in same place; uncovered	96·2
Over middle portion of thigh, about two inches from the course of the femoral artery; the bulb covered	94
Ditto, in the same place; the bulb uncovered	93
Lower part of groin	98·2
Umbilical region	99
Axilla	98·1
Bend of fore arm	97·3

Whilst the observations on both these occasions were made, the body was most lightly clad, and in great part was naked; the skin was moist with perspiration; there was very little evaporation, the difference between a thermometer with a moist and dry bulb being only 2°; and in the last noticed instance, as well as in the first, towards the end of the observation, there was a slight increase of the temperature under the tongue.

5. *Of the early Morning Temperature of the Body within the Tropics.*

According to the observations which I have made in England, the temperature of the body, as measured by a thermometer placed under the tongue, is lowest at night, about midnight, and highest on rising, after the rest of the night; the average difference was 0·82. The limited observations which I have as yet made within the tropics, lead me to infer that the early morning temperature, just after waking and quitting bed, is lower than the night temperature before retiring to rest, and decidedly lower than the day temperature. The following are a few of the observations which I have hitherto made leading to this conclusion. Of these some were made at sea,—those preceding the 7th of July; the rest after landing:—

	Temp. under tongue.	Temp. in hand.	Pulse.	Respirations.	Temp. of air.		Temp. under tongue.	Temp. in hand.	Pulse.	Respirations.	Temp. of air.
July	Deg.	Deg.			Deg.	July	Deg.	Deg.			Deg.
1. 6 a.m.	98·5	97·5	56	14	80	15. 11½ p.m.	99·4	98·5	78	16	77
2. ,,	98·5	97·5	60	15	79	18. 6½ a.m.	98·4	97·25	60	15	78
3. ,,	98·7	98	56	14	80	18. 10½ p.m.	99·5	98·5	70	17	77
4. ,,	98·7	98	60	14	83	19. 6 a.m.	98·4	97·5	62	16	77
6. ,,	98·7	98	60	13	82	19. 10 p.m.	99	97·5	60	16	79
12. ,,	98·35	97·5	58	17	79	20. 6 a.m.	98·1	97.25	56	15	77
12. 10½ p.m.	98·5	98	64	16	79	20. 10 p.m.	98·7	97.25	52	15	79
14. 6 a.m.	98·4	97·5	58	15	75	21. 6 a.m.	98·6	97·5	54	15	78
14. 10 p.m.	98·95	98	70	17	80	21. 10½ p.m.	98·8	97	60	16	81
15. 6 a.m.	98·7	98	60	15	78						

These observations give an average early morning temperature of 98·33°, whilst in England it was 98·74°; and an average night temperature of 98·88°, whilst in England it was 97·92°.

Should more extended observations confirm these conclusions, the differences of temperature at different times of the day, in the two climates, may perhaps be connected with the manner of sleeping in a tropical country, merely covered with a sheet and commonly in a current of air; and in part to the moist state of the skin, presenting a constantly evaporating surface of large extent, especially when wet or moist with perspiration; and partly to the subsidence of functional action in sleep, after undue excitement when waking. By day, in all the instances mentioned, the temperature had risen to 99°, or above that. However it may be explained, should the fact be established, that the temperature of the body falls in sleep far more in a tropical than in a temperate climate, in a hot than in a cool one, it should be suggestive of precautions in sleeping, especially as to too free exposure to the wind, or currents of air, and very thin or no bed clothing,—precautions commonly observed by the natives of hot climates, and too commonly neglected by strangers unaccustomed to tropical heats; and very often I believe with bad effect, especially when malaria is rife, and exposure to the night air is especially dangerous.

6. *Of the Effect of a High Artificial Temperature on Man.*

I availed myself of the opportunity which the steamer offered to notice the effect of a comparatively high temperature on men exposed to it for a considerable time. On the 4th of July, about 11 a.m., when the temperature of the air on deck was 80°, I descended with the principal engineer to the hottest place in the ship, where the men work who are employed about the engines. It was aft, over the boilers, its floor the plate, ventilated by one port-hole only, and a small circular opening in the deck little larger than sufficed to allow a man to enter. The temperature of the air in it was 111°, and I was told it was often higher; the floor was hotter, disagreeably so to the feet even protected with moderately thick soled boots. The men employed here in removing coal, of which it is a receptacle, seldom remain above twenty, or at most twenty-five minutes.

1. A healthy man, 24 years of age, who had been employed about the fires three hours, and during the last quarter of an hour was in this hot atmosphere, had his temperature tried just on leaving it. Under the tongue it was 100·5°; in the closed hand 98·9°; his pulse 112, and strong. He had on trousers and

shoes, was otherwise naked, and his skin was wet with sweat standing in drops, or running off in small streams.

2. After exposure to the same temperature for above fourteen minutes, dressed as usual, I found the temperature under my tongue 99·5°; of the closed hand 98·5°; the pulse 83; the respirations 16. There was profuse perspiration; no uneasy sensation. After exposure of about 25 minutes, sweating profusely, and still without any well marked unpleasant sensation, the temperature under the tongue was found to have risen to 100·2°; of the hand to 99·9°; the pulse to 102; and the respirations to 18. It may be mentioned, that an hour before, after walking gently for about half an hour, exposed to the wind at 80°, the temperature under the tongue was 98°; of the closed hand 98°; the pulse 60; the respirations 15.

3. The chief-engineer, a robust healthy man, about 30 years of age, who had been below nearly two hours seeing to the engines, but not in the hottest parts, was the next subject of observation. After exposure of twenty-five or twenty-six minutes to a temperature of 111°, his temperature under the tongue was 102·3°; of the closed hand 100°; his pulse 142, and not feeble. He was profusely sweating. He said he had no uneasy sensation.

4. The last subject of observation was a man about 25 years of age, active and healthy, who had been exposed to the high temperature more than a quarter of an hour, and elsewhere to a less high temperature about two hours. His temperature under the tongue was 101°; of the hand 100°; his pulse 102. He was profusely sweating.

It may be worthy of mention, that when resting in my cabin after this exposure, on making a deep inspiration and expiration, a sensation of cooling was experienced in the region of the lungs, contrary to what is felt, according to my experience, in breathing in the same manner, when the body is cold, when I have always noticed, directing attention to the part, a distinct feeling of warmth.

I may give the results of two other trials of temperature, made in the after stokehole, where the fires of the furnaces are fed, and where, when the furnace doors were closed, the temperature of the air was about 104°; when opened, about 112°, the moistened bulb-thermometer falling to 94°. After having been there about seven minutes, a profuse perspiration occurring, I found the temperature under my tongue 99·4°; hand 97·75°; pulse 70; respirations 20. A quarter of an hour before, the temperature under the tongue was 98·3°; of hand 97·5°; pulse 52; respirations 15. The temperature of a stoker was 99·5°. He had been employed a considerable time in attending to the fires, dressed in trousers and shirt, and was profusely perspiring.

It is said that the firemen, and the men employed about the engines generally, notwithstanding the very high temperature to which they are exposed, have whilst in the West Indies better health than the common seamen, and are, especially, less liable to fever. Their less liability to fever seems to be a well established fact; and it is certainly a curious, and I cannot help thinking a valuable, fact. Does it not tend to shew that a high temperature of about 110°-112° is destructive of malaria? This would be in accordance with what seems to be ascertained relative to the effect of high temperature in the instance of the contagion of plague and of vaccine lymph,* rendering them inert. Those who do not adopt this conclusion, may, perhaps, refer the escape, which the engine men commonly experience, from fever in the West Indies to their not being exposed to the night air, as the common sailors are on this station, and where (on account of the numerous islets and reefs rendering the navigation arduous and difficult) it is necessary for them to be much on the alert, and to exert themselves more than is usual in the open ocean.

As regards general health, and wear and tear of the constitution, it seems difficult to imagine that the exposure to so high a temperature as the firemen are obliged to undergo is not injurious. Statistical returns of the diseases to which this class of men are subject, and of the average length of years they are capable of serving may be mentioned as desiderata. They will probably be found to suffer in a high ratio from diseases of the

* Vaccine lymph sent from England to the West Indies in the steam packets, I am informed has always proved inert. Sent under cover through the post office as a letter, it is put into the mail bags which are kept in a very hot part of the ship where wax melts. This temperature may render the lymph useless. In transmitting lymph to tropical countries, especially in steamers, it seems very desirable that the parcel should be addressed to the purser or surgeon, with a "N.B. Vaccine lymph: to be kept in the coolest place." At present in the West Indies, I am told, that, owing to the inefficiency of the lymph imported, there is a considerable number of persons, especially negroes from the western coast of Africa, requiring to be vaccinated, and many of them in the public service.

heart and brain, and to be specially subject to sudden deaths from rupture or over-distension of the blood vessels. Two days before reaching Barbados, it may be noticed, that a man, just as his four hours of duty in attendance on the engine expired, was struck down by apoplexy of a severe kind, and which it is likely would have proved fatal, but for the active and judicious treatment employed by the surgeon of the ship. The blood abstracted from the arm in this case, it is remarkable, was reported to have coagulated almost instantly, and to have become putrid in a very short time, emitting an offensive smell, it was said, in less than a quarter of an hour.

7. *Of the Ventilation and Cooling of Steam Ships.*

The steamers employed in the West Indian packet service, as also in the Oriental, are many of them noble vessels, fitted up and conducted so as to afford a very tolerable degree of comfort to the passengers, especially the West India packets with single berths. What they seem most faulty in is ventilation, and the means (essentially connected with a due supply of air) of keeping them cool. To this part of their construction the help of science does not appear to have been applied. The means of ventilation available are only the ordinary ones of ports, sky lights, and wind sails,—all precarious, and often not admitting of use. Even in moderate weather, it was necessary on this voyage to have commonly the ports in the lower berths closed, and always at night, and often likewise the ports in the saloon,—the common dining room,—to the no trifling discomfort of the passengers, after so suddenly passing into a tropical climate. If the saloon and lower berths are ill-ventilated, some other compartments of the ship are worse, especially those near the engine, heated by communicated heat, not being protected by bad conducting surfaces. The mess-room of the officers of the vessel may be mentioned as an example. In dimensions it is about 8 feet by 6; and besides the entrance door it has only one port for the admission and exit of air. At two o'clock in the afternoon of the 3rd of July, when the temperature of the air on deck was 80°, in this confined space it was 90·1°. It was just after the officers' dinner. A young midshipman present, who was bathed with perspiration, submitted to have his temperature tried; under the tongue it was 99·8°; in the closed hand 98·5°. I no sooner entered than I was also in a profuse perspiration, but without experiencing any oppressive feeling of heat. Such a feeling the officers did not speak of experiencing; but they did say,—and they are men not given to complain,—that they were subject to get cold going suddenly from their vapour bath into the open air.

It is to be hoped, that the company which has shewn so much enterprise in establishing a regular communication with the West Indies by such noble steamers, and so much perseverance in contending with difficulties, and resolution in striving against disasters considered almost ruinous, will not be satisfied with having brought their establishment of steamers into tolerable order, but will assiduously apply themselves to improve them to the utmost extent; and first, and most of all, as regards defective ventilation. Simple means, I am confident, might be suggested by which this could be effected, without the necessity of incurring any great expense, and without risk of impairing the strength of timbers. A very ingenious machine for ventilation has lately been invented by Dr. Arnott, of which I had the satisfaction of seeing a model before leaving town. It is recommended by its cheapness and freedom from complexity. It is admirably fit to introduce fresh and cool air into any part of a steamer. By means of it, at the expense of a few shillings, or a few pounds at farthest, aided by air tubes judiciously placed, and bad conducting surfaces, I have no doubt, all objection, as regards want of air and excess of heat, might be removed.

The Company, it appears to me, ought to look forward to a time when in the favorable season, at the beginning of our winter, a voyage to the West Indies will be as commonly recommended to invalids requiring a mild winter climate, as to Pisa, Naples, or Malta at present. Were their steamers ventilated as they might be, and kept of a mild temperature, having the means of introducing warm air into the cabins as well as cool air, they would be admirably fitted for conveying invalids, and the comfort afforded by them would be vastly increased, and could not fail of being duly appreciated by the passengers; and I will add, it may be for the interest of the Company thus to improve them, thereby giving them still greater advantages than they at present possess over sailing vessels.

IV. ON THE TEMPERATURE OF MAN WITHIN THE TROPICS.

In a paper on the Temperature of Man, which I had the honor to submit to the Royal Society in 1845, and which was published in the Philosophical Transactions for the same year, I expressed the hope of being able to continue the inquiry in the West Indies, to which I was then about to proceed.

In the present communication I propose to lay before the Society the results of the trials made whilst there, viz. during a period of about three years and a half, exclusive of trifling interruptions, and of one prolonged through several weeks, between June 1847 and October of that year, owing to illness.

In making the trials, the same instrument as that before used was employed, and with like precautions to insure accuracy, and as then, the subject of the observations was the same individual; also, as then, the pulse and respirations were noticed at the time, and invariably in the sitting posture.

The greater number of the observations were made in Barbados, in a house situated about half a mile from the sea-shore and a few feet only above its level,—where the mean annual temperature of the atmosphere is about 80° Fahr., and the range of temperature throughout the year from about 10° to 18° in the open air.

For the sake of comparison, I shall follow as closely as possible the order observed in my former paper in stating the results.

1. *Of the Variation of Temperature during the twenty-four hours.*

To ascertain this, observations were made commonly three times a day, viz. immediately on rising, about 6 a.m., before taking any food, and before making any exertion, even in dressing, clad merely in a light dress consisting of loose drawers and gown, which in that climate are almost always sufficient for comfort;—next, about 2 p.m., sometimes an hour earlier or later, and generally after occupation, either within doors at home, or at an office a mile and a half distant nearly, to which I went in a carriage,—the occupation being chiefly that of reading or writing, or some other requiring little bodily exertion;—next, the last thing before retiring to rest at night, which was commonly at 10 o'clock, rarely later than 11.

It may be further premised, that the manner of living, as to diet and the time of meals, was much the same as in England,—breakfasting commonly at 9 o'clock, dining about 5, without intermediate luncheon, and drinking tea about 7.

The following Table exhibits the mean monthly results of the daily observations made in accordance with the above, during a period of thirty-six months: in an Appendix, the observations as daily recorded will be given, omitting only those which may be considered abnormal, either owing to bodily ailment, or other cause of a special kind, requiring particular notice :—

.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
1845.	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
July	98·2	99	98·2	56	61	60	14·7	15·5	16	78·3	81·5	79·4
Aug.	98	98·7	98·2	53·8	53·6	57·9	15	15·7	15·8	78·7	84	80·3
Sept.	98	98·9	98·9	54·3	55	62·1	15·2	15·6	15·6	78·6	84·7	80·5
Oct.	97·9	99	98·8	53·4	56·3	61·3	14·6	15·9	16·8	78	85·4	81
Nov.	98	98·6	98·7	53·3	56·5	61	14·8	15·6	16	78	83·4	79.9
Dec.	98	98·6	98·6	53·8	55·2	66	14·4	15·5	15·7	75	81·8	77
1846.												
Jan.	98·2	99	98·7	54·3	58·5	61·7	14·6	15·7	15·6	74·3	82	78·2
Feb.	98·1	99·1	98·6	55·1	57·1	59·9	14·3	15·5	15·2	74	83	76·6
Apr.	98·1	98·7	98·6	54	54·5	59·5	14·4	15·6	15·4	77	84	79
May	98·2	98·9	98·9	56·2	55·2	61·4	15	15·8	15·6	78·7	85·8	80
June	98·2	99	98·8	55·1	55·6	59·7	14·6	15·7	15·4	79	84·7	81
July	98·2	99	98·9	54·5	54·5	60·7	14·7	16	15·3	78·8	84·8	81·4
Aug.	98·1	99	98·9	54·2	55·6	60·5	14·6	15·9	15·6	77·7	85·5	81·7
Sept.	98	99	98·2	55	54	60·8	14·3	15·9	16	78·7	85·1	81·3
Oct.	98·2	99	99	52	55	61	14·4	15·4	15·3	78·2	85·1	81·2
Nov.	98·1	98·9	98·8	56·2	55·4	60·1	14·6	15·8	15·6	77·4	84·4	80
Dec.	98·1	98·9	98·9	55·3	55·3	63·5	14·5	15·8	15·8	76	80·5	77·4
1847.												
Jan.	98·1	98·9	98·8	54·8	55·3	60·8	14·4	15·4	15	74·6	81·3	77·3
Feb.	98	98·8	98·8	55·5	57·6	61·7	14·2	15·5	15·3	73	81·5	76·3
Mar.	98·1	98·8	98·9	54·8	59·2	62·7	14·5	15·6	15	74·6	83·5	77·1
Apr.	98·1	98·7	98·8	55	56·4	62·3	14·2	15·5	15·6	74·7	82·2	77·1
May	98·1	98·9	98·8	56·6	57·8	61·3	14·5	15·8	15·6	76·7	85	79·2
Oct.	98	99	99	54·7	56	55·7	14·5	15·1	15·7	77·7	84·8	80
Nov.	98·1	98·8	98·8	53·3	54·8	59·2	14·5	15·1	15·2	76·7	82·3	79
Dec.	98	98·8	98·3	52·9	53·5	58	14·1	14·6	15·3	74·5	81·1	76·9
1848.												
Jan.	98·1	98·9	98·8	54·5	56·8	59·3	14·1	14·9	15·2	74·1	80·5	76·6
Feb.	98	98·8	98·8	54·9	57	59·7	14·5	15·1	15·5	73·6	82·1	77·5
Mar.	98	98·9	98·9	54·9	57.6	59·7	14·3	15·5	15·4	75·1	81·9	78·2
Apr.	98	98·9	98·9	54·9	57·7	60·9	14·7	15·2	15·3	74·8	82·9	78·1
May	98·1	99	98·9	55·1	56·2	60·6	14·3	15	15·6	78·2	85·6	80·6
June	98	98·8	99	51	56·1	56·8	14·1	14·8	14·9	77·5	84·7	80·4
July	98	98·3	98·9	53	54·2	59·5	14	14·7	14·8	77·7	84·3	79·4
Aug.	98·1	99·1	99·1	55·4	56·3	59·5	13·9	15	14·7	78·4	85·3	80·9
Sept.	98·1	99·1	99·1	54·4	57·3	60·4	14	15	15·2	78·9	86·2	81·7
Oct.	98·2	99·1	99	54·3	55·8	61	14·2	15	15·3	78·9	84·8	80·8
Nov.	98·1	99·1	98·6	55·3	57·7	59·2	14·2	15·4	15·4	77	83·5	79
	98·07	98·9	99	54·4	56	60·3	14·4	15·4	15	76·7	83·6	79·8

These results, compared with those obtained in England, shew marked differences, as will best appear by presenting them together.

Mean temp. under the tongue in England.			Pulse.			Respirations.			Temp. of room.		
7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.	7-8 a.m.	3-4 p.m.	12 p.m.
Deg. 98·74	Deg. 98·52	Deg. 97·92	57·6	55·2	54·7	15·6	15·4	15·2	Deg. 50·9	Deg. 54·7	Deg. 62
In Barbados :—											
6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
Deg. 98·07	Deg. 98·9	Deg. 99	54·4	56	60·3	14·4	15·4	15	Deg. 76·7	Deg. 83·6	Deg. 79

The observations from which these mean results are deduced are so consistent, as may be seen by reference to them in detail in the Appendix, as hardly to admit of doubt in relation to their accuracy.

Probably the low morning temperature of the body within the tropics, as shewn above, may be owing principally to three circumstances,—to the depressing or lowering power of sleep,—to the light bed-covering used there, and the free circulation of the air in the room. A sheet and a thin coverlet were the only protection from the air; and the windows mostly being open, the outer air had free access, so that towards morning the temperature of the air of the room did not differ from that of the external air commonly more than two or three degrees. Moreover, the higher temperature observed at night, before going to rest, may have conduced to the lower morning temperature noticed on rising, as the lower temperature at night in England, with the opposite circumstances as to bed-clothing and air of sleeping-room, may have conduced to the higher morning temperature recorded there,—fluctuations these in accordance with well-known physiological laws.

2. *Of the Variation of Temperature during different seasons of the year.*

The following Table, formed from the first, exhibits the mean of the observed temperature during the several months comprised therein, as well as the mean of the in-door temperature.

	Mean temp. under the tongue.	Air of room.		Mean temp. under the tongue.	Air of room.
1845.	Deg.	Deg.	1847.	Deg.	Deg.
July	98·43	76·00	January	98·60	77·70
August	98·30	80·60	February	98·53	80·26
September	98·60	81·20	March	95·60	77·40
October	98·56	81·46	April	98·53	78·00
November	98·46	80·56	October	98·66	80·83
December	98·40	77·90	November	98·56	79·66
			December	98·36	77·36
1846.			1848.		
January	98·63	78·16	January	98·60	77·06
February	98·60	77·88	February	98·53	77·73
April	98·46	80·00	March	98·60	78·40
May	98·66	81·50	April	98·60	78·26
June	98·66	81·56	May	98·66	81·46
July	98·36	81·00	June	98·60	80·86
August	98·70	81·60	July	98·36	80·46
September	98·40	81·36	August	98·76	81·56
October	98·73	81·50	September	98·76	82·26
November	98·60	80·60	October	98·76	81·50
December	98·63	77·96	November	98·60	79·83
				98·54	79 75

Comparing the temperature under the tongue with that of the air of the room, it will be seen that there is some accordance between them, though not a strict one, as if other circumstances than differences of atmospheric temperature have a marked effect in modifying the temperature of the body within the tropics, where the variations of atmospheric temperature throughout the year are so inconsiderable—circumstances, for instance, of state of health, kind of life led as to exercise, diet, etc., and quality of atmosphere, as to degree of moisture or dryness, and the direction and force of the winds.

3. *Of the Effect of Active Exercise on the Temperature and of Rest after Exercise.*

On most occasions, when active exercise was taken, especially when on military duty, the dress worn was heavier and warmer than that in common use, to which partly the increase of temperature observed may be justly referred. This remark applies to the majority of the following observations :—

	Tongue.	Pulse.	Respirations.	Air of room.
	Deg.			Deg.
December 17, 1846, 1 p.m. After attending an inspection of a regiment on parade, exposed to the sun about half an hour	99·3	66	17	82
December 28, 1846, 3 p.m. After returning from office in Barbados, with some difficulty in carriage, the latter part of the road being flooded, a torrent rushing over it, 4·39 inches of rain having fallen between 9 a.m. and 3½ p.m.	99	56	17	75
December 28, 1846, 4 p.m. The last hour sitting lightly clad	98·1	52	16	77
January 5, 1847, 1 p.m. After visiting hospitals	99·3	54	16	82
January 5, 1847, 4 p.m. The last hour sitting lightly clad, reading	98·0	50	14	78
January 6, 1847, 1 p.m. After inspecting an hospital	99·2	54	15	82
January 6, 1847, 4 p.m. Since 1 p.m. chiefly reading, lightly clad	98·2	50	14	81
March 26, 1847, 4 p.m. After inspecting hospitals, etc., occupied several hours, the greater part of the time walking	99·8	70	16	85
March 29, 1847, 6 a.m. After rising, before any exertion	98·4	56	15	68
March 29, 1847, 9 a.m. After a walk of three hours, ascending and descending a hill in the neighbourhood of Port of Spain, Trinidad; perspiring	99·1	68	17	79
March 29, 1847, 7½ p.m. After a ride of about four hours, most of the way at a quick pace; perspiring	99·6	78	17	80
April 3, 1847, 6 a.m. Just risen	98·2	54	14	73
April 3, 1847, 1 p.m. At the foot of the fall of the Marraca, in Trinidad, approached by a steep ascent through forest, an hour's walk, preceded by a ride at a quick pace of several miles; perspiring, temperature of hand 99·5°; that of the air (81° at the fall) was comparatively low from the evaporation of copious spray	100	110	20	81
April 3, 1847, 6¼ p.m. On return from Marraca; the last few miles in an open carriage	98·7	68	16	79
April 10, 1848, 3½ p.m. After inspecting the hospital at St. Vincent, and riding an hour or two	99·4	62	16	85
April 11, 1848, 3½ p.m. After visiting the barracks; perspiring	99·4	78	17	84
July 15, 1848, 1 p.m. After a ride of about an hour from Courland Bay, Tobago	99·6	64	16	83
July 15, 1848, 6½ p.m. After visiting barracks, etc.	99·7	64	15	81

These results (many more of the like kind might be given) shew how readily, when in health, the temperature of the body rises from active exercise and subsides on rest,—both, very much in the same manner and degree as in a cooler climate; and also, unless the exercise be severe, how the temperature is proportionally more affected than either the pulse or respiration,

4. *Of the Effect of Carriage Exercise on the Temperature.*

The carriage used was a light one adapted to the climate, open behind as well as in front, and well protected from the sun, except when low, by a deep hood: the rate of going was commonly between six and seven miles an hour; a servant drove.

	Tongue.	Pulse.	Respirations.	Air of room.
	Deg.			Deg.
January 28, 1846, 9 p.m. After coming from Mount Wilton, about 900 feet above the level of the sea	97·9	70	15	73
August 27, 1846, 5 p.m. After a drive of about sixteen miles, going to and returning from Villa Nova, in Barbados, in a cool situation about 700 feet above the level of the sea	97·8	48	14	80
September 3, 1846, 11 p.m. After coming from Villa Nova	97·7	58	16	79
November 18, 1846, 5 p.m. After a drive of about twenty-four miles in the heat of the day, going and returning, and some walking exercise in a low district of Barbados, fasting	98·3	60	16	82
November 18, 1846, 10 p.m. Feeling weary, after dinner, followed by tea	99·1	58	15	79
November 21, 1846, 11½ p.m. After coming from Villa Nova	97·7	58	16	79
December 18, 1846, 5 p.m. After coming from Blackman's, situated like Villa Nova, and about the same distance; took a good deal of walking exercise there; the wind high	98·6	70	15	80
December 18, 1846, 9 p.m. Feeling weary, hot and thirsty	99·7	80	16	79
February 4, 1847, 12 p.m. After coming from Villa Nova	97·5	58	15	74
April 25, 1847, 10½ p.m. After coming from Villa Nova	97·1	54	14	77
June 14, 1848, 10 p.m. After coming from Villa Nova	98·2	58	15	82

Many other instances, of which I have notes, might be adduced to shew the effects of carriage exercise in lowering the temperature,—an effect previously observed in England.* Those given, which were chiefly at night, were best marked, no doubt owing to the temperature of the air then being lower than by day, and the heat from the reflected and direct rays of the sun being then entirely avoided. When coming from the higher grounds of the interior of Barbados at night, and often when ascending them by day, the feeling of coolness was such as to render agreeable some additional clothing, which was always

* The effect of railway exercise, so far as I have had an opportunity of observing, has been different, that of an exciting kind, so far as the heart's action is concerned—most in second class carriages, least in first class: I have commonly found, indeed I may say, I have always found, the pulse accelerated;—but, in my own case, without any appreciable residual bad effect.

provided, though the difference of atmospheric temperature was only of a very few degrees. The latter observations, those of the 18th of November, 1846, and of the 18th of December, are given as shewing the tendency to augmentation of temperature of body in an undue degree after unusual depression, preceded by fatiguing exercise. The rise, so far as it was abnormal, was probably owing to the influence of the fatigue, for it was not witnessed if such exercise had not been taken.

5. *Of the Effect of Gentle Walking Exercise on the Temperature.*

The following observations were made when convalescent from illness (an anthrax with extensive sloughing and general derangement of health), a time when the body in a delicate state seems to be very readily acted on and to shew the effects with unusual distinctness :—

	Tongue.	Pulse.	Respirations.	Air of room.
	Deg.			Deg.
March 7, 1847, 6¼ a.m. Just risen	97·8	52	14	74
March 7, 1847, 3 p.m. During the last two hours taking gentle exercise within doors, making some chemical experiments	98·3	56	15	82
August 17, 1847, 5¾ a.m. Just risen	97·8	62	15	77
August 17, 1847, 7¾ a.m. During last half hour walking slowly in shaded gallery, exposed to the wind; feelings agreeable; feet and hands warm	97·8	68	16	80
August 19, 1847, 5¾ a.m. Just risen	98·6	62	15	79
August 19, 1847, 8 a.m. Last half hour walking in gallery; not exposed to the wind; perspiring gently	98·4	70	17	81
August 21, 1847, 5¾ a.m. Just risen	98·4	60	15	78
August 21, 1847, 7½ a.m. After walking about half an hour; hands and feet glowingly warm	98·2	76	17	81
August 22, 1847, 5¾ a.m. Just risen	98·5	62	16	79
August 22, 1847, 7¾ a.m. After walking in gallery about half an hour, very lightly clad, exposed to the wind; hands and feet very warm	98·2	78	17	82

To appreciate the effect of gentle exercise on the temperature, it requires to be mentioned and kept in mind, that had not such exercise been taken, the thermometer would have risen on its second application from ·3° to ·5° higher than at the first.

The feeling of increased warmth in the hands and feet from gentle exercise has been noticed in three instances. The diffusion of heat to the extremities, owing no doubt to a freer circulation of the warming medium—the blood—may partly account for the cooling effect under the tongue, and probably in the deep-seated parts from the exercise under consideration. The high temperature commonly observed in the extremities within the tropics, is a circumstance very deserving of note. In the daily observations appended, a record is given of the temperature of the hand during two months, noticed three times a day; from which it appears that on an average it was less than that under the tongue by only 1°. For the purpose of ascertaining the heat of the hand, the bulb of the thermometer was placed between the middle of the palm and ball of the thumb, and so covered was gently pressed.

6. *Of the Effect of Change of the Temperature of the Atmosphere on the Temperature.*

In illustration of this effect, I shall notice only a very few of the observations obtained in the West Indies, corroborative of those collected many years ago in the island of Ceylon.

	Tongue.	Pulse.	Respirations.	Air of room.
	Deg.			Deg.
November 15, 1847, 6 a.m. Just risen, at Villa Nova......	98	56	15	76
November 15, 1847, 2 p.m.	98·7	50	15	76
November 15, 1847, 9½ p.m..........	97	58	16	76
November 16, 1847, 6 a.m.	98·4	54	15	76
November 16, 1847, 1½ p.m..........	98·5	50	16	78
November 16, 1847, 9½ p.m. Windows closed; feet and hands agreeably warm..........	97·8	58	16	76
November 17, 1847, 6¼ a.m..........	98·3	54	14	75
November 17, 1847, 9.25 a.m. At home, in the neighbourhood of Bridgetown..........	98·8	54	16	77
November 22, 1847, 1½ p.m. At home, variously occupied	99·2	62	16	79
November 22, 1847, 4 p.m. Last hour sitting reading; raining; a feeling of cold; hand 85°	97·9	48	14	75
December 24, 1847, 6 a.m. At home; just risen..........	98·1	52	14	74
December 24, 1847, 1.3 p.m. At home; variously employed	99·1	58	16	80
December 24, 1847, 9½ p.m. Villa Nova; arrived there 5 p.m.	97·8	56	14	74
December 25, 1847, 6¼ a.m. Villa Nova; just risen; slept warm; windows closed	98·9	60	14	73
December 25, 1847, 12 a.m. Agreeably cool..........	98·5	54	14	76
December 25, 1847, 5 p.m. A feeling almost of cold	98·5	52	14	74
December 25, 1847, 9½ p.m..........	97·85	52	15	74
December 26, 1847, 6¼ a.m..........	98·5	60	15	73
December 26, 1847, 9¼ p.m. At home; left Villa Nova at noon	98·7	64	16	80

The situation of Villa Nova has already been mentioned: it was there that most of them were made, that spot being well-fitted for trials of the kind, the temperature there by day rarely exceeding 80°, even at the hottest time, and always below 80° at night, often requiring the closure of the bed-room windows, and occasionally a blanket in addition to the bed-clothes.

The observations made at Villa Nova, if compared with those made in England, will be found to approximate, especially as regards the morning and night temperature, and may be adduced in confirmation of the explanation given under Section 1, regarding the contrast of observed temperatures there adverted to.

7. *Of the Effect of Excited and Sustained Attention on the Temperature.*

A few instances may suffice to illustrate this effect, selecting those in which it was most strongly marked; between which and those least notable there is a fine gradation, according to degree of exertion of mind, which it would be difficult to appreciate on account of interfering disturbing circumstances.

	Tongue.	Pulse.	Respirations.	Air of room.
	Deg.			Deg.
December 22, 1846, 3½ p.m. After delivering a discourse in public, in a well-aired room, lasting about an hour...	99·8	78	16	82
December 22, 1846, 5.20 p.m.	98·9	58	15	80
December 22, 1847, 6 a.m. Just risen	98·1	52	13	75
December 22, 1847, 3¼ p.m. After delivering a discourse in the same place as the preceding, and of about the same length	100·3	90	15	82
December 22, 1847, 9½ p.m.	98·9	66	15	77
December 23, 1847, 6 a.m.	97·9	54	14	75
June 24, 1848, 3½ p.m. After occupation similar to that last mentioned, and in the same place	99·9	82	15	81
August 30, 1848, 2 p.m. After delivering a chemical lecture in a close and crowded room	100·1	84	15	88
August 30, 1848, 9 p.m.	99·4	62	14	82
August 31, 1848, 6 a.m.	98·2	58	14	80
September 6, 1848, 2 p.m. Similarly occupied; the heat of the room greater; immediately after the pulse was 100	100·4	80	16	88
September 6, 1848, 9 p.m.	99·2	64	15	81
September 7, 1848, 5¾ a.m.	98·2	54	14	78

The additional observations—those made some hours after the exertion—are given as illustrating an important fact, viz. the rapid manner in which, when the body is free from disease, the functions on rest return to their ordinary state; whilst, on the contrary, when disease is present, especially of an organic kind, even though latent in relation to the most obvious class of symptoms, indications of it may be obtained by attention to the temperature, pulse and respiration; and often, as in instances of pulmonary disease, in a very remarkable and decided manner.

8. *On the Effect of Cool and Well-ventilated Rooms, and of Close and Heated Rooms on the Temperature.*

As regards ventilation, and consequently coolness where there is a concourse of people, the cathedral church of Barbados, and Harrison's school-room adjoining, in which lectures were given on the opening of the "Reid School of Practical Chemistry," may be mentioned as good and bad examples. In illustration of the effects of each, I shall give a few observations on temperature, noted down when made, which was within about half an hour after quitting the church and the school, conveyed in a carriage, attending in the one divine service, in the other a lecture. The temperature of the air marked, as usual, was that of my sitting-room, which was cooler by several degrees than the school-room, but less cool commonly than the church.

	Tongue.	Pulse.	Respirations.	Air of room.
	Deg.			Deg.
February 28, 1847, 1 p.m. Just come from church	98·4	54	16	83
March 7, 1847, 1 p.m. Just come from church	98·5	54	15	84
March 5, 1848, 2¾ p.m. Just come from church	98·5	56	15	82
August 2, 1848, 2½ p.m. Just come from school-room	99·8	66	13	85
August 9, 1848, 2 p.m. Just come from school-room	99·8	60	16	82
August 16, 1848, 2½ p.m. Just come from school-room......	99·9	64	16	87
October 18, 1848, 3½ p.m. Room less crowded	99·5	58	15	82
October 25, 1848, 5¼ p.m. Room less crowded	99·4	62	15	83

9. *On the Effect of taking Food and Wine on the Temperature.*

The effect of a meal in moderation, whether the light one of breakfast or the fuller and heavier one of dinner, was to raise the temperature; and this also when wine was sparingly used at the latter, viz. to the extent of two or three glasses; but when more freely drunk, as it commonly is in company, then often its influence on temperature appeared to be depressing. I shall give a few instances in illustration :—

	Tongue.	Pulse.	Respirations.	Air of room.
	Deg.			Deg.
December 21, 1846, 10 p.m. Dinner at 6; wine chiefly champagne and Madeira; no headache; no malaise......	98·1	70	15	77
February 6, 1847, 10 p.m. Dinner at 7; the wines similar	97·8	70	15	71
February 13, 1848, 11½ p.m. Dinner at 6; about seven miles distant in the country, from whence just returned; kind of wine not noticed	97·3	64	16	78
March 9, 1848, 10½ p.m. Dinner at 7; kind of wine not noted down	97·9	64	15	77
May 29, 1848, 12 p.m. Dinner at 7; wines chiefly champagne and claret	98	72	16	79
August 9, 1848, 11 p.m. Dinner at 7; the wines like the last	98	70	15	79

It is deserving of remark, that whenever wine was used, except in great moderation, though never to the excess of an inebriating effect, on the following morning the temperature under the tongue was found to be more or less above the average, and the pulse commonly quicker than usual. It is also worthy of remark, that occasionally the effect at night was to increase the temperature of the body, and that in a marked manner; but whether from some peculiar quality of the wine used, or from some deranged state of the system or other adventitious circumstance, I have not been able to determine.*

10. *Of the Effect of Sea-sickness on the Temperature.*

During a voyage from Barbados to St. Christopher, and from thence to Barbados in May and June, 1848, in a transport, stopping at some of the intermediate islands, an opportunity offered of making some observations on the effect of sea-sickness, from which I suffered more or less on the several days noticed in

* These results seem to me rather to favor the inference that wine in great moderation may act like fat and starch, be consumed in respiration, and so administer to animal heat; and in excess *vice versâ*,—and, as is well ascertained, diminish the amount of carbonic acid expired. Because alcohol in any excess, or in its pure state, if not in excess, be exhaled in part by the lungs, it does not follow that the same happens to wine,—and that it no wise acts as a sustaining drink,—and by its consumption supports the animal temperature.

the subjoined Table. The degree of sickness, often amounting to vomiting, was such as to render rising disagreeable. Little food was taken on those days, except chicken broth with some bread; no wine was drunk. The observations were made in the sitting posture in bed in a well-ventilated cabin.

	Temp. under the tongue.			Pulse.			Respirations.			Temp. of cabin.		
	6-7 a.m.	12-2 p.m.	6-10 p.m.	6-7 a.m.	12-2 p.m.	6-10 p.m.	6-7 a.m.	12-2 p.m.	6-10 p.m.	6-7 a.m.	12-2 p.m.	6-10 p.m.
May	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
14	98·7	98·9	...	58	68	...	16	16	...	81	83	...
15	98·8	...	...	60	...	...	14	...	...	80	...	...
18	98·2	98·6	...	54	64	...	14	15	...	80	81	...
26	98·7	...	...	60	...	...	15	...	...	79	...	...
27	98·8	...	...	60	...	...	15	...	...	79	...	...
31	...	98·7	99·1	...	58	58	...	15	14	...	82	82
June												
1	98·6	98·8	99·3	58	56	58	14	15	15	81	83	83
2	98·7	98·3	...	56	58	...	14	14	...	81	80	...
3	98·7	99·2	99·4	58	56	60	15	16	15	80	82	82
4	98·7	...	...	58	...	...	14	...	...	80	...	...
	98·65	98·75	99·23	57·55	60·28	58·66	14·44	15·14	14·66	80·24	81·71	82·33

It appears from these few observations, that under the influence of sea-sickness, the morning temperature was higher than ordinary and the pulse somewhat quicker; when the former was lowest, as on the 18th of May, then the vessel being in smooth water, there was scarcely any uneasiness experienced. Comparing the several observations, perhaps the inference may be justifiable, that the tendency of sea-sickness, when not in its severest form, is either of a slightly lowering, or of an equalizing kind in relation to the temperature, pulse and respiration,—a tendency no doubt promoted by the little variation to which the sea atmosphere is liable, especially within the tropics. On so obscure a subject, however, as sea-sickness, this remark is offered with some hesitation.

11. *Of the Temperature at Sea, when not under the influence of Sea-sickness.*

In returning from the West Indies in the well-appointed steam-packet Clyde, I availed myself of the opportunity to continue the observations on temperature. The results are given in the following Table, commencing on the day after leaving St. Thomas, when all feeling of sea-sickness had ceased, and ending on the 2nd of December, the day before coming in sight of the coast of England. The weather during the greater part of the voyage was favourable : after leaving Fayal, on the 27th of November, it was more or less tempestuous; on the 29th and 30th, it was necessary to have the cabin ports closed. The observations were made in a well-ventilated cabin in the sitting posture,—the port commonly open excepting at night. The diet was fuller and more nourishing than that used in the West Indies, the appetite increasing on passing into a cooler climate; rather more wine was used, viz. a pint of sound Bordeaux at dinner; and the clothing was rendered warmer as required by diminution of warmth of atmosphere :—

	Position of ship at noon.		Temp. under the tongue.			Pulse.			Respirations.			Temp. of cabin.		
	Lat. North.	Long. West.	6-7 a.m.	12-2 p.m.	10 p.m.	6-7 a.m.	12-2 p.m.	10 p.m.	6-7 a.m.	12-2 p.m.	10 p.m.	6-7 a.m.	12-2 p.m.	10 p.m.
Nov.	dg. mn.	dg. mn.	deg.	deg.	deg.							deg.	deg.	deg.
16	22 39	61 8	98·5	98·8	98·9	54	50	60	14	15	16	78	81	78
17	24 16	58 11	98·2	98·3	98·4	56	54	66	14	16	15	76	77	77
18	25 39	55 30	98·2	98·7	98·2	56	52	60	14	16	15	75	77	77
19	27 33	53 13	98·6	99·3	...	52	72	...	14	15	...	77	79	...
20	29 19	50 45	98·6	98·1	97·7	60	56	60	14	15	15	77	77	77
21	31 6	48 9	98·5	97·9	97·8	56	50	62	14	15	15	75	75	73
22	32 53	45 16	98·7	97·7	97·7	54	54	60	14	14	15	71	73	72
23	34 43	41 18	98·2	98	98·1	54	56	58	15	15	15	71	71	72
24	36 23	37 21	98·5	98·1	98·6	58	54	60	14	15	15	69	67	67
25	37 21	33 34	98·3	98·5	...	52	56	...	14	14	...	64	66	...
26	38 28	30 2	98·5	98·7	...	56	56	...	14	14	...	66	69	...
27	38 32	28 40	98·3	98·5	97·5	54	54	54	14	15	15	68	67	66
28	41 2	25 18	98·4	98·7	98·3	52	58	60	14	16	15	66	61	65
29	43 19	21 32	98·6	99·4	98	56	62	58	14	15	15	65	63	68
30	44 59	17 13	98·1	98·9	97·3	54	56	54	14	15	14	61	59	64
Dec.														
1	46 33	12 7	98·4	98·9	97·9	54	60	56	14	14	14	59	59	63
2	48 33	6 44	98·7	98	97·6	58	62	48	14	15	...	58	57	59

Comparing these results with those obtained in England and in the West Indies, they will be found to accord more with the former than the latter, the morning being on an average higher than those obtained at night. The small range of temperature is also worthy of note, and its decrease with diminution of atmospheric temperature. In those instances in which the variation was greatest, the higher temperature noted down, was in the middle of the day, commonly in connection with active walking exercise just before taken,—and the lower than ordinary at the same period of the day, was, after sitting for an hour or two exposed to the wind on deck.

Conclusions.

The following are some of the conclusions that appear to be either proved or rendered probable by the preceding results, or by those given in the Tables appended,—supposing, as it is believed, that, were the observations extended to many individuals, no material discrepancy would be witnessed.

1. That the average temperature of man within the tropics is a little higher, nearly 1°, than in a temperate climate, such as that of England.

2. That within the tropics, as had before been found in cooler regions, the temperature of the body is almost constantly fluctuating,—varying according to the variety of agencies to which it is subject, some of which are distinct, others obscure.

3. That the order of fluctuation observed there is different from that in a cooler climate, the minimum degree of temperature being commonly early in the morning, after the night's rest, and not at night previous to going to rest.

4. That all exertion, whether of body or mind, except it be very gentle, coming under the designation of passive rather than active exercise, has a heightening effect on the temperature, while the latter, the passive kind, has rather a lowering tendency, especially carriage exercise.

5. That heavy clothing, especially if tight and close, obstructing the admission and circulation of air, tends to raise the temperature unduly, especially under active exercise; and that close, ill-ventilated rooms, especially when crowded, have in a marked manner the same tendency.

6. That when the body is in a healthy state, then on rest after exercise or exposure to any other exciting cause, it rapidly recovers its normal condition as to temperature.

7. That when labouring under disease, however slight, the temperature is abnormally elevated; and that—judging from observations made, but not recorded, in the Tables—its undue degree is some criterion of the intensity of the diseased action,

8. That within the tropics there is comparatively little difference of temperature between the surface of the body, especially the extremities and the internal parts;—and that there the skin is more active in its function of transpiration and the kidneys are less active as secreting organs; with which it may be conjectured is connected a rapid production and desquamation of cuticle, and the absence, in great part or entirely, of lithic acid in the urinary secretion. This latter fact, however, may be explained in a different manner, on the supposition that the acid is not formed in the blood, or if formed, in a greatly diminished quantity.

9. That the effect of wine, unless used in great moderation, is commonly lowering, that is as to temperature, whilst it accelerates the heart's action, followed after a while by an increase of temperature.

10. That the tendency of sea-sickness is to check what may be considered the natural fluctuation of the temperature, and when severe, like disease, to elevate the temperature.

11. That the tendency of a sea voyage, apart from sea-sickness, is to equalize the temperature without elevating it, an equalization that is best witnessed in voyaging in a tropical sea, where the atmospheric temperature is so little variable.

12. That even at sea, with a change of atmospheric temperature, there is a tendency to change of temperature of the body, the average increasing in proceeding towards the tropics, and diminishing in receding from them.

These conclusions obviously admit of application, and that variously in relation to health and disease. It would be unsuitable to the occasion to dwell on this part of the subject; I shall merely remark, that it is a happy circumstance for man, and seems wisely ordered, that fluctuation of temperature should be connected with a healthy state of the system, and probably conducive to it, in whatever manner produced, whether by change of climate, or atmospheric variation, or by exercise, whether of body or mind. The excellent health which the crews of the West Indian steam-packets have, that are in constant transition from heat to cold, is a striking proof of this, and other instances of a like kind, were it necessary, might be adduced in confirmation.

The thermometer used in making the observations contained in these Tables, was compared with a standard one, and was found to require no correction in its scale for the degrees of temperature indicated when placed under the tongue.

	Temp. under the tongue and in hand.						Pulse.			Respirations.			Temp. of air of room.		
	6-7 a.m.		12-2 p.m.		9-11 p.m.		6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
	Tongue.	Hand.	Tongue.	Hand.	Tongue.	Hand.									
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
July, 1845. 12	98·7	97·5	99·5	98	98·25	97·5	64	66	62	15	15	16	80	84	82
13	98·35	97·5	99·1	98	98·5	98	58	54	64	17	15	16	79	78	80
14	98·4	97·5	99·1	98	98·95	98	58	60	70	15	16	17	77	84	80
15	98·7	98	99·3	98·5	99·2	98	60	76	76	15	16	16	78	83	80
18	98·4	97·25	99·3	98	99·5	98·5	60	64	70	15	16	17	78	83	77
19	98·4	97·5	99·2	98	99	97·5	62	62	60	16	16	16	77	83	79
20	98·1	97·25	98·6	97·5	98·7	97·25	56	52	52	15	15	16	77	74	79
21	98·6	97·5	99	97·5	98·8	97·5	54	64	54	15	15	16	78	83	80
22	97·5	96	98·4	97·5	98·3	97	56	56	60	15	16	16	78	81	81
23	97·8	97	98·9	97·5	...	...	54	60	...	16	16	...	79	84	...
24	97·9	96·25	...	...	98·4	97	54	...	54	15	...	16	79	...	79
25	97·8	97	98·8	97·5	98·3	97·5	50	62	62	15	16	16	78	85	80
26	98 1	97·5	99·1	98	98·7	97·25	62	60	54	14	15	16	78	85	81
27	98·2	97	98·6	97	98·1	96·5	52	54	54	15	15	15	80	86	79
28	98·2	96·5	98·7	97·25	98·4	97·5	54	54	64	15	15	16	78	84	80
29	98·2	96·75	...	...	98·7	97·5	50	...	64	14	...	16	78	...	80
30	98·1	96·5	98·9	97·5	98·4	97·5	50	62	50	14	16	15	78	85	79
31	98·2	97	99·1	97·7	98·3	...	56	61	61	15	16	16	79	85	80
	98·2	97·1	99	97·7	98·2	97·35	56	61	66·5	14·7	15·5	16	78·3	81·5	79·4
August, 1845. 4	98·4	97	99	97·5	99	97·5	56	64	60	15	16	16	79	83	82
5	98	96·5	98·7	97·25	98	96·5	52	52	52	15	15	15	78	85	78
6	97·7	97	99·1	97·5	98	97	54	54	60	14	16	16	78	84	79
7	98·25	97·5	98·7	97·7	98·7	98	60	54	54	16	16	15	78	85	82
8	97·7	96·7	98·3	96·5	98·3	97·5	50	52	60	15	16	16	78	85	81
9	98·2	97	98·5	97	98·6	97·5	52	54	62	15	16	16	79	84	80
10	93·9	97	98·7	97·5	97·8	96·7	52	58	64	16	16	16	79	85	80
11	98·5	97·5	98·8	97	98·5	97·25	60	52	56	16	15	16	80	85	80
12	97·8	97	98·9	98	98·9	98	54	54	62	15	15	16	79	87	81
13	97·9	97	98·8	97·5	98·3	97·5	54	52	58	14	15	16	78	83	80
14	97·7	96·5	99	97·5	98·7	97·5	50	54	56	15	16	16	78	88	80
15	97·8	97	98·7	97·75	98·7	97	52	52	54	14	15	16	79	85	81
16	97·9	96·5	98·7	97·5	98·7	97·5	52	54	54	15	16	15	79	86	81
17	97·9	97	98·4	97·25	98·2	97·3	54	50	62	15	16	15	79	82	80
18	98·1	97	98·7	97	98·7	97·5	54	50	60	14	15	16	77	87	82
19	98·3	97	98·4	97·5	98·5	97	54	50	60	14	15	15	78	87	82
20	98·3	97·5	98·7	97·5	98·8	97·5	52	56	56	15	16	15	78	77	78
21	98·2	97	98·5	97	98·4	97·5	56	54	58	15	15	16	78	82	78
22	98	97	98·7	97·7	98·6	97·7	52	54	54	15	16	15	78	83	80
23	98	97·7	99	98·5	98·5	97·7	52	54	54	15	16	15	78	85	81
24	98·2	97·7	99	98	98·6	97·5	52	50	54	15	15	15	76	84	81
25	98·1	97	98·7	97·5	98·7	97·5	54	56	56	16	15	16	78	84	77
27	97·9	97	98 8	97·5	98·8	97·5	54	52	62	15	16	16	78	86	80
28	98·1	97	...	...	...	...	60	...	...	16	...	...	78	...	...
30	98·2	...	99	...	98·9	...	54	58	60	15	16	15	79	86	81
31	97·9	...	98·6	...	99·2	...	54	50	60	15	15	16	80	86	82
	98	97	98·7	97·4	98·16	97·4	53·8	53·6	57·9	15	15·7	15·8	78·7	84	80·

Month	Day	Temperature under the tongue. 6-7 a.m.	12-2 p.m.	9-11 p.m.	Pulse. 6-7 a.m.	12-2 p.m.	9-11 p.m.	Respirations. 6-7 a.m.	12-2 p.m.	9-11 p.m.	Temperature of room. 6-7 a.m.	12-2 p.m.	9-11 p.m.
		Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
September, 1845.	1	98	98·9	99·3	54	52	66	15	16	15	78	86	81
	2	98·3	99·3	99·9	60	56	70	14	15	15	79	86	81
	3	98·1	98·6	99·3	54	50	58	14	16	15	78	82	80
	4	98·1	99·1	99·1	60	56	66	15	15	16	79	85	80
	5	98	98·9	98·9	52	54	60	15	16	14	78	85	79
	6	98	98·3	99·1	46	50	66	14	15	16	79	80	81
	7	97·95	98·7	99	50	48	64	15	16	16	77	84	80
	8	98·2	99·1	98·6	54	50	60	15	15	16	77	86	80
	9	97·9	99·1	99	52	56	62	15	16	16	78	83	80
	10	98·1	98·9	98·5	54	56	70	15	16	16	78	85	80
	11	98·6	99	98·4	70	54	52	15	15	16	78	80	80
	12	97·8	98·9	98·6	52	52	60	15	15	15	78	85	78
	13	98·1	99·2	99	52	56	62	15	16	16	78	82	80
	14	97·9	98·6	98·9	52	52	62	15	16	15	78	84	80
	15	98·1	99	98·7	52	60	54	15	16	16	78	85	82
	16	98	98·5	98·6	60	60	60	16	16	15	78	85	82
	17	98·1	98·9	98·8	60	56	58	16	16	15	78	85	81
	18	98·2	99	99·3	56	60	70	15	15	16	79	87	82
	19	98·3	99	99·2	56	56	62	14	15	15	79	86	82
	20	98·4	99·5	99·3	54	58	60	15	16	16	77	86	77
	21	98·5	98·8	98·9	52	56	58	14	15	15	80	85	82
	22	98·2	99·4	99·4	52	62	76	15	16	15	79	84	81
	23	98·3	99·4	99	56	60	68	16	16	16	78	85	81
	24	98	98·8	98·3	52	52	51	15	16	15	80	86	80
	26	98·1	98·4	99	52	52	62	15	16	16	77	85	81
	27	97·9	98 5	98·7	54	58	62	15	16	16	78	85	82
	28	97·7	98·5	98·7	52	54	58	15	15	16	80	87	81
	29	97·9	98·9	98·9	52	52	62	15	15	15	78	86	80
	30	97·9	99·3	99·3	54	58	62	15	15	16	78	86	82
		98	98·9	98·9	54·3	55	62·1	15·2	15·6	15·5	78·6	84·7	80·5
October, 1845.	1	97·6	99·1	98·9	54	54	58	15	14	16	78	86	82
	2	97·9	99.3	99	54	62	60	15	15	15	78	86	81
	3	98	99	99	52	54	66	15	16	16	78	87	82
	4	97·9	98·8	98·7	51	56	62	14	16	16	78	86	80
	5	98·1	98·6	98·4	56	56	54	14	16	15	79	85	80
	6	97·9	99·1	98·7	56	66	60	14	15	15	79	84	84
	7	98·2	...	98·7	52	...	64	14	...	16	80	...	82
	8	98·5	98·6	98·8	52	56	56	15	15	16	78	86	82
	9	97·8	98·6	98·4	53	54	60	15	16	16	77	86	82
	10	98·2	...	98·9	54	...	68	15	...	16	78	...	82
	11	98	98·7	98·9	52	58	56	14	16	16	78	87	82
	12	97·9	98·7	...	52	52	...	15	16	...	78	86	...
	13	98	98·7	98·8	53	56	62	15	16	16	78	87	82
	14	98	99·1	98·8	52	56	62	14	15	16	77	87	81
	15	98·1	99	98·9	54	58	56	15	16	16	78	85	80
	16	98·2	98·8	98·9	54	62	62	15	17	15	79	86	81
	17	98·2	99·6	99·4	56	58	64	15	16	16	80	87	83

		Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
		6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
		Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
October, 1845.	18	97·9	98·9	99	52	60	76	14	16	17	80	82	77
	19	98	99·4	98·8	64	66	56	16	16	15	78	83	79
	20	98	99·7	98·6	56	62	70	15	16	16	77	84	79
	21	98	99·5	99	54	60	58	15	17	16	77	84	80
	22	97·9	98·9	98·8	50	60	60	14	17	17	77	86	82
	23	98·2	99·1	98·9	56	60	60	15	16	16	79	87	82
	24	98·1	99	99·4	58	60	62	14	16	16	79	87	82
	25	97·9	98·9	99·3	56	54	60	15	16	16	78	86	82
	26	97·9	98.7	98·9	50	60	52	15	16	15	78	84	80
	27	97·9	99·1	99·1	53	60	60	15	16	15	80	84	80
	28	97·7	...	...	54	...	...	15	...	...	78	...	...
	29	...	98·7	99	...	54	56	...	16	16	...	86	81
	30	97·8	98 7	99	52	58	62	15	17	16	78	85	81
	31	97·8	98·6	98·7	52	56	60	15	16	16	78	83	81
		97·9	99	98·8	53·4	56·3	61·3	14·6	15·9	16·8	78	85·4	81
November, 1845.	1	97·8	98·7	99	54	56	60	15	16	16	78	85	81
	2	97·8	98·6	98·8	52	56	66	15	16	16	78	85	81
	3	97·9	98·8	99	52	60	66	15	17	16	79	86	82
	4	97·8	98·9	98·9	53	58	62	15	16	16	78	86	81
	5	97·9	99·1	99·1	54	56	64	15	16	16	80	85	82
	6	98	98·7	98·6	53	56	60	15	16	15	77	83	80
	7	98	98·7	98·7	54	56	60	15	15	16	78	80	80
	8	98·1	99·4	99	52	58	68	15	16	15	78	84	81
	9	98	98·6	98·5	52	54	56	15	15	15	77	83	80
	10	98·1	98·6	98·8	56	56	64	15	15	16	78	86	81
	11	98·2	98·8	98·8	56	56	64	14	16	16	79	86	81
	12	97·8	98·2	98·5	54	52	62	14	15	16	79	79	78
	13	98·2	98·8	98·5	54	56	58	15	16	16	79	83	81
	14	98	99·2	98·6	50	60	54	14	15	15	78	82	80
	15	97·9	98·6	98·2	54	54	58	15	15	16	78	81	79
	16	98	98·5	98·4	56	56	60	15	15	15	78	84	78
	17	98	98·5	98·9	52	56	62	15	15	15	78	81	79
	18	98	98·5	98·7	50	54	58	15	16	16	78	82	79
	19	98·1	98·9	98·6	54	60	60	15	16	15	77	84	80
	20	98	99	98·6	54	56	58	15	15	16	77	84	80
	21	97·9	98·3	98·9	54	54	60	14	16	16	77	84	80
	22	98	99·2	98·3	54	62	60	14	16	16	79	85	79
	23	98·5	98·8	98·5	54	56	60	15	15	16	78	85	77
	24	97·9	99	99·2	50	58	66	14	16	17	77	83	81
	25	98·4	99·1	98·6	54	58	58	16	17	16	78	85	78
	26	97·8	98·3	99	52	54	56	15	16	15	77	83	80
	27	98·5	98·6	99	52	54	62	14	16	16	78	83	80
	28	98·2	99·2	98 2	56	60	70	15	16	16	78	81	78
		98	98·6	98·7	53·3	56·5	61	14·8	15·6	16	78	83·8	79·9

		Temperature under the tougue.			Pulse.			Respirations.			Temperature of room.		
		6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
		Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
December, 1845.	1	97·9	98·6	98·4	48	54	58	15	15	16	78	80	77
	2	97·9	98·4	98·5	52	52	60	14	15	16	77	82	79
	3	97·8	98·9	98·8	50	56	56	14	16	16	77	81	77
	4	98·1	98·7	98·5	54	56	60	14	16	16	77	79	79
	5	97·9	98·4	98·5	52	56	70	15	16	17	77	81	79
	6	98·1	98·7	98·4	52	56	60	14	15	16	77	81	79
	7	97·9	98·3	98·2	50	52	76	15	16	17	77	82	77
	8	98	98·5	...	64	60	...	14	15	...	75	82	...
	9	98·4	98·5	98·9	52	56	56	14	15	16	75	83	77
	10	97·9	98·4	98·8	54	52	60	14	15	15	76	82	78
	11	97·9	98·9	98·4	56	56	62	14	16	15	75	83	79
	12	97·8	...	...	56	...	...	14	...	...	77	...	...
	13	97·9	98·4	98·8	54	54	62	15	15	15	97	81	78
	14	98	98·5	98·6	54	54	56	13	15	16	75	81	78
	15	97·7	98·3	98·9	50	54	60	14	15	15	70	80	77
	16	98·2	98·8	98 2	50	54	60	15	16	16	76	82	78
	17	98·4	98·8	98·6	52	58	66	15	16	16	77	83	78
	18	97·9	98·6	...	50	52	...	15	15	...	75	82	...
	19	98·4	98·8	98·7	58	52	64	15	16	16	73	80	78
	20	97 8	98·8	...	52	50	...	15	16	...	74	82	...
	21	98·1	98·9	98 6	66	56	60	16	16	15	73	80	74
	22	97·6	98·6	98·5	52	56	60	14	16	16	74	82	77
	23	97·9	98·6	99	60	54	56	14	16	15	73	82	76
	24	97·8	98·6	98·6	54	58	56	14	16	15	74	82	78
	25	98·3	99·2	98.9	52	60	66	15	16	16	74	81	76
	26	98·1	98·7	99·2	52	56	58	14	14	15	74	80	76
	27	98·2	99·3	98·4	54	58	60	15	15	16	73	82	75
	28	98·3	...	98·6	56	...	70	14	...	15	72	...	75
	29	98·1	98·4	98·5	52	52	58	14	15	16	72	82	78
	30	98·3	98·5	98·4	54	58	76	15	16	16	75	82	78
	31	98·2	98·6	98·6	60	56	66	15	16	17	76	82	76
		93	98·6	98·6	53·8	55·2	66	14·4	15·5	15·7	75	81·3	77
January, 1846.	1	98·2	98·5	99·2	54	56	78	15	15	16	72	83	79
	2	98	...	...	52	...	...	14	...	...	76	...	...
	3	98·5	99	98.8	56	62	66	15	15	17	74	80	78
	4	98·1	98·8	98·8	56	56	62	15	16	16	76	82	77
	5	98·2	99·1	98·5	56	60	76	15	16	15	75	83	78
	6	98·3	98·9	98·7	54	60	70	15	16	16	72	81·5	78
	7	98·3	99	98·7	56	60	62	14	16	15	76	81	78
	8	98·2	98·8	98·5	54	56	62	15	16	16	74	82	77
	9	98·3	99·3	...	58	66	...	15	16	...	76	82	...
	18	98·2	99·1	98·6	52	60	58	15	16	15	76	84	78
	19	98·2	98·7	98·9	54	58	60	14	16	15	76	82	79
	20	98·2	99·1	98·7	54	60	60	14	16	16	76	83	79
	23	98·3	98 9	98·5	56	60	56	14	15	15	75	83	79
	24	98·1	98·9	99·2	56	60	60	15	16	15	76	82	79
	25	98·1	98·7	98·9	50	52	52	14	15	15	75	81	78
	26	98·2	...	...	54	...	...	14	...	...	75	...	...
	29	98.3	98·9	...	52	52	...	14	16	...	72	82	...
		98·2	99	98·7	54·3	58·5	61·7	14·6	15·7	15 6	74·3	82	78·2

	Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
February, 1846. 3	98·3	99·6	98·7	54	62	74	15	16	16	72	82	76
4	97·9	98·8	...	58	54	...	15	15	...	73	78	...
5	98·5	99·1	98·5	60	56	56	14	16	15	74	82	75
6	98·3	99·3	98·5	56	58	58	14	16	15	73	81	76
7	98·3	99·3	98·8	52	56	66	14	16	16	72	82	76
8	98·2	99·3	98·2	52	58	70	14	16	16	72	85	76
9	98·3	99·2	98·9	62	62	64	15	15	16	76	85	78
10	97·9	99	...	50	56	...	14	16	...	75	82	...
12	98	98·8	98·8	52	52	50	14	15	15	72	82	75
13	98·2	98·6	98·6	52	54	62	14	15	15	73	84	75
14	98·3	99·1	98·5	58	56	56	14	16	15	75	84	76
15	97·9	...	98·8	52	...	60	14	...	15	75	...	81
16	98	99·3	99·1	52	60	62	14	16	15	75	84	78
17	98·1	98·8	98·4	58	58	60	14	15	15	75	83	76
18	98·1	99	98·5	56	56	56	15	16	15	75	83	78
19	98	99·1	98·6	52	56	60	15	16	15	75	83	78
20	98	99·3	98·4	56	60	66	14	15	16	75	85	79
21	98·3	99·3	98·8	56	58	70	14	16	16	76	83	78
22	98	98·8	99·1	58	56	62	15	15	15	75	85	80
23	98·4	99·2	98·8	56	58	56	14	16	14	77	85	78
	98·1	99·1	98·6	55·1	57·1	59·9	14·3	15·5	15·2	74	83	76·6
April, 1846. 9	97·8	98·3	98·5	52	54	56	14	15	15	74	79	77
10	98·2	98·5	98·6	56	56	52	15	14	15	77	83	78
11	98·2	98·8	...	56	58	...	14	16	...	77	84	...
12	98·4	99	98·6	56	54	64	14	16	16	77	85	80
13	98·1	99·2	99	54	56	72	15	16	16	78	84	77
14	98·1	99·3	98·2	52	58	70	14	16	16	76	83	79
15	98·3	98·7	98·5	54	52	62	14	15	15	77	84	79
16	97·9	98·8	98·7	56	52	64	14	15	16	77	85	79
17	98·1	98·6	98·4	54	52	60	14	15	15	77	84	79
18	98·1	99·3	98·5	52	56	58	14	16	15	77	84	80
19	98·2	98·6	98·3	56	54	56	14	16	16	77	82	79
20	98·2	98·6	98·5	50	52	52	15	15	15	77	83	79
21	98·3	98·9	98·8	52	54	58	15	16	15	76	84	79
22	98·3	98·9	98·6	56	56	54	15	15	14	75	83	79
23	98·3	99·3	...	52	56	...	14	15	...	77	85	...
24	98·3	99·1	98·6	60	54	60	16	16	16	77	86	80
25	98·1	99	98·8	52	56	62	14	16	16	78	85	81
26	98·3	99·1	98·8	54	54	56	14	17	16	77	85	82
27	98·2	99·1	99	52	56	58	14	17	15	79	85	81
28	98·2	98·8	...	54	56	...	15	16	...	78	86	...
29	...	98·9	98·7	...	50	54	...	16	15	...	85	80
30	98·1	98·6	98·6	54	50	62	15	15	16	77	82	79
	98·1	98·7	98·6	54	54·5	59·5	14·4	15·6	15·4	77	84	79

	Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p m.	9-11 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
May, 1846. 1	98·1	98·5	99	54	54	62	15	16	16	77	82	79
2	98·1	99·1	99	54	56	66	14	16	15	76	85	78
3	98·4	98·9	99·2	54	56	64	15	16	15	77	85	80
4	98·1	99	99	52	56	62	15	16	16	76	85	81
5	98·2	99	98·8	56	54	66	15	16	16	77	84	81
6	98·1	98·9	...	52	54	...	15	15	...	77	86	...
7	98·3	99	98·5	60	56	72	16	16	16	79	86	81
8	98·2	98·8	98·7	56	56	58	15	16	16	78	86	80
9	98·2	98·8	...	56	56	...	15	16	...	79	86	...
10	98·1	99·1	99·3	54	56	60	15	16	16	79	86	81
11	98·1	98·9	99·1	56	56	64	15	17	16	78	86	81
12	98·4	99·1	98·7	54	56	58	15	16	16	79	86	82
13	98·1	98·5	98·4	54	48	54	15	16	15	78	84	81
14	98·6	99·1	98·9	54	60	60	15	16	16	78	86	81
15	98·3	98·9	98·4	52	54	80	14	15	16	78	86	81
16	98·1	99·1	98	56	58	60	16	16	16	79	87	82
17	98·3	98·9	98·1	62	54	72	14	17	16	80	88	81
18	98	99·2	98·6	70	56	56	14	16	16	77	88	82
19	98·1	98·7	98·9	54	54	60	15	16	16	80	87	82
20	98·1	...	...	56	...	...	14	...	...	79	...	...
21	98·2	99	98·4	54	58	54	15	16	15	80	87	81
22	98·4	98·7	98·8	54	50	58	15	16	16	79	88	77
23	98·4	99·1	98·7	60	56	70	15	16	16	81	87	82
24	98·2	99	98·5	58	56	56	15	16	15	79	88	82
25	98·2	99·1	...	56	56	...	14	16	...	79	88	...
26	98·3	98·6	98·5	60	52	58	15	15	15	79	79	81
27	98·4	99·1	98·7	56	56	56	14	14	15	80	82	82
28	98·3	99	99	56	56	60	15	16	14	81	86	84
29	98·4	99·4	98 8	60	60	60	15	16	16	82	88	83
30	98·5	99·3	99·3	56	56	62	15	16	16	80	87	83
31	98·2	99	99	54	52	58	15	15	16	80	85	76
	98·2	98·9	98·8	56·1	55·2	62	15	15·8	15·6	78·7	85·8	80
June, 1846. 1	98·3	99·5	99·2	52	56	62	14	16	16	79	87	83
3	98·4	99·4	98·9	58	58	56	15	15	15	80	88	83
4	98·2	99·5	98·7	58	68	64	15	17	16	82	88	82
7	98·2	98·7	98·9	60	54	70	15	15	16	78	82	82
8	98·4	99·1	98·7	54	58	60	15	15	16	79	86	81
9	98·4	99	98·8	54	54	60	15	16	15	79	86	81
10	98·3	98·9	98·7	50	56	58	14	16	16	79	84	79
11	98·2	...	...	56	...	...	15	...	...	78	...	...
12	98·5	99	98·6	58	54	80	16	14	17	79	86	82
13	98·4	98·9	98·7	66	56	60	14	16	15	80	85	82
14	98·4	98·5	...	58	52	...	15	15	...	80	87	...
15	98	98·7	98·9	52	50	58	14	16	16	80	85	82
16	98.2	99·1	98·7	52	56	56	14	16	16	79	82	80
17	98·4	99	98·7	58	58	56	15	16	14	79	85	81
18	98·1	98·7	...	52	52	...	15	16	...	77	81	...
20	98·1	...	...	56	...	...	15	...	...	79	...	...
23	98·3	98·6	98·9	50	54	58	14	16	15	79	82	79
24	98·1	98·8	98·8	58	54	56	15	16	15	79	85	82

	Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
June, 1846. 25	98·1	99·1	...	56	56	...	15	16	...	79	85	...
26	98·3	98·5	98·8	54	54	54	15	16	16	79	84	81
27	98·2	98·9	98·8	54	56	52	14	16	14	79	86	78
28	98	98·7	98·5	52	52	52	14	15	15	79	85	81
29	98·1	99·2	98·7	52	54	62	14	16	15	79	83	80
30	97·9	99·1	...	54	62	...	14	15	...	77	86	...
	98·2	99	98·8	55·1	55·6	59·7	14·6	15·7	15·4	79	84·7	81
July, 1846. 2	98·3	99	99·3	62	56	62	15	16	16	79	84	81
3	98·3	98·9	...	54	54	...	15	15	...	79	86	...
4	98·1	98·9	98·5	54	52	75	14	16	16	79	86	82
5	98·3	98·6	...	52	54	...	15	16	...	79	86	...
6	98·5	99	98·8	60	56	56	16	16	16	79	86	82
7	98	98·6	98·4	52	56	80	15	17	16	79	85	80
8	98·3	98·8	98·6	60	50	52	15	15	15	78	83	81
9	98·2	98·9	99	54	56	60	14	16	15	79	86	81
10	98	98·6	98·8	54	54	54	14	17	16	79	86	81
11	98	...	...	56	...	...	14	...	...	80	...	...
12	98·4	99	99·3	54	54	60	16	15	15	78	85	82
13	98·2	99	99	52	56	60	15	15	16	79	86	81
14	97·7	98·7	98·9	54	54	56	15	16	15	79	86	80
15	98·4	99	98·9	56	56	80	14	17	15	80	86	82
16	98·4	98·9	98·9	56	56	58	15	16	16	79	83	82
17	98·1	98·8	99·1	50	56	56	14	15	16	76	84	81
18	98·1	98·6	99	52	56	58	15	16	16	79	86	82
19	98·1	...	99	52	...	58	14	...	16	79	...	82
20	98·3	99·1	99·3	52	58	56	15	17	15	78	86	82
21	98	98·8	...	54	56	...	14	17	...	79	85	...
23	98·3	98·8	99	56	56	58	15	15	17	79	84	81
24	98·3	98·8	99·1	54	50	64	15	16	15	78	83	82
25	98·2	98·6	99	54	52	56	15	17	16	79	83	81
26	98·2	98·8	98·8	50	50	58	15	15	15	79	85	81
27	98·1	99	98·9	54	54	56	15	17	15	79	84	82
28	98·2	99·2	98·7	52	58	68	14	16	15	78	84	81
29	98·4	98·9	...	54	52	...	14	16	...	79	84	...
	98·2	99	98·9	54·5	54·5	60·7	14·7	16	15·3	78·8	84·8	81·4
August, 1846. 1	98·2	98·8	98·5	56	54	70	15	16	17	78	86	82
2	98·1	98·7	99·2	54	54	62	15	15	16	79	87	81
3	98·2	99·1	99·2	52	58	60	14	17	16	79	86	82
4	98·1	98·9	98·9	54	56	64	15	16	16	79	86	82
5	98·2	99·2	98·9	52	54	60	15	16	15	79	83	79
6	98·2	99	99·1	54	56	70	15	16	16	75	84	81
7	97·9	99·1	99	52	60	60	15	16	15	77	86	82
8	98·3	98·7	98·7	58	56	60	15	16	16	79	83	80
9	98·1	98·6	98·8	54	54	56	15	15	16	79	85	81
10	98·2	98·9	98·9	54	54	60	15	16	15	79	87	83
11	98·2	98·4	99	54	54	62	14	14	16	78	83	81
12	98·3	99·2	98·9	56	60	64	15	16	16	80	86	82
13	98·5	99·1	98·8	56	54	60	15	15	16	79	84	81

		Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
		6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
		Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
August, 1846.	14	98·1	98·9	99·2	54	58	64	15	17	16	79	86	82
	15	98·4	99	98·7	54	58	54	15	16	15	80	87	82
	16	98·3	...	98·8	54	...	56	15	...	16	80	...	83
	17	98·3	98·9	98·9	54	56	54	14	16	16	79	85	79
	18	98	98·8	98·9	54	52	64	15	16	16	77	86	83
	19	98·4	99·2	99	56	56	60	14	16	16	80	86	82
	20	98·3	98·9	...	54	52	...	15	16	...	79	86	...
	21	98·4	99·3	99·1	62	60	62	16	17	16	80	88	83
	22	98·2	99	99·3	54	54	60	14	16	16	79	86	81
	23	98·2	98·8	98·8	54	54	60	14	17	16	79	87	78
	24	98·2	99·1	99·2	54	54	54	14	16	16	79	87	82
	25	98·2	99·4	98·8	52	54	60	14	16	16	78	82	80
	26	98·3	98·9	99	54	54	62	14	15	15	78	84	83
	27	98·2	98·7	98·8	54	58	60	14	16	16	78	82	78
	28	98·3	99	98·5	54	54	54	15	16	16	78	86	82
	29	98·3	98·9	98·9	54	56	60	14	15	15	79	84	81
	30	98·2	...	...	56	...	...	15	...	...	79	...	...
	31	98	99·1	99	58	58	64	15	16	16	79	86	82
		98·1	99	98·9	54·2	55·6	60·5	14·6	15·9	15·6	78·7	85·5	81·7
September, 1846.	1	98·2	99	98·8	56	58	56	14	17	16	79	86	79
	2	98·2	98·8	99	54	56	56	14	16	16	79	85	83
	3	98·2	99·1	...	54	54	...	15	16	...	78	85	...
	4	98	99·2	99·2	52	56	62	15	16	16	78	85	84
	5	98·2	98·9	98·9	54	54	78	15	17	17	80	87	83
	6	98·1	98·9	98·8	56	56	60	15	15	15	80	87	79
	7	98·1	98 9	98·4	54	52	54	15	15	16	80	84	79
	8	97·7	98·8	99·2	52	54	60	15	15	16	78	86	83
	9	98·4	98·9	98·7	56	56	62	15	15	15	79	86	82
	10	98	99	98·9	54	56	54	14	15	15	80	87	83
	11	98·3	99·1	98·8	54	60	62	15	16	16	81	83	81
	12	98·2	98·6	99·1	56	52	58	14	14	14	80	82	81
	13	98·2	98·7	98·5	54	50	56	15	15	16	78	86	82
	14	98·3	99·1	98·1	56	54	64	15	16	16	78	86	81
	15	98·5	98·8	98·9	52	56	56	15	16	16	79	85	82
	16	97·9	...	98·5	56	...	60	15	...	16	78	...	80
	17	98 2	99·3	98·6	58	60	60	15	16	16	79	85	80
	18	98·2	99·1	98·9	54	56	60	15	16	16	78	84	81
	19	98·2	99	99·2	54	52	58	14	15	15	79	86	82
	20	98·3	99·2	98·7	52	58	56	14	15	15	77	86	80
	21	98·3	99·2	99	58	60	60	15	16	15	79	86	78
	22	98·2	98·9	98·8	56	56	56	15	16	16	79	85	79
	23	98·6	99	99	54	52	80	14	15	17	79	85	81
	24	98·5	99·4	99·2	62	58	60	15	15	16	78	86	82
	25	98·3	99·1	98·7	60	62	60	15	16	15	80	82	79
	26	98·1	99·2	98·5	56	56	80	14	16	16	77	84	79
	27	98·5	99·1	99	62	54	62	15	16	16	78	85	82
	28	98·2	99	99·1	54	56	62	14	15	15	79	85	81
	29	98	99·1	98·7	52	56	56	15	15	16	77	85	80
	30	98·2	98·7	98·7	54	58	56	15	16	16	77	85	81
		98	99	98·8	55	54	60·8	14·3	15·9	16	78·7	85·1	81·3

		Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
		6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
		Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
October, 1846.	1	98·1	...	...	52	...	...	15	...	...	79	...	...
	2	98·4	98·8	98·7	54	54	54	15	16	15	78	82	80
	4	97·9	98·5	98	52	54	65	15	15	15	77	83	79
	5	98·3	99·1	99	56	56	64	15	15	16	77	85	80
	6	98·1	98·7	...	52	54	...	14	15	...	78	86	...
	7	98·5	99·1	98·9	62	58	60	15	16	16	79	84	80
	8	98·4	99·3	99	52	56	74	15	16	16	78	85	81
	9	98·3	99·2	99·3	52	56	62	15	16	15	78	86	82
	10	97·9	98·9	98·8	54	56	72	15	15	16	78	86	79
	11	98·2	...	...	56	...	...	15	...	...	78	...	...
	13	98·2	99	99·4	50	56	64	14	16	15	79	86	82
	14	98·3	98·7	...	50	56	...	14	15	...	78	87	...
	15	98·4	98·9	98·9	60	54	62	14	16	16	80	86	81
	16	98·5	98·7	99	52	56	62	14	15	16	80	85	81
	17	98·1	99	98·7	50	52	56	14	16	15	78	86	81
	18	98·2	98·9	99	50	56	56	14	14	15	77	86	80
	19	98	98·9	99·1	52	54	60	14	15	15	76	86	80
	20	98·3	99·1	...	52	56	...	14	16	...	78	86	...
	21	98·2	98·9	99·2	60	52	60	16	15	16	79	88	83.
	22	98	98·8	98·8	52	50	60	14	16	15	80	87	82
	23	98·1	99	98·8	50	56	56	14	16	16	78	87	81
	24	98·3	99	98·4	54	60	64	15	15	17	79	83	80
	25	98·2	98·8	99	52	54	60	14	16	15	80	83	82
	26	98·2	98·9	...	54	52	...	14	15	...	78	84	...
	27	98·2	98·7	99·2	54	54	62	14	15	17	78	85	81
	28	97·8	98·8	98·9	52	54	56	14	15	16	77	85	82
	29	98·2	...	98·6	54	...	70	14	...	15	79	...	81
	30	98·3	98·6	...	60	58	...	14	16	...	79	85	...
	31	98·1	99·4	98·7	60	62	56	15	16	16	76	82	78
		98·2	99	99	52	55	61	14·4	15·4	15·3	78·2	85·1	81·2
November, 1846.	1	98·2	99·2	98·6	56	58	58	15	16	16	75	84	79
	2	98·1	99·2	98·7	54	58	56	14	16	15	77	85	81
	3	98	98·7	98·8	54	52	58	14	14	16	77	85	81
	4	98·3	99·1	...	54	54	...	15	16	...	78	85	...
	5	98	99·3	99·1	54	56	60	15	16	15	79	85	80
	6	98	98·7	...	54	52	...	14	15	...	78	86	...
	7	97·9	99·1	98·8	56	50	52	14	16	15	75	82	78
	8	98	98·5	98·9	52	52	54	14	15	15	77	83	80
	9	98·2	98·8	99·1	56	56	60	15	16	15	78	86	81
	10	98	99·2	99·2	56	60	62	15	16	16	78	87	82
	11	98·1	99·2	99	56	60	60	15	16	16	79	87	82
	12	98	...	...	52	...	...	14	...	...	78	...	...
	13	98·3	99·1	99·4	52	54	64	15	16	16	79	87	82
	14	98·3	98·9	...	56	56	..	15	17	...	78	87	...
	15	98·5	99·1	98·9	66	58	60	15	15	16	78	86	82
	16	98·3	99	98·8	56	56	60	14	16	16	79	87	80
	17	98·2	98·2	98·6	56	52	56	15	15	15	79	78	79

		Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
		6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
		Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
November, 1846.	18	98·4	...	...	56	...	...	...	14	...	...	...	...
	19	98·1	99	99	58	60	66	15	16	16	79	79	80
	20	97·9	...	98·8	66	..	62	16	...	16	79	...	81
	21	98	98·7	...	56	54	...	14	16	...	78	85	...
	22	98·4	98·7	99·1	58	58	58	14	16	15	78	85	81
	23	98·2	98·7	98.9	56	56	62	15	16	16	77	84	81
	24	98·2	98·9	98·8	56	56	58	15	16	16	78	85	80
	25	98	98·8	99	56	54	60	15	15	15	76	83	78
	26	97·9	99·1	98·4	56	60	70	15	17	16	76	83	79
	27	98·3	98·8	98·7	56	56	58	15	17	16	78	84	81
	28	98·1	98·6	99·2	54	54	62	14	15	16	75	84	80
	29	98·1	98·9	98·8	54	54	64	15	16	16	77	83	80
	30	98·3	...	98·8	54	...	62	15	...	16	76	...	80
		98·1	98·9	98·8	56·2	55·4	60·1	14·6	15·8	15·6	77·4	84·4	80
December, 1846.	1	98·3	98·9	98·6	54	54	62	15	16	16	78	79	78
	2	98·3	99·2	...	56	64	...	15	16	...	78	77	...
	3	98	98·8	98·9	54	54	68	15	15	16	75	79	79
	4	97·9	98·5	...	54	52	...	15	15	...	77	77	...
	5	98·3	98·7	98·8	64	56	74	16	16	17	77	81	78
	6	98·2	98·4	99	60	54	64	15	16	15	78	80	78
	7	98·1	98·8	98·8	54	50	76	15	15	16	76	79	78
	8	97·9	98·8	98·9	50	50	60	15	15	16	76	80	75
	9	97·9	99·3	98·7	52	56	60	14	16	16	75	82	78
	10	98	99	98·9	50	56	56	15	15	15	75	82	78
	11	97·9	98·7	98·5	56	52	60	15	15	16	75	82	77
	12	97·9	98·7	98·8	54	56	60	15	16	15	75	82	78
	13	98·1	98·7	99·2	56	56	64	15	16	16	75	82	77
	14	97·9	98·8	99·1	54	56	58	15	16	16	75	82	79
	15	98·1	98·5	98·5	56	54	60	15	16	15	75	77	76
	16	98·1	99·2	99	54	60	64	15	16	16	76	82	78
	17	98·3	...	98·7	56	...	56	15	...	16	77	...	78
	18	98·1	...	...	56	...	...	15	...	...	76	...	...
	19	98·2	98·8	99·2	54	56	62	14	17	16	76	84	79
	20	98·3	98·7	...	56	62	...	15	16	...	78	83	...
	21	98·3	98·7	...	56	62	...	15	16	...	78	83	...
	22	98·7	...	...	68	...	...	16	...	...	77	...	...
	23	97·9	98·9	98·9	62	54	62	15	17	16	76	82	78
	24	97·9	98·8	99·1	52	54	62	14	15	16	76	82	78
	25	98·3	98·7	...	52	54	...	14	15	...	76	81	...
	28	98	99	99	52	56	70	15	17	16	75	75	77
	29	98 1	...	...	52	...	...	14	...	...	77	...	...
	30	97·9	98·9	98·6	60	56	58	14	15	16	73	81	71
	31	98·1	98·8	98·9	52	56	56	15	16	14	73	80	78
		98·1	98·9	98·9	55·3	55·3	63·5	14·5	15·8	15·8	76	80·5	77·4

		Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
		6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
		Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
January, 1847.	1	98·4	98·8	98·7	56	54	66	14	16	16	73	82	78
	2	98·3	98·8	...	64	56	...	14	16	...	76	82	...
	3	98·5	...	99	60	...	60	15	...	15	76	...	78
	4	98·1	98·9	98·9	56	56	60	15	16	16	77	82	79
	5	98·1	99·3	98·7	54	54	56	14	16	14	76	82	78
	6	98·3	99·2	99·1	54	54	60	14	15	16	76	82	78
	7	98	99·1	98·9	50	60	60	14	16	15	76	82	77
	8	98·3	98·9	98·9	52	58	62	14	16	16	76	83	78
	9	98·5	99·5	98·9	58	62	62	15	16	16	75	83	77
	10	97·9	98·4	98·5	56	52	56	14	15	14	75	79	76
	11	98·4	98·8	98·5	60	54	54	15	16	15	75	79	77
	12	97·9	98·9	98 9	54	56	60	15	16	16	74	81	77
	13	98	99	98·9	54	56	62	15	16	15	74	81	78
	14	97·9	98·9	99·1	50	56	64	14	16	15	74	82	77
	15	98	98·8	98·9	52	52	58	14	15	15	74	81	77
	16	98·3	98·8	99	52	52	58	14	14	14	74	82	77
	17	98·5	99	98·8	60	54	66	15	15	14	74	81	77
	18	98	99	98·9	54	54	62	14	14	14	74	81	77
	19	98·3	...	...	56	...	...	15	...	...	75	...	...
	21	98·3	99	98·7	56	54	62	15	16	15	73	79	77
	22	98·2	...	...	56	...	...	15	...	...	74	...	..
	23	98	98·6	99	54	48	64	14	14	16	74	81	78
	24	98	98·4	98 6	52	54	60	14	15	15	74	81	77
	25	98·5	99·2	99·1	54	56	68	14	15	15	75	82	77
	26	97·9	98·9	98·7	54	56	56	14	16	15	74	82	77
	28	98·1	...	...	56	...	...	14	...	...	74	...	...
	29	97·9	99·1	...	52	64	...	14	16	...	73	80	...
	30	98·5	98·9	99·3	58	58	66	14	15	15	73	82	76
	31	97·9	98·4	98·5	54	52	58	15	15	15	74	81	77
		98·1	98·9	98·8	54 8	55·3	60 8	14·4	15·4	15	74·6	81·3	77·3
February, 1847.	1	97·9	99·2	98·7	52	60	60	14	16	15	73	82	75
	2	97·9	99·2	98·3	56	60	60	14	16	15	72	82	74
	3	98·1	...	...	56	...	...	14	...	...	71	...	...
	4	97·9	98·9	...	56	62	...	14	15	...	71	81	...
	5	97·9	99	99·1	56	56	64	14	16	16	71	81	74
	6	97·7	99	...	58	56	...	14	15	...	70	79	...
	7	98·1	98·5	99·1	58	60	60	15	15	15	71	81	75
	8	97·8	98·7	99	54	56	64	14	16	16	73	82	75
	9	97·8	99·2	98·9	54	60	64	14	16	15	72	81	78
	10	98·2	98·8	98·9	56	54	64	14	15	15	74	82	78
	11	98·2	98·9	99·1	56	54	62	14	16	16	73	83	77
	12	98·2	98·7	98·6	58	56	58	15	16	15	74	80	76
	13	98·3	98·5	...	56	50	...	14	14	...	72	80	...
	14	98·2	98·7	98·8	58	56	56	16	14	15	74	79	75
	15	98·1	98 8	98·7	54	60	58	14	16	15	71	82	76
	16	97·7	98·5	98·8	58	54	60	15	16	15	74	82	78
	17	98	98·8	99·1	58	58	58	15	16	15	75	82	77
	18	97·8	98.9	98·6	54	58	76	14	16	16	74	82	78
	19	97·8	98·8	98·8	58	58	60	16	16	15	74	81	77

	Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
February, 1847. 20	98·2	…	…	58	…	…	14	…	…	74	…	…
21	98·2	…	…	54	…	…	15	…	…	75	…	…
23	98	99	98·9	56	54	64	14	16	16	76	83	79
24	97·9	98·9	98·9	52	60	62	14	15	15	75	83	77
25	98·2	99·3	…	54	64	…	14	16	…	74	82	…
26	98·1	98·9	99·2	56	56	62	14	14	15	74	81	76
27	97·9	98·8	98·1	56	56	60	14	15	16	72	82	76
28	98	98·4	…	54	54	…	14	16	…	74	83	…
	98	98·8	98·8	55·5	57·6	61·7	14·2	15·5	15·3	73	81·5	76·3
March, 1847. 1	98	98·9	99·3	52	54	64	15	16	15	74	83	76
2	98·1	98·6	98·7	54	56	56	14	16	15	75	84	78
3	98·2	99	98·6	54	60	80	14	15	16	75	83	78
4	98·4	98·7	99	62	60	66	15	16	16	76	83	78
5	98·1	98·8	99·2	52	54	68	14	16	16	75	84	77
6	97·9	98·8	98·5	54	56	62	14	15	15	75	84	77
7	98·2	98·5	98·9	54	54	64	14	15	15	74	84	77
8	98·1	98·8	98·9	54	54	62	14	15	16	74	84	76
9	97·9	98·7	98·9	54	52	56	14	16	15	74	83	78
10	98·4	98·8	98·9	56	56	66	15	16	16	75	84	78
11	98.1	98·8	98·9	56	50	64	15	16	16	74	83	76
12	98·1	…	…	54	…	…	15	…	…	74	…	…
13	98·2	99·2	99·3	56	58	60	15	15	15	74	83	78
14	98·2	98·9	98·5	56	60	60	15	15	15	75	84	79
15	98	99·1	99·2	56	62	60	15	16	16	76	85	78
16	98·3	98·8	98·4	54	54	60	15	16	15	75	83	76
17	98·4	99·2	98·8	56	54	62	15	15	16	73	75	76
18	98	98·8	99·1	52	56	70	14	16	16	75	84	78
19	98·2	98·7	98·8	52	54	60	15	16	16	75	83	78
20	98·2	98·8	98·9	56	52	62	15	15	16	76	83	77
21	98·2	98·7	98·9	52	52	60	14	15	15	74	83	77
23	98·1	99·1	98·7	58	60	60	14	16	15	75	83	76
24	98·1	99·2	…	60	64	…	14	16	…	75	82	…
	98·1	98·8	98·9	54·8	59·2	62·7	14·5	15·6	15	74·6	83·5	77·1
April, 1847. 1	98·3	99	99·1	52	70	66	14	17	15	72	78	75
2	98·2	98·7	98·1	54	56	66	14	16	16	76	81	74
3	98·2	98·7	99·2	54	68	70	14	16	16	73	79	75
4	98·2	98·6	98·3	54	52	66	15	14	16	72	86	72
5	98	…	…	54	…	…	14	…	…	70	…	…
8	98·1	98·3	98·6	54	54	60	14	15	15	74	83	77
9	97·9	98·7	98·8	54	54	60	14	15	15	75	83	78
10	98·2	98·2	99	52	52	66	14	15	16	74	78	76
11	98·3	98·6	98·8	50	54	56	14	15	16	74	81	78
12	98	98·7	98·4	56	54	56	15	16	15	75	83	77

		Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
		6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
		Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
April, 1847.	13	97·8	98·7	98·7	54	54	64	14	15	16	73	77	78
	14	97·9	98·7	98·7	56	56	64	14	15	15	76	82	77
	15	97·9	99·2	99	54	64	70	15	15	15	75	82	78
	16	98·3	98·5	98·8	54	54	60	15	15	16	75	82	78
	17	97·7	99	...	58	56	...	14	15	...	75	83	...
	18	98·2	98·4	99·1	60	56	60	15	15	15	75	83	79
	19	98·3	98·7	99	56	56	62	15	16	16	75	84	78
	20	98·2	98·9	99·1	56	58	54	13	16	15	75	81	77
	21	97·5	99	98·8	54	60	58	14	17	16	76	83	78
	22	98·2	98·7	98·5	56	54	62	14	16	16	75	83	78
	23	98·1	98·7	99·1	58	54	70	14	16	16	76	84	79
	24	98·3	99	98·8	54	56	70	14	15	15	76	84	78
	25	98·2	...	...	56	...	...	14	...	...	75	...	...
	26	98·6	98·7	99	56	56	62	14	16	16	76	83	79
	27	98	99	98·9	54	54	60	14	16	15	76	84	78
	28	97·9	99	...	56	56	...	14	15	...	75	84	...
	29	98·4	98·9	98.9	58	54	56	15	16	15	76	85	78
	30	98·2	98·8	99·2	58	54	58	14	16	15	76.	83	78
		98·1	98·7	98·8	55	56·4	62·3	14·2	15·5	15·6	74·7	82·2	77·1
May, 1847.	1	97·8	99·2	98·5	56	58	58	14	16	16	76	84	79
	2	98·3	98·7	98·7	56	52	62	14	15	15	77	84	79
	3	98·4	98·8	99·2	54	58	60	14	16	16	76	85	79
	4	98·1	98·7	98·3	56	60	52	14	16	15	75	83	77
	5	98·4	98·9	98·9	58	56	60	14	16	16	75	84	79
	6	98·4	98·7	...	56	58	...	15	17	...	76	84	...
	7	98·4	98·9	99·2	56	58	58	15	17	15	77	85	79
	8	98·2	99·2	99·2	54	60	72	15	16	16	77	85	79
	9	98	99·2	99	54	60	66	15	16	16	77	84	78
	10	98·1	98 9	98·7	60	58	58	15	15	16	75	83	78
	11	98	98·7	...	56	58	...	15	16	...	75	84	...
	12	98·2	98·8	98·9	58	56	66	14	16	15	76	84	79
	13	97·9	99·1	98·9	56	56	64	15	15	16	77	84	79
	14	98·1	98·9	98·6	54	64	58	15	16	15	78	87	80
	15	98·1	99·1	98·7	56	62	58	15	16	15	78	86	79
	16	98·2	98·8	99·2	54	58	62	15	15	16	78	86	80
	17	98·1	99·1	99·2	56	58	64	15	16	16	77	87	80
	18	98·3	98·6	98·7	56	54	58	14	16	15	77	84	79
	19	98·4	99	...	56	62	...	14	16	...	77	87	...
	20	98·1	98·7	99·2	60	56	64	15	16	16	78	86	81
	21	98·1	98·9	...	58	60	...	14	15	...	78	86	...
	22	98·3	98·8	98·6	62	56	66	14	16	16	78	87	82
	23	98·2	98·8	99·1	60	54	60	15	15	15	79	85	80
	24	98 4	...	...	58	...	...	14	...	...	77	...	...
		98·1	98·9	98·8	56·6	57·8	61·3	14·5	15·8	15·6	76·7	85	79·2

	Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
October, 1847. 1	98·2	99	99·2	56	56	60	15	16	17	77	85	81
2	98·2	99	99·1	56	58	60	15	15	17	78	86	83
3	98	98·9	98·6	56	56	56	14	15	16	78	85	81
4	98·3	98·9	99·2	58	58	60	15	15	16	78	87	81
5	98·5	98·8	98·8	56	56	56	15	15	16	78	83	78
6	98	98·7	99	56	52	64	14	16	17	77	85	79
7	98	99	98·5	54	56	54	15	15	16	78	86	78
8	98·2	99·6	98·9	56	62	58	15	16	16	77	85	81
9	98	99·1	98·8	56	54	54	15	16	15	78	86	82
10	98·3	99	98·7	54	56	58	15	15	16	79	87	81
11	98·5	98·8	99·1	56	58	56	15	16	16	80	83	81
12	98·4	98·8	98·7	56	58	54	15	14	16	78	86	79
13	98	99·2	98·8	52	60	60	14	15	16	78	85	80
14	98·2	99	98·8	58	58	55	16	16	16	77	85	80
15	98	98·8	98	54	56	58	15	15	16	77	84	80
16	98·1	99·1	98·6	56	54	56	15	17	16	78	86	80
17	98	98·9	99·1	54	56	60	15	15	16	78	86	80
18	97·9	98.7	98·6	54	52	56	15	15	16	78	86	80
19	98·1	99	98·7	54	52	54	14	15	16	78	87	81
20	98	99·2	98·6	52	58	54	14	15	15	78	86	82
21	98	99	99·1	52	56	60	14	15	16	78	85	80
22	98·1	99	98·4	58	54	56	14	15	15	78	84	79
23	97·9	99·6	98·9	52	58	60	14	15	15	77	86	80
24	98·1	98·9	98·5	56	56	54	15	15	15	77	85	80
25	97·7	98·5	98·3	54	50	54	15	15	15	79	79	78
26	97·9	98·2	98·6	52	54	54	14	15	15	77	80	79
27	97 9	99·2	98·4	56	56	54	15	14	15	77	85	77
28	98·1	98·7	98·7	54	52	56	14	15	15	77	83	79
29	98	98·8	98·8	54	58	58	14	14	15	77	82	80
30	98·1	98·8	99	56	58	60	14	15	16	76	85	80
31	98	98·9	98·8	52	60	58	14	14	15	77	85	81
	98	99	99	54·7	56	55·7	14·5	15·1	15·7	77·7	84·8	80
November, 1847. 1	98·2	99·7	99·1	56	66	60	15	15	16	77	84	79
2	98·3	99	98·9	54	62	58	14	15	15	77	83	82
3	98·1	98·7	98·9	56	50	58	15	15	16	78	86	82
4	98	98·9	99·1	54	52	58	14	14	15	78	86	82
5	98	98·8	98·9	50	56	56	15	15	15	78	86	80
6	98·2	99·4	98·8	56	54	62	14	15	16	78	86	82
7	98·3	99·3	...	56	62	...	14	16	...	79	87	...
8	98	98·8	98·9	56	56	60	14	14	15	78	84	79
9	98	98·5	98·9	56	52	54	14	16	16	78	86	81
10	98·2	99	98·9	56	56	56	14	15	16	79	86	81
11	98·4	98·8	98·8	58	58	56	14	15	16	79	85	81
12	98·2	99·3	99	56	56	62	14	15	15	79	84	81
13	98·4	99·5	99	58	60	58	15	16	16	79	86	81
14	98·6	99·1	98·6	60	58	56	15	16	15	78	83	78
15	98	98·7	97·7	56	50	58	15	15	16	75	76	76
16	98·4	98·5	97·8	54	50	58	15	16	16	76	78	76

	Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
November, 1847. 17	98·3	98·5	98·8	54	52	54	14	15	16	75	79	77
18	98·2	99·4	...	54	62	...	15	16	...	77	83	...
19	98·2	98·5	98·6	58	52	58	15	15	15	75	82	76
20	98·1	98·9	99	52	56	58	15	15	15	74	83	78
21	98	98·2	98·5	56	54	64	14	15	16	74	77	77
22	98·2	99·2	98·6	56	62	60	14	16	16	75	78	77
23	98·2	99·2	98·7	60	70	60	15	15	16	75	83	78
24	98·2	98·8	99·3	54	54	64	14	15	16	75	75	79
25	98·2	99·2	99	54	56	66	15	15	17	77	83	79
26	98·2	98·9	98·5	54	56	56	14	15	15	77	82	78
27	98	98·6	98·7	54	56	62	15	15	16	74	81	78
28	98	98·6	98·3	54	54	56	14	15	15	75	82	76
29	98·1	98·9	99	52	54	68	15	15	15	77	77	78
30	98·4	99·3	98·6	58	58	54	15	14	15	76	81	78
	98·1	98·8	98·8	53·3	54·8	59·2	14·5	15·1	15·2	76·7	82·3	79
December, 1847. 1	98·1	98·7	98·9	52	56	62	14	15	15	77	83	77
2	97·9	98·6	98·6	52	52	58	14	15	16	74	81	68
3	97·9	99·2	99·2	52	58	68	14	14	16	75	73	77
4	97·9	98·7	98·7	52	54	60	15	15	16	74	82	73
5	97·7	99·1	97·8	54	60	56	14	15	14	68	82	73
6	98·2	98·4	98·5	56	58	56	14	15	16	72	83	78
7	97·9	98·6	98·2	56	54	56	14	15	16	77	83	78
8	98·1	99·1	98·8	54	58	58	14	15	15	77	84	78
9	98	99·2	98·5	54	62	58	15	16	15	76	83	77
10	98·2	98·7	98·5	54	54	60	15	15	16	76	83	78
11	98·3	98·8	98·6	54	54	60	15	15	16	77	81	78
12	98·3	98·9	98·7	54	50	58	14	14	16	77	81	77
13	98·2	99·1	98·8	56	56	60	14	15	15	79	80	78
14	98	98·9	98·4	52	52	64	14	14	16	73	77	77
15	97·9	99·2	98	52	58	64	14	15	16	76	80	77
16	97·9	98·8	98·6	52	56	60	14	15	16	75	80	76
17	98·1	98·7	98·7	52	54	60	15	15	15	76	81	77
18	97·9	98·2	98·5	54	50	56	14	15	15	76	81	77
19	98·1	98·9	98·5	52	60	58	14	15	15	75	82	77
20	98·2	98·7	98·6	56	56	60	14	14	16	76	82	78
21	98	98·8	98·7	52	56	58	14	15	15	75	82	78
22	98·1	...	98·9	52	...	66	13	...	15	75	...	77
23	97·9	98·8	98·1	54	56	62	14	14	15	75	82	76
24	98·1	99·1	...	52	58	...	14	16	...	74	80	...
27	98·1	98·5	98·7	54	52	56	14	14	15	73	81	76
28	97·9	98·4	98·3	52	56	58	14	15	15	74	82	76
29	98	98·4	98·9	50	52	62	14	14	15	73	81	75
30	97·7	99·2	99·4	50	60	58	14	15	15	73	81	78
31	98·1	98·5	98·4	52	50	58	14	15	15	75	79	78
	98	98·8	98·3	52·9	53·5	58	14·1	14·6	15·3	74·5	81·1	76·9

		Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
		6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
		Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
January, 1848.	1	98	99	98·8	56	60	66	14	15	14	76	80	77
	2	98·1	99·2	98·9	54	56	60	15	15	15	75	81	78
	3	98	99·1	99	54	60	66	14	15	15	76	82	78
	4	98·2	99	98·8	52	56	58	14	15	15	75	80	76
	5	98·1	99·1	99	52	58	58	14	14	16	75	80	76
	6	97·9	99·1	98·5	54	58	58	14	15	15	75	80	78
	7	98·2	98·7	98·8	56	54	58	14	15	15	74	81	78
	8	98·3	99·1	98·8	56	62	64	14	16	16	75	83	78
	9	98·3	98·8	99·1	52	56	60	15	15	16	74	82	78
	10	98·4	98·3	98·8	54	58	60	14	15	16	73	81	71
	11	97·9	98·4	98·5	52	54	60	14	16	15	73	81	75
	12	98·1	98·3	98·7	58	54	66	14	15	16	73	82	77
	13	97·9	98·6	98·5	54	58	64	14	16	15	74	80	77
	14	98	99·1	98·9	56	60	56	14	16	14	73	81	77
	15	98	98·9	99	56	62	60	14	16	15	73	81	78
	16	98·3	99	98·7	56	52	58	14	15	16	75	80	78
	17	98·1	98·8	98·8	54	56	60	15	15	15	75	75	75
	18	97·9	98·6	98·9	56	56	56	14	15	16	73	77	77
	19	98·3	98·8	98·8	54	54	58	14	15	15	74	80	77
	20	98·3	99	98·5	56	58	56	14	15	16	74	81	77
	21	97·9	98·6	98·8	54	58	58	14	16	16	74	80	78
	22	98·1	98·7	98·7	56	56	58	14	15	15	74	82	78
	23	98·1	99·1	98·7	56	58	54	14	14	14	73	81	76
	24	98·3	99	98·8	52	54	60	14	14	15	73	79	76
	25	97·8	98·9	98·4	52	54	60	14	15	15	73	80	76
	26	98·2	98·9	99	53	56	56	14	15	16	73	81	77
	27	97·9	98·8	98·8	56	54	56	14	15	15	69	80	74
	28	98	98·8	98·9	54	56	62	14	15	15	68	80	76
	29	98	99·3	99·1	54	50	62	14	16	15	72	80	74
	30	98·3	99·3	98·8	56	56	56	14	14	15	71	82	77
	31	98·1	98·9	99	54	56	54	15	15	15	74	82	78
		98·1	98·9	98·8	54·5	56·8	59·3	14·1	14·9	15·2	74·1	80·5	76·6
February, 1848.	1	98·4	99	98·8	56	56	56	14	15	15	73	81	78
	2	98·2	99	98·8	54	58	56	15	15	15	73	83	75
	3	97·9	99·2	98·9	56	60	58	15	15	15	75	82	78
	4	98	98·8	98·6	54	62	56	14	16	15	74	81	78
	5	98·1	98·8	98·9	56	60	62	14	16	15	73	83	77
	6	98	98·8	98·8	56	56	60	14	15	16	72	82	77
	7	98	98·9	99·3	56	54	62	14	14	16	73	81	78
	8	98	99·2	98·8	57	58	56	14	16	15	73	83	77
	9	98·1	98·7	99·2	56	58	64	15	15	16	74	83	78
	10	97·9	99·6	99	54	54	56	14	15	16	74	81	77
	11	98	98·5	98·9	55	56	62	15	15	16	74	80	78
	12	98·1	98·5	99	56	54	64	15	16	17	73	81	77
	13	98	98·4	98·5	52	56	56	14	15	16	73	81	77
	14	98	99·4	98·9	56	58	60	14	15	16	74	83	78
	15	98	98·6	99·3	52	56	64	15	15	15	74	83	77
	16	98	98·7	98 9	57	56	62	15	15	16	73	82	78

	Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
February, 1848.												
17	98·3	99	99·2	58	58	58	16	16	16	75	81	78
18	98·1	98·3	98·8	54	52	58	15	15	15	73	77	77
19	97·9	99·2	98·7	52	56	56	14	15	16	73	81	77
20	98	99	98·7	56	54	56	14	15	15	74	81	78
21	97·7	98·9	98·9	52	56	58	15	16	16	73	81	77
22	97·8	99·2	99·1	52	58	62	15	14	15	73	83	78
23	98·2	99	98·9	56	56	64	15	15	16	75	82	79
24	97·9	99·1	99	54	64	64	14	16	16	74	83	79
25	97·9	98·7	99	58	56	58	15	15	15	74	82	78
26	97·9	98·7	...	54	56	...	15	16	...	74	81	...
27	97·8	99·2	99	56	56	60	14	15	15	74	82	78
28	98·1	98·8	98·8	54	54	62	14	15	15	75	77	77
29	97·8	98·6	98·9	52	56	62	15	15	15	74	81	78
	98	98·8	98·8	54·9	57	59·7	14·5	15·1	15·5	73·6	82·1	77·5
March, 1848.												
1	98	98·8	99·3	54	56	62	15	16	16	75	83	79
2	98·1	99·6	99·2	56	58	66	15	16	16	76	84	80
3	98·2	98·8	99	56	56	64	15	15	17	76	83	80
4	97·8	98·9	...	52	56	...	14	15	...	73	81	...
5	98·1	98·5	99	60	56	60	14	15	16	74	82	78
6	97·9	98·7	98·6	56	56	56	14	15	15	74	82	79
7	97·8	98·3	98·9	52	56	56	14	15	15	74	82	77
8	97·9	99·5	99	52	74	58	14	16	15	74	83	79
9	98·1	99·4	. .	54	60	...	14	14	...	76	82	...
10	98·4	99·1	99	58	56	62	15	14	15	75	83	78
11	98·1	99	98·7	54	60	62	14	16	16	74	81	78
12	98·1	98·9	98·9	54	62	60	15	15	16	74	83	78
13	98	98·9	99·1	56	56	60	14	15	16	73	82	78
14	98·1	98·9	99	58	60	62	15	16	15	75	82	79
15	98	98·8	98·7	56	56	56	14	15	15	76	83	78
16	98·3	98·7	99·2	56	58	62	15	16	16	76	80	77
17	98·1	98·7	98·9	54	54	62	14	14	15	75	83	79
18	98·1	...	...	56	...	...	14	...	...	76	...	...
19	98·2	99·2	99	56	60	54	15	15	15	75	83	82
20	98·2	98·7	98·6	54	54	54	14	16	15	75	76	77
21	98	98·7	99·1	52	56	56	15	16	15	73	82	78
22	98	99	99	56	56	58	15	15	15	75	82	79
23	98·1	98·8	98·9	54	58	60	14	15	15	77	83	79
24	98·3	98·9	99	54	56	62	14	15	16	75	82	78
25	98	98·6	98·9	56	56	56	14	15	15	76	79	78
26	98·2	98·9	98·5	54	58	60	14	15	14	75	81	79
27	97·9	98·7	99·1	52	52	56	14	15	15	75	83	78
28	97·9	99·1	98·8	52	54	60	14	15	15	76	84	80
29	98	99·2	99 2	54	64	66	14	16	16	77	85	77
30	98·2	99·1	99·1	54	56	60	14	15	14	77	83	78
31	98	99·4	99	58	58	62	14	15	15	78	79	75
	98	98·9	98·9	54·9	57·6	59·7	14·3	15·5	15·4	75·1	81·9	78·2

		Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
		6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
		Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
April, 1848.	1	98·2	99	98·6	56	58	58	15	15	14	75	79	78
	2	98·1	99·2	99	60	56	58	14	16	15	73	81	77
	3	97·9	98·8	98·9	54	56	60	15	15	16	71	81	77
	4	98	98·7	...	54	58	...	14	15	...	71	81	...
	5	98·2	98·8	98·9	60	60	64	15	16	15	72	83	78
	6	98·1	99·3	98·7	56	58	64	14	15	16	73	85	79
	7	98·2	99·2	99·2	56	58	64	15	15	16	76	85	79
	8	97·8	99·3	99·3	54	58	58	15	15	15	77	83	81
	9	98·4	...	...	58	...	...	15	...	...	75	...	...
	25	98·1	98·9	98·9	54	54	62	14	15	15	75	80	77
	26	98	98·7	98·9	52	56	60	15	16	16	77	85	81
	27	98	98	99	54	58	60	15	15	15	78	84	79
	28	97·9	99	98·9	52	58	60	14	15	15	77	85	79
	29	97·7	98·5	98·8	52	56	62	14	16	15	76	84	79
	30	97·9	99	99 1	52	64	62	15	16	16	77	85	82
		98	98·9	98·9	54·9	57·7	60·9	14·7	15·2	15·3	74·8	82·9	78·1
May, 1848.	1	98·3	99·1	98 7	52	58	60	14	15	15	78	86	81
	2	97·9	98·7	98·6	52	56	58	15	15	16	79	85	79
	3	98·1	98·7	99	56	54	60	14	15	16	77	85	80
	4	97·9	98·8	99	52	56	54	14	16	16	78	87	82
	5	98	99·2	99	56	55	60	15	16	15	79	85	81
	6	98·2	99	99·1	54	56	70	15	14	16	79	86	81
	7	98	99·1	99	58	56	60	14	15	16	78	86	82
	8	98·1	98·8	99·1	54	56	60	14	15	15	78	86	82
	9	98·3	99·3	98·4	56	58	58	15	15	15	79	85	78
	10	98·4	99·2	99	58	58	66	15	14	16	78	84	80
	11	98·2	...	...	58	...	...	14	...	...	78	...	...
		98·1	99	98·9	55·1	56·2	60·6	14·3	15	15·6	78·2	85·6	80·6
June, 1848.	7	98	98·6	...	56	60	...	15	16	...	78	81	...
	8	98	98·9	98·6	52	58	62	14	15	15	78	84	81
	9	97·9	98·6	98·8	54	58	64	14	16	14	77	85	82
	10	98·8	98·9	99	54	58	60	15	15	16	78	85	80
	11	97·8	99	98·9	52	56	56	14	14	14	77	86	81
	12	97·9	98·9	98·9	52	54	58	14	15	14	78	83	80
	13	98·1	99	98·7	54	62	58	14	15	15	78	86	81
	14	98·1	98·9	...	54	58	...	14	14	...	78	86	...
	15	98	98·5	99·3	50	52	56	14	14	15	79	85	78
	16	97·9	98·7	99·1	52	54	62	14	15	15	77	86	80
	17	97·9	98·8	99	54	60	76	14	15	15	77	86	81
	18	98	98·9	98·9	56	58	56	14	14	16	77	85	81
	19	98	98·7	99	52	54	58	14	15	15	78	79	80
	20	98	99·1	98·7	50	56	62	14	15	15	77	85	81
	21	97·9	98·8	99	50	56	60	14	15	15	78	85	80

		Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
		6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
		Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
June, 1848.	22	98	98·7	99·2	50	56	60	14	15	15	77	85	82
	23	98	98·9	99	54	54	56	14	14	14	78	86	82
	24	98·1	...	98·9	54	...	62	14	...	15	79	...	79
	25	97·8	98·6	98·8	52	56	56	14	16	15	78	83	81
	26	97·9	98·9	99·2	52	52	60	14	15	14	78	84	81
	27	98·2	98·9	98·8	54	54	58	14	15	14	77	86	80
	28	97·9	98 9	98·8	50	54	58	14	15	15	77	85	79
	29	97·9	98·8	99·2	52	56	60	14	14	16	75	86	89
	30	97·8	99	99·3	54	54	62	14	14	15	77	86	80
		98	98·8	99	51	56·1	56·8	14·1	14·8	14·9	77·5	84·7	80·4
July, 1848.	1	97·9	99·1	98·9	48	54	58	14	15	15	79	86	82
	2	98·1	98·3	98·9	58	52	56	14	15	14	78	87	82
	3	98·2	99	99·1	52	56	64	13	13	15	78	84	81
	4	98·1	98·6	98·5	52	54	78	13	15	15	76	79	79
	5	98·3	98·9	98·5	60	54	58	14	15	15	75	84	79
	6	98·1	99	99·1	52	52	58	14	14	14	78	85	81
	7	97·9	98·9	99·1	52	56	58	14	14	15	78	83	81
	8	98·1	99	98·5	51	54	72	14	15	14	78	84	81
	9	98·2	...	99·3	56	...	58	14	...	15	78	...	82
	10	98·2	98·9	99·1	52	54	58	14	16	15	77	85	80
	11	97·8	98·7	99	50	56	54	14	14	14	77	85	80
	12	98	...	...	50	...	...	14	...	...	77	...	...
	26	97·9	99	99·4	54	56	58	14	15	15	77	85	80
	27	98	98·9	98·7	54	56	58	14	15	15	77	85	80
	28	98·2	99	98·9	54	56	56	15	15	15	79	85	82
	29	98·2	98·4	98·9	54	50	52	14	15	15	79	85	81
	30	98·1	98·9	98·5	54	50	54	14	13	15	78	83	81
	31	98·1	99·2	99	52	58	58	14	14	15	78	85	80
		98	98·3	98·9	53	54·2	59·5	14	14·7	14·8	77·7	84·3	79·4
August, 1848.	1	97·8	99	98·7	52	54	56	14	15	15	78	85	80
	2	97·8	...	98·5	52	...	76	13	...	15	78	...	79
	3	98·3	98·7	98·7	54	54	54	14	15	14	78	86	80
	4	98·1	98·7	98·6	54	52	54	15	15	14	79	82	81
	5	98·1	99	98·9	56	52	56	14	15	15	78	85	80
	6	97·8	99	99	52	54	58	13	14	15	76	85	81
	7	98·2	99	99·6	56	58	66	14	15	15	78	86	81
	8	98·1	99·1	99	56	54	60	14	16	15	79	85	80
	9	98·2	...	...	54	...	...	15	...	...	79	...	...
	10	98·3	98·9	98·7	66	54	58	15	15	15	77	83	80
	11	97·9	99·1	99·2	54	54	58	14	15	16	77	86	81
	12	98·1	99·3	99·1	54	56	58	14	15	15	79	86	82
	13	98·2	98·9	98·9	56	54	60	14	14	15	79	86	83
	14	98 2	99	99·6	56	58	62	14	15	15	80	85	82
	15	98	99·2	98·8	56	58	62	14	15	16	79	86	80
	16	98·2	...	...	56	...	...	14	...	...	79	...	...

	Temperature under the tongue.			Pulse.			Respirations.			Temperature of room.		
	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.	6-7 a.m.	12-2 p.m.	9-11 p.m.
	Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
August, 1848. 17	98·2	99·7	99·7	58	60	58	14	14	14	79	84	81
18	98·1	99·2	99·1	54	58	58	15	14	15	79	86	79
19	98·1	99·3	99·2	54	56	56	14	15	15	79	86	81
20	98·2	99	...	54	54	...	14	15	...	78	81	...
22	98·2	99·1	99·5	54	56	66	14	16	15	80	87	82
23	98·1	...	...	56	...	...	14	...	...	78	...	...
24	98·1	99·4	99·4	60	60	60	14	16	15	77	86	82
25	98·1	99·4	98·8	56	56	54	14	15	15	79	86	82
26	98·2	99·3	99·2	54	66	60	14	16	15	79	87	82
27	98·1	99·1	99·2	52	60	56	13	15	15	78	86	79
28	98·1	99·4	99·6	56	60	66	14	15	15	78	86	82
29	98·2	...	98·9	56	...	70	14	...	14	79	...	80
30	98·2	...	99·4	56	...	66	14	...	14	79	...	82
31	98·2	99·1	99·4	58	60	66	14	15	14	80	87	82
	98·1	99·1	99·1	55.4	56·3	59·5	13·9	15	14·7	78·4	85·3	80·9
September, 1848. 1	98	99·2	99	52	56	64	14	15	16	78	87	81
2	98·1	99	99·2	54	52	60	14	15	16	78	88	81
3	98	99·3	99	52	54	60	14	14	14	79	88	84
4	98·1	99·6	99·4	52	62	60	14	14	15	80	87	82
5	98·2	99·4	99·4	56	66	64	14	15	16	79	87	80
6	98	...	99·2	54	...	64	14	...	15	79	...	81
7	98·2	99·6	99·6	54	60	62	14	16	15	78	88	82
8	98·5	99·5	99·2	56	60	58	14	15	15	79	88	86
9	98·3	99·5	99·6	56	60	66	15	15	15	83	89	84
10	98·3	99	99·3	56	56	66	15	16	15	80	89	82
11	98·3	97·9	99·2	56	60	66	14	15	15	80	89	82
12	97·9	99·2	99·3	52	58	60	14	15	14	80	88	83
13	98·1	...	...	54	...	...	14	...	...	79	...	...
14	98	99·3	99	56	60	62	14	15	15	78	86	81
15	98·5	99·2	98·9	54	56	72	14	15	16	79	85	81
17	98	99	98·9	54	54	66	14	15	15	76	85	82
18	98·3	99·4	99·9	54	58	74	14	15	16	79	84	82
19	98·5	98·8	99·3	58	56	70	14	15	14	79	80	81
20	98·3	98·9	98·7	58	54	66	14	15	16	80	84	82
21	98·3	99	98·8	58	52	56	14	15	16	81	86	82
22	98·1	98·7	99·2	52	52	60	14	15	16	81	86	82
23	98	98·9	...	52	56	...	14	16	...	78	86	...
24	98·5	98·7	99·1	62	56	62	14	15	15	80	86	80
25	98	99·3	99·1	52	56	56	14	16	15	78	85	82
26	98·2	99 2	98·8	54	52	56	14	15	15	78	85	80
27	98·1	...	...	52	...	...	14	...	...	77	...	...
28	98·3	98·8	99·3	54	56	58	14	16	15	75	85	80
29	98·5	99·2	99·3	54	58	60	14	15	15	78	86	82
30	98·2	98·8	99	52	58	66	14	16	15	79	85	82
	98·1	99·1	99·1	54·4	57·3	60·4	14	15	15·2	78·9	86·2	81·7

Month	Day	Temperature under the tongue. 6-7 a.m.	12-2 p.m.	9-11 p.m.	Pulse. 6-7 a.m.	12-2 p.m.	9-11 p.m.	Respirations. 6-7 a.m.	12-2 p.m.	9-11 p.m.	Temperature of room. 6-7 a.m.	12-2 p.m.	9-11 p.m.
		Deg.	Deg.	Deg.							Deg.	Deg.	Deg.
October, 1848.	1	98·3	98·8	98·8	56	56	66	14	15	16	79	86	83
	2	98·2	99	98·7	52	56	60	14	14	15	79	82	80
	3	98·2	99	...	52	56	...	14	15	...	78	86	...
	4	98·4	99·3	99·2	60	56	60	15	15	15	79	87	84
	5	98·5	...	99·3	54	...	72	14	...	15	82	...	83
	6	98·1	99·3	99·1	50	56	58	14	16	15	80	87	82
	7	98·3	99·2	99·4	54	54	60	15	15	15	80	87	80
	8	97·9	99·1	98·8	52	58	58	14	16	15	78	84	80
	9	98·1	99·4	99·4	54	56	60	14	15	15	76	87	81
	10	97·9	99·2	98·7	54	52	58	15	15	15	80	84	81
	11	98·3	99·5	99·9	54	58	60	14	16	15	79	87	81
	12	98·1	99·4	99·3	54	58	64	15	15	16	79	86	80
	13	98·1	99	99·1	56	58	62	14	16	15	79	87	78
	14	98·3	99·3	...	54	58	...	14	16	...	78	88	...
	15	98·2	98·9	99·3	56	58	66	14	14	16	78	86	82
	16	97·9	98·8	98·6	54	52	58	14	14	15	80	80	78
	17	98	99·1	99	52	54	60	14	16	15	77	86	80
	18	98·5	...	98·6	58	...	58	15	...	16	78	...	80
	19	98·1	98·9	99·2	56	58	62	15	15	16	78	85	82
	20	98·1	98·9	98·7	58	58	64	14	15	16	79	80	79
	21	98·4	98·6	98·9	54	52	62	15	15	16	77	86	82
	22	98·3	...	99·1	56	...	58	14	...	16	79	...	80
	23	98·2	98·8	98·9	52	52	56	14	15	15	79	82	80
	24	98·4	99·1	98·4	56	54	60	14	15	15	77	83	79
	25	98·5	...	...	52	...	...	14	...	...	77	...	...
	26	98·2	99·3	99·3	58	56	66	14	15	15	76	86	82
	27	98	99·1	98·8	54	56	58	14	14	15	77	86	81
	28	98·1	99·1	99·1	52	56	62	14	15	16	78	87	83
	29	98·4	99·5	...	54	58	...	14	15	...	80	83	...
	31	98·3	98·8	98·8	52	52	60	14	15	15	78	77	79
		98·2	99·1	99	54·3	55·8	61	14·2	15	15·3	78·9	84·8	80·8
November, 1848.	1	98·2	99·3	99	50	58	60	14	16	15	77	85	81
	2	98·1	99	...	52	56	...	14	15	...	78	82	...
	3	98·5	98·8	98·5	58	56	58	15	15	15	77	82	78
	4	98·2	99·1	...	50	58	...	14	15	...	77	84	...
	5	98·1	99·4	98·5	62	58	58	15	16	16	78	86	79
	6	98·1	99·2	98·4	56	58	58	14	15	15	77	83	78
	7	98	98·9	...	58	56	...	14	15	...	77	83	...
	8	98·2	99·3	98·9	54	62	62	14	16	16	76	83	79
	9	98·1	...	...	58	...	...	14	...	...	78	...	...
		98·1	99·1	98·6	55·3	57·7	59·2	14·2	15·4	15·4	77	83·5	79

Postscript.—The thermometer with which the preceding observations were made was broken immediately after my return to England, when travelling by railway, no special precaution having been taken in the packing of it, as by inclosing it in elastic horse-hair; and in consequence I had not the means at once of making further trials on temperature for the purpose of comparison. Recently, having had another thermometer constructed as delicate as that before used and divided with the same minuteness—each degree of Fahrenheit into ten parts—I have been enabled to continue the trials; as yet, however, only for one month, that of April, without interruption. They have been made thrice daily, at about the same hours as those recorded in my former paper. The results, under ordinary circumstances of health, exercise, etc., have accorded with those then obtained, the highest temperature having been found to be immediately on rising in the morning after the night's rest, and the lowest at night, just before retiring to rest. This accordance will probably be received in proof that the difference of results in the West Indies and in England has been mainly owing to difference of climate and the habits of life connected therewith; and apart from these, to no change in the individual, the subject of the trials.

V. MISCELLANEOUS OBSERVATIONS ON THE TEMPERATURE OF ANIMALS.

These observations like those in the preceding papers have been made at very different times and places; and like them are offered as a small contribution to an interesting subject, the knowledge of which in its vast details—these commensurate with the animal kingdom—can only be advanced by the joint aid of many inquirers.

1. *On the Temperature of the Bonito, Shark, Salmon, and Dolphin.*

In a former volume,* I have given one observation on the temperature of the bonito (*Th. pelamys*) and one on that of a shark (*Carcharias communis*) made during a voyage to Ceylon in 1816. Referring to the notes taken at the time, I find some other observations, which I shall now give, trusting that there is a warrant for so doing in the nature of the subject, and the degree of obscurity attached to it, especially in the instance of the first named fish.

The bonito, the temperature of which was tried, was caught at five o'clock in the afternoon in lat. 1° 14′ N., long. 63° 30′ E. The weather during the day had been fair and pleasant. The fish weighed about 15 lbs., and was amazingly thick in comparison with its length. The trials with the thermometer were made about a minute after its capture. The air at the time was 78°; the sea, at its surface, 80·5°. The heart of the bonito lies near the surface, and being, like that of the tunny, very large and powerful, is well fitted to effect a vigorous branchial circulation. The thermometer introduced into it through a small incision rose to 82°. The large vena cava, on the contrary, situated centrically and surrounded by bad conducting parts, is well protected from the cooling influence of the sea. The thermometer introduced into it about half an inch, the blood flow-

* Res. Anat. and Physiol., vol. i., p. 170.

ing, rose to 95° ; and inserted into the deeply seated muscles of the back, rose to 99°.

The sharks, the temperature of which was tried, were all of the same kind.

The first caught was a female, taken by line and hook on the 11th of March, in lat. 8° 23′ N. When the trials were made the fish was alive, but its strength was nearly exhausted. The thermometer, inserted through a small incision into the muscles near the tail, rose to 77° ; placed in the vena cava, it fell to 76° (the blood then flowing coagulated on rest) ; in the muscles of the exterior part of the body it was the same. The air at the time was 71·75° ; the sea at the surface 74·75°.

The second was taken on the 15th of March in lat. 4° 9′ N. It was a "small shark," so designated, and was taken by the harpoon. Tried immediately ; the temperature of the thick muscles of the back was found to be 82·5° ; the ventricle of the heart 82°. The air at the time was 79° ; the sea at surface 80·5°.

A third fish was taken the following morning at day-break ; this one by hook and line, and a considerable time elapsed before it could be drawn into the ship. It was about seven feet long. The thermometer in the thick muscles of the back stood at 81° ; the sea was 79·75°.

A fourth was taken on the 9th of August in lat. 2° 30′ N., long. 77° 53′ E. The thermometer placed in the heart rose to 82° ; in the thick muscles of the body it fell to 77°. The air and the water at the time were the same, 81°.

Relative to the low temperature of the deeply seated muscles in this last fish, 5° lower than that of the heart,—I remark in my notes that it was contrary to the former observations. This fish, it was conjectured, might have come rapidly to the surface from deeper and cooler water. The mode of capture in this instance is not mentioned. Whether a difference in the manner of effecting this, can have any effect on the temperature I am unable to say : it seems probable that it may : when taken with the harpoon, the struggle is commonly short ;—when with hook and line, especially if the fish be large, the struggle is longer protracted.

These several observations on the temperature of sharks shew how difficult it is to determine the true temperature of an oceanic fish.

Relative to the bonito, it seems strange, that there should be so great a difference as was observed between the temperature of the deeply seated muscles and the blood in the vena cava and the temperature of the heart; and only explicable on the idea that in the heart the temperature was rapidly reduced by the agency of the cooler water, that organ lying so near the surface and not protected from the loss of heat like the vena cava by the contiguity of thick layers of bad conducting substances, such as the muscles and viscera.

On the temperature of the salmon, the observations I have made have been limited to four fish, taken with the rod and fly, and that recently, viz. in September, 1862, in the river Crede in Lews of the Hebrides.

Of a male fish, the temperature under the liver, ascertained immediately, was 59°; in the heart, still pulsating, 58°; the river 56°.

Of other two salmon, caught when the river was 58°, the temperature was 60°, and of a grilse captured the same day, was 59·5°. In these three instances the thermometer was introduced into the wound made by the gaff in the thick muscles of the back, from which blood was exuding at the time.

The only observations I have to offer on the temperature of the dolphin (*Coryphæna hippurus*) was obtained on the same voyage as that on which the trials on the sharks were made. The following is a note of it:—"The thermometer inserted into the deep muscles of the back, just above the heart, stood at 74°. The fish was about three feet in length and was alive at the time, having been just caught, pulled suddenly on deck as soon as struck. This was in lat. 30° 25′; long. 21° 46′; the air 70°, the sea 71·25°.

2. *On the Temperature of the Tortoise* (*Testudo græca*).

In a former work, I have given the temperature as ascertained of three species of chelonidæ of a warm or tropical climate, in each of which it exceeded that of the atmosphere, and in the instance of the green turtle, was at one time as much as 8·5° in excess. The following observations were made on a tortoise, the inhabitant of a cool climate, and which as a hybernating animal, passes the cold months of winter in a torpid state.

The subject of the trial had wintered in a wooden house, under the ramp, a part of the fortification of Fort Pitt, Chatham, where it is probable that, during the hybernating time, the temperature of the air was often near, if not below, the freezing point.

On the 1st of March the tortoise first awoke from its winter sleep, thrusting out its head, and moving its limbs feebly. The thermometer in the open air was 50°; in the room (into which the tortoise had just been brought) 55°; introduced into the rectum about half an inch it fell to 48°. Portions of the carapace and plastron were now removed from over the thorax and abdomen. During the operation the animal was not roused, remaining in almost a torpid state;—its blood probably nearly saturated with carbonic acid. When the heart was exposed it acted in the feeblest manner;—there was not a single vigorous pulsation; its slight motion was most like the peristaltic motion of a portion of intestine: no motion was perceptible in the stomach or intestines. The thermometer, with as little delay as possible, placed in the blood flowing from the right auricle, rose to 50°; in the left ventricle to 51·5°. It was noticed that little blood was found in the heart and vessels;—that the red corpuscles were comparatively few in number, the coagulable lymph, the fibrin, minute in quantity and very tender. No fat could be detected in any part of the body. The lungs were collapsed. The urinary bladder was much distended with lithate of ammonia, partly of a soft consistence, and partly hard and gritty: it contained also a few drops of a brownish fluid, in which urea was detected. This abundance of lithic acid, it may be remarked, seems significant of the hybernating period, and reminds one of the like excretion which takes place in the chick *in ovo*, and in the chrysalis stage of insects in both which the functions in action sustaining life are maintained *ab interno*.*

* The tenacity of life of the tortoise is well known. The following is a remarkable example, which came under my notice at Malta in December, 1835. "A small specimen of *testudo græca* brought from Dalmatia was put into spirit of wine after having been under water twenty-four hours previously without apparent injury, though a land animal. To-day, after having been about forty-eight hours submerged in spirit, it was apparently dead; yet on opening it, the heart and muscles generally were found to be irritable and contracted when punctured." This note was made at the time.

3. *On the Temperature of the Toad* (*Bufo vulgaris*).

In the work already referred to, some observations will be found on the temperature of the frog, shewing how variable it is according to the condition of the animal and the circumstances in which it is placed. The same is true of another batrachian, the toad. I have found that when its skin has been dry, and the reptile active, its temperature *in ano* has been 1° higher than that of the atmosphere at 63°. On the contrary, when its skin has been more damp than usual, and the animal languid, its temperature has been from 1° to 2° below that of the air. The observations affording these results were made in the last week of August and the first of September, in the Lake-district.

That the temperature of the toad should not be higher than 1° above that of the air, and occasionally lower, is no more than might be expected, considering the comparatively small proportion of oxygen it consumes in respiration, and the cooling effect of evaporation. In illustration of the former, I shall state the results of a trial made with a toad just taken and vigorous. In volume it was equal to three cubic inches; placed in a receiver over water holding sixty-three cubic inches of common air, and taken out at the end of twenty-two hours, when not apparently the worse, it was found to have consumed less than double its volume of oxygen.

In another trial, a larger toad, one the volume of which was equal to five and a quarter cubic inches, confined over water in seventeen cubic inches of atmospheric air, was nowise apparently distressed till after sixteen hours: five hours later it was found dead.*

I shall mention a third trial: this was on a female toad in high condition in April. Confined in common air over water, beginning at 10.40 a.m.; at midnight it was still alive, breathing slowly. Lime had been added to the water to absorb the carbonic acid as it was produced in respiration: now, from absorption by the lime a considerable diminution of the con-

* After death there was a copious exudation of acrid creamy fluid from the cutaneous glands, which shewed no alkaline or acid reaction. Opened, large masses of adipose matter were found attached to its mesentery. The stomach was distended with food, chiefly millepeds of two kinds (*Julus complanatus* and *sabolosus*), some of them partially digested. The contents shewed an alkaline reaction; what remained of the millepeds effervesced with an acid. The cœcum was distended with dark, almost black fœcal matter, of very offensive smell.

tained air had taken place. The following morning the animal was found dead. The residual air proved to be nearly pure azote, barely a trace of oxygen could be detected in it.

These results, while they shew how small is the want of this reptile for oxygen, and the power it has of living in an atmosphere containing very little of this gas, equally demonstrate that like all other known animals, unless certain infusoria are exceptions, the presence of some oxygen is essential to its vitality; and, thereby rendering the popular accounts of live toads having been found imprisoned in solid blocks of stone, of coal, and of wood extremely improbable.

4. *On the temperature of the dormouse* (*Myoxus avillanarias*).

I have given the temperature of one hybernating animal, that of the cold-blooded kind; I shall now offer the few observations I have made on a warm-blooded hybernating animal, the dormouse, in which the connection between animal heat and respiration is displayed in a very striking manner.

On the 25th of March, at 8 a.m., when the temperature of my sitting-room was 55°, a dormouse, which I had obtained some days before, and was nestled in cotton-wool, as well as I can recollect, for there was no note made of its bed at the time, lay coiled up and motionless; there was no appearance of respiration. The bulb of the thermometer hid in its coil stood at 56°. Now stirred with the bulb, it moved its limbs, and a feeble respiratory motion became perceptible. After a while the thermometer rose to 66°. It was now put into the open air, outside the window, where the temperature was 44°; at the time it was slightly raining. It now opened its eyes partially, as if half awake, but did not stir its limbs. Presently the thermometer in contact with it rose to 72°. Taken back into the room, it was thoroughly roused into activity by pinching the skin of its neck and pulling its whiskers. The thermometer, its bulb buried in its fur, soon rose to 88°. Two hours later, when quite awake and active, the thermometer again applied rose to 99·5°. Later in the day, when it was lying motionless, coiled up, its head hid, its respiratory motion quick, but distinct, its temperature had fallen to 85°. The following night it was exposed to the open air in its box, well supplied with food; the thermometer during the time varied but little from 48°. The following morning at eight o'clock it was found very active; it had eaten a good deal of bread, and a portion of some paper which was in its box. Its temperature was 99°.

The last-mentioned observation, shewing the high temperature maintained, after an abundant meal, though exposed under otherwise depressing circumstances, might be adduced to illustrate the effect of food in sustaining animal heat. The want of such sustenance is strongly marked in young animals. I shall give the example of a brood of young dormice, four in number, which were taken from their nest on the 19th of July, and were brought to me the following day, nestling, each coiled up, in dry hay—it was in a hay field that they were found. After having been some hours in my room, the temperature of which was 60°, I found their temperature to vary from 62° to 66°; one was 62°, two were 64°, the fourth was 66°. They were not actually torpid; their eyelids were closed, they moved their limbs sluggishly, but could not stand. The thermometer in the trials made was pressed against the abdomen, and was encircled by the bent limbs. One of them tried with milk would not taste it. The day after, one of them died, and the next day the others.

When describing the tortoise awakening from its winter sleep, I offered the suggestion that its blood probably abounded in carbonic acid. Even the attitude which hybernating animals assume in their long rest seems favorable to this idea. From the admirable researches of Dr. Brown Séquard, it would appear that venous blood, which is most impregnated with carbonic acid, promotes most muscular action, and consequently the contraction of the limbs, the abductor muscles generally being more powerful than the extensor: hence, may it not be inferred, results the coiled up position of many of the animals in question, at the time of their hybernation, and the tenacity of the hold of others, as of the bat-kind, which hybernate in a pendant posture? And is not the same query applicable in an inferior degree to other animals, even when taking their ordinary rest? Most mammalia in sleep, by drawing up their limbs and letting fall the head place themselves in a position adapted best for the retention of warmth:—birds do the same, and many of them even in a more marked manner, burying their head when they sleep under their wing-feathers; and often, especially in cold weather, resting on one leg only, its claws with the firmness of a vice, owing to powerful muscular action, keeping hold of the perch, the other leg, owing to the same muscular force, being withdrawn from exposure. What an adaptation is thus displayed! What a harmony of means to an end! Diminished respiration, excess of carbonic acid in the blood, the hybernating state and sleep, contraction of limbs, and thereby the best possible attitude of body to check radiation and counteract the cooling influence of the atmosphere! Even in the fœtus *in utero* and in the egg—that external uterus as called by Harvey—may not the same cause be operative in conducing to the coiled posture of the body and limbs?

5. *On the Temperature of Birds of different ages and sex.*

The few additional observations which I have made since the publication of my Anatomical and Physiological Researches tend to support the commonly received conclusion, that the temperature of very young birds, like that of very young animals in general, is lower than that of adults, and the temperature of males somewhat higher than that of females.

A chick of the common fowl hatched in the morning (July 24, at Lesketh How), a very fine one, the tips of its wings scarcely yet dry, brought from the yard wrapped in flannel, was 104°. On the following day, when the air was 60°, the temperature of the same chick was 102·5°. The day after it had risen to 106°. Now, the provisional tip of its upper mandible was easily detached.*

The temperatures of four chicks of the same brood, taken on the same day as the last, were as follows:—One was 105·5°; another 105·75°; another 107°; the fourth 102°.

The temperature of a sitting hen, the parent of the preceding,

* This admirable provisional contrivance, by which the chick *in ovo* facilitates its exit, may be compared to the steel-pointing of an iron implement. I have examined one, taken from the chick above mentioned. It weighed $\frac{1}{100}$ of a grain; its hard point was dull white, nearly opaque. Under the microscope, using a one-inch power, the marginal portion appeared nearly transparent, dotted with granules, which increased in frequency towards the harder apex. Before the blow pipe it burnt with flame, leaving on the platina support a very minute residue,—distinct under the microscope, and which was dissolved by an acid, and precipitated by ammonia: whence it may be inferred to have been chiefly phosphate of lime. Another, put into a weak acid, did not effervesce perceptibly; indicating the absence of carbonate of lime.

taken from her nest for a few minutes, two days before the event of hatching, was 106·5°. Another hen, a full-grown one, at large, was 106°. The day after the leaving of her nest, the former was 107°. A full grown cock was 108·5°.

A young duck, taken from under the hen which had performed the office of incubation, was 102°. This was on the 6th of August.

On the 1st of August the temperature of a full-grown duck was 107°; of a drake 108°.

In the foregoing trials of temperature, the thermometer, which was a very delicate one with a very small bulb, made expressly for the purpose, and compared with a standard, was introduced into the rectum, and there kept till stationary; previously it had been warmed, so as to be about 100°, thus tolerably correcting any cooling effect from it.

The inferences from these results as to the care which should be taken to preserve the warmth of young animals are so obvious as not to require to be insisted on. Commonly Nature herself, ever provident, amply provides the means in the instance of birds and most mammals in the bad conducting covering of down or fine hair with which they are born, and in the instinct productive of motherly care which is imparted to the parent, and which is oftenest most strongly displayed in the most timid and defenceless animals.

6. *On the Temperature of different parts of the Bodies of Animals.*

On this subject, too, I have but few additional observations to offer, and these have been confined chiefly to birds.

At Malta, in December, when the temperature of the air was about 56°, the thermometer in the cloaca of a full grown turkey was 107°; in the folds of the skin of the neck, wrapped over the bulb, it was 103°.

Of a turkey cock, tried as soon as possible after death, the temperature in the pharynx was found to be 99°; in the stomach 109°; beneath the pectoral muscle 108°; the observations were made in the order mentioned.

In the cloaca of a common fowl, tried the same day as the first mentioned turkey, the temperature was 108°; under the feathers of neck 104°; the bill was cool.

At Fort Pitt, Chatham, on the 17th of November, the temperature of two pigeons was ascertained, an old and a young one, first *in ano*, next in the brain. The calvaria was divided with a portion of brain by a strong scalpel; death instantly followed without convulsions. In the old bird *in recto* the thermometer was 109°; in the brain 106°; in the young bird fully grown, in the former it was 110°; in the latter 108°. It is noted that, in the trial on the first, the scalpel and thermometer were used cold; but in that of the second, warm, of 105°, taken from water of that degree. It is also noted that blood flowed freely from the parts incised of the brain.

In the first volume of my Researches, many observations are to be found on the temperature of the brain of sheep, tending to prove, that whilst it differs 2° or 3° from the deeply seated parts abundantly supplied with blood, such as the heart, lungs, and liver, it varies but little from that of the rectum.

It is admitted that intelligence depends on cerebral action. Is it not a problem worthy of consideration, whether temperature has any modifying influence? In the instance of man, those races who clothe the head most, such as the Orientals, are not preeminent for intellect; nor, I believe, are those animals, especially birds, which have the longest necks and least feathers on their heads, most marked for their stupidity.

Conclusions.

In all the observations on the temperature of animals, it is interesting to see the constant relation which exists between the degree of animal heat and the quality and quantity of the circulating blood, and the force exerted by the heart by which this fluid is propelled.

In birds, of all animals those possessed of the highest temperature, the circulation is especially vigorous; the proportion of red corpuscles large, as indicated by the high specific gravity of their blood;* and thus, it may be, compensating for the quantity of this fluid being, as I believe, less in them than in most of the mammalia. In birds, I fancy we see the most perfect economy of means, and were we to speak of mechanism,

* The specific gravity of the blood of the common fowl I have found to be 1,064; of a turkey 1,061; of a goose 10,589.

the highest, as it were, invention to accomplish the ends in view with the least expenditure of force. Though their temperature is so high, it seems very doubtful that in them more carbonic acid gas is formed in respiration than in mammalia of the same size, but `of lower temperature. Their natural covering of feathers, such bad conducting substances; the small expenditure of heat from surface-evaporation, as denoted by the dryness of their integuments; the little loss of heat from their ejecta, both their urine and fæces being all but solid, may with the perfection of their blood and their powerful heart, tolerably account for their distinctive high temperature irrespectively of greater functional respiratory action.

In the mammalia, we have blood, somewhat less rich in red corpuscles than in birds, and of somewhat lower specific gravity, the blood itself, I believe, in the former being more abundant in quantity, with a force of heart also inferior, but adequate to a vigorous circulation, and a temperature generally, though lower than that of birds, only a few degrees lower; their respiratory apparatus being more complete, if we may so say, and their consumption of oxygen as large, if not proportionally larger.

In reptiles the theory becomes more distinct, less open to doubt and objection, as it is also in part in the hybernating mammalia. As the proportion of the red corpuscles diminishes in reptiles, and as the heart's action becomes languid, so is their temperature lowered, and *vice versâ*, ranging only a very few degrees above or below that of the atmosphere, and in about the same ratio is their consumption of oxygen.

In fishes we find the same relation tolerably established between the richness and poverty of the blood of different species and their temperature. Thus in all the cartilaginous fish, so far as our knowledge extends, the red corpuscles are comparatively few, the specific gravity of the blood low,* its quantity small, and their temperature correspondingly low: whilst in certain osseous fishes of the tunny-family, the red corpuscles are plentiful, the specific gravity of the blood high, its quantity considerable, and their temperature proportionally elevated.

* See for specific gravity of the blood of the tunny and of a cartilaginous fish, pp. 2 and 3. The blood of the holibut (*Hippoglossus vulgaris*) I have found to be 10,285; its serum 10,155; the blood of the plaice (*Platessa vulgaris*) 10,33; of the salmon (*Salmo salar*) 10,366.

Whether the nucleated condition of the blood-corpuscles in birds, reptiles, and fishes is any wise concerned as favoring their function as absorbents and carriers of oxygen is, as before remarked, open to question. On the supposition that the corpuscle with its included nucleus is a galvanic kind of combination, it seems not improbable that, so formed, it may be more energetic than the corpuscle without a nucleus in the instance of the mammalia.

There is a scope for question too as regards the influence of nerves in connection with respiration. The lungs of birds, as far as I have been able to ascertain, are but scantily supplied; in this respect approximating to reptiles. The warm blooded fishes, if I may so call the scomberidæ, on the contrary, have their gills amply supplied, and in the instance of the tunny, almost rivalling in size the nerves of the special organs of the electrical fishes.

The subject of animal heat is confessedly a most difficult one, and in its production, there seems reason to believe that many agencies are concerned. But, do they not all involve the consumption of oxygen and mainly the formation of carbonic acid; so that the quantity of this acid gas given off in respiration, whatever the class of animals, is the best criterion of the calorific process.*

* It may be a question whether the formation of lithic acid, so abundant in the urinary excretion of birds, is not concerned in the production of animal heat: the quantity of this acid produced in the fœtal bird, seems favorable to the idea that it is in some degree operative.

VI. ON THE INFLUENCE OF SLIGHT CHANGES OF TEMPERATURE ON BUTTERFLIES; IN A LETTER ADDRESSED TO WILLIAM SPENCE, ESQ., F.R.S., ETC.

My dear Sir,—It was from you I learnt that no exact thermometrical observations had yet been published on the degrees of temperature at which hybernating insects, or those having properties analogous, pass from a torpid to an active state, and consequently, that a record of any such observations, made with exactness, would not be devoid of interest.

The observations which I have hitherto made on this subject have been chiefly confined to two species of butterfly, viz. *Vanessa Urticæ* and *Io*. These I shall briefly relate.

The first-named butterfly I found active within doors in a window on the 18th of March, 1850, when the weather was unusually mild for the season. It was in untarnished beauty, as if fresh from its puparium. It was placed in a thin glass vessel, and lightly covered with paper, so as to prevent its flight, and yet allow of a sufficient access of air. Thus confined, it was put into a dark cupboard, the temperature of which, even when there was a fire in the room, was below 60° Fahrenheit. It remained alive about a month, and during that time it was observed almost daily, and occasionally oftener,—two or three times in the same day, and its place changed. The following are the only notes that were taken down, shewing the effects of changes of temperature, in rendering it active from being torpid, and *vice versâ*.

April 11th.—Since first taken it has been found torpid at 58°, as if dead, shewing no indications of life even when shaken; at 64°, or thereabouts, it has become active, and that even when brought from darkness into an obscure light,—the mere degree of light, apart from heat, seeming to have little effect: replaced where the temperature was about 58°, and observed an hour or two afterwards, it was found to have resumed its torpid state.

April 14th.—Found it standing, risen from having been recumbent on its side, but not active; temperature of the dark cupboard then 59°.

April 18th.—At 60° it was torpid: it seemed indeed dead, and shewed no signs of life till the temperature was raised to about 75° by placing it on a stove; after a few minutes at this temperature it exhibited marks of languid life, by a tremulous motion and partial opening of its wings. Two days later it was found dead.

Another butterfly of the same kind was found active on the 28th of April, at a temperature of 62°. It became torpid at 56°.

May 1st.—It was found torpid at 58°; it became active at 63°. On the following day it was not roused by a temperature of 95°; it was dead.

A *Vanessa Io* found torpid on a garden walk, when the temperature on the 29th of April (the morning of the day on which it was found) must have been below 40°, was placed under the same circumstances for observation.

On the 2nd of May it was torpid at 59°: after ten minutes at 66° it became active.

May 4th.—Torpid at 62° in the dark; after a few minutes' exposure to a temperature of 67° in light it became active.

May 11th.—Torpid at 57°; slightly active at 63°; became again torpid by a reduction of temperature to about 60°.

May 13th.—Torpid at 57°; on exposure to a dull light at 61° it rose on its feet, before recumbent on its side, shewing when thus standing only slight marks of vitality; yet in a few minutes, after gently touching its antennæ, and breathing on it, it became pretty active.

May 15th.—Torpid at 58° in the dark; became active in a few minutes at 62° in a dull light, the sky being overcast. Two days after it was found dead at a temperature of 59°, its wings expanded, seeming to denote that it had not died in a torpid state.

In describing the above observations I have used the word torpid rather than a state of sleep, from the belief that the butterflies, the subjects of them, were, when motionless, not under the influence of sleep, but of that kind of torpor to which certain animals are subject in their hybernating condition,—one in which the vital functions are all but suspended, and in consequence the corporeal waste is very small,—permitting retention of life without the use of food,—the object, no doubt, for which the hybernating faculty is by nature intended. All that came under my notice in the instances of these butterflies seemed to accord with this view, such as the rapid transition from activity of organs to rest, and that so perfect as to simulate death on reduction of a few degrees of temperature; and the length of time life was sustained without the support of food, that is, compared with the short time insects of the same family live at a high and uniform temperature in a state not of torpor, when confined and deprived of food: thus, in Barbados, butterflies so confined I have commonly found dead in two or three days.

It is my intention, if leisure and opportunities permit, to prosecute the subject further, and to make some experiments on the effects of gases not capable of supporting life on such insects as seem to have the power of hybernating. Should the results be at all decisive, I shall have pleasure in communicating them to you, to do with, like the present, as you may think best.

I am, my dear Sir,
Yours very truly,
J. Davy.

VII. ON THE EFFECTS OF CERTAIN AGENTS ON INSECTS, IN A LETTER ADDRESSED TO WILLIAM SPENCE, ESQ., F.R.S., ETC.

My dear Sir,—In a letter which I had the honor to address to you last year relative to the effects of change of temperature on insect life, in a very limited way, I expressed the hope of being able to continue the experiments and extend them to other agencies on the same class of beings. This I have in some measure accomplished, and I propose now to make known to you the results, which be pleased to use in any way you may think proper.

The enquiry was entered on in the last week of November, 1850, and owing to the extraordinary mildness of the winter months has been continued almost uninterruptedly to the present time, viz. the middle of March. During the whole of this period the thermometer here has never for twenty-four hours been below the freezing point; it has oftener, by day, been above 40° of Fahrenheit than below that degree. This mildness of the atmosphere has been accompanied with unusual humidity and an extraordinary fall of rain. In November the amount of rain here was 13·26 inches; in December, 5·46 inches; in January, 19·54 inches, as measured by the pluviometer; and scarcely a day has passed during the whole of the period that insects of several kinds have not been seen on the wing, or abroad in the open air. For the sake of order, and to be better able to compare the results, I shall first notice those experiments which I have made on the effects of temperature on insects; next, of gases; and lastly, of vapors. Should my account of the trials be found tedious, I trust it will be borne with, as without some minuteness of detail, accuracy on such a subject is hardly attainable. For the names of the species, when they are assigned, you know that I am indebted to an able entomologist, Francis Walker, Esq., who, at your request, was so obliging as to examine most of the individuals, the subjects of the experiments, and to return them labelled.

1. *On the Effect of Changes of Temperature on certain Insects.*

I shall first notice the effect of reduced temperature, that is, of a reduction of many degrees below the annual mean, and also below the freezing point.

On the 28th of November exposed a bee (*Bombus hortorum*) in a languid state as to activity, in a room, the temperature of which was 54°, to the open air of the temperature 32°; it immediately became more active, endeavouring to get from under the glass which confined it, even spreading its wings and attempting flight. How long it remained thus active I did not stop to watch. Left over night, it was found the following morning quite torpid, as if dead. A register thermometer that had been placed beside it on the grass indicated 25° as the minimum. After having been brought into a room of the temperature 52°, in about half an hour it revived, and, when touched, feebly moved its legs and walked. The same bee, on the morning of the 30th, at a temperature of 48°, moved its limbs slowly when touched. Exposed to the open air at 22°, rising in the course of the day to 28°, it soon became completely torpid, moving no part when touched. It revived as before at a temperature of 56°.

A fly (*Musca vomitoria*), on the 8th of December was active, flying about within doors, at a temperature of 52°; it became dull and averse from motion at 40°, and more so at 33°, at which temperature it did not move till touched, and then sluggishly. Even at 28° it was not torpid, and that after exposure during the night to a temperature of 23°, as indicated by a register thermometer. On the following day it was exposed under the same circumstances, in company with a bee (a *Bombus*), to the open air at 28°; the bee presently became torpid, the fly not, moving its legs languidly when touched. The bee in its torpid state bore a temperature of 22°: it remained motionless at 30°; for two hours it did not revive at 54°. When placed upon the warm hand and breathed upon, it presently began to move one of its legs, and shortly after another, and in less than a minute it was walking on the hand. Removed from the hand, in a few seconds it became motionless, shewing no signs of life even when touched, and this at 54°. Replaced on the hand, it again became active, but not so soon as before; its hind legs first moved. The movement was that of passing one over the other, as in the act of cleaning them; next the fore legs were moved in the same manner, the wings remaining motionless. Now, removed from the hand, a state of torpor was induced in a few seconds.

A fly (*Musca stabulans*), found within doors on the 13th of December, exposed to the open air at 41°, became motionless, except when touched, when it was tolerably active: even at 54° it appeared to be in the same state, not torpid, moving only when touched, and excited to motion by the gentlest touch. At 31° it became quite torpid, so that when roughly moved it shewed no signs of life. It was exposed to a temperature of 22° without fatal effect.

A small fly (*Piophila casei*), pretty active in a window at 44° on the 19th of December, did not become torpid at 38°, moving its legs when touched, nor at 33°; but at 31° torpor was produced, and it bore without fatal effect a reduction of temperature to 22°. After this it was active at 36°, and flew away when let into the open air in the shade.

On the 23rd of February exposed a fly (*Anthomyia mitis*) to a night temperature of 25° (the lowest). The following morning, at 7 a.m., it was languid at 26°, moving its legs sluggishly when touched. On the following night it was again exposed to a temperature of 22°. When taken out of the tube, which was covered with hoar-frost, it was motionless for several seconds, but in a minute or two it moved its legs when touched, and brought into a warm room it soon became active.

A honey bee (*Apis mellifica*), active in the open air in sunshine on the 9th of March, became motionless at 40° in the shade. In a room at 55° it moved its legs and wings languidly; some hours later at night, at 51°, it was found torpid; breathed on, a languid movement of its wings was reproduced; placed where the temperature was 65°, it presently became active. The following morning at 50° it was found motionless; breathed on, on the warm hand, a feeble revival was indicated by a slight motion of one of its antennæ, and occasionally of one of its legs. In this state, as it were between life and death, it continued during five days, at a temperature between 50° and 60°, at the end of which it ceased to give any signs of vitality, the same means being employed to excite it.

These are all the trials I have made on the influence of reduction of temperature. They are smaller in number than I could have wished, owing to the want of opportunities from the extreme mildness of the season, as already adverted to; and even in the few instances in which the reduction of temperature was below the freezing point, it may be worthy of remark that it was owing, not so much to the coldness of the atmosphere as to that of the surface, the effect of radiation from the grass plat on which the insects were placed during clear and calm nights, of which, for the time of the year, there were unusually few.*

On the effect of elevation of temperature, to which I shall now proceed, I have also but a few results to describe.

On the 19th of January exposed a fly (*Anthomyia mitis*), in a tube of thin glass to a temperature of 126°; in two or three minutes it was motionless, and was found to be dead. The same effect, in as short a time, was produced on another fly of the same kind by a temperature of 113°. On the 28th of January exposed another, also of the same kind, to a temperature gradually rising (the tube being immersed in hot water) to 96°. After about an hour the fly, that was at first active, was found lying at the bottom of the tube, seemingly incapable of standing. In another minute it ceased to move, and did not revive on exposure to the air of the room. Another of the same kind bore well a temperature of between 80° and 90° for two hours; after exposure to a temperature between 90° and 100° less than an hour, it was found dead.

A fly (*Musca lanio*), on the 30th of January, was exposed to a temperature between 80° and 90°, and 90° and 100°, for several hours; so long as the air in the tube was below 100°, the fly did not appear to suffer; above 100°, it shewed a tendency to become torpid; at 105°, after a very few minutes, it became motionless, and did not revive on exposure to the air of the room.

On the 16th of March, a honey bee, just taken from the open

* Since the above was written I have made a few additional experiments on the exposure of insects to cold. Some small flies bore a reduction of temperature to 19°, others to 18°, without loss of life. They were active at 55°. The time was February. Not having had their species determined, I need not give the details of the trials. The same flies, I may mention, also sustained with apparent impunity exposure to the drying effect of strong sulphuric acid for more than 24 hours, having been suspended over it in a muslin bag in a stoppered bottle.

air at 45°, when in sunshine actively on the wing, was exposed to a gradually increasing temperature. Its activity was not apparently diminished at 80°; at 90° the sound of its wings had ceased: it was at rest at the bottom of the tube, but in nowise torpid. After a few minutes, at 96°, it became motionless; then taken out and exposed to the air of the room, at 55°, it did not revive; no part of it afterwards moved.*

What is known of the habitats of insects in relation to climate, I need hardly remark, amply proves the vast range of temperature at which different species can exist, each probably restricted to a certain range; and consequently that a very large number of trials on different species would be required to arrive at any satisfactory conclusion on the influence of temperature on insects generally. One remark I beg to make, in which I think I am warranted by my limited experience, as to the effect of change of temperature, viz., that on insects it is different from what it is on hybernating mammalia; not, as in them, producing a quick transition from active to torpid life, but more commonly a graduated one from great activity to diminished, till motion is altogether lost; and this gradation, whether in becoming torpid from cold, or in recovering from a state of torpor on elevation of temperature.

2. *On the Effects of certain Gases.*

Carbonic acid gas.—On the 19th of December a fly (*Musca stabulans*) was placed in a tube in which this gas was in process of being rapidly generated by the action of dilute nitric acid on fragments of limestone. The fly was supported by a perforated diaphragm, so as to be kept above the fluid, and the tube was corked, but not so tight as to prevent entirely the escape of gas, and immersed in water, so as to exclude the admission of com-

* On the effect of elevated temperature I have also made a few more experiments, The subjects of them were put into a thin glass tube and plunged into heated water. Two small flies which had borne a cold of 25° the preceding night were immersed in water at 148°, taken out after half a minute. They were motionless, and so continued for several hours. The next day they were found moving feebly. Other two flies were left in a minute at 110°; taken out they speedily revived from a state of torpor. Returned into the tube and water, the latter falling in 25 minutes to 103°, now taken out, one was tolerably active, the other very feeble, just moving its legs, but in a few minutes it recovered so as to be able to walk. Again replaced and left in an hour and ten minutes, the temperature falling from 102° to 94°, both were active when taken out.

mon air. In a few seconds the fly became motionless: taken out after a few hours, it seemed dead; it did not move even when placed on the warm hand, and breathed on for a minute or two. Soon after, the breathing on it being continued, a slight tremulous motion was perceived in its feet, and in a few minutes more decided animation was restored, and it moved when touched, and walked.

December 21st, a fly (*Heteromyza buccata*) was put into a tube full of carbonic acid gas. It became instantly motionless. Kept in the gas no longer than two minutes, it revived after being taken out after about five minutes, and seemed not less active than before. Replaced in the gas after an interval of twenty-four hours, in a few seconds it became motionless; left in the gas another twenty-four hours, it was found dead when taken out.

On the same day a small fly (an *Anthomyia*), confined in a wine glass inverted on a plate of glass, was so active (the temperature of the room 56°) as to fly from side to side. Introduced a bit of limestone, and added a little nitric acid; the fly at the same time was standing on the side of the glass. In two or three seconds, without making any efforts to escape, it lost its hold, and fell into the effervescing fluid, where it was motionless. Taken out after about two minutes, in two or three minutes more it began to move its legs, and shortly was tolerably active considering its clogged state from its wet wings and legs.

On the 25th of December put a fly (*Musca lanio*), in an active state at the temperature of the room, into the tube with carbonic acid gas. It immediately became motionless. After about three or four hours' exposure to the gas, on being taken out and placed on the warm hand and breathed upon, a slight motion of the legs was perceived, but this only for a minute or two, when all marks of life ceased.

On the 29th of December, placed two flies, both of them pretty active (a *Heteromyza buccata* and a *Musca lanio*), in carbonic acid gas. They became motionless in a few seconds; left in about an hour; the former did not revive immediately on being taken out, nor even when placed on the warm hand and breathed upon; yet, in half an hour it was nearly as active as before. The other fly, similarly treated when taken out, revived in less than a minute on the warm hand. Again immersed in the gas, and now left there about twenty hours, both were found to be dead when taken out.

On the 29th of December, repeated the experiments on two flies, similar to the preceding, with a like immediate result, they becoming motionless in two or three seconds. Taken out after two hours, they did not recover immediately even when placed in the warm hand and breathed upon; in ten minutes they had revived, and were much in the same state as to activity as before immersion; the one alert (the *Heteromyza*), the other sluggish. Thrown again into the gas, they instantly became motionless; not the slightest movement even of the legs could be perceived. Had the air in the tube been common air, they would have been excited by the fall; taken out instantly, they presently revived. Returned into the gas on the following day, and taken out after three hours, both were found motionless; the *Heteromyza* after six hours was found active, the other fly was motionless, not moving even when touched; nine hours later it was revived and active.

On the 9th of March, placed two honey bees, rather languid at 55°, in the tube in which carbonic acid was in the act of being disengaged; common air was mixed with the gas. They at first became excited and active, used their wings, as if trying to escape; gradually and pretty rapidly, their activity diminished, and in less than a minute they both dropped down on the diaphragm motionless. Taken out after remaining in about a quarter of an hour, they were motionless for about an hour and a quarter; now breathed on, they began to move, and shortly, at a temperature of 65° to which they were transformed from one of 51°, they became active.

Hydrogen gas.—On the 30th of December immersed the same flies, which had revived after exposure to carbonic acid gas on the preceding day, in hydrogen whilst in the act of being generated by means of zinc and dilute muriatic acid. They did not become instantly motionless, but they were so in less than a minute. Taken out after being two hours in the gas, the *Heteromyza* revived in about a quarter of an hour; the other remained motionless about three hours, then, when touched, it moved freely.

On the 10th of March made a similar experiment on a honey-bee. As the hydrogen, from the expulsion of common air from the tube, became predominant, the bee became restless; in two or three minutes it fell down, and after a tremulous movement of its limbs for a few seconds, it ceased to move. Taken out after being in five minutes, it exhibited in a few seconds marks of reviving, its limbs moving in the same tremulous manner as that observed previously to its becoming torpid. On the following day it was alive and tolerably active at 55°, as if nowise injured by the gas.

Azote.—Put a fly (*Heteromyza buccata*) into this gas, obtained from common air by the removal of the oxygen by phosphorus, after which removal it was allowed to stand twenty-four hours over water. The fly in coming in contact with the azote immediately fell from the side of the tube, and, except a slight motion of one of its legs, it appeared to have been rendered instantly torpid. Taken out after two hours, it revived in about ten minutes, its activity nowise impaired.

Common air.—Put a fly of the same kind as the preceding into a tube with common air confined by a little cotton-wool, in a room where all the indoor experiments were made, varying in temperature between 52° and 60°. From the 5th of February, when it was introduced, till the 26th, it seemed little affected; on the 28th, it was found dead.

Put a fly (*Musca lanio*) into a tube, with a small quantity of air, a few times its own volume. This was on the 4th of January; on the 5th it was languid; on the 16th it was motionless. About two hours after being taken out it shewed marks of vitality, and was soon tolerably active. Hardly an appreciable quantity of oxygen, by the test of phosphorus, was found in the residual air; most of it had been converted into carbonic acid.

On the 19th of January put three flies of the same kind as last mentioned, pretty active at 45°, into a tube full of water, and inverted it in water; a little air adhered to their wings. Shortly after they were motionless, the temperature of the room 56°. In about ten minutes after being taken out they revived, and when dry, they appeared to be as active as before.

Oxygen.—Introduced a fly of the same kind into this gas, over water obtained from the decomposition of chlorate of potash. Though swimming with its feet and part of its body immersed in the water for twenty-four hours, its activity was no wise impaired: a few hours later it was found under the water and motionless; on exposure to the air of the room it did not recover.

On the 27th of January introduced another fly of the same kind into oxygen. Twenty-four hours after, it was standing dry on the side of the tube, as if in common air, and in like manner on the 31st. On the 1st of February it was found swimming on the water; by inclining the tube it got out of the water, attaching itself to the side of the glass; on the following day it was found motionless; it did not revive on being taken out. There was a slight diminution of the volume of the oxygen, many times the volume of the fly.

Coal gas.—Put a fly (*Heteromyza buccata*) into a tube half full of water, above which was common air, and introduced a little coal gas as it was generated by the action of heat: no sooner had a few bubbles come in contact with the fly floating on the surface of the water, than from a state of activity it became motionless. Taken out without loss of time and exposed to the air, it revived in about ten minutes. The same fly, exposed to the action of the gas nearly pure, became motionless in two or three seconds. Taken out after about a minute, it gave no signs of life for half an hour; three hours later a feeble movement of its legs was perceptible, soon terminating in death.

Sulphuretted Hydrogen.—On the 31st of December immersed the same flies that had been exposed to the action of hydrogen on the preceding day, in sulphuretted hydrogen as it was slowly disengaged in a tube by the action of dilute muriatic acid on sulphuret of iron. After a few seconds both flies were motionless, and they did not recover when taken out, and that immediately. The effect was the same on another *Heteromyza buccata*), and as rapidly.

On the 1st of January immersed a fly (*Musca hortorum*) in a mixture of sulphuretted hydrogen and common air: in a few seconds it became motionless, and though instantly taken out it did not revive.

On the same day repeated the experiment on a *Heteromyza* and with like effect: the air in the tube consisted of about two parts of common air and one of sulphuretted hydrogen.

On the same day introduced into the tube in which the gas was in process of being generated, two flies (*Trichocera hiemalis*, very active, *Musca vomitoria,* less so). After about an hour both were motionless, nor did they revive when taken out. So small was the proportion of sulphuretted hydrogen, that when an attempt was made to ascertain it, it was hardly appreciable.

On the 20th of March introduced an active honey bee just taken, into a mixture of sulphuretted hydrogen and atmospheric air. The very instant it entered it dropt motionless; and not the slightest motion of any part was seen afterwards. Taken out, after being in the tube ten minutes, it did not revive on exposure to the air. The same day repeated the experiment on another honey bee; after a few minutes of restlessness it fell down motionless. Taken out, after about eight minutes, and exposed to the air, in about half an hour a languid motion of the abdomen was visible, and the same was occasionally seen to occur for about thirty hours, soon after which time it was found dead. The proportion of sulphuretted hydrogen mixed with the atmospheric air in this instance was very minute. The experiment repeated under the same circumstances with another active honey bee, the result was similar; the state of torpor, the effect of the very dilute gas, on exposure to the air was followed by slight signs of revival soon ending in death.

Sulphurous acid gas.—On the 24th of January exposed a fly (*Heteromyza buccata*) to this gas very much diluted, by kindling a minute portion of sulphur under a wine glass, in which the fly was confined. In a few seconds it tottered and fell, and in a few more it became motionless: it did not recover on exposure to the air.

Chlorine.—On the 22nd of January exposed a fly of the same kind as the last to this gas, as disengaged in the tube by the action of dilute sulphuric acid on a mixture of common salt and black oxide of manganese. In less than two minutes the fly became motionless. Taken out immediately and exposed to the air, it did not revive.

3. *On the Action of Vapors.*

Ammonia.—On the 3rd of January put a fly (*Heteromyza buccata*) into a tube with a few drops of strong *aqua ammoniæ.* At first it was active and restless; in about half a minute it fell down, moving its legs; in another half minute it was motionless.

It had not come in contact with the fluid. Taken out it did not revive.

Exposed another fly (*Musca lanio*) in the same manner to the ammoniacal vapor; it presently exhibited convulsive movements; in less than a minute it was motionless, and it remained so for several hours after exposure to the air; the following morning it was found revived and active.

Muriatic acid.—Exposed a fly of the same kind to the vapor of strong muriatic acid: during the first half hour the fly was active; it gradually became less so; after about two hours it was found motionless. Taken out of the tube, after shewing no signs of life for an hour or two, it revived and recovered its activity. Replaced and kept exposed to the acid vapor for twenty-four hours, it was found dead when taken out.

A fly (*Piophila casei*) was put under a small wine glass on a glass support, and with it a portion of cotton-wool moistened with the acid. In less than a minute the gait of the fly became tottering; and in less than five minutes more it was motionless. It was immediately taken out, but it did not revive. A similar experiment was made on another fly (*Heteromyza ?*), somewhat larger and more vigorous. After about a quarter of an hour it became motionless. Then, taken out, in a few minutes it became pretty active.

Nitric acid.—The results of two trials with the vapor of this acid, one on a *Piophila*, the other on a *Heteromyza*, differed chiefly from those last mentioned in being in each instance fatal, exhibiting after deprivation of motion no sign of revival, though taken out immediately.

Alcohol.—Put a fly (*Musca lanio*) into a tube with a little alcohol of sp. gr. ·84 underneath, and raised its temperature to 74°. For a few minutes the fly shewed increased activity; in a few more it became nearly motionless; after about a quarter of an hour it appeared to be torpid. Now, exposed to the air of the room, in a few minutes a slight motion of its feet was seen; after a couple of hours it was nearly as active as before the experiment: two hours later it was found dead.

On the 8th of January, temperature of room about 58°, placed two small flies (*Sciara vitripennis* and *Psychoda nervosa*) under a wine glass on a glass support, with a portion of cotton-wool moistened with alcohol. The first effect was an increase of activity;

after a few minutes their irregular movements and occasional falls suggested an inebriated state. A drop of alcohol poured on them rendered them motionless instantly, and proved fatal to them.

On the 11th of January a similar trial was made on another fly (*Heteromyza buccata*), at a raised temperature of 70°. The excitement first produced, marked by increased activity, was soon followed by feebleness, irregular movements, and death. Similar effects were produced by the vapor of alcohol in another fly of the same kind at the temperature of the room 57°, but not quite so rapidly.

Ether.—On the 12th of January placed a fly (*Musca lanio*) in a tube with a little sulphuric ether, at the temperature of the room 56°. It immediately became motionless. Taken out after a few minutes, it soon revived, and seemed as active as before. Replaced in the tube after an interval of two hours, it did not become torpid so soon as at first, two or three minutes elapsing. It revived in about a quarter of an hour after being taken out.

Repeated the experiment on another fly (*Heteromyza buccata*), at a temperature of about 54°. In a few seconds the fly dropped from the side of the glass motionless; so it continued till taken out. When taken out it was found to be dead. On another fly of the same kind, the immediate effect was the same; taken out in less than a minute, in about an hour it revived, and was active as before.

Chloroform.—On the 14th of January, at the temperature of the air of the room, placed a fly of the same kind as the last under a wine glass, with a portion of cotton-wool moistened with chloroform. For a second or two the insect was active, then it suddenly became motionless. Taken out, it was tolerably revived in an hour.

Repeated the experiment on a small gnat, with a like effect in every respect. Repeated on the fly and the gnat, keeping them in the vapor about eight minutes; they soon became motionless as at first, but they did not revive on exposure to the air.

Camphor.—A fly (*Heteromyza buccata*) put under a wine glass at the temperature of the room, with a piece of camphor, became torpid in about half an hour; after exposure to the air for about the same time it revived, moving sluggishly. Replaced and left over night under the glass with the camphor, on the following morning it was found dead. In another experiment with a fly of the same kind, in rather less than half an hour the activity of the fly was diminished. Some five hours later it was motionless; it did not revive on exposure to the air.

Oil of Turpentine.—On the 20th of January placed two flies of the same kind as the last under a wine glass, with a portion of cotton-wool moistened with this volatile oil. In a minute or two they walked unsteadily, as if intoxicated; in less than a quarter of an hour they were found motionless; they had become smeared with the oil. They did not revive on exposure to the air.

Another fly of the same kind placed in a tube with a portion of oil, separated by a diaphragm pervious to the vapor, at a temperature of about 75°, soon became motionless, and did not recover when taken out. Repeated the experiment on another fly of the same kind, at the temperature of 54°. The immediate effect was the same. In a few seconds it had lost the power of walking on the perpendicular side of the glass tube, and was more restless than before. Accidentally a small portion of its surface came in contact with the oil; in less than half a minute it became motionless. Immediately taken out, it did not revive. The experiment repeated on another fly of the same kind, not allowed to come in contact with the oil, it became motionless in a minute, after having been instantly affected in the same manner as the preceding. Taken out after about two minutes, it did not revive.

Strong Acetic Acid.—A fly (*Heteromyza buccata*) exposed to the vapor of this acid at about 56°, was not immediately apparently affected, nor after five hours; after ten hours it was found motionless. It did not revive when taken out. The trial was repeated on another fly of the same kind with a like result, and also on a gnat. In about five minutes the gnat became motionless, and when taken out was found to be dead. Repeated on another fly (*Heteromyza buccata*), in about half an hour it ceased to move; allowed to remain in about two hours, it did not revive when taken out.

Attar of Roses.—Placed a fly (*Heteromyza buccata*) under a wine glass, with a portion of paper on which a drop of this essential oil had been let fall; the temperature of the room 54°. At first the fly did not appear to be affected; in about an hour it had become dull, moving languidly; five hours after it was found motionless. Taken out, it shewed marks of revival in about half an hour; after three hours it had recovered its activity. Repeated the experiment on a small fly, a gnat. After an exposure of about two hours it was found motionless; taken out it did not recover.

Musk.—Placed a fly (*Heteromyza buccata*) under a wine glass with a portion of this substance, adding a few drops of water to favor the rise of the odorous effluvia. After twelve hours no appreciable effect was produced: the activity of the insect was not impaired. Repeated the experiment on a gnat and on another fly, the species of which was not determined, and with a similar result in each instance; one was exposed to the odour for twelve hours, the other—the latter—for twenty-four. This insect, on lifting up the glass, took wing and escaped.

Iodine.—Exposed a fly (*Musca domestica*) to the vapor of this substance at 70°. The air in the tube was only just perceptibly colored. During the first half hour the fly was active and seemed little affected; after another half hour it was found motionless. Whether it revived or not is not mentioned in the note of the experiment. The experiment was repeated, at the temperature of the air of the room 55°, on two flies (*Heteromyza buccata*); one, which was feeble when put into the tube, became motionless in about a quarter of an hour; the other, in the same time, excepting that it had become dull, seemed but little affected. In about an hour its gait was unsteady, as if its muscles were no longer under control; after four hours it was found motionless. Neither of them revived.

On the results described in the two last sections I beg to offer in conclusion a few remarks.

Comparing the effects of the several agents, no two seem to have acted precisely in the same manner; and probably were a larger number of experiments made, and with minute attention, each agent would be found to possess some peculiarity in its influences. Those most fatal to life appear to have been sulphuretted hydrogen, ammonia, chlorine, nitric acid, iodine, camphor, and oil of turpentine, each varying in degrees of rapidity of effect, but so far analogous that no perfect revival ensued on exposure to the air after a motionless state had once been induced. Those less fatal to life appear to have been azote, hydrogen, carbonic acid, coal gas, muriatic acid-vapor, ether, and chloroform, all of them producing immobility, and probably insensibility, with different degrees of rapidity, but not commonly terminating in death, revival in most instances following on exposure to the open air. Whether oxygen belongs to either is more than doubtful; it seems to stand alone as regards its effect on the functions of life throughout all classes of animals. That death sooner occurred in the trial with it, than in that with atmospheric air, may have been owing to exhaustion connected with increased vital action of the insect, unsupported by nourishing food.

How the effects of the several agents are produced, it may be difficult to explain in the present state of our knowledge. Some of them probably act chemically in suspending respiration, or in preventing the due aëration of the fluids, such as azote, hydrogen, carbonic acid; and others, it may be, in a more complicated manner, and in part through their action on the nervous as well as the respiratory system, such as sulphuretted hydrogen, ether, chloroform, alcohol, oil of turpentine, and camphor.

It is worthy of remark that most of the substances which in minute portions mixed with common air prevent the slow combustion of phosphorus, as indicated by its shining in the dark, have on the insects on which they were tried the effect of suspending animation.

The revival which occurred in so many instances after suspended animation, may probably be connected with that tenacity of life and long-enduring excitability for which insects are remarkable: thus, even when decapitated, the common house fly, or the flesh fly, will not unfrequently move its limbs twenty-four hours after the loss of the head, on being touched, and commonly can be incited to action beyond twelve hours after such a mutilation.

Some of the results may not be undeserving notice for practical purposes—such as those in the instances of sulphuretted hydrogen, oil of turpentine, and camphor—in relation to the destruction of parasitical insects, whether infesting plants or animals, or to the preservation of substances from the attacks of insects. To be applicable to the preservation of plants, of course it is necessary that the agents to be used should not have on them any injurious effect. This must be determined by experiments made expressly for the purpose. The few trials I have yet made on seeds seem to shew that the steeping them in a solution of sulphuretted hydrogen in water has not prevented their germination. The seeds tried were mignionette, cress seed, and that of a *Nemophila:* analogy, viz., that of steeping the seed of the *Cerealia* in a solution of the white oxide of arsenic, is in favor of the same conclusion. Further, for the preservation of articles, whether of clothing or furniture, it is hardly less necessary that the substances to be employed should have no offensive odour. Judging from the effects of attar of roses, and from what we know of scented woods not being liable to be attacked by insects, the probability is that any volatile oil of an agreeable perfume will answer the purpose required, and form a true instance of the *utile dulci.**

As carbonic acid gas, and some of the other agents mentioned, produce merely a temporary torpor, it may be a question whether this gas, or simple immersion in water, may not be advantageously substituted for the fumes of burning sulphur, destructive of life, at the yearly gathering of honey; the former, indeed, may be said to be used in the Levant, where the smoke of the fire of leaves, in which the carbonic acid generated may be considered as chiefly operative, is employed to stupefy the bees preparatory to the spoiling of their hives.

The tenacity of life in insects, as exemplified in their suspended animation, whether from want of free air, as when immersed in water, or buried in the earth, or surrounded by azote or carbonic acid, may serve a useful purpose in the economy of nature. They are the favorite food of many birds and fishes; and, in their torpid state, with just sufficient life to preserve them from decomposition, they are most easily obtained, and that at a time when such a supply is peculiarly wanted; and even their quitting their state of torpor in temperate weather in

* The experienced angler knows that Russian leather binding of his fly-book preserves its contents from the destructive attacks of moths. Lavender flowers, I find, are fatal to some small moths, such as commit depredations on clothes. On an humble-bee, and on one or two of the larger flies (*Musca*), the odour appeared to have no injurious effect. As in the instance of the moths, these flies were placed in a small tube with freshly-gathered flowers—the tube loosely corked so as to admit some air.

winter, when they themselves appear to exist without food, may conduce to the same end—the affording food to other animals higher in the scale of organization, and especially such as are roused from a like state of torpor by elevation of temperature.

I am, my dear Sir,

Yours very truly,

J. Davy.

P.S.—In the note with which you have favored me of the 15th April, you have called my attention to the effect of the vapor arising from the bruised leaves of the common laurel on insects, and also to that of prussic acid. Before describing the experiments I have made in consequence, I shall, with your permission, quote your words:—"There is," you remark, "one vapor, on which, though now employed to kill insects, we want more exact facts—that of the prussic acid let loose* from the bruised leaves of the common laurel. Entomologists find that insects introduced into a wide-mouthed phial, having a few of these bruised leaves at the bottom, die almost instantly; but there is this inconvenience in employing them, that the watery vapor, which they also give out, condenses on the side of the phial, and injures the wings of the flies, etc., put into it." You add: "It would be desirable to know how small a quantity of the vapor of prussic acid suffices to kill an insect of medium size; whether some insects (as beetles) are less affected than others (as flies, etc.), and whether the size of the insect has anything to do with the result."

Preparatory to making any trial with the prussic or hydrocyanic acid, I thought it right to institute one or two with the bruised leaves of the laurel.

Two leaves just gathered, torn into small pieces and bruised in a mortar, were put under an inverted glass, in which were confined a honey bee and two small insects of the gnat kind. In about five minutes they all became motionless. One of the small flies did not revive; the other and the bee recovered in a few hours, symptoms of revival appearing even before they were

* This expression "let loose," seems a just one, as I believe the prussic acid in a very dilute state in the leaf of the laurel is contained in a vesicle or cell, much in the same manner as is the essential oil, to which flowers and the leaves of certain plants owe their fragrance. In every instance in which I have examined such leaves and flowers under the microscope, I have noticed the cells or vesicles referred to.

taken from beneath the glass into the open air. Probably had the experiment been made at another season, in summer or autumn, the effect would have been different.* Be this as it may, the result was not of a kind to induce me to make at the time any further trials of the leaves, especially after instituting some with the hydrocyanic acid, the fatal agency of which exceeded my expectations.

With a view to answer your inquiries respecting this acid, I paid attention to quantities. Two glass tubes, each closed at one end, a small and a large one, were selected for the experiment—the larger of three cubic inches and a half capacity, and seven-tenths of an inch wide; the smaller only one-tenth of an inch in width, and a little more than an inch long. The small tube, charged with three-tenths of a grain of prussic acid, was introduced into the larger, in which just before had been placed three flies. The tube was finally closed with a cork.

In less than two minutes one of the smaller flies, and also the larger, dropped down, and, after a few convulsive movements, became motionless, as did the other in about half a minute more (its place in the tube was more distant from the acid than that of the other two), but without convulsive action. Taken out in less than half a minute and exposed to the air of the room at 55° Fahr., they did not recover. The small tube immediately weighed had not sustained any appreciable loss. The balance used was readily affected by ·01 grain.

* I find the leaf of the laurel in September (the 10th), especially the young tender leaf, more active than the old leaf which I tried in winter. Prepared as above described, it took effect on two or three different kinds of flies in a few seconds, rendering them motionless; and kept in the tube about a quarter of an hour, at a temperature of 64°, they did not revive. The effect was nearly similar, though not quite so rapid, on two kinds of spiders. . It proved fatal to one of them; the other, the larger, was found the following morning revived and active. The fully formed leaves at this season are somewhat slower in their operation than the young leaves, but more rapid and powerful than the winter leaves, judging from such trials as I have made. The prussic acid being very volatile, the bruised leaf, as might be expected, soon becomes inert from exposure to the air. So powerful is the poison of the young leaves, that a quantity of them, equal to about 70 grs., merely cut into small pieces, and slightly bruised, proved fatal to a toad in about two hours. The toad was confined under a tumbler to which air had access, and was then surrounded by the leaves. The breathing of the toad first ceased. On opening it about half an hour after, it had become motionless, no muscle contracted, except the heart. This toad was a female, and full of ova. On the 13th of October the young leaves were tried on a cockroach (*Blatta orientalis*): it died in about three minutes. The same insect had not apparently suffered from being confined first with a portion of the tobacco of commerce, and afterwards from partial immersion in a pretty strong infusion of the fresh leaf from a plant grown in Westmoreland.

Repeated the experiment, using the same portion of acid, on another fly (a *Heteromyza*). It became motionless in as short a time, but it revived when exposed to the open air. Placed again in confinement, with only ·04 grain of prussic acid, in the small tube, in five minutes it became motionless. Immediately taken out, it was found revived after an hour, and so active that it escaped by taking flight.

Placed a honey bee, just caught in the garden at noon, under a wine glass inverted, of about four cubic inches capacity, with the small tube charged with ·16 grain of prussic acid. In about two minutes the bee fell down, and was convulsed in its legs and abdomen: all motion ceased in about eight minutes. Taken out immediately, symptoms of revival appeared in about three hours; soon after it was standing on its legs, but not active. The following morning it was found to be dead.

Placed the small tube containing the prussic acid, instantly after the experiment on the bee, under another wine glass, in which was confined a *Bombus* (*B. hortorum*), just caught and very active. During the first minute or two its wings were in constant motion; during the next four or five it moved about in an irregular manner, its legs alone acting; in about ten minutes motion ceased, except a slight tremulous one of the legs. Now several parasitical insects (*Acarus* —— [?]) dropped from it deprived of motion themselves. In about twenty minutes it was altogether motionless. The tube taken out and weighed was found to have sustained no appreciable loss; it was replaced under the wine glass. In about three hours, though still confined under the wine glass, the humble bee began to revive, and the following morning it was pretty active. None of the parasitical insects revived, excepting one.

Placed another *Bombus* under a wine glass, with the small tube charged with ·08 grain of prussic acid. In less than a minute it was motionless, as were also two or three of the parasitic insects (of the same kind as the preceding), which fell from it when the poison began to take effect. In less than two hours it had tolerably revived. Now added a single drop of the acid, in about ten minutes the humble bee, after being slightly convulsed, became motionless. On the following morning it was found alive, moving its feet feebly.

Substituted for the small tube one a little larger, its mouth two-tenths of an inch in diameter, its capacity one-tenth of a cubic inch. After pouring into it 7 grains of prussic acid, it was suspended by a thread in the larger tube, and so used in the following trials.

Introduced a large vigorous beetle (*Geotrupes stercorarius*), and closed the receiving tube with a cork so as to prevent the escape of the vapor of the dilute acid. In about a minute the beetle was motionless. Taken out after two hours and a quarter it was found to be dead.

Next introduced an active *Bombus*. In a few seconds it became motionless, entirely so. Taken out after an hour, it was dead.

Introduced another active *Bombus*. It, too, in a few seconds was rendered motionless. Taken out after about a minute, it shewed no signs of revival for many hours; but the following morning it was found tolerably revived. Replaced in the tube with the acid, and left there three hours, it did not recover when taken out. The tube now weighed was not found to have sustained any appreciable loss, a circumstance which perhaps might have been anticipated, considering that the prussic acid used was the dilute hydrocyanic acid of the London Pharmacopœia, which contains 2 per cent. only of real acid.

These few experiments may perhaps suffice to shew the great efficacy of the acid, the intensity of its action, and the very minute quantity requisite to occasion death, and that in a few seconds, of such powerful and comparatively large insects as the *Bombus hortorum* and *Geotrupes stercorarius*, or at least a motionless state, terminating in death, exposure to the acid vapor being prolonged; and, further, in conjunction with qualities of the acid, especially its great volatility, the boiling point of the pure acid being only 79° Fahr., the manner in which it should be used when employed for the purposes of the entomologist.

As to precautions, the poison being so volatile, it need hardly be remarked that a close tube (one made air-tight by a good cork will be best) ought to be used for confining the insects to be acted on; and, on the same account, that the receiving tube should not be very capacious, nor the small tube or bottle to be included in it charged with the acid, one of a narrow or contracted aperture. The dimensions of the two duly proportioned, the effect, deprivation of life, even from a few grains of the acid, should be almost instantaneous and quite certain, and this at the ordinary temperatures of our rooms, supposing them to range throughout the year between 50° and 70°.

As to bad effect from moisture, there appears to be no ground for apprehension, insomuch as in the short time the effect is being produced, even should the insect be allowed to remain in the tube more than an hour, there will be no appreciable loss from the ascent of aqueous vapor. And even if there were, it would be most easy to counteract its influence by the introduction in another small tube of a few drops of strong sulphuric acid, or grains of the chloride of calcium.

As, in the instance of the *Bombus*, the prussic acid proved destructive to the parasitic insects infesting it, is it not probable —I might say, almost certain—that it will be equally fatal to parasitic animals generally, and their ova and larvæ? And, if so, would not this be an additional inducement to employ it, and no small advantage?

VIII. ON THE VITALITY OF THE COCKROACH (*BLATTA ORIENTALIS*), OF THE SLUG (*ARION EMPIRICORUM*), AND OF THE EARTHWORM (*LUMBRICUS TERRESTRIS*).

Since the preceding experiments were made I have instituted others, both on insects and some other animals. At present I shall give the results of the trials on three only, those above-named, belonging to different classes, and so widely apart as to organization.

1st. Of the Cockroach.

Those I have experimented upon were found in a box from Naples containing maccaroni. A few of them were put into a tumbler with a little bread late in December: now, in the first week of May, they are alive and active, and yet the portion of bread is not perceptibly diminished. The temperature of the room during the whole time has not varied more than ten degrees—from about 50° to 60° of Fahr. During the whole period, whether seen by day or night, they have been not only quite free from torpor, but apparently awake, judging from their antennæ acting when in the slightest degree disturbed.

1. *On the Effect of Reduction of Temperature.*

Three cockroaches, placed in a small glass receiver, were exposed to the open air when the register-thermometer on the snow was 28°. They immediately became restless, as if desirous of escaping, and so continued for several minutes, until a thermometer with a large bulb placed close to them had fallen to 32°. Seen an hour after, they were found collected together in a heap, adhering to one another, and were motionless. Snow fell during the night, and the vessel was buried in it. On the following morning the thermometer under the snow was at 36°; the depth was about eight inches, and at that depth were the insects. Brought into a room, at 50°, they all became active.

Replaced, plunged again in the snow, and left there all night, they were not rendered torpid, judging from their not being so when taken out the following morning.

Other two cockroaches were exposed to the open air at a temperature of 28°. After an hour they became torpid; at a temperature of 50° they revived.

The same were exposed to the open air during the night, the register-thermometer falling to 17°. They were found in the morning motionless and dead.

Three were placed in the snow; during the night there was rain. In the morning the vessel was standing half immersed in snow and half full of water. One cockroach at the top showed signs of life, moving its antennæ and feet; the others, submerged, appeared to be dead. They were motionless when taken out, but in a short time, in about an hour or less, they revived and became as active as ever.

2. *On the Effect of Elevation of Temperature.*

The same cockroaches, confined in a small glass receiver, were put into water at 95°. In half an hour the temperature had risen to 100°. The insects displayed an increase of activity. After three hours and a half, the thermometer falling to 98°, they continued active, and did not appear to have suffered in the least.

The same were placed in water at 115°. After twenty minutes, when the water had fallen to 107°, one of them was active, another just moved its legs; another moved its legs very feebly. After five hours, when the thermometer had fallen to 93°, two were active; the other languid. On the following day, the air of the room 50°, they were all active. And, not only did they bear the high degree of temperature above-mentioned, but they endured also without loss of life exposure afterwards to degrees of cold sufficient to render them torpid.

3. *On the Effect of Privation of Air.*

A cockroach was immersed in water at 50° for twenty-four hours. When taken out it was dead.

Another cockroach immersed struggled for about half a minute and then became motionless. Taken out it did not immediately revive; seen about an hour after it had recovered its activity.

Immersed again shortly after, it made no struggle; taken out after an hour, it was not quite motionless—one of its legs was seen to move; gradually, in rather less than an hour, it had become tolerably active.

The same, immersed two hours and a-half, was rendered torpid in four or five minutes, *i.e.*, had become motionless. Taken out and left in this state, when seen again, six hours later, it was found active.

The same, immersed at 8·20 a.m., immediately became motionless; taken out at 5 p.m., it was apparently dead; seen at 11 p.m., it had not revived; on the following morning, at 9 a.m., it was alive, but not active.

To sum up the results, it would appear that this insect, the cockroach, can sustain life many weeks at a temperature between 50° and 60°, fasting, or nearly so; that it is always on the alert, as if it never slept; that it becomes torpid about 32°, and can bear a reduction of temperature below the freezing point of water without losing life; that it can tolerate also a temperature as high as 115°, its activity increasing with increase of temperature up to 100°, and diminishing towards 115°, seeming to become, about that degree, torpid; and further, that it is retentive of life for many hours when deprived of air, immersed in water.

Do not these qualities well account for the cockroach having a wide range of habitat? It is stated on good authority, that it is not only spread over Europe, but is to be met with in most parts of the world.*

* From the results of a few experiments which I have made on spiders of this country (their species not determined), they have appeared almost as retentive of life under the same circumstances as the cockroach.

One was not rendered torpid at 32°, although exposed to the open air a whole night: this was in February.

Another, inclosed in a thin glass tube, was immersed in water at 96° for ten minutes; it was rendered torpid, but, taken out, it soon recovered its activity. Another was active, similarly confined at 98°; the temperature of the water slowly rising to 99°, it became torpid; when taken out it recovered in some degree its activity. The following day it was found dead.

A spider, uninclosed, was immersed in water at 54°; it almost immediately became motionless; its legs were retracted. Taken out after five hours and a quarter, it soon became active. Two spiders were immersed in water at 52°. Taken out after twelve hours, they were dead.

2nd. Of the Slug.

The trials I have made of the slug, using individuals of different colors and sizes, have been of the same kind as those just described on the cockroach. I shall relate them in the same order.

1. *On the Effect of Reduction of Temperature.*

Some slugs taken from the ground on the 17th December, close to a sheltered garden wall, and confined in an earthenware pot, merely covered with paper, appeared to be torpid, in a room, at 55°, the air of which was moderately dry. Transferred to a receiver in which was a vessel containing some water, so as to secure a complete saturation of the air with aqueous vapor, they soon became active, moving about, the temperature remaining the same.

From the room they were taken into the open air. When the thermometer had fallen to 40°, two or three of them were still tolerably active; later, when the thermometer was at 35°, all of them appeared to be torpid.

They were left out during the night, the thermometer falling to 30°; water close by in a small pan was frozen, but not water within the receiver. Seen in the morning, several slugs were found to have changed their place during the night, as was indicated by their slimy track, and were still adhering to the glass; brought into the room, they soon put out their feelers (*tentaculæ*), showing that a temperature just above the freezing-point, to which they must have been exposed during the night, was neither fatal to them nor rendered them torpid.

They were replaced in the open air, and left there till the following morning. At night the register-thermometer fell to 30°, and water in the receiver was frozen. All the slugs, after this night's exposure, were found frozen and dead; gradually thawed there was not the slightest appearance of revival.

2. *On the Effect of Elevation of Temperature.*

In August a slug* was put into a small glass receiver, and immersed in water at 115°; taken out after half an hour, it was

* Its temperature was 58°, the air 60°.

contracted in volume and motionless, nor did it revive on exposure to the air at 60°.

On the 27th April, a slug was put into a thin glass tube, where it was confined by cotton-wool, and immersed in water at 104°; taken out, after a quarter of an hour, when the water had fallen to 95°, it appeared to be dead, nor did it revive.

On the following day the experiment was repeated on two slugs, exposed to a temperature of 100°, falling in three-quarters of an hour to 90°; when taken out, at the end of this time, they were motionless and dead.

On the 11th of August a large slug was subjected to a temperature of 100°, falling to 85°. Taken out it was motionless, and so it remained, as if torpid, the whole of the day. The day following it was again in motion.

3. *On the Effect of Privation of Air.*

On the 29th of April a slug was immersed in water at 60° : an hour after it was seen in motion; a quarter of an hour later it was motionless. Taken out after twelve hours it was dead.

On the 1st of May another slug was immersed in water of the same temperature. After an hour and forty minutes, taken out, it was lifeless.

The results of these trials on the slug, whilst they shew that it has a certain hardihood and power of resisting certain injurious agencies, also demonstrate that it possesses them in a less degree than the cockroach; and therefore may they not well account for the former, in relation to habitat, being less widely spread than the latter?

The circumstance of the slug not becoming torpid, except at a temperature within two or three degrees of the freezing point of water, may deserve to be kept in mind by gardeners, if desirous of checking the breed of one of their most destructive enemies. One of the favorite haunts of the slug is the sheltered ground at the foot of a wall with a southern aspect on which fruit trees are trained. Sought for in such a situation towards the end of November or the beginning of December, they may be met with in large numbers of various sizes near the surface, and easily destroyed. As the cold increases they descend deeper and deeper till beyond the reach of frost, and otherwise secure, reascending as the spring returns and the buds open.

3rd. Of the Earthworm.

This worm subjected to trials similar to the preceding afforded the following results.

1. *On the Effect of Reduction of Temperature.*

On the 3rd of March, some worms of average size and active, taken from the ground, about a foot from the surface (the ground was covered with a layer of decaying weeds during the winter), where the temperature at the depth mentioned was about 40°, as ascertained by the thermometer, were exposed to the open air at 34°. For a while they were more or less active after their manner, as if the cold was painful to them, and they were endeavouring to escape. After little more than an hour they were found coiled together in a mass motionless and torpid. They had thrown out a good deal of their peculiar mucus: the temperature of the air was 33°. Taken into a room, the temperature of which was 50°, they soon revived.

One of the same size, and on the same day, was put on ice. It instantly became very restless, and for some minutes continued its wreathing motion: it soon became torpid and stiff, as if frozen. After less than an hour's exposure on the ice, the open air 33°, it was taken into the room with the others: it did not recover.

2. *On the Effect of Elevation of Temperature.*

Three of the earthworms which had been exposed the preceding day to the open air were placed in a glass jar with a few drops of water, and immersed in water at 85°. They became very active, much in the same manner as the one placed on ice, twisting themselves in all directions. In ten minutes, the temperature of the water the same, they were less restless. An hour and a half later, the water 93°, to which it had gradually risen, the vessel standing on a stove, they were found motionless, and indeed dead; for when taken out and exposed in moist air to the temperature of the room, not one of them revived. On examination the following morning, they were found smeared with thin red fluid, their blood, from a rupture of the integuments, accompanied by a solution in part of the stomach and intestine, owing, it may be inferred, to the action of the gastric juice. From these and other trials and observations I have made, I am disposed to consider the earthworm analogous to the slug, and that a temperature either at or below 32° or 33°, or at or above 100°, is fatal to it. It differs, however, from the slug, I believe, in not hybernating. This last March I found an earthworm which had been frozen dead. Early in the night there was a heavy shower, ·18 inch of rain-fall; towards morning the sky became clear. At 9 a.m. the thermometer was at 20°, and the rain water was frozen in the gauge. It may be inferred that the rain tempted the worm from its underground retreat, and that, owing to the sudden change of temperature, it became benumbed and frozen before it could effect its retreat beneath the surface. From such information as I have obtained from gardeners, the earthworm is active throughout the year; in summer living near the surface of the soil, in winter descending, and the deeper according to the degree of cold.

3. *On the Effect of Privation of Air.*

Considering how the function of respiration is performed by the earthworm, chiefly through the integuments, and that its blood is probably capable of storing up oxygen for use underground, it might be expected that immersion in water could be borne by it without loss for a considerable time. This I find is the case, the length of time it can remain so immersed varying with the volume of water.

IX. OBSERVATIONS ON THE SNAIL (*HELIX* * * *) DURING ITS TORPID STATE.

A SNAIL is sold in the markets of Constantinople, as an article of food, which, from the information I there received, remains torpid during many months of the year, becoming so at the beginning of the dry weather in June, and remaining in that state until the following spring.

I procured some which were offered for sale in the last week of August. The following were the few observations I made on them.

Of each the capacious aperture was closed, covered by a membrane on a level with the margin of the operculum and firmly attached to the shell. This broken through, another similar one was found within. Both were of an opaque white; both effervesced strongly with an acid; the inner seemed to differ from the outer only in having on it a thicker coating of carbonate of lime.

A portion of the membrane moistened with water, placed under the microscope, and seen with a one-eighth-inch object glass, appeared translucent, formed of an irregular, delicate tissue, spotted with little white masses. No crystals were to be seen, nor were there any visible pores. The same membrane, after the action of a dilute acid by which the little masses (those of carbonate of lime) were dissolved, was rendered nearly transparent.

Immediately after the rupture of the membranes a just visible movement of the small portion of the snail that was visible was perceived, but so faint was it that it was hardly distinct without the aid of a lens, and it was continued in the same feeble manner, whilst under observation, for about a quarter of an hour. At the same time there was an exudation of a little fluid.

On the following day the aperture was found closed by a new membrane similar to the old, the shell partially adhering to the wood on which it had been placed, and by a matter the same as that of the membrane.

On the 28th August, the following experiment was made on one of the snails, one that weighed 247 grains. It was put into a glass drinking-vessel (a tumbler), with a little water, and covered slightly with paper: the air of the room was about 78°. Seen the following morning, the operculum was complete; showing that moisture of air, and the temperature stated, had no effect in inciting the snail into action.

The outer membrane was now broken through, and four in succession within. They had all become moist. In each the coating of lime was on the outer surface. The body of the snail in this instance was retracted far within the shell. No movement of its surface could be perceived.

After two hours, during which it had continued motionless, pressure was made on it with a probe. Now a just perceptible motion was witnessed, and the end of the probe became moistened with a yellowish matter, which in dilute acid effervesced; thus indicating that the animal was not only alive, but was also pouring out a fluid, in the way of reparation, for the forming of a new protecting membrane. On the following day the membrane was complete; it was deep within the shell.

Examined on the 7th September, three of the six were found dead and putrid. Two of these had broken through their opercula, and had moved to a short distance. Beyond this time I had not an opportunity to watch the remaining three.

Those who are interested in the subject, especially as regards the influences of causes which are concerned in hybernation and its opposite, I would refer to a paper on the hybernation of the *Cochlea pomatica*, by M. Gaspard, M.D., which was originally published in "Majendie's Journal de Physiologie Experimental," and republished, translated, in "The Boston Journal of Philosophy and the Arts," No. VI., for April, 1824. The particulars the author gives of the long sleep of this snail for many months—and even for more than a year under peculiar circumstances—or rather its suspended animation, are very remarkable.

X. ON THE EXCREMENT OF SPIDERS.

In the more recent zoological arrangements spiders have been removed from the class of insects. Their urinary secretion, from the experiments which I have made on it, appears to be another distinctive mark compared with that of the different species of insects which I have hitherto examined. In the secretion of the spider I have not been able to detect any lithic acid, but what I believed at the time to be the zanthic oxide.* Whether obtained from a spinning or from a hunting spider, the character of the matter voided in confinement has been similar: abundant as to quantity; semifluid at first, soon becoming solid; of the lustre of wax; most readily diffusible in water, to which it imparts a milky hue; neither acid nor alkaline, as tried by test-papers; and, under the microscope, appearing to be composed of very minute granules varying in size from $\frac{1}{20000}$ to $\frac{1}{30000}$ inch in diameter. It was found soluble, and that readily, in the nitric, sulphuric, and muriatic acids, and also in the acetic and the oxalic. It did not impart color to either of these acids when the solution was made without the aid of heat; but with heat, in the instance of nitric acid, a compound of a yellow color was obtained; and in that of the sulphuric a slight yellow tinge was produced. I have found it also soluble in aqua potassæ, and in a solution of the sesquicarbonate of this alkali, but not in aqua ammoniæ, nor in a solution of the bicarbonate of potash. Heated on a platinum foil, it bore a pretty high temperature without apparent change; more strongly heated, it consumed without flame, leaving a very minute quantity of matter, which, exposed to nearly a white heat, melted, and exhibited under the microscope minute globules. Subjected to a decomposing temperature in a glass tube, it yielded pretty much water and carbonate of ammonia, with a residue of carbonaceous matter.

I have examined the excrement of at least three different species of spider, and of several individuals of the same species, and I have found it in its general qualities and compositions remarkably

* Since ascertained to be guanine.

uniform. Sometimes its color has been a little greyer than at others, which I believe to be owing to a minute portion of alvine excrementitious matter being mixed with the urinous. In one instance, under the microscope, a few very small prismatic crystals were seen, resembling those of phosphate of lime, mixed with the granular matter. The proportional quantity in which this excrementitious matter is voided is also remarkable. It suggests the idea that almost the whole of the food of spiders is digested, and that their principal secretion and excretion is the urinary. And, further, as the food of these animals is entirely insects, and chiefly flies, and as I have not been able to detect any traces of lithic acid in the excrement in question (an acid which, it may be inferred, is contained in the cloacæ of many of the insects consumed), the idea is suggested that in the digestive process in the spider, this acid is either assimilated, so as to form a part of the nutritive fluid, or is altered and converted into some other compound.

In this tropical region (the West Indies), teeming with animal life, and equally so with vegetable, the quality of the urinary excrement of spiders as well as of insects, considering their composition, seems to be peculiarly in harmony with an adaptation of means to ends, and affording an example of that happy economy which is so often to be witnessed in the processes of nature. Both appear to be specially fitted to form a part of the food of plants. Both are only slightly soluble in water. Both in their unmixed state appear to be rejected by animals of every description when in search of food. Ants here, which may be considered as the principal scavengers of the tropics, especially as regards putrefactive animal matter, leave untouched, as I have observed, the urinary excrement both of spiders and of insects. This exemption from destruction may be said to insure to the soil productive of vegetables a constant source of manure; the vegetables in such a climate as this supporting innumerable insects, and the insects in their turn supporting in great numbers spiders and other animals—such as birds, lizards, batrachians—the excretions of which have similar qualities, and are well adapted to perform the same part. Ought not this to be a lesson to man to husband excrementitious matters generally, making no exception, and to bestow them on the soil as its peculiar and appropriate fertilizers?

XI. ON THE TEMPERATURE OF THE SPIDER; AND ON THE URINARY EXCRETION OF THE SCORPION AND CENTIPEDE.

In a former communication made to this journal,* I have spoken of spiders as belonging to the class of cold-blooded animals. Though this, I believe, is generally admitted as a fact, I am not aware of any precise observations that have been made and published confirming it. I am induced in consequence to give the following, which, although the number is very limited, may in part at least supply the supposed deficiency.

The subject of the first trial I shall mention was a large species of *Mygale*, not uncommon in Barbados, the body of which was about an inch long. A small thermometer with a projecting bulb applied to its abdomen was stationary at 86·25° Fahr., when a similar one placed under the glass vessel in which the spider was confined stood at 86°. The following day, the difference between the two thermometers was a little greater; that in contact with the spider was 88·5°, that under the glass was 88°. On this occasion the spider was placed on cotton-wool, and the bulb of the instrument was between this bad conductor of heat and the abdomen of the animal. I may add, that the previous night it devoured the soft parts of a beetle, and voided a considerable quantity of urinary excrement. The trial of the thermometer on this spider was repeated several times, and with the same result.

In Trinidad, as well as in some of the other West Indian islands, a very large spider (*A. avicularia Linn.*) is met with. At my request, Mr. Longmore, stationed in Trinidad, was so good as to institute some trials on it to determine its temperature. In the first he made he informed me that a delicate thermometer, the bulb of which was grasped by the spider so as to be surrounded, rose in one instance from 85° to 86·5°, in another from 83° to 85°. In some other trials made on the same spider, taking precautions which I suggested, he found the

* The Ed. New Phil. Jour., No. 87.

thermometer under the abdomen and in contact with it from one-half to three-quarters of a degree higher than in the air of the box in which the spider was confined; and he stated that the same results followed the experiment when made at different atmospheric temperatures. These last results of course I consider, as did also Mr. Longmore, the most accurate.

In connexion with its temperature, I have thought it worth while to ascertain the proportion of carbonic acid formed by the spider in a certain time. The spider, the temperature of which I had tried, was confined in a glass jar over water of the capacity of 23 cubic inches, and holding this quantity of common air. After twenty-four hours and a-half (the temperature of the room the same at the beginning and end of the experiment), calculating the absorption that had taken place—the diminution of the volume of air, and the after diminution by agitation with lime-water—it was found that there was a total diminution of 2·11 cubic inches of air, so that about this quantity of carbonic acid, it may be inferred, had been formed. In a second experiment on another species of *Mygale*, about half the size of the former, after three days' confinement in 13·6 cubic inches of atmospheric air, nearly a cubic inch (·97) appeared to be formed. The spider at the end of three days was alive and active. These results on the quantity of oxygen consumed by these spiders in the formation of carbonic acid, or the animal-heating process, may be considered in accordance with the degree of temperature they have been observed to possess. I have mentioned that the spider first used in the trial on temperature devoured the soft parts of a beetle: of this I satisfied myself by careful examination of the remains. And, I may add, that from repeated observations, I am sure that spiders do not, according to popular belief, feed on the juices of the animals they make their prey, but on their muscle and other soft parts. The same remark, the result also of observation, applies to the scorpion.

As regards this last-mentioned animal there is another popular belief, which I have no doubt is equally ill-founded, viz., that if an attempt be made to confine it, it will destroy itself by thrusting its sting into its head. I apprehend that most scorpions that die shortly after they have been placed in confinement, have been hurt in being taken, and die in consequence. The committing of self-destruction on loss of liberty, excepting it be done instinctively, implies not only a reasoning power and process, but a knowledge of the effects—that is of death—a knowledge which we can hardly suppose any animal but man to possess. But, apart from this general consideration, I may mention that the only scorpion I have had, that I have taken uninjured, has lived and is now living in confinement, and feeds well, eating the soft parts of flies and of small cockroaches which it kills; and seems to be in the enjoyment of perfect health, and nowise repining under the loss of liberty.

This scorpion, a small brown one, with transverse stripes on its back of a lighter hue (*S. Americanus*), has enabled me to make trial of its urinary excrement. Like that of spiders it is voided in a semi-fluid state; shortly becomes solid from the evaporation of the aqueous part; is of a greyish hue; under the microscope is seen to consist chiefly of spherical granules of from about $\frac{1}{8000}$ to $\frac{1}{12000}$ of an inch in diameter; and examined chemically, is found to have the properties of xanthic oxide.* I have never been able to detect in it any traces of lithic acid, which I have sought for in several specimens. It is remarkable that almost the whole of the excrement of this animal appears to be urinary. It is voided often, and, in proportion to the size of the scorpion, in large quantity. A like remark applies to the spider, but not to the centipede. Hitherto my observations on this secretion of the scorpion have been limited to one example; and in the instance of that of the centipede they have been equally limited.

The centipede (*Scolopendra morsitans*) from which some excrement was obtained for examination, was about six inches long; it died after having been about a fortnight in confinement without eating; it had been hurt in being taken. During this time it twice voided excrement—a dark small cylindrical mass, partly covered with a whitish incrustation. This whitish matter, separated and examined apart, was found to consist chiefly of lithate of ammonia. Under the microscope it was seen to be composed of granules of about $\frac{1}{8000}$ inch in diameter; and acted on by nitric acid and heat, it yielded the purple compound characteristic of lithic acid. The dark, almost black matter, composing the principal part of the excrement, under the microscope appeared as a very mixed debris, amongst which

* Guanine, a compound nearly allied to xanthic oxide.

sand in very fine grains was observable, but nothing else well defined. No doubt it was fæcal matter; and it would seem to indicate that the centipede is a coarse feeder.

Since my return to England I have examined the excrement of the common millipede: in one instance it afforded a trace of lithic acid; in another, I could not satisfy myself of the presence of this acid. The excrement was in little pellets, and formed chiefly of the debris of vegetable matter.

XII. MISCELLANEOUS OBSERVATIONS ON THE CENTIPEDE (*SCOLOPENDRA MORSITANS*) AND ON THE LARGE LAND SNAIL OF THE WEST INDIES (*HELIX OBLONGA* [?]).

In a preceding paper I gave an account of the urinary secretion of the centipede, how it consisted chiefly of lithate of ammonia. Then I had obtained no certain information respecting its food. Since, I have had an opportunity of watching one in the act of eating, and of ascertaining that its food is insects. It was caught without being hurt, and was confined under a glass vessel—a common drinking-glass inverted on a porcelain plate—where it remained under observation a month, when it effected its escape. During the whole time it shewed a voracious appetite. The first day of its captivity it ate two house flies; and each day after, this number or more; one day it devoured nine. It ate the flies piecemeal, leaving nothing but portions of the wings. So intent was it on the act, that it did not relinquish its prey even when somewhat roughly touched, appearing, when so engaged, regardless of everything else. After eating, it commonly seemed listless and disinclined to move, almost as if torpid or asleep. The excrementitious matter it voided was abundant, not unlike that of small lizards, being in little cylindrical masses, in part nearly white, consisting of lithate of ammonia, and in part of a darker hue, the latter alvine, formed chiefly of the wings and other undigested parts of the flies it had consumed.

This centipede weighed 24·46 grs. (weighed in a glass tube). This was after it had been two days in confinement, and had devoured nine flies. The excrement voided in the same time weighed, after having become dry from exposure to the air, ·44 of a grain. The larger portion of it was lithate of ammonia, as it always was whenever examined. The quantity of it voided did not diminish with the confinement of the centipede, but seemed rather proportionally to increase, being in accordance nearly with the quantity of food used; and it was noticed that commonly after devouring a fly a small excrementitious mass was discharged. Between the 12th and the 25th July the matter voided was found equal to 2·55 grs., weighed when dry.

I am induced to give these particulars thus minutely for two reasons—one, the difficulty there is in procuring a centipede uninjured in its capture so as to be fit for such observations as I have made; the other, on account of the observations themselves, denoting a great activity of the digestive and assimilating power of the animal, and the rapid formation and abundant quantity of lithate of ammonia. This activity of functions was, I believe, connected with the growth of the centipede, for at the end of the month it appeared decidedly increased in size. Had it not escaped, this would have been determined with precision by weighing.

The snail, which I believe to be the *Helix oblonga* of Linnæus, is very common in the island of Tobago, less so in St. Vincent, and is not met with in Barbados. The specimens I have seen have been about four inches in length and about two inches in width.* It burrows, hiding itself underground during the dry season. There, too, it deposits its eggs. It appears abroad in damp nights, and by day in rainy or showery weather. It is believed to feed entirely on vegetables.

Its eggs, those I have seen, have been about an inch long, and about six-tenths of an inch in their short diameter. They have a brittle, semi-transparent shell, which I find is composed of carbonate of lime, with a little animal matter and a just perceptible trace of phosphate of lime.† Their contents in their

* They vary very much in weight, according to the thickness of their walls; one weighed 375 grs., another and a smaller 732 grs.

† They, too, vary much in weight, and owing to the same circumstance—the thickness of their shell: one weighed 23 grs.; another of about the same size weighed only 12·3 grs. These shells were dry after twelve years' keeping: in one, the heavier, was a fœtus with its shell formed in part, and emitting an offensive smell; in the other there was no fœtal development, and no putrefactive change. The shell of the young snail, as formed in the egg, is at first a transparent membrane; carbonate of lime is deposited on it; when hatched, it is completely incrusted; one in its membranous state weighed 3·6 grs. The carbonate of lime is probably chiefly derived from the shell of the egg separated by solution by means of the carbonic acid formed by the respiration of the fœtus. Is not part of the lime which enters into the composition of the bones of the chick *in ovo* similarly derived? The brittleness, the weakness of the shell after hatching, would seem to show that it has lost in the process of incubation a portion of its earthy material; and thus, whilst administering to the wants of the fœtus rendering the exit of the imprisoned chick the more easy. Another circumstance may be mentioned in favor of the same inference, that of the

early stage, judging from one I have examined, are a viscid fluid of uniform color, white, with a just perceptible tinge of yellow. There was no appearance of yolk, if the whole may not be so considered. Carefully weighed, it was found to be of specific gravity 1,060. It was coagulated by heat, and was rendered opaque by corrosive sublimate and nitric acid, much in the same manner as the albumen ovi of the common fowl, but the coagulum was less firm, yet, as formed by heat, sufficiently so to bear the inversion of the vessel without its flowing.*

The excrement which this snail voids is large in quantity, and in consolidated cylindrical masses, casts seemingly of the tube from whence they are discharged. That which I have examined I have found to consist chiefly of two kinds of matter, viz., one, the chief portion, of a dark olive green, formed principally of

lining membrane of the egg after incubation separating more readily than that of the egg which has not produced a chick, and under the microscope presenting a somewhat different appearance, as if more porous or less uniformly incrusted with calcareous matter.

* In an egg which I have kept twelve years, without any precautions, deposited merely in a dry place, I have found, with the exception of its desiccation, little change. Its contents were reduced to a small bulk; the dried substance was of a light yellow. This substance, with water, formed a slightly viscid milky fluid, having a faint alkaline reaction. It was coagulated by heat. It was examined for oily or fatty matter by means of boiling alcohol, and also by means of ether, but with no satisfactory positive result: if either be present the proportion must be very minute: I am most inclined to think that they do not enter into the composition of this egg. In thus resisting change this egg resembles that of the common fowl, which may be kept many months with little alteration, except loss of weight from the evaporation of its aqueous portion. I shall give an example:—An egg newly laid on the 19th of May, 1859, then weighed 1088 grs.; it was placed in a dark press, to which air had pretty free access, in a room the temperature of which throughout the year ranged from about 50° to 65°. Weighed again on 2nd August, 1860, it was reduced to 401·5 grs. Broken, it had no unpleasant smell; the yolk and albumen (the latter had acquired a rich yellow hue) were distinct, much reduced in volume, and the containing membrane strong. The albumen was of sp. gr. 1125; it was perfectly transparent and very viscid; mixed with quicklime, no ammoniacal odour was perceived, and only a very faint fume when brought near a glass rod dipped in nitric acid. The yolk, too, had no appreciable smell; mixed with quicklime, however, it gave off a faint ammoniacal odour. I may further remark, that even when the white of the egg is taken out of the shell and freely exposed to the atmosphere, it absorbs oxygen very slowly and undergoes change proportionally slowly. The albumen of two eggs kept in a pneumatic apparatus afforded proof of this: introduced on the 24th April, 1859, and taken out on the 2nd August, 1860, it was still transparent, but was less viscid, and it had only a slight offensive smell; heated it coagulated; mixed with lime it gave off a pretty strong ammoniacal odour. A very different result was obtained when the white and yolk were mixed together. The mixture poured into a bottle, in which was a certain quantity of common air, soon underwent change and became putrid, disengaging much gas of an extremely offensive kind—the one acting on the other as a ferment. We see in the grape and many other fruits analogous results: provided the cells be not broken down and the several parts mixed, there will be no fermentation, and in a dry air the fruit may be desiccated and preserved for a length of time—the dried fig, a dried slice of apple, are good examples of the kind; and the same holds good of roots.

the debris of the vegetable food, as was indicated by its appearance under the microscope ; the other, almost white, soft when voided, of uniform appearance and consistence, and composed of globules chiefly, mixed with a few epithelium scales. The globules were about $\frac{1}{2000}$ of an inch in diameter. Acted on by nitric acid and heat, I have found them to contain lithic acid, probably in the form of lithate of ammonia, and probably also combined with some animal matter. That it is so combined, I infer both from the large size of the globules and from the purple color, the result of the action of the acid and heat being less intense than in the instance of pure lithate of ammonia similarly tested. A third kind of matter is occasionally met with of a grey color, or nearly white, which, chemically examined, appears chiefly to consist of earthy substances, carbonate of lime and silicious sand ; which, it may be inferred, has been taken in with the food, either intentionally, instinctively, after the manner that a like matter is swallowed by fowls to afford materials for the formation of the shell of the egg ; or accidentally, adhering to what is eaten, still serving in part the same purpose. This matter occurs mixed with the dark olive green intestinal excrement. The white globular matter containing lithic acid, which may be considered a secretion of the urinary kind, is not mixed with the intestinal matter ; it is merely attached to it much in the same manner as in the instance of the mixed excrement of the centipede, but in greatly smaller proportion.

As it is probable, reasoning from analogy, that the excrement of all snails and slugs* is similar in composition, and contains lithic acid, it may be inferred that their casts are even more useful, considered as a natural manure, than has commonly been supposed, viz., by storing up nitrogen in lithate of ammonia, and in its slow decomposition, affording an element of support for growing plants.

As this large snail seemed a good subject for ascertaining

* In one instance, since my return from the West Indies, I have detected lithic acid in the excrement of the common garden slug (*Limax ater*). I found it amongst my books ; strange to say, the slug had been feeding on the back of an 8vo. unbound volume, which in one night it had laid bare. Its excrement was in small pellets, on which was a slight incrustation, consisting chiefly of lithate of ammonia. In the fluid, voided by another slug kept in confinement, hippuric acid was detected in minute quantity. This was in July. The slug was of large size, and its intestine was loaded with the debris of vegetable matter.

its temperature, I instituted a few trials, the results of which I shall give. It may be premised that the two snails on which the observations were made were brought from Tobago to Barbados, and had been several days fasting, refusing to eat the leaves given them. They were kept under a glass vessel to which air had free access, placed in a well ventilated room. Their temperature was ascertained by placing the projecting bulb of a delicate thermometer so as to be covered by their soft parts. The temperature of the air of the room was at the same time noted down, and also, except on the first day, that of a bottle of water standing by, for the purpose of comparison.

Time of observation.	Temperature of air of room.	Temperature of water.	Temperature of one snail.	Temperature of another.
	Deg.	Deg.	Deg.	Deg.
July 27, 3 p.m. ...	85	...	85·25	85
" 27, 9½ p.m. ...	80	...	81	81·5
" 28, 6 a.m. ...	79	80	81	81·25
" 28, 3½ p.m. ...	85	85	85·5	85·5
" 29, 6 a.m. ...	79	80	80·5	80·5
" 30, 3½ p.m. ...	83	82·75	83·25	83·25

These results seem to shew that these snails had a temperature commonly exceeding a little that of the atmosphere in which they were.*

The two snails were equal in volume to ten cubic inches. On the 2nd of August they were put under a jar of the capacity of 240 cubic inches full of atmospheric air, and water was poured on the stand so as to cut off all communication with the atmosphere. In this moist air, moist to saturation, the snails, the first day, were very active, being in constant motion. On the second day they were less so. On the third day they were found dead. The air now on examination was found to be largely vitiated with carbonic acid gas. Thirty-two cubic inches of this gas were absorbed by lime-water, leaving 198 cubic inches, 10 cubic inches having been displaced by the

* The temperature of a large slug of the same kind as that mentioned in the preceding note, was found in July half a degree higher than that of the air. The bulb of the small thermometer used was enveloped in its folds. The air of the room at the time was 60°; the moistened bulb was 2·5° lower. The slug was confined under a glass tumbler, so that evaporation from its surface could have had little effect in reducing its temperature. After exposure to the free air of the room for an hour, its temperature was found to have fallen, owing to evaporation from its moist surface, 2°, *i.e.*, only half a degree less than the moistened bulb thermometer.

volume of the snails. Hence it appears that nearly two-thirds of the oxygen had been consumed, converted into carbonic acid gas, which may tolerably account for the temperature of the snails being, as recorded, a little higher than that of the air they breathed.

The comparatively short time that the snails lived in a confined atmosphere may have been owing to their activity excited by the humidity of the air, so congenial to their nature and habits when free. In this, their speedy death, they remind one of hybernating animals, perishing speedily at a low temperature, if unduly stimulated to exertion, roused from their dormant state, in which, if undisturbed, they would have remained secure.

XIII. ON THE COLOSTRUM OF THE COW.

THE colostrum—the new milk of the cow after calving—differs, as it is well known, from ordinary cow's milk, in being of a richer yellow color, less liquid, of greater specific gravity, and in possessing the property of coagulating when heated.

Resembling in the last-mentioned property the serum of the blood, or the substance of the egg, it has been supposed by some physiologists to be more animalized than common milk, and to contain even serum, and that its being coagulable by heat, is owing to the presence of serum.

This inference not having appeared to me to be proved in a satisfactory manner, and, *à priori*, not very probable, I have been induced to subject it to some trials, the results of which I shall briefly describe, tending to shew, as I believe they do, that serum does not enter into the composition of the colostrum, and that its coagulability by heat depends on a peculiar modification of its caseous portion.

The milk, the subject of the experiments I am about to describe—selecting, as is desirable, a particular instance—was from a healthy cow that calved for the first time on the 19th of July, and was drawn about an hour and a half after, the udder having begun to be distended about three weeks previously.

Its appearance under the microscope differed principally from that of common milk in presenting larger oil globules—a very few irregular flakes, probably epithelium scales—a little granular matter, like curd, and a small number of granular corpuscles—the granular bodies of the colostrum first described by Mandl, the largest of which were about $\frac{1}{500}$ of an inch in diameter.*

It reddened, slightly, litmus paper. Its specific gravity, carefully ascertained before any separation of cream had taken place,

* The nature of these bodies seems to be somewhat doubtful—whether they are aggregation-globules, formed chiefly of albuminous matter—or large oil globules, with particles of curd, or even of oil attached to their surface: from their apparent specific gravity, and some other circumstances, I am disposed to adopt the first opinion; when colostrum mixed with water is put by in a cool place, after a few days, when the greater part of the cream has collected at the surface, a sediment is found at the bottom, consisting chiefly of these globules.

was found to be 1,075, and as high as 1,080 after a portion of its cream had risen, and had been removed.

To determine the degree of temperature at which it coagulated when heated, a portion of it, contained in a glass tube, was immersed in water, the temperature of which was gradually raised. At 160° Fahr. it was unaffected; at 163° it coagulated: the coagulum was rather soft—but admitted of being inverted without flowing; it yielded readily to gentle pressure. It did not become hard when boiled, nor was its consistence materially increased by boiling. It may be deserving of mention, that the neutralization of the little free acid it contained by the addition of sesqui-carbonate of ammonia, had no marked effect on its coagulation by heat.

It was coagulated by rennet, and more readily than common milk. In a comparative experiment, whilst the latter required a temperature of 110°, the former underwent the change at the temperature of the atmosphere, then about 65°. The coagulum of the colostrum was much softer than that of common milk, and the proportion of whey which separated from it was very much less.

Mixed with ordinary milk and heated, the colostrum acted like rennet; twelve measures of the colostrum mixed with forty-six of milk, formed a soft coagulum when the temperature of the mixture was raised nearly to the boiling point.

The colostrum was coagulated by all the acids that I tried on it, and this at ordinary temperatures, as the acetic, muriatic, nitric, and sulphuric; and also by the citric, tartaric, and oxalic, when mixed with it in the state of powder. The coagulum it formed with the acetic and the mineral acids was soft, and of a grumous consistence; that which it formed with the solid vegetable acids was firm, like strong jelly, and diaphanous. When the mineral acids were added in large excess, the coagulum was dissolved, leaving the butteraceous part, which rose to the surface: ammonia, in the form of aqua ammoniæ, added largely in excess, formed a gelatinous mass of pretty firm consistence, which did not dissolve on immersion in water.

The colostrum left at rest underwent change slowly. After three days it still retained throughout its yellow hue; a part only of its cream had risen to the surface. After seven days, the lower part had become of the color nearly of ordinary milk, retaining its liquidity, and having acquired increased facility of coagulating when heated,* whilst its upper part was of a richer hue, and less fluid, being covered with a thick pellicle of the consistence of cream-cheese. This had the peculiar smell of cream-cheese, and was spotted with mildew, consisting of that kind of byssus which forms on cheese. After thirteen days, the inferior white portion had coagulated, having acquired the consistence with the properties of soft curd—the part above it having undergone little apparent change, that immediately over the curd retaining its yellowness or semifluidity and the butteraceous cheese-like crust, preserving the appearance before noticed with increased firmness and increase of mildew.

Mixed with water (about two parts of water to one of colostrum), and agitated, and then allowed to remain at rest, cream rose to the surface, and curd formed and subsided, leaving, after about a week, a transparent or nearly transparent fluid between the supernatant cream and the sediment of curd. This fluid was acid, and held some curd in solution. It had a cheesy smell, was not rendered turbid by boiling, nor by acetic or the citric acid, but yielded a precipitate with the three mineral acids; and when kept, a white crust formed on its surface. The formation of this crust was accompanied by a disagreeable smell, similar to that of decaying cheese, but rather more offensive, partaking of the putrid odour, owing probably to the presence of some granular corpuscles. When the crust was separated by filtration, a fresh one formed on the fluid in a few days, and this successively for many days. The crust, or pellicle, consisted chiefly of little cylindrical masses, rounded at their extremities, about $\frac{1}{1000}$ of an inch in length, by about $\frac{1}{2000}$ in width. After the fluid had ceased to yield this pellicle, it was rendered turbid by nitric acid, as is also the whey of ordinary milk, whether coagulated by rennet, or spontaneously on its becoming sour by the absorption of oxygen.

* In this respect resembling common milk, which, after keeping a time, varying with the temperature of the air, coagulates even below 160°: as to coagulability indeed presenting a complete gradation—when quite fresh, resisting it, as is well known, at the boiling point; and after keeping some time, coagulating at a low temperature, not exceeding that of the warm hand; in this state offering a subject for a pleasing experiment: it may be poured perfectly liquid into a thin glass tube, and merely by grasping the tube with the hand the effect is produced—rapidly, almost in an instant, the liquid is converted into an apparent solid, and that without the disengagement of heat.

Other specimens of colostrum which I have examined have afforded similar results. I may notice two in particular, the specific gravities of which were ascertained with care, and the degree of temperature at which they coagulated. The one was from a cow that calved in September, and was drawn almost immediately after: its specific gravity was found to be 1,070. Whilst still warm, it reddened litmus. It was pretty firmly coagulated at 160°; at 150° it formed a soft coagulum; and at 140° it became thicker, but was still fluid. At the lower temperatures, a much longer time was required for the effect to be produced than at the higher. The other specimen was from a cow that calved in December, and was drawn about a quarter of an hour after. It was less thick than the colostrum usually is, was of sp. gr. 1,057, and coagulated at about 162°.

What are the inferences to be drawn from the preceding experiments? Do they not shew that the peculiarities of the colostrum are not dependent on the presence of serum, and, in brief, that the colostrum is destitute of serum? The effect of rennet on it, which I find does not coagulate serum,* the effect of the vegetable acids on it, and the changes which it undergoes on keeping, when partially exposed to the action of the atmosphere, are the facts most to be insisted on. And, in corroboration, I may mention the results of trials on the heating of mixtures of common milk and serum in different proportions. Serum, in certain proportions, like the white of egg, I find does occasion the coagulation of milk—giving rise to custard; but it requires a pretty large proportion: five parts of serum, with fifty-five of milk, even when boiled, had no effect; and even when the former was increased to ten, and the latter reduced to fifty, the mixture did not coagulate till its temperature was raised to about 195°.

Whilst all these results are opposed to the idea that serum forms a part of new milk, they favor the conclusion that the caseous portion of the colostrum is in a state somewhat different from its ordinary condition in the milk of the cow, modified in some manner, either in consequence, it may be, of a slight difference in composition; or owing to the influence of the other ingredients of the milk with which it is mixed, especially the

* See Researches Physiol. and Anat., ii., p. 97; the experiments related there have been repeated with the same negative result.

granular bodies, if admitted to be aggregation-corpuscles, which may perform the part of rennet, in promoting its coagulation; or to the butteraceous ingredient, more abundant in it than in common milk, separating more slowly, and which must protect it more powerfully from the action of the atmospheric air, and from the changes consequent on the absorption of oxygen. And that the caseous portion of milk does admit of modification, and that not inconsiderable, is shewn in a remarkable manner, on comparing it, as existing in the human milk and in cow's milk; in the latter, even in its ordinary condition so readily coagulable, whilst in the former it resists this change, whether acted on by acids or rennet, and yet it is easily obtained by means of evaporation, when freely exposed to the air, and without evaporation from the slow and long continued action of atmospheric air.*

Physiologically considered, the most marked circumstances belonging to the colostrum are the concentration of nutritive matter in it; the greater facility of its coagulation by rennet, compared with older milk, and its greater power of resisting change when exposed to the action of atmospheric air. These are qualities which may be eminently serviceable, viewing it as the first food of the young animal. Its easy coagulation may suit it to the stomach, in which probably the gastric juice at first is in small quantity and feeble. Its power of resisting change and remaining semi-fluid may adapt a part of it to the intestines, to promote the removal of the meconium. Whilst its concentration as nutritive matter may fit it to perform for the calf the same part that the substance of the egg serves which enters the intestine during the latter stage of fœtal development in the instance of birds, reptiles, and fishes.

The change which takes place in the cow's milk, watching it after calving, is not unfavorable to this view of the uses of the colostrum. Recurring to the first example, the milk of the cow

* A portion of milk from a healthy young woman, five weeks after delivery, of sp. gr. 1,033, kept in a stoppered phial, to which air had access, a thin slip of paper having been placed between the stopper and its neck, and kept in a dark place, of about 55° temperature, from November till May, had lost its milk-white hue, and deposited curd as well as thrown up cream. Strained, the whey was found of sp. gr. 1,020: 8·1 grains of dried curd were obtained from 312 grains of milk. The whey reddened litmus paper, and was not coagulated either by boiling or by nitric acid. The curd was very like that which constitutes the chief portion of the alvine evacuation of infants at the breast.

which calved in July, that drawn after 24 hours, was of sp. gr. 1,039, which was less thick and less colored, and coagulated when heated to about 180°. The milk of the following morning, of sp. gr. 1,038, did not coagulate even when boiled, subjecting it to heat even five hours after it was drawn; but after having been kept thirty hours, then it coagulated like the preceding, at about 180°. Milk of the following morning, drawn on the 21st of July, about 60 hours after calving, was of sp. gr. 1,033, differed hardly perceptibly from common milk in appearance, and did not coagulate on boiling, even after having been kept about fifty-four hours, at a temperature little below 70°, and by day amounting to that.

If the special use of the colostrum of the cow is such as I have inferred, it may be expected that the first milk of other animals will be found to be similar in its properties, at least, of all those the young of which, like the calf, are born fully formed and vigorous, with good use of their limbs almost immediately after birth. And, so far as I have been able to learn, this is so. The new milk of the ewe, of the mare, and of the sow, I am informed by intelligent farmers, is as rich and thick, and, in the instance of the two first, coagulates when heated. But whether the milk of the sow has the same property, I have not been able to ascertain.* I may mention another instance—it is the milk of the giraffe. Mr. Gulliver informs me that a small quantity "drawn within an hour after the young one was born (May 20, 1841), was excessively rich in fat-like cream, not rendered clearer by any quantity of ether, and became thicker or clotted in a watch-glass over a candle."

Whether the first milk of those animals, the young of which are born helpless and feeble, as of the carnivora, is also like the preceding, cannot, that I am aware of, be at present determined, for want of facts. I am disposed, however, to conjecture that it is similar. This conclusion is founded on analogy, having found that the first milk of the bitch is so, coagulating when heated, and yielding a large proportion of animal matter (35·2

* Since the above was written I have examined the first secreted milk of the ewe and sow, and have found them very like that of the cow. The ewe's was of sp. gr. 1,060, reddened, slightly, litmus, coagulated at about 165°, and yielded, when evaporated to dryness, 35·7 per cent. solid matter. The sow's was of sp. gr. 1,057, and coagulated at 160°. Milk from another sow, the first drawn also, was found of sp. gr. 1,062.

per cent.) when evaporated to dryness. I could not procure sufficient to determine its specific gravity. In the instance of the carnivora, it may be requisite for the first milk to be rich, from the manner in which these animals feed, having to leave their young to procure food, and to be absent, it may be, an uncertain time. And in accordance with this, is the fact that the human milk, the first drawn, is not unusually rich, and does not coagulate when heated; at least these are the results of the few trials of it that I have made. The milk of a healthy young woman, who had been confined five weeks, nursing her first child, I found of the sp. gr. 1,033; it yielded on evaporation 10·22 per cent. of solid matter. The first milk of a healthy woman, ætat 40, after giving birth to her sixteenth child, was of sp. gr. 1,033, and yielded on evaporation 12 per cent. solid matter. The first milk of another woman, of whose age or condition I omitted to make a note, was of sp. gr. 1,022. Examined four days after, this woman's milk was found to be of sp. gr. 1,035, and to yield on evaporation 11·6 per cent. solid matter. This peculiarly dilute state of the human colostrum, if proved to be general, seems equally suitable to the condition of the offspring and mother, the one helpless and feeble, not requiring concentrated nourishment, the other commonly, from a certain degree of exhaustion during labor, ill fitted to yield such a supply; and, moreover, being designed for domestic life and support, and not under the necessity of separating herself from her offspring to go in quest of precarious food; offering in this point of view another instance of the vast number of examples of harmonious adaptation; and it may be also one circumstance more by which man as an animal is distinguished from every other.

XIV. ON THE COMPOSITION OF THE MECONIUM AND OF THE VERNIX CASEOSA.

The microscopical character of the meconium is very distinctive, and well displays its compound nature. It may be examined advantageously either mixed with water, or in a saturated solution of common salt, or merely compressed between two plates of glass. Using either method its appearance is much the same—it exhibits a confused mixture of globules, plates, and molecules. The globules, about $\frac{1}{3000}$ of an inch in diameter, are very abundant, and form a principal part of the whole. Judging from their form and size, their insolubility in water and alcohol, they may be inferred to consist chiefly of mucus.

The plates, which are tolerably abundant, are of two kinds. One kind is of irregular form, somewhat granular, varying in size from about $\frac{1}{2000}$ to $\frac{1}{1000}$ of an inch in diameter, insoluble in water and alcohol, whether hot or cold, and in the dilute acids and alkalis, after the manner of epithelium scales, which I believe them to be. The other kind are of a regular form, chiefly rhomboidal, of great thinness and perfect transparency, insoluble in water and acids and in cold alcohol, but readily soluble in hot—properties sufficiently distinctive of cholesterine.

The molecules vary in size from $\frac{1}{8000}$ to $\frac{1}{2000}$ of an inch in diameter; and, as they are insoluble in water, and in most part soluble in an alkaline ley, they may be considered as consisting chiefly of fatty matter. They constitute a very small part of the whole.

Besides these ingredients admitting of being distinguished by the microscope, to which the meconium owes its thick consistency and viscid nature, there is another portion, the soluble part, with which they are imbued, and from which the mass derives its color and taste and probably its power of resisting putrefaction,* and which seems identical with the coloring

* This property of meconium is remarkable; after more than three months, a portion put by in a bottle containing a good deal of air, closed to prevent the drying of the substance, was found unaltered in color, and presenting the same appearance under the microscope as when first examined; the only perceptible difference was, that its upper surface was covered with a mould or mucor like that of cheese, formed of connected globules, each about $\frac{1}{3000}$ of an inch diameter.

and sapid matter of the bile, being soluble in water and alcohol.

The specific gravity of meconium deprived of air exceeds that of water.* It sinks in a saturated solution of common salt of the sp. gr. 1,148. It may be mentioned in confirmation of what has been already stated, that this mixture of meconium and brine affords, after standing some time, a kind of mechanical analysis or separation of its ingredients. The mucus-globules and epithelium scales, dyed of a dark green by the coloring matter, find their place of rest at the bottom; whilst in the supernatant fluid, slightly turbid and of a bright yellowish green hue, numerous plates of cholesterine, and a smaller number of fatty globules and molecules, are found suspended.

The quantities of meconium which I have obtained have been too small to admit of accurate analysis, and the determination of the ingredients of any one specimen. It may be briefly mentioned that every specimen that I have examined (some voided just after birth, others taken from the intestines of still-born children) has been very similar, and, in accordance with the results of the microscopical examination, composed chiefly of mucus-globules and epithelium scales, and of biliary matter containing, besides the coloring and sapid matter of the bile, a small portion of cholesterine, of margarine and oleine, with a little free acid, probably the carbonic, judging from the want of effect of nitrate of silver in precipitating it, and from the circumstance that the redness imparted to litmus paper was removed by heat.

In one instance—a specimen obtained from a healthy child immediately after birth—the proportion of water was determined; also of matter soluble in hot alcohol, separating on cooling, chiefly cholesterine and margarine; of matter soluble in cold alcohol, chiefly oleine and the coloring and sapid matter of the bile; and of matter insoluble in this fluid, whether hot or cold, chiefly epithelium scales and mucus: the results per cent. were about as follow:—

* It is readily deprived of air by crushing it under water by the pestle in a mortar, more readily than by the air-pump, owing to its tenacity; in this respect resembling cork, which, composed of elastic cells, floats *in vacuo* on water many months, and yet immediately sinks when forcibly compressed, using small portions.

23·6	Mucus and epithelium scales.
·7	Cholesterine and margarine.
3·0	Coloring and sapid matter of bile and oleine.
72·7	Water.
100·0	

These proportions, I believe, may be considered pretty correct; that of the coloring matter indeed is a little too low, owing to the difficulty there is in extracting it from the mucus and epithelium scales, with which it appears to combine as a dye, staining them permanently.

A portion of the same meconium was incinerated. It burnt, after becoming semi-fluid, with a bright flame, and left ·69 per cent. of reddish ash, chiefly peroxide of iron and magnesia, with a trace of phosphate of lime and common salt: the magnesia seemed to be the predominant ingredient, and uncombined.

The character of the vernix caseosa under the microscope is not less distinctive than that of the meconium. Being immiscible with water, it can only be examined by using the compressor: thus seen, compressed between two surfaces of glass, it is found to be composed of granular plates and molecules—the plates constituting the principal part, and producing a tesselated appearance not unlike the representation of an old Roman pavement or rude cyclopian wall. The plates are insoluble both in weak acids and alkaline leys, and in cold and hot alcohol; are of irregular form, varying in size from about $\frac{1}{666}$ to $\frac{1}{1000}$ of an inch in diameter, and very thin. Their granular character is greatly diminished by the action both of a solution of potash, and by boiling alcohol after drying, by which also the molecules are dissolved.

The vernix caseosa is apparently lighter than water, on which it floats; but this is owing to air entangled in it, as is proved by subjecting it, immersed in alcohol, to the action of the air-pump, after which it sinks in water at 60° Fahr. A specimen thus treated was found to be of sp. gr. 1,039; and it probably still contained a little air, it being extremely difficult to exhaust the whole from a substance so constituted; of which proof is afforded by the circumstance that if the trial is made with the air-pump, using water instead of alcohol, although a considerable portion of air is exhausted, as is indicated by the ebullition produced, yet sufficient remains to keep it buoyant. Proof of the same is afforded by boiling it in water; even after several hours boiling, the whole did not sink. It may be worthy of remark, in confirmation of its specific gravity being only a very little above that of water, that towards the boiling point most of it rises towards the surface (its specific gravity diminishing with elevation of temperature like that of fatty matter), and again subsides as the water cools down to the temperature of the air.

From the circumstance also of the epithelium scales being coated with, or enveloped in, fatty matter, the vernix caseosa is retentive in a remarkable degree of the water which forms a part of it. It required ten hours' exposure over a steam-bath to expel from eight grains the whole of the water belonging to it, when it was reduced to 1·77 gr.

Of a butteraceous consistence in its ordinary state at a temperature of 60°, it hardens on reduction of temperature, and becomes almost semifluid when its temperature is raised, as to 100°, admirably adapting it for a lubricating substance in parturition. But, when the whole of its water is expelled, then, even at the temperature of 212°, it loses its quality of lubricity, and is converted into a hard mass of a greasy feel—the dried epithelium scales, doubtless, absorbing the portion of fatty matter, in the same manner as flour absorbs the butter or lard mixed with it, forming when baked a crisp paste. When thoroughly dried, the fatty matter which it contains is readily extracted by the action of boiling alcohol, of sp. gr. ·838. From what is witnessed when thus treated, it may be inferred that the fatty matter is of two kinds, one being deposited on the cooling of the alcohol, the other being retained in solution; the former having the character of margarine, the latter of oleine.

A single specimen of the lubricating matter, of great purity, taken from a healthy infant immediately after birth, subjected to analysis, with the intent of determining the proportion only of the principal ingredients, was found to consist of—

13·25	Epithelium scales.
5·75	Oleine.
3·13	Margarine.
77·37	Water.
100·00	

A portion of the same was incinerated: it burnt with a bright flame, and left a very small quantity of white ash, hardly $\frac{1}{50}$ of a grain, although 40 grs. was the quantity consumed, weighed before drying. This ash, in a drop of dilute muriatic acid, dissolved, emitting a distinct smell of sulphuretted hydrogen; and the solution was clouded by adding a little ammonia; thus indicating the presence of a minute portion of phosphate of lime and sulphur—the latter in union probably with lime or potash.

Theoretically considered, as regards the origin of the two substances treated of, the preceding results seem to point out distinctly that both are excretions, the meconium chiefly derived from the liver, as I believe is commonly admitted by physiologists, and the lubricating matter from the skin.

M. Raspail is of opinion that a portion of the meconium consists of intestinal villi, founding his conclusion on microscopical observations.* I have sought in vain for the appearances which he describes; the utmost I have seen has been a solitary filament now and then mixed with the plates: often indeed there has been an appearance of a greater number, but these on careful inspection have proved to be the margins of plates of cholesterine from their position having a linear or filamentous appearance.

Vauquelin and Buniva, after examining the vernix caseova were led to infer that it is not an excretion from the infant, but a deposit on its surface of a peculiar nature from the liquor amnii, derived from its albuminous part by a certain change.† This view, I apprehend, cannot now be sustained, and does not require to be controverted. Bichat saw the unreasonableness of it, and rejected it merely from the circumstance that no such deposit is found on the umbilical chord, or on the inner surface of the amnios, and came to the conclusion, which seems most just, that it is derived from the skin of the fœtus, and is a secretion similar to that which takes place after birth in many parts of the cutaneous system.‡

* Nouveau Système de Chime Organique, ii. 466.

† Ann de Chim., xxxiii. 274.

‡ Anatomie Descriptive, v. 393.

XV. MISCELLANEOUS OBSERVATIONS ON THE TADPOLE, ON THE ALBUMEN OF THE NEWLY-LAID EGG, ON THE GROWTH OF BIRDS AND THEIR SPECIFIC GRAVITY, AND ON THE STOMACH OF FISHES IN RELATION TO DIGESTION.

1. *On the Tadpole.*

I SHALL offer a few preliminary observations on the jelly in which the ova of the frog are enveloped, a matter somewhat peculiar in its nature and remarkable in its properties, and performing no doubt an important part in the early stage of batrachian life.

The quantity of solid matter that this jelly contains is extremely small—less than 1 per cent.; one specimen evaporated to dryness yielded ·49 per cent. It had been kept before evaporation in a moist filter, covered, to draw off what might be considered superfluous water. Another yielded only ·37 per cent.; and another even less. The dry films, on the addition of water, rapidly expand from imbibition, but they absorb less than they have lost, and consequently do not recover their original bulk. A portion that before drying weighed 40 grains, after absorption of water weighed only ten grains.

The refractive power of the jelly differs so little from that of water, that, when immersed, the ova detached, it is hardly discernible. Under the microscope, it appears perfectly transparent; but when evaporated on a glass support, so as to be moderately concentrated, granules and spindle-shaped forms become visible. By boiling in water for several hours, it is in great part dissolved, a just perceptible fibrous matter remaining. The solution is very slightly affected by infusion of nut-galls and corrosive sublimate; by both a perceptible turbidness is produced. A like effect is occasioned by nitrate of silver. A solution of this salt—12 grains to the ounce of water—occasions a slight contraction of the jelly, and its discoloration on exposure to light; now, seen under the microscope, it seems to be composed of globular masses of unequal dimensions, having somewhat the character of cells. It is acted on by the mineral acids. At first, on immersion in them, it contracts a little; it is slowly dissolved without change of color. In the same acids heated, it dissolves rapidly; in the nitric imparting a yellow color, in the muriatic without change of color, in the sulphuric occasioning a brown, almost black, discoloration, with the disengagement of sulphurous acid gas. These acid solutions, when largely diluted, become slightly turbid.

Mr. Brande, who was the first to examine this substance—and I am not aware that it has been examined since—was led by his experiments to the conclusion that it is a peculiar matter, and, as regards its chemical properties, intermediate "between albumen and gelatine."* From the results I have mentioned, in some particulars not entirely agreeing with his, I am disposed to view it as a variety of albumen; and, physiologically considered, the part it performs seems to be tolerably in accordance. Its uses appear to be, first to protect the ovum, and, after its hatching, to afford food to the tadpole. As regards the first, by its glairy adhesive quality, it helps to retain the ova in the water in which they have been shed, and to defend them from the attack of insects and fishes. It may also retard their freezing, and so preserve their vitality; for I have found that after having been frozen, their death has ensued, denoted by their development on thawing being altogether arrested. That the jelly serves as the food of the tadpoles, is proved by its rapid disappearance after their birth from the egg. It may be useful also in preserving the ova from desiccation (the water in which they are all but failing), by its imbibing power—and this although I do not find that it retards the evaporation of its aqueous portion—equal quantities of water and jelly exposed to the air having been reduced to dryness in about the same time. May not this property aid in explaining some of the statements made with the intention to prove that the frog and toad may be produced without passing through their larva stage?†

The ova of the frog in some of their properties nearly resembles those of fish. They appear to be composed chiefly of an albuminous matter and oil, the former coagulable by the admission

* Philosophical Transactions for 1810, p. 219.

† See Proceedings of Royal Society, vol. vi., p. 292.

of water, and, after coagulation, redissoluble by admixture with common salt, or with any other of the neutral salts; and further, like the ova of fish, not coagulable at a temperature of 180°, or even of 212° Fahr., if not immersed in water. Their loss of vitality is denoted by their becoming of an opaque white; thus, too, resembling the same ova, and from the same cause, the imbibition of water.

From the observations made by Mr. Higginbottom—these in opposition to those of Dr. Edwards—it would appear that neither the ova nor the tadpoles of the batrachians are influenced by light;* with which the trials I have made on those of the frog agree; as they do also with his results on the latter, shewing how materially they are affected in their progress by temperature, and food and air—a low temperature and a scanty supply of food retarding their development, and *vice versâ*. The influence of air, I may remark, is well shewn by placing the spawn of the frog in a deep glass vessel full of water: the ova nearest the surface will first be hatched, those lower next, and so on, many days intervening between those first and last appearing.

Mr. Higginbottom found tadpoles kept in a deep cellar, the temperature of which varied from 50° to 54°, still in their larva state so late as the middle of November, or about eight months from the time of their leaving the egg; I have found some in this state so late as the 13th of February, or about eleven months from their birth. These were kept in the open air in an earthenware pan, were supplied with very little food, and with no water, except by rain. The last survivor was active when the temperature of the water was only 33°. On the 1st of March it was found dead, and then its hind extremities were partially protruding. Dr. Edwards, it would appear, has kept tadpoles equally long, and under the same circumstance of a very scanty supply of food.†

It is well known that the metamorphosis of the tadpole is attended with a diminution of bulk. The following results which I have obtained may be adduced in illustration, and for the further purpose of shewing that, though there is a decrease of volume, there is a proportional increase of solid matter in the transition.

* Philosophical Transactions for 1850, p. 401.
† On the Influence of the Physical Agents of Life (English Trans.), p. 52.

The ovum of a frog, carefully detached from the including jelly, weighed 0·1 gr.; dried over steam, it was reduced to 0·04 gr., or 4 per cent. solid matter.

A young tadpole, its branchial filaments and tail distinct, deprived of excess of moisture by blotting-paper, weighed 0·24 gr.; dried, was reduced to 0·06 gr., or 6 per cent. solid matter. It measured 0·40 inch in length, including its caudal fin, which was 0·24 inch long.

A tadpole, its hind feet formed, and protruding, its tail but little diminished, weighed 5·3 grs.; dried, was reduced to 0·4 gr., or 7·54 per cent.

A tadpole, its fore and hind feet formed, its tail but little diminished, weighed 2·8 grs.; dried, it was reduced to ·28 gr., or 10 per cent. solid matter.

A young frog, fully formed, its tail entirely absorbed, weighed 1·4 grs.; dried, it was reduced to ·18 gr., or 12·35 per cent. solid matter.

This proportional increase of solid matter, I find, is associated with the formation of bone. This is well shewn by the result of incineration of the tadpole, and of the young frog. In the one instance, when the animal matter is entirely consumed, the residue appears without any regular form; in the other (however young, provided it be fully formed), a distinct skeleton is obtained. And, examined chemically, I have found it to be composed chiefly of phosphate, with a little carbonate of lime. In the residual ash obtained from the tadpole, a reddish spot was observable, owing its color to peroxide of iron, and, it may be inferred, derived from blood in the heart. There was also seen a convoluted marking, attributable, it may be conjectured, to the intestine; and in confirmation, it may be remarked, when this portion of the ash was microscopically examined, some infusorial forms were to be seen in it, which might have constituted part of the food.

From seeing the reddish spot just alluded to, I was induced to make trial of the blood corpuscles. They were incinerated on slides of glass fit for use under the microscope, exposed to heat in a crucible. The whole of the animal matter consumed, the minute residual ash, in a finely granular state, exhibited the forms of the corpuscles, with a partial reddish tinge just perceptible, some circular, some elliptical.

On the temperature which the tadpole and young frog are capable of bearing, I have made a few trials, the results of which are briefly the following: — A temperature gradually rising to 94° has been borne by tadpoles for a considerable time without loss of life, or sensible loss of activity. At about 96° or 97° their activity has diminished, and they have become torpid. In this torpid state they have sustained a temperature of 100°, not long continued; on its reduction they have become active. The less advanced in growth, the more tolerant they have proved of the influence of heat. Thus, when two tadpoles well advanced in size, and other two of smaller size, were kept in water of 100° for the space of thirty minutes, gradually cooling from this degree, the first were found motionless and never recovered; the second, from a torpid state soon recovered their activity. At a temperature between 110° and 115°, even the youngest have been found to die, and that rapidly; in the instance of the larger, the abdomen has been seen to burst, and the intestines to protrude. The full-grown tadpole and the young frog are in a remarkably less degree tolerant of heat. A temperature so low as 88° has proved fatal to both.

The influence of a certain degree of heat, in rendering the tadpole and young frog torpid, brings to recollection what we read of the effect of tropical heat with drought on the alligator, which is said to remain in the passive condition just mentioned so long as the hot and dry season lasts.

I have also made trial of the action of salt water on the tadpole. In a solution of common salt, of a sp. gr. 10,425, some lived about half an hour; in a very much weaker solution, viz., of sp. gr. 10,130, the activity of others was not diminished; this after half an hour; after two hours they became languid; half an hour later they were found dead. In a still weaker solution, one of sp. gr. 10,065, they lived about forty hours.

May not this effect of salt-water, and so dilute as that used in the two last trials, inferior to that of the sea, be considered as one of the causes tending to narrow the habitats of the species?

2. *On the Albumen of the Newly-laid Egg.*

The albumen of the egg of the common fowl, newly laid, has properties differing in some particulars from those of the albumen of the stale egg. One of these, and that which is best known, is the milkiness which it exhibits when dressed for the table, provided the egg be not put into water of too high a temperature, and kept there unduly long. Another is seen in the manner of coagulating.

Though the differences alluded to may be well known, I am not aware that they have been elucidated by experiment. This I mention as a reason for entering upon the inquiry—if I may use that term in reference to a subject which to many may appear too trivial to deserve the name.

1*st. Of the Milkiness.*—I may premise, that if the albumen of two eggs—one newly laid, one kept for a week or more—be compared before being boiled, each will be found to be equally transparent, commonly little or no milkiness will be appreciable in either of them, proving that the milkiness which appears in the newly laid one, after being boiled, is the effect of heat. The temperature required is about 150° Fahr. After immersion in water of this degree of heat for about two minutes, the white of the newly-laid egg will be found on examination to have very much the appearance of milk, being liquid, with little or no viscidity, and mixing with water, and imparting to it, and to a large quantity, the same quality, and for a considerable time. Seen under the microscope with a one-eighth-inch power, the milky albumen is found to abound in granules of great minuteness—so minute, indeed, as to be only just distinctly visible—with which are intermixed a few particles of a larger size, as if aggregates of granules.

These granules, which are the cause of the milkiness, appear to be a modification of albumen. This is inferred from not being able, by treatment with ether, to extract from the milky fluid any oily or fatty matter, and from the circumstance that the granules disappear when acted on by a solution of potash.

2nd. Of Coagulability.—The difference as to this property, seen in comparing the white of the newly-laid egg with that of an egg kept some time, though only small, is pretty well marked. It is shewn by the following trial. To two similar glass tubes, equal portions of the white of an egg laid the same day, October 26, which may be called No. 1, and of one laid on the 23rd April, No. 2, were introduced, and were subjected to different degrees of temperature, by immersion in heated water for the space of two minutes.

At 140° no change was seen in either.

At 150° falling to 145°, No. 1 had become in great part opaque, *i.e.* of milky opacity; No. 2 only at the surface, and very slightly.

At 162° falling to 159°, No. 1 shewed a milky opacity throughout—had lost little of its fluidity, as shewn by being very easily poured out; No. 2 was in part so coagulated, as to admit of inversion; a gelatinous coagulum adhered to the inner surface of the tube.

The results of another trial were the following:—

At 155° falling to 145°, about half of No. 1 acquired a milky opacity—its upper portion; No. 2 had become so to less than the extent of one quarter.

At 166° falling to 160°, No. 1 had become opaque throughout, retaining its fluidity. No. 2 had also become opaque; it had lost its fluidity—at least it did not flow when the tube was inverted.

At 175° falling to 168°, No. 1 was still fluid.

At 195° falling to 185°, No. 1 was coagulated throughout: the coagulum moderately soft. These results seem to shew that the white of the newly-laid egg is more readily affected by heat of a certain temperature than that of an egg exposed some time to the air, as indicated by the appearance of milkiness it exhibits, and yet that, within a certain range of temperature, the amount of coagulation or the degree of firmness is less. Many other trials which I have made have given similar results. I shall confine myself to the mention of one.

A portion of the white of a newly-laid egg was poured into a tube, and over it a portion of one that had been laid seven days; they were kept apart by a thin pledget of cotton. At 162° falling to 152°, an opacity appeared at the surface of both at the same time; but it spread through No. 1 more rapidly than through No. 2. The former was opaque throughout, when the latter was transparent except at the surface. Kept in water till it had fallen to 148°, No. 2 also had become opaque throughout; it was so firmly coagulated that it had to be broken up before it could be removed from the tube. No. 1, on the contrary, was of very soft consistence, and most easily removed. The trial was repeated, reversing the position of the two, the white of the newly-laid egg over that of the older. The results were similar. The former was soonest affected; the latter, in the end, was most firmly coagulated.

That the difference of qualities which I have described is owing to exposure to, and the action of, atmospheric air, can hardly be doubted.* The proofs seem to be sufficiently clear. The newly-laid egg contains little or no air;† and if atmospheric air be for most part excluded, its absorption checked, as by lubricating the shell with oil or any oleaginous matter, the albumen retains for a considerable time the qualities of the newly-laid egg. This is a fact well known to dealers in eggs.

The exact time required for the change to take place, owing to the absorption of air, I cannot exactly say; it varies, I believe in some measure, according to the season—a shorter time in winter being required than in summer; the egg in the former season, owing to lower atmospheric temperature, contracting more in bulk, as regards its substance, than in the former. A very few days, as from five to six at furthest, seem to be sufficient. In April I have observed the milkiness on the fourth, but not later. On the contrary, if air be for most part excluded, I do not know how long the quality of the newly-laid egg may not be preserved. I have found an egg laid in the month of April, and then smeared with butter, hardly appreciably changed at the end of six months.

Nor can I speak with any exactness respecting the amount of air, of oxygen absorbed, or of other alterations that may be effected in the composition of the egg by its action. All that I have yet ascertained is, that, with the absorption of oxygen in the instance of the stale egg, carbonic acid is formed and ammonia, and the color of the albumen is darkened, it becoming of a light brownish yellow, and at the same time acquiring a smell somewhat unpleasant, and a taste, as is well known, not agreeable. The putrefactive process, I believe, does not take place, however long the egg may be kept, unless there be some

* May not the disappearance of the curd which is seen in the salmon when dressed fresh from the sea, the clean fish of the angler, be owing to the same cause, viz., the absorption of oxygen on being kept exposed to the air, and the liquefaction of the curdy matter, a consequence of that absorption—a liquefaction similar to that which the fibrin of the blood undergoes from the action of oxygen.

† See Anatomical and Physiological Researches, by the author, vol. ii., p. 222.

admixture of the yolk and white. Further on this subject will be resumed.

In conclusion, I may add that though I have confined myself in the account of the foregoing observations to the albumen of the egg of the common fowl, the trials I have made have been extended to that of the egg of the duck, the turkey, and guinea-fowl, and that the results have been similar. Slight variations, indeed, have been observed, but not, I think, greater than have been witnessed in experimenting upon different eggs from the same fowl, or upon eggs of different fowls of the same kind.

3. *On the Growth of Birds.*

The observations I have to offer on this subject have been confined chiefly to four birds—the martin, the common fowl, the turkey, and goose; the first feeding on insects, and never but on the wing; the last on vegetables, subsisting chiefly on grass; the barn-door fowl and turkey using a mixed diet of seeds and insects.*

1. *Of the Martin* (*Hirundo urbica*).—The young of this bird, I am led to infer, is capable of taking flight in from fifteen to twenty days from the time of hatching; for in about this time, reckoning from the day that I have found the broken egg-shells thrown out of the nest, I have seen the young birds first on the wing.

On the 16th of July, a young martin taken from its nest, not in full feather, weighed 346 grains; opened, much fat was found within its abdomen. Thoroughly dried on a steam-bath, its weight was reduced to 112 grains.

On the 9th of July, a young martin shot on the wing, supposed to have taken flight for the first time that morning, weighed 303 grains; thoroughly dried, it was reduced to 110 grains.

On the 4th of August, a young martin, which it was believed had left its nest many days, shot on the wing, weighed 270 grains; some flies were found in its stomach. There was less fat within its abdomen than in that of the imperfectly-fledged nestling. By thorough drying it was reduced to 110 grains.

* Although the food of these birds is so different, their temperature is but little different, leading to the conclusion that no special kind of food is needed for the production of animal heat.

On the 7th August an old martin was shot. It weighed 273 grains. No fat was found within its abdomen. By thorough drying it was reduced to 105 grains.

2. *Of the Common Fowl.*—The average weight of six eggs from a hen of the Dorking breed was 986 grains; the average weight of the same number of chickens, the produce of these eggs, ascertained a few hours after hatching, was 717 grains.* One of this brood, when a week old, had increased in weight to 950 grains. Two of the same brood, a cock and a hen, when three months old, had increased, the former to 3lbs., the latter to 2¾lbs.

By a gentleman who has paid much attention to poultry, I am informed that in Yorkshire the average weight of barn-door fowls, hatched in spring and killed about Christmas, is from 6 to 8 lbs.: the difference chiefly owing to the sex; the males the heaviest.

3. *The Turkey.*—I have only one trial, the result of which I can give, on the growth of this bird. I am indebted for it to a friend, on whose accuracy I can depend. A young turkey, from an egg which before incubation weighed 2¾ oz., on the day it was hatched (including the empty shell), weighed 1¾ oz.; when a fortnight old, it had increased in weight to 3¾ oz.; when four months, to 10 lbs. It was one of a flock allowed to roam the woods; it had not been put up to fatten, and, as I am informed, was only moderately fed.

From the gentleman before mentioned, I learn that the weight of the turkey in Yorkshire at Christmas varies from 16 lbs. to 25 lbs.

4. *The Goose.*—The eggs of this bird vary greatly in weight —more than any others which I have tried. Thus, two taken from the nest at the same time differed as much as 191 grs.—

* The weights of the several eggs, including the shell, and of the chickens were as follows, in grains:—

973·5	727
1042	691
1000·5	692·5
989	757
931	727·5
984	706·5

The weight of the shells, they being broken and mixed, could not be ascertained: they vary so much that an average would be of little use for the purpose of ascertaining the loss during incubation.

one weighing 2,920 grs., or about 6·08 oz., the other 2,729 grs., or about 5·66 oz. A gosling hatched on the 26th April, on the 30th of that month weighed 1,987 grs., or 4·14 oz.: another hatched on the 17th of the same month, tried on the same day as the first, weighed 5,579 grs., or 11·60 oz. For the following results I am indebted to an intelligent farm-servant:—An egg, a few days after it had been laid, weighed 7 oz.; the gosling, the same day that it was hatched, weighed 4 oz.; after four days it had increased in weight to 6 oz.; after thirty-four days, to 6 lbs.; after sixty-eight days, to 7 lbs. My Yorkshire correspondent estimates the weight of the goose at Christmas at from 12 lbs. to 14 lbs.

These few examples may suffice to shew how rapid is the growth of birds. Comparing the growth of the swallow, fed by the parent birds, with that of the young of the turkey, common fowl, and goose, which have to find their food, a marked difference is observable—the growth of the one being so much more rapid than that of the other; and it can hardly be doubted that the same will be found to be the case more or less generally. Again, if we compare the eggs of those birds which feed their young till they are capable of taking wing, with the eggs of other birds the young of which, so soon as they are hatched, have to provide for themselves, a difference also will be found, the eggs of the latter being, I believe, without exception, proportionally larger. How little, as regards size, does the egg of the eagle differ from that of the goose; how very small is the egg of the cuckoo compared with that of the partridge—the latter birds differing but little in size. And is there not design in this as well as in the different degrees of rapidity of growth? Is not the gosling, during the first days of its existence after leaving the egg, more dependent for nourishment on the residual included yolk than the eaglet? And is it not so in other instances?*

* The subject of the size of the egg in relation to the size of the parent bird is an interesting matter, and well adapted for inquiry. Probably, were a strict comparison made, a variety of circumstances would be found to have an influence in regulating the relation—circumstances chiefly referrible to the manner of feeding the young birds, as alluded to above—and the supply of food, whether scant or abundant. By way of illustration, I would ask, may not the eggs of the missel-thrush be so large, having to be hatched early in the spring? May not those of the turkey be smaller than those of the goose, their time of hatching being considerably later? May not those of the Cochin-China variety of the common fowl be smaller than those of any of our northern varieties—the former being the breed of a warm climate, where food,

In the mammalia there appears to be a relation between the period of growth, that required for the attainment of their maximum power, and their duration of life; but in birds this relation seems to be entirely set aside. And is it not because a rapid attainment of their full power is essential to their existence? This is strikingly the case in the instance of the swallow; and in other birds we witness it in degree according to their habits and wants. An advocate for final causes might here find ample argument in support of his doctrine. If the means of this rapid growth be considered, I believe they may be referred to two things mainly, viz., abundance of food, and activity of the digestive organs of the young birds, other circumstances aiding. Take the instance of the martin, the growth of which is so rapid, that in the short space of a fortnight or three weeks it acquires a bulk and weight exceeding those of the parent bird. Those who have watched a swallow's nest cannot but have been struck by the wonderful industry and assiduity displayed by the old birds in supplying their brood with insect food; and the quantity of droppings from the nest, evacuated by the young birds, is hardly less remarkable; in twenty-four hours as many as forty-one have been counted, which had fallen on the flags below. These, collected and dried, weighed 70 grains; on examination they were found to be partly fæcal, the undigested remains of insects, and partly urinary, the latter consisting chiefly of lithate of ammonia, with a very little urea. In the other birds of which notice has been taken, the same conditions as to digestive power and the supply of food exist, varying only in degree. And the same, we believe, may be said of birds generally, whether their young are sustained for a time by the parent birds or have to find their own sustenance, the breeding season of all of them, and the spots selected for breeding being suitable—the one a time of plenty, the other affording a kind of food best adapted to the wants of the several species. In the other examples, as in the instance of the swallow, the activity of digestion is also con-

it may be inferred, is always abundant. The small size of the egg of the cuckoo is a striking example of the adaptation. Amongst the peculiarities of this remarkable bird, I am not aware that its diminutive testes have been described by any author. In one instance that I examined them during the breeding season, I found that they bore in relation to size a proportion very like that of their eggs to those of other birds, they were so small. Is not this in harmony with the character of the bird—so little amatory and philoprogenitive?

firmed by the copiousness of the evacuations; the fæcal (as in that also), consisting chiefly of matter of an indigestible kind, from which all that is nourishing has probably been extracted—the urinary chiefly of lithate of ammonia. I have alluded to aiding circumstances: these, I believe, are to be found in some of the peculiarities of organisation of birds and their functions, promoting a high temperature, with comparatively a small waste of matter in respiration.* A powerful heart, a quick circulation, richness of blood in red corpuscles, cutaneous and mucous tissues of little activity, may be mentioned as some of these; and as resulting from these, little need of fluid ingesta and little loss of fluid, either cutaneously or by the action of the kidneys and intestines.

4. *On the Specific Gravity of Birds.*

The experiments I have made on this subject have been limited to the following birds—the martin, water-ouzel, snipe, wood-owl, merlin-hawk, and wren.

The trials made have been of two kinds—one on the birds after the removal of their feathers, to secure which they were skinned; the other with their feathers on. I shall notice the former first.

The specific gravity of a martin (*Hirundo urbica*) was found to be as nearly as possible that of the water in which it was weighed. Its cylindrical bones contained no air, but a colourless marrow.

The specific gravity of a snipe (*Scolopax gallinago*) was 1,038. Its bones were destitute of air. The marrow in the long bones of the legs and wings was reddish-yellow.

The specific gravity of a water-ouzel (*Turdus cinclus*) was 1,200. Its long bones contained a reddish marrow.

The specific gravity of a wood-owl (*Strix stridula*) was a little less than that of water; the body, which was a little heavier, having been buoyed up by the head, that being of less specific gravity, owing to the cellular construction of its bones.

* According to the experiments of Messrs. Allen and Pepys (Phil. Trans. for 1819 and 1829), in tho instance of the pigeon and guinea-pig, allowing for difference of bulk, the quantity of carbon consumed by each, indicated by tho carbonic acid produced in respiration, varies but little—the bird, per minute, producing ·5 cubic inch; the other ·62 inch; the volume of the former 28 cubic inches, of the latter 39 cubic inches.

The specific gravity of a merlin-hawk, its head cut off, was about the same as that of water; its head was a little lighter, from its cellular structure; air was found in its long bones, the femoral and humoral; and a yellowish fat in its tibiæ and radii. Notwithstanding the presence of air and marrow in these bones, the wings and legs, deprived of their feathers, sank in water.

The specific gravity of a wren (*Motacilla troglodytes*) was 1,017.

The results of the trials on these birds with their feathers on, have been, as might have been expected, less precise. I need hardly observe, that they all floated in water, or that as their feathers became wet on submersion (submerged by a weight attached), the more they became wet the more air was detached, and the more the specific gravity was increased. I may mention an instance, that of the wren: when first submerged, its specific gravity was found to be 0·890; after being under water twelve hours, it had increased to 0·960.

The least unsatisfactory trial I have made has been that on the water-ouzel. Its specific gravity was found to be 0·724. When under water, few air-bubbles escaped from its feathers, owing probably to their resisting wetting, from the oil with which they are pruned, *that* being abundantly supplied by the large oil-gland with which this bird is provided.

The bird the specific gravity of which I have found lowest—if I may so speak of an approximate result—has been the merlin-hawk; it was 0·570.

In conclusion, it may be remarked, that, judging from the foregoing results, the specific gravity of the body of birds is concerned but in a very subordinate manner with their aptitude for aërial locomotion. That aptitude seems to depend on other circumstances, such as the great lightness of their feathers, owing to the air which they contain; the little tendency of water to adhere to them, even when exposed to rain; their form and arrangement, so admirably adapted for the purpose of impulse—the high temperature of the body expanding the contained air, and the immensely powerful muscles, the pectoral, belonging to the wings. Is not the power of flight of each species in a great measure proportional to these conditions?*

* The wild swan deprived of its feathers, I find from a note made in March, 1838, sinks in water, with the exception of its long neck, which floats. A series of osseous

5. *On the Stomach of Fish in Relation to Digestion.*

The observations I have to offer on this subject have been made chiefly when on angling excursions. They were begun in consequence of having my curiosity excited by so often finding the stomach of the salmon and sea-trout perfectly empty of food. This, indeed, is a well-known circumstance, giving rise to the popular notion that these fish, after leaving the sea, abstain altogether from food.

This empty state of stomach led to the question, Is it accompanied or not by the presence of the gastric juice? To endeavour to find an answer I employed test-papers, taking it for granted, that if any gastric juice were present, it would be denoted by an acid reaction. For the sake of precision, I shall relate a few of the many instances in which I used this test, beginning with the salmon (*Salmo salar*) and the sea-trout (*Salmo trutta*).

On the 24th August, four salmon taken by net in the sea, at the mouth of the Crede, one of the salmon-rivers of the isle of Lewis, were opened, and about three hours after their capture. The stomach of each was empty, that is, contained no solid food, nor indeed any liquid of any kind, merely a little adhering mucus. No effect was produced on litmus paper, applied to different parts of its lining membrane.

The stomachs of other four, taken in the sea at the same place on the 26th of the same month, were also found empty. Tested by blue litmus paper, no effect was perceptible; tested by reddened paper, a very slight alkaline reaction was indicated.

On the 27th August, the 3rd and 6th of September, the stomachs of two salmon and of one grilse, all taken with the

air cells extends from the dorsal and thoracic vertebræ to the cervical. A little of the air obtained from these cells was not diminished by agitation with lime water: it consisted of 83·3 azote, 16·7 per cent. oxygen. The structure of this bird is very admirable. Its carotids pass up in a groove in the anterior surface of the spinal column, so as to be well defended from violence and loss of temperature. Its abdominal aorta above the origin of the emulgents is enclosed in a dense ligamentous sheath: below, "it loses this sheath and acquires what I believe to be an internal muscular sheath, one composed of reddish fibres, following the course of the vessel—these somewhat elastic, easily broken, and partially divided by the application of a ligature, not unlike the muscular coat of the human intestine. Under the microscope their appearance is much the same as those of the muscular coat just mentioned: they are of extreme fineness, are parallel in their course, and longitudinal." Thus was noted the appearance of the structure at the time it was examined.

fly in fresh water at Loch Morsgael, also in Lewis, were opened immediately after their capture. As in the preceding instances, they were found empty; and tested in the same manner, they gave like results.

With the young of the salmon, before their descent to the sea, the parr and the smolt, the results of all the trials I have made have been different. These fish, it is well known, are greedy feeders. I have always found food in their stomach, chiefly flies, and the stomach has always exhibited a distinct acid reaction. The fish I tried were most of them from a tributary of the Teith and from the Welsh Wye, and the examination was made as soon as they were taken from the water.

The number of sea-trout which I have opened has been large, all of them taken with the fly in fresh water, and chiefly in the lakes of Lewis and Harris, which I had the privilege of fishing through the kindness of Sir James Matheson, the proprietor of the former, and of Lord Hill, the lessee of the latter. In the great majority of instances the results were the same as those afforded by the salmon, the stomachs being found empty, and without effect on test-paper. In a few, the results were different; these may require notice. Of twenty taken in Harris on the same day, in Loch Vosmet, September 8th, three had food in their stomach, believed to be small trout (they were partially digested, so that their character was obscure), an acid reaction was distinct in each. Of forty-two taken in Loch Morsgael, August 28th, the same reaction was witnessed in the stomach of two; a few minute flies, and these only, were found in each. In one instance only have I ever noticed a reaction of this kind in the sea-trout with an empty stomach, and that was in one of the forty-two just mentioned.

I shall now pass to the common trout (*Salmo fario*), on which I have made very many observations, and these at different seasons of the year, and on individuals of different sizes, from the brook-trout of two or three ounces to the lake-trout of as many pounds. The results have been remarkably uniform. In the great majority of instances, food has been in the stomach,*

* One exception since the above was written I have met with. The instance was a trout of $2\frac{1}{2}$ lbs. taken in Windermere in excellent condition. Its stomach shewed a distinct alkaline reaction, and yet there were several small fish—minnows—in it; but these, excepting one, exhibited no marks of incipient digestion, and that only very slightly, and test-paper applied to it indicated a very feeble acid action. The

and in all a distinct acid reaction has been witnessed. On the contrary, in the very few in which it has been found empty, that reaction did not take place; the lining membrane was either neutral, or showed a very slight alkaline reaction.

On the charr (*Salmo umbla*) and the grayling (*Thymullus vulgaris*), the former taken in Windermere, the latter in the Wye, the Teme, and the Lugg, the observations I have made have been few. As the results accord with those already described, it may suffice to mention, that when food was found in the stomach an acid reaction was detected, and *vice versâ*.

On the perch (*Perca fluviatalis*) my observations have been limited to four; these were female fish, each of about a quarter of a pound. In the stomachs of two in which small fish were found partially digested, there was a strong acid reaction: the same acid reaction was seen in the two appendices pyloricæ of one fish; in those of the other it was alkaline. In the other two, the stomachs were found empty: one of them shewed an alkaline reaction, the other had no effect on test-papers. The appendices pyloricæ of both these fish had an alkaline reaction.

On the fario (*Coregonus fera*) of the lake of Geneva, my observations have been restricted to two; these were made at Geneva on the 26th June. Each fish was of about half-a-pound. Broken up food, which effervesced with an acid, was found in the stomach of each. As might be expected, there was no acid reaction, nor was there any alkaline exhibited. The fluid of the appendices pyloricæ was alkaline.

A barbot (*Lota vulgaris*) from the same lake, was examined on the same day. Its stomach was empty, and shewed an alkaline reaction.

On sea-fish, the trials I have made have been fewer still; they have been limited to the dog-fish (*Scyllium canicula*), the haddock (*Morhua æglefinus*), the sea perch (*Perca marina*), and red gurnard (*Trigla cuculus*). The trial with the dog-fish (it was a solitary one) was made in June in Sutherland. The fish, before it was opened, had been taken over twelve hours. In its stomach there was a considerable quantity of food in a pultaceous state; its acid reaction was decided. Of five haddocks examined, food was found in the stomachs of four; it consisted of pulta-

conclusion I drew was, that the trout was laying in a store of food to be ready for digestion when needed, and was not hungry at the time he captured the minnows.

ceous gritty matter, with which in one instance were mixed broken spines, like those of the aphrodita; in the others, the remains of crabs and some bivalve shells, the latter little altered. In each the reaction was alkaline. This, I apprehend, was no more than might have been expected, considering the presence of carbonate of lime in the spines and in the shells, with the additional circumstance that many hours had intervened between the taking of the fish and the experiments made on them. To endeavour to ascertain whether any free acid had been secreted by the stomach, the contents of one were subjected to chemical examination, and with positive results, chloride of lime being detected. In the instance in which the stomach was found empty, a very slight acid reaction was just perceptible.

The results of the examination of the stomach of two sea perch were similar to those last described. Each fish weighed about three-quarters of a pound, and both were females. Shrimps were found in the stomach of each, and each stomach had an alkaline reaction.* The stomachs of two gurnards, taken off the coast of Cumberland in November, contained shrimps and the fragments of other crustacea; each shewed an alkaline reaction; the fluid of the appendices pyloricæ of one of them had an alkaline reaction.†

Though my observations have been principally directed to the stomach of the fish I have examined, they have not been entirely confined to that organ; in many instances they have been extended to the intestines. In all these, their contents have been found, as might be expected, invariably alkaline; and this whether the part tested was near the pylorus or distant from it. The same alkaline reaction, with the exceptions mentioned, was witnessed in the fluid, the secretion of the appendices pyloricæ—the test-paper being applied to any one of those tubes after having been divided transversely, when a little liquid commonly exuded.

* The roes were very small. In the cavity of the abdomen of each there were masses of fat, which I first took for milts, so close were their resemblance to these organs in their forward stage, but I soon satisfied myself of the mistake. Is it possible that Cavolini, who describes this fish as a hermaphrodite, could have made the same mistake? The fish was from the coast of Cumberland, and were taken in the first week in October.

† In the œsophagus of the fish, just above the cardia, there is on each side a kind of tooth, which in my notes is described as flat, circular, and rough. I am not aware that this has hitherto been noticed by any ichthyologist.

In connection with these observations, I have made a few on a subject nearly allied to them, viz., the action of the gastric juice, and of the other fluids supposed to be concerned in digestion after death, on the secreting organs and parts adjoining them—an action first observed and described by John Hunter in the instance of the human stomach.

In the common trout, the stomach of which is so rarely without food or without a free acid, indications of the action in question must be familiar to every angler in the habit of opening the fish he takes. According to what I have observed, several hours commonly intervene between the capture of the fish and the witnessing the effects, varying as to time with the temperature of the air and other circumstances of a less appreciable kind. The organs or parts most liable to suffer I have found to be not the stomach itself, but the parieties of the ribs on the side contiguous to the appendices pyloricæ and the upper portion of the intestinal canal. These bones have been seen bare and projecting internally, with softening, and often a breach of the intestine, and yet the stomach, at the time, has been little changed—softened only in a slight degree. The only instances that I can recall to mind of its having been partially dissolved and ruptured have been of young fish, especially the young of the salmon—the parr—taken at a season when they were feeding greedily.

These remarks on the trout are applicable to the charr and the grayling; in fish of each kind I have witnessed effects similar to the preceding.

The observations I have made on the salmon and sea trout, in relation to this action, have been fewer than I could wish; and it has so happened that they have been confined to those the stomachs of which were empty and shewed no acid reaction. In all but one no *post mortem* effect was perceived; it was one of seven sea trout taken on the 10th September, and opened on the following day. In this exceptional one, the bones contiguous to the appendices pyloricæ were partially laid bare, as if from solution of their covering. Neither in the stomach of this fish nor of the other six was there any food found, or any traces of acidity. I may mention the results of a comparative experiment made with the common trout and the sea trout, shewing the marked difference of effect in the two instances. The subjects of the trial were a trout of the common kind, from the river Rothay, of about half a pound, and a sea trout from Maryport of three pounds. After a ligature had been applied to the gullet and also to the lower portion of the intestine of each, the viscera of both were taken out, and placed in a glass vessel and covered. The temperature of the air was little variable, about 65°. After twenty-four hours an examination was made. In the stomach of the common trout, pultaceous food was found with an acid reaction; its coats were but slightly softened; the intestine was reduced to a shreddy state, most remarkable in its upper portion, and its contents had escaped; these shewed an alkaline reaction. The stomach of the sea trout was empty, with the exception of a little adhering milkwhite mucus, as were also the intestines, with the exception of a little yellow slime. The stomach was neutral; the intestine slightly alkaline. Neither exhibited any appearance of softening.

As some of the conclusions deducible from the preceding observations, I would beg to submit the following for consideration:—

1. That the gastric juice, and probably the other fluids concerned in the function of digestion in fishes, are not secreted till the secreting organs are stimulated by the presence of food—a conclusion in harmony with a pretty general physiological law, and in accordance with what has been best ascertained respecting the gastric juice in other animals.

2. The probability that the gastric fluid—a fluid with an acid reaction—is less potent in the instance of fishes as a solvent than the alkaline fluid of the appendices pyloricæ;* and that, even as regards the gastric fluid, its acidity is not essential to it, as its action does not appear to be arrested when it is neutralised by the presence of articles of food abounding in carbonate of lime.

Lastly, as a corollary from the first, may it not be inferred that the migratory species of the salmonidæ, such as the salmon and sea trout, which attain their growth and become in high condition in the sea, there abundantly feeding and accumulating adipose matter, though not always abstaining in fresh water,

* From the Researches of M. L. Corvisart it would appear that the fluid of the pancreas of the mammalia has a high digestive power. See Dr. Brown Sequard's Journal de la Physiologie, No. XI., 1860.

which they enter chiefly for the purpose of breeding,* are at least capable of long abstinence there without materially suffering? And may not this be owing to none of their secretions or excretions, with the exception of the milt of the male and the roe of the female, being of an exhausting kind? And, further, owing to the empty and collapsed state of the stomach and intestines, are they not, when captured, less subject to putrefaction, and thus better adapted to become the food of man?

* That salmon and sea trout enter fresh water and leave it for the sea without breeding is, I think, pretty well established. I have found both taken in a river in September, and "fresh run," with the testes and ovaries, as it were, in a rudimentary state, leading to the inference that they breed not yearly, but probably only every second year.

XVI. ON THE EXCREMENT OF INSECTS, IN A LETTER ADDRESSED TO WILLIAM SPENCE, ESQ., F.R.S., ETC.

My dear Sir,

The excrement of insects has not, I believe, had that attention paid to it, chemically, which the subject deserves, especially taking into account the vast number of insect species, how they abound, the manner in which they are diffused, or the part, as a class, which they appear to perform in the economy of nature. This, too, being your opinion as expressed in a letter with which you have favored me, I am induced to collect and submit to you such results as I have obtained in a limited number of trials, with the request that you will communicate them to the Entomological Society, should they appear to be of sufficient interest to have the attention of that learned body.

The inquiry I commenced when I was in the West Indies, between 1846 and 1849, and have since continued from time to time as opportunities have offered.

I shall first bring under notice the results of the experiments made on the excrements of insects in their first stage of development, that of the larva or caterpillar. Even at the risk of being tedious, for the sake of accuracy, I must be more particular than I could otherwise wish, and shall have to describe individual instances. I have to express regret at the same time for my inability, without aid in those distant colonies, to give the specific names of the specimens which yielded the excrementitious matter examined.

1. The caterpillar of a butterfly, resembling *Papilio aphrodite*, Linn., common in Barbados, in December, voided excrement in abundance when actively feeding, in the form of little green pellets. A certain quantity of these, dried and acted on by proof spirit, yielded a residue on evaporation, in which hippuric acid, or a matter having similar properties, was detected; thus, to mention one, with muriatic acid, on slow evaporation, it afforded prismatic crystals, shooting from a centre, and which did not deliquesce in a moist atmosphere.

2. A large caterpillar of a moth, resembling *Sphinx atropos,* when voraciously feeding, voided much excrement in the form of cylindrical masses, of a dark olive green, some of which were partially covered with a yellowish crust. The matter of this incrustation was found to be chiefly lithate of ammonia. Under the microscope it was seen to consist of spherical granules, most of them about $\frac{1}{10000}$ of an inch in diameter; these were dissolved immediately in dilute nitric acid, and when heated (a drop of the solution being placed on a slip of thin glass), acquired the rich purple hue characteristic of lithic acid under this treatment.*

3. A large caterpillar of a *Sphinx,* after it had ceased feeding, about to assume the chrysalis state, put under a glass cover, in less than twenty-four hours parted with its enveloping integuments, and became completely incased; in doing so, it voided a good deal of brownish fluid. This, tested for lithic and hippuric acid, afforded no traces of the former, but pretty distinct ones of the latter.

4. Another large caterpillar of a *Sphinx,* in a state very similar to that of the preceding, was very restless in confinement, and shortly voided some dark fluid excrement, which, on examination, gave results like those last mentioned.

This caterpillar was killed by immersion in spirits of wine. Opened, its stomach was found to be the most conspicuous organ; it extended nearly the whole length of the abdominal cavity, and was distended with a dark mucous fluid. Under the microscope a tubular structure was seen contiguous to it, and connected with it. Conjecturing that these tubes might be renal ones, the part was taken out, and digested in water, to which a little nitric acid had been added: the solution formed was found to contain lithic acid; thus a drop of it evaporated, and heated on a support of thin glass, acquired the rich purple tint distinctive of this acid.

5. A large caterpillar of another species of *Sphinx,* taken from the leaves of the sweet potatoe on which it was feeding voraciously (in confinement still feeding), voided a great deal of almost black excrement, in cylindrical masses, rounded at their ends. On none of them was there any appearance of incrusting

* I have in this paper and in the following, when lithate of ammonia is mentioned, applied the term as used above, to designate a compound containing lithic acid in a granular state: that the compound was truly lithate of ammonia was commonly inferred, and not actually proved.

matter, as in the instance of No. 2. They yielded a brownish solution to proof spirit; and this evaporated, afforded an extract which, with nitric and muriatic acid, afforded crystals, some of them of the form of hippuric acid similarly combined.

6. A silkworm actively feeding on mulberry leaves voided excrement in the form of small black pellets. On one of them there was a brown incrustation; this, agitated with a drop of water, rendered the water slightly turbid. Under the microscope it exhibited granules; these were soluble in dilute nitric acid, and afforded, when evaporated and heated, in the color produced, a slight but distinct trace of lithic acid.

These few are all the trials I have to notice on the excrements of larvæ. With the exception of the last, in this country, they were all made in Barbados.

The next I have to mention were on the excrement of the perfect insect, immediately after quitting its puparium; these likewise were made in Barbados.

7. The pupa of the caterpillar, No. 1, I may premise, occurs attached by a fine short thread, and hangs perpendicularly from the branch of the shrub to which it is fastened. Before the escape of the imago the color of the crysalis changes from apple green to a dull blue, passing into brown, becoming when empty of a light grey. On quitting its case, the butterfly adheres to it, its head upwards, its wings hanging down. Thus it remains motionless, with the exception of occasionally expanding its wings, till the intestine has been unloaded, fitting it for flight, secure from its position of not being soiled in the slightest degree by what it voids. The discharged matter I have found to vary in different instances, and this at the same season, and when voided by individuals similarly reared, and detached from the same shrub. The excrement of one was a turbid liquid of a purplish hue, as if tinged by the purpurate of ammonia. Under the microscope, there were to be seen purple patches, some colorless rhomboidal plates, as if of lithic acid, and numerous granules, as of lithate of ammonia. By the test of nitric acid and heat, manifest proof was obtained of the presence of lithic acid, a strong purple color being produced. The excrement of another was of a brownish hue, and turbid. In it were detected traces of hippuric acid, of lithic acid, and of urea, judging from the form of the crystals obtained from an alcoholic solution, from the

effect of nitric acid, and of that of this acid and of heat. The excrement of a third, also a brownish fluid, appeared to contain little else than hippuric acid, with perhaps a trace of urea. The extract obtained from the alcoholic solution, of the dried matter emitted, on the addition of nitric acid, an odour like that of the urine of the horse, and afforded crystals on evaporation similar to those from the same urine when treated in like manner.

8. A *Sphinx*, from the larva No. 4, on quitting its puparium voided a considerable quantity of brownish turbid fluid. This excrement afforded distinct traces of lithic acid in the form of lithate of ammonia, and also of hippuric acid; the former in suspended granules, the latter in solution. The lithic acid was indicated by the effect of nitric acid and heat; the hippuric acid by the crystals obtained on evaporation after the addition of muriatic acid. On this addition being made, a smell was perceived like that from the urine of the horse. The *Sphinx* shortly after, and before it had taken any food, was killed and examined; its stomach and intestine were found empty.

9. A *Sphinx*, from larva No. 3, less than a month in undergoing its metamorphosis, voided, on leaving its puparium, a good deal of turbid, reddish-brown fluid, which, subjected to examination, was found to contain lithate of ammonia, a trace of the purpurate of ammonia and of hippuric acid. Under gentle pressure, when laid hold of, this *Sphinx* discharged pretty much semifluid matter of a brick red color, which, under the microscope, was found to be composed of spherical granules, varying in diameter from $\frac{1}{12000}$ to $\frac{1}{15000}$ of an inch. After standing a little while many of them coalesced and formed larger granules. Tested by nitric acid and a regulated heat, they had the character of lithate of ammonia. I may mention further, that in the forsaken puparium there was a good deal of whitish matter; this washed out with water and collected, under the microscope was seen to consist of granules of about $\frac{1}{10000}$ of an inch in diameter; and tested, was found likewise to be of lithate of ammonia. The quantity of excrement accumulated in this moth during its change of state, and voided on acquiring its perfect form, was truly surprising, and not less so the abundance in it of lithate of ammonia.

I shall now pass to the results obtained from insects caught, and consequently of uncertain age in relation to their last birth, or time of quitting their puparia.

10. A *Sphinx*, after about twenty-four hours' confinement, under glass, with a free supply of air, died without yielding any excrement. On opening it, the lower portion of its intestine was found distended with a brownish opaque fluid. The opaque matter, it may be inferred, was lithate of ammonia; for under the microscope it exhibited the finely granular condition of this compound, and, when acted on by dilute nitric acid and heat, was first dissolved, and then acquired the rich purple color distinctive of lithic acid.

On examining the abdomen, opening the *Sphinx* under water, some minute cells, with delicate tubes proceeding from them—part, I believe, of the renal apparatus—were observable by means of the microscope; they contained an opaque matter, probably lithate of ammonia, for, tested for lithic acid, proof was obtained of its presence.

Whilst in Barbados other large moths of the *Sphinx* tribe were caught and placed in confinement; I have notes of six. From all of them either excrement was voided during life, or was found in the intestine on examination after death, and of a nature similar to that last mentioned, composed chiefly of lithate of ammonia. No other solid substance, no crystals were observable when submitted to the microscope. In one, as in the instance last described, on opening the abdomen some delicate tubes were seen ramifying on the intestine externally. These, too, contained an opaque whitish matter, which, from the effect of nitric acid and heat, appeared to be lithic acid, but more probably was lithate of ammonia.

11. A yellow butterfly, in confinement, voided a little semi-fluid brownish excrement. Seen under the microscope, diluted with water, it exhibited many well-formed rhomboidal plates, or low prisms, as if of lithic acid, with which were intermixed minute granules, as of lithate of ammonia. Acted on by nitric acid and heat, the purple tint was produced indicative of lithic acid.

12. A black beetle, with suckers or cushions to its feet and claws, voided a considerable quantity of fawn-colored matter, in little grains about the size of mustard-seed. By the same test as the preceding, they were found to consist chiefly of lithate of

ammonia. No appearance of crystals was seen under the microscope.

13. A *Mantis* (*M. bicornis*, Linn.), in confinement, yielded a minute portion of excrement. This, mixed with a little water on a glass support, exhibited under the microscope a few minute rhomboidal crystals, and, acted on by nitric acid and heat, acquired a pink tinge, denoting the presence of a little lithic acid.

14. A fire-beetle (*Lampyris ignita*, Linn.) voided, in confinement, a little brownish excrement. It appeared under the microscope to consist chiefly of epithelium-scales and of granules; the latter, of lithate of ammonia, being dissolved instantly by dilute nitric acid, and acquiring when heated the color marking lithic acid.

15. A light yellow moth, during the night, in confinement, voided a large quantity of excrement, that is, large in proportion to the small size of the insect: it was white and semifluid. Under the microscope it was found to abound in granules of about $\frac{1}{3000}$ of an inch in diameter; they were completely dissolved by dilute nitric acid, and afforded on evaporation when heated the rich purple hue distinctive of lithic acid.

16. A brown moth, with white spots, weighing 1·6 gr., voided a good deal of excrement in confinement, some portions of which were whitish, some reddish. In the former, under the microscope, a few crystals were seen, as of lithic acid, and numerous granules, about $\frac{1}{3000}$ of an inch in diameter, as if of lithate of ammonia. The latter contained no crystals, only granules. Heated with nitric acid, lithic acid was strongly indicated in both.

17. A brown moth, of a lighter color than the preceding, of about the same size, voided a pretty copious fawn-colored excrement, which, mixed with water, under the microscope exhibited aggregated masses as if formed of granules, and some crystals, square plates, and one low four-sided prism. The granulated masses and crystals dissolved in nitric acid, and the rich color, the mark of lithic acid, was produced by exposure to a regulated heat.

18. A dark brown moth of medium size, in confinement during twenty-four hours, voided a good deal of reddish excrement, partly in minute granules, and partly in little masses, probably aggregates of the granules; such they appeared under the micro-

scope. No crystals were seen. By the nitric acid and heat test, lithic acid was found to abound. Twenty-four hours longer in confinement this moth was found dead, after having voided a good deal more of red excrement, like the preceding, excepting that in one small portion of it crystals of lithic acid (hexagonal and quadrangular plates) were observable. The nitric acid test gave the same result as the last.

19. A small white moth, that died after being confined twenty-four hours, voided a pretty considerable quantity of semifluid, almost colorless excrement, composed chiefly of granules, which, from the action of nitric acid, it may be inferred were of lithate of ammonia.

20. A small grey moth voided a minute portion of brownish excrement, consisting, as seen under the microscope, of little granular masses, and dispersed granules without crystals: from the effect of nitric acid and heat it would appear that they were composed principally of lithate of ammonia.

21. A brown moth, about half an inch in length, in confinement, before it died voided a portion of excrement so minute in quantity that it was not easy to collect; notwithstanding, it afforded satisfactory proof of the presence of lithic acid by the nitric acid test. The rich distinctive hue was visible to the naked eye, and well shewn under the microscope.

22. A delicate white moth, with a tufted tail, deposited in confinement three portions of semifluid excrement, each similar, composed chiefly of granules of about $\frac{1}{3000}$ of an inch in diameter, which, by the test of nitric acid, appeared to be of lithate of ammonia.

23. A large butterfly supplied with syrup, which it sucked up greedily, voided a drop of fluid excrement of a light brownish hue. This collected with care, after having been diluted with water to increase its bulk, was allowed to evaporate spontaneously on a glass support. Thus prepared, seen under the microscope, it exhibited in a transparent medium some minute plates and fine granules. A very little dilute nitric acid was added; on its evaporation stellaform groups of crystals appeared, reminding of nitrate of urea, and there was a urinous smell, not unlike that from human urine with nitric acid. Heated carefully the purple hue indicative of lithic acid appeared in specks fading from them as centres.

24. A black beetle common in Barbados, about half an inch in length, of impetuous flight, striking against objects, when it enters a room at night, with a force, considering its size, almost incredible, in confinement voided a large quantity of very light fawn-colored excrement, in a semifluid state. It was composed of spherical particles from $\frac{1}{6000}$ to $\frac{1}{8000}$ of an inch in diameter, as seen under the microscope, without crystals or any other form of matter. Tested, it was found to contain lithic acid, and, it may be concluded, in combination as lithate of ammonia, for it dissolved more readily in hot than in cold water, the hot solution in cooling becoming slightly turbid, and the extract obtained on evaporation, after filtration, acquiring, when heated with nitric acid, the characteristic color due to lithic acid.

25. A brown grasshopper, found amongst Guinea-grass, in confinement, voided two kinds of excrement: one, it may be inferred, fæcal, in small cylindrical masses, almost black; the other urinary, at least in part, of the same form, of a light fawn-color. These, the latter, dissolved without effervescence in dilute nitric acid, and acquired, when the solution was evaporated and subjected to a regulated heat, the color denoting lithic acid.

26. A field-cricket, in confinement, voided some excrement in small black pellets, in which no lithic acid could be detected, and which was probably altogether fæcal. Bread was given, which it ate freely. During the following twenty-four hours it voided more excrement: some, like the preceding, black; some in oval pellets, smeared with a semifluid matter, brown, and with a urinous odour. These, broken up and diluted with water, exhibited, under the microscope, spherical granules, as of lithate of ammonia, globules like those of starch, and which were colored blue by tincture of iodine, and irregular fragments as of vegetable matter. The presence of lithic acid was detected by the usual test. It was tested for urea; but the presence of this substance was not demonstrated, which may have been owing to the smallness of the quantity subjected to experiment.

27. A cock-roach just killed, taken from the apothecary's store-room, was found, on being opened, to have its stomach and intestine distended with small dark fragments, amongst which were some possessing the color and lustre of Spanish flies. A system of tubes, containing a white opaque matter, was seen on each side, and at right angles to the intestine. This matter examined

was found to contain lithic acid, and was probably in combination with ammonia.

28. A large dragon-fly, in confinement, voided pretty much reddish excrement, which, under the microscope, appeared in little aggregate masses, with (when broken down and diffused in water) some very thin colorless and transparent hexagonal plates. It dissolved in part in nitric acid, and heated acquired a rich purple color.

29. Another dragon-fly, a smaller species, voided in confinement several small cylindrical masses, in part brick-red, and in part blackish. Broken and mixed with water, under the microscope they appeared to be composed of the *debris* of insects, portions of wings, legs, etc., and of granules. Acted on by dilute nitric acid, a partial solution was immediately effected, in which lithic acid was clearly detected by the ordinary test.

30. A large *Mantis* (*M. siccifolia?* Linn.), in confinement, voided a good deal of excrement in small pellets, some blackish, some brown. The latter, under the microscope, after admixture with water, shewed numerous granules larger than those of lithate of ammonia commonly are, being about $\frac{1}{2000}$ of an inch in diameter, yet having the properties of this compound, as tested in the ordinary way.

31. A large humble-bee, its prevailing color black, in confinement voided pretty much excrement in a semifluid state. Under the microscope it appeared to consist chiefly of corpuscles, reminding one of the pollen of flowers. Acted on by dilute nitric acid and heat, the presence of a little lithic acid was demonstrated.

32. Several wasps, together with their comb, placed under a glass shade, were found dead on the fourth day of their confinement, after having voided some excrement, in which lithic acid was detected by the ordinary test. During the first two or three days it was remarked that they fed on their comb.

The cells of the comb contained larvæ nearly in a state to pass into the perfect form. One taken out and killed was examined. Its intestine, the lower portion, was found full of a chalk-like matter, which on examination proved to be lithate of ammonia.

The comb, freed from the old wasps, was placed on a clean plate and covered with a glass shade. In a few hours a young wasp made its appearance, having broken down the lateral portion of its cell. It soon voided some excrement, which was of two kinds, one almost black, of an offensive smell, a kind of meconium; the other of a light fawn color. This, the latter, had what appeared to be a mucous covering, within which was a fluid, and in that a little mass of soft consistence, about the size of a barleycorn. The fluid was brownish and transparent; with nitric acid it emitted a smell like that from impure urea or human urine similarly acted on. The included little mass was found to consist principally of lithate of ammonia.

33. Several flies, such as are common in Barbados within doors, somewhat smaller than the common English house-fly, voided in confinement a little semifluid excrement. By the ordinary means a distinct trace of lithic acid was detected in it. The liquid part afforded indications of urea, yielding a honey smell on the addition of nitric acid, and minute crystalline plates on evaporation in sunshine, which deliquesced in moist air.

34. *Musquitoes.*—These insects, averse to light, harbour in dark places;* the case of my microscope was a favorite place of resort: its brass stand became spotted with their minute droppings—so minute, indeed, as hardly to be distinguishable without a magnifying glass. The specks were nearly of the same size and appearance, except that some were darker than others. A good many of each color were collected; heated apart with nitric acid, both proved rich in lithic acid, judging from the purple color produced.

A single musquitoe was confined under a wine glass, inverted on a porcelain plate. Shortly after, on careful examination with a magnifying glass, a speck of excrement was detected of a light color and semi-globular form, as if voided in a semifluid state. Removed carefully to a slip of thin glass, and nitric acid added, it dissolved completely, and, cautiously evaporated and heated, a distinct mark of the presence of lithic acid was obtained; there was a circular patch of a bright rose hue, which was dissolved by water. A single musquitoe weighed was found equal to about

* At p. 98 a few results are given, shewing that certain flies, at a temperature of about 100°, either become torpid or die; probably the musquito is similarly affected by a high temperature. This conjecture seems to be confirmed by what is stated by Dr. Wallich in his recently published and very interesting accounts of "The North Atlantic Sea Bed," how at "North-west River" station in Labrador, musquitoes, at other times abounding, entirely disappear during the extreme heats which occasionally occur there in July, when the thermometer has stood in the shade at 110°, and at night at 90°.

·008 of a grain. The balance used was a delicate one, of Robinson's construction.

35. A large fly (four white bars on its thorax, white spots on abdomen, yellow about the eyes), in confinement, voided some excrement, partly in small cylindrical masses of a fawn color, partly spread out and semifluid, of a light brownish hue. In the former, under the microscope, two forms of crystals were seen; one like those of lithic acid; the other like those of ammoniaco-magnesian phosphate. In the latter, granules only were seen. Both acted on by nitric acid and heat, afforded proof of the presence of lithic acid.

36. A mason-bee, in confinement, voided a very minute portion of excrement, semi-transparent, semifluid, and of a brownish hue. By the usual test, it was found to contain lithic acid.

37. An elegant beetle with cushioned feet, in confinement, voided several small pellets, some of them with a brownish incrustation. These, the incrusted ones, acted on by nitric acid and heat, afforded distinct traces of lithic acid. Examined under the microscope, they appeared to be composed principally of vegetable matter. In those without incrustation, and of a darker hue, no lithic acid could be detected; they were, probably, entirely fæcal.

38. A brown speckled moth, its wings spotted white, in confinement voided a comparatively large quantity of brownish excrement of soft consistence. Under the microscope, fine granular matter—the granules about $\frac{1}{3000}$ of an inch in diameter—was observable, and many crystals; of these, some were reddish brown, some colorless; some, the majority, were rhomboidal plates of moderate thickness; others nearly cubical; one plate, a colorless one, was hexagonal. They varied in size; they were large microscopical objects as seen with an $\frac{1}{8}$th-inch power. A drop of nitric acid added, the granules were instantly dissolved, the crystals slowly. The solution evaporated and heated, acquired a rich purple hue. The granules, it may be inferred, were of lithate of ammonia; the crystals, probably, for most part, of lithic acid.

All these observations on perfect insects, with one exception, were made in Barbados; the single exception was that on the fire beetle, which was made in Trinidad. The observations which I am now about to offer were made in this country, and the greater number of them in the neighbourhood of Ambleside. In recording these latter results, I have had the advantage in most instances of being able to assign correct names to the insects, their species having been determined by Francis Walker, Esq., and by J. W. Douglas, Esq., who, at your request, were so obliging as to examine them.

39. A butterfly (*Vanessa Urticæ*), on pressure being applied to its abdomen when in a torpid state (it had been caught on the wing), a comparatively large quantity of semifluid excrement was ejected, of a rose color, as if from the presence of rosacic acid. Under the microscope it was seen to be very uniformly composed of spherical granules, of about $\frac{1}{10000}$ of an inch in diameter. It dissolved immediately in nitric acid, and when evaporated and heated, acquired the rich purple hue indicative of lithic acid, or of lithate of ammonia.

40. Another butterfly, of the same kind, voided in confinement a small quantity of reddish excrement; in which, besides granules, as of lithate of ammonia, rhomboidal plates, more or less truncated at their angles, probably of lithic acid, were seen under the microscope. The granules dissolved rapidly in dilute nitric acid; the crystals slowly. The whole when evaporated and heated acquired the color indicative of lithic acid.

41. A *Vanessa Io*, in confinement, voided some greyish excrement, which, under the microscope, and the action of nitric acid and heat, was found similar to the first of the two preceding instances.

42. A moth (*Smerinthus Populi*), in confinement, voided pretty much reddish excrement, which was found to consist principally of lithate of ammonia.

43. Another moth (*Crambus culmellus*), pressure being applied to its abdomen, a minute quantity of light-colored excrement was discharged, which, on examination, proved similar to that last mentioned.

44. A moth (*Triphæna pronuba*), in confinement, voided a good deal of fawn-colored excrement. Under the microscope, it exhibited spherical granules, as of lithate of ammonia, with which were intermixed low four-sided prisms or cubes of a pretty large size. Acted on by nitric acid and heat, the rich purple, marking lithic acid, was produced.

45. Another moth of the same kind as the last, in confinement, during one night voided excrement in three separate portions—one reddish, one brown, one of a fawn color. They were found to

consist principally of lithate of ammonia. On pressing the abdomen of this moth, a reddish brown fluid was obtained, which had the smell of human urine, and which was changed to a honey smell on the addition of a little nitric acid. On slow evaporation the solution yielded crystalline forms, rhomboidal plates, resembling those of nitrate of urea.

46. A moth (*Cerapteryx graminis*) voided no excrement in confinement. After its death, the anal portion of its abdomen was detached and digested for a few minutes in dilute nitric acid; the solution, evaporated and exposed to a graduated heat, afforded a faint but clear trace of lithic acid.

47. A fly (*Anthomyia platura*), in confinement, voided a minute portion of excrement; which, on examination, was found to consist principally of lithate of ammonia.

48. Another fly (*Calliphora vomitoria*), in confinement, voided a very little excrement, which was ascertained to be like the last.

49. A butterfly (*Pontia Napi*) yielded in confinement some excrement; found to consist chiefly of lithate of ammonia.

50. Two cow-dung flies (*Scataphaga stercoraria*), in confinement, voided a little excrement in reddish patches, which, under the microscope, exhibited the usual granular appearance of lithate of ammonia, the granules about $\frac{1}{10000}$ of an inch in diameter, and was similarly acted on by nitric acid and heat.

51. Several small flies (*Musca rudis*), in confinement two or three days, voided pretty much greyish excrement deposited in patches. Under the microscope, after being diluted with water, it exhibited, in addition to granules, some prismatic slender crystals, as of phosphate of lime, and some scales, as of epithelium. Acted on by nitric acid and heat, proof was obtained of the presence of lithic acid.

52. A honey bee (*Apis mellifica*) died in confinement without voiding any excrement. On pressing its abdomen a colorless drop of fluid was obtained, in which a trace of lithic acid was detected.

53. Three bees, taken from their hive in October, presently died at a temperature of about 40° Fahr. The anal portion of the abdomen of each was separated and digested in dilute nitric acid; on evaporating the solution at a graduated heat, a trace of lithic acid was detected.

54. A wasp—the common English wasp—which in confinement voided no excrement, yielded after death, the abdomen

being pressed, a drop of fluid, in which, under the microscope, a few granules were seen, as of lithate of ammonia, and in which a trace of lithic acid was found by the ordinary means.

55. A beetle (*Geotrupes sylvaticus*), in confinement, during one night voided many small cylindrical masses of a brown color, with a sprinkling of a matter on their surface of a lighter hue. One broken up, mixed with water, under the microscope exhibited minute granules, as of lithate of ammonia, particles of an irregular form, as of earthy matter, a few forms as of infusoria, and fibres, etc., as of vegetable matter. The pellets, digested in dilute nitric acid, separated into smaller ones, seeming to shew casts of the intestines. The solution (the greater portion of the excrement remaining undissolved), evaporated and heated, afforded proof of the presence of lithic acid.

After having been killed by a vapor of camphor, an opaque filament was seen adhering to its anal extremity. This, digested in water, and slightly agitated, under the microscope exhibited granules like those of lithate of ammonia, with a filamentous substance, probably mucus. The granules, about $\frac{1}{10000}$ of an inch in diameter, were immediately dissolved in dilute nitric acid, and on the application of heat the purple hue was produced denoting lithic acid.

56. A beetle (*Geotrupes stercorarius*), in confinement, voided some excrement of a soft consistence, and nearly white, which was found to consist principally of lithate of ammonia. Killed by immersion in water, and opened, white vessels were seen on the intestine containing an opaque matter, which, tested by nitric acid and heat, proved to be lithic acid or lithate of ammonia.

57. A beetle (*Blaps mortisaga*), in confinement, voided some excrement in the form of pellets of a dark grey color, partially covered with a crust of lighter hue. Broken up and mixed with water, under the microscope the prevailing color was light greenish, from vegetable matter in fragments, amongst which were interspersed many granules, as of lithate of ammonia. Digested for a short time in dilute nitric acid, and the solution evaporated and heated with care, proof, in the color produced, was obtained of the presence of lithic acid.

58. A female *Telephorus pilosus* voided in confinement a little excrement, which was found to consist principally of granular lithate of ammonia.

59. A male *Telephorus melanurus* (?), taken in company with the preceding, and in the act of coitus, died in confinement without voiding any excrement. The anal extremity detached, acted on by nitric acid and heat, afforded a trace of lithic acid.

The trials on the four following insects were made about twelve months after their death, kept, put by in a drawer, without any care; they were some of those on which experiments had been tried to shew the effects of different agents on insects, as described in a letter which I had the honor to address to you in April, 1851, and which was published in the Transactions of the Entomological Society for the same year.

60. Of a *Musca lanio* the anal portion was cut off, and digested in dilute nitric acid; the solution formed, carefully heated gave a distinct trace of lithic acid in the color produced.

61. A *Musca domestica,* similarly treated, afforded a like result.

62. A *Musca stabulans,* the like.

63. A fly (*Heteromyza buccata*) yielded a very slight trace of lithic acid, requiring microscopic examination to distinguish the color.

64. An *Eristalis tenax* afforded a slight but yet a distinct trace of the acid.

65. A large moth taken on the 20th of July, at Lesketh How, placed in confinement, died shortly after laying 120 eggs.* Much reddish excrement was voided, which was found to consist chiefly of lithate of ammonia.

On these, the preceding observations, having now described all I have to offer, I would beg to make a few remarks in conclusion.

* These eggs, of a light apple green, were of an oval form: their long diameter, $\frac{5}{50}$ of an inch; their short, $\frac{3\cdot5}{50}$—14 of them weighed ·64 gr. Immersed in a glass tube in water raised to the boiling point, they rapidly became shrivelled. Immersed in water at 150° Fahr. they did not coagulate; but this effect was produced at 200°. The shells were transparent, colorless, and brittle. I could not detect in them any earthy matter, neither carbonate nor phosphate of lime. Their contents were somewhat thick. Under the microscope the appearance presented was not unlike that of the yolk of the common fowl; oil globules and granular matter were well seen. The matter of the egg did not coagulate like that of the ova of certain fish when mixed with water. A certain number that were put aside were hatched in about four or five days from the time of their being laid. The caterpillars produced were greenish, and about $\frac{1}{5}\frac{3}{0}$ of an inch in length. They all died.[1] The empty shells seen under the microscope appeared to be composed of plates bounded by four lines, enclosing a central nucleus: in form they were somewhat irregular.

1 Probably owing to want of proper food. Different kinds of leaves were given them, none of which they ate of, except that of the currant, and of this very sparingly.

Considering the properties of the excrementitious matter examined, I apprehend it may be admitted that in almost every instance a part of it, and in most instances the larger portion, was urinary,—a renal secretion.

Adopting this conclusion, the urine of the insects in their earlier stage, their larva state, would appear to differ considerably from that of the same insects in their imago or perfect form. Thus, whilst in the latter it was found to consist chiefly of lithate of ammonia, in the former lithate of ammonia was sparingly detected, or not at all; what seemed to be hippuric acid being more abundant. Should further inquiry be confirmatory of this, will not an interesting analogy be established, viz., of one, the perfect insects, in their urinary secretion, to birds, which they resemble in so many other particulars; of the other, the insects in their larva state, in relation to the same secretion, to the mammalia, to which also, especially in their mode of feeding, they bear a certain resemblance—a resemblance that may be traced through several orders, according to their diet? Even in their transition state, that is, when passing from the larva to the imago, comparing the pupa of the insect with the excluded ovum of the bird, the analogy seems to be sustained—both, in the process of hatching to evolve the perfect animal, being independent, with the exception of atmospheric air, of any external material supply. The renal secretion of the fœtal bird is, I believe, always lithate of ammonia; at least, I am not aware that any other has yet been detected. In the insect we have seen how, when fully formed and quitting its puparium, the same compound has abounded.

The proportional quantity of the urinary secretion of birds, and the large quantity of lithate of ammonia which exists in it—is indeed its principal part*—is remarkable; we have proof of it, whether we examine the excrement of any single bird, or direct our attention to the immense beds of guano, of which the urine of birds, variously changed, appears to be the chief ingredient. Nor is the urine of insects in relation to quantity less remarkable. In examining it, I have often been surprised at its abund-

* Without any exception, I believe the urinous secretion of birds is principally lithate of ammonia. I have found it such in every instance that I have examined it, whatever the kind of food; in the instance of the graminivorous birds, such as the goose and the swan, the lithate incrusts the fæcal excrement commonly much in the same manner as I have found it incrusting the same excrement from beetles.

ance. In my notes, when mentioning the excrement of the moth, No. 16, which weighed little more than a grain and a half, I find the remark, that its excrement exceeded in quantity—it was similar in kind—that of a humming-bird which I was examining at the time, and which weighed 92·5 grs. The musquitoe, and its urinary secretion, may be adduced as another illustration, as well as of the delicacy of the test employed to detect the organic acid. In your letter to me, that already referred to, adverting to the importance of insects in the economy of nature, after noticing their number, how probably 250,000 species may be estimated to exist, you specially point to one function of this great class—the eating of plants and the converting them into animal matter fit for the food of birds, fishes, etc. Another part, in harmony with this, may be pointed out, viz., how by their excrement, especially the urinary portion of it, they contribute to manure and fertilize the earth for the production of plants, on which so many of them depend for subsistence. We have seen in the examples last given—the four last—that the peculiar urinary secretion may be detected in the dead insect after many months, in accordance with the character of lithate of ammonia. This quality of endurance, I need hardly remark, fits it admirably for a persistent manure.*

I am,

My dear Sir,

Yours very truly,

JOHN DAVY.

* In an earlier paper on the same subject, adverting in conclusion to the probable uses of insects, I remark that, besides manuring where they have depastured, they may be beneficial in another way, viz., by feeding on the leaves of some plants in preference to other plants (one species of caterpillar mostly choosing for its food the leaves of only one species of plant), checking thereby the extension of one kind and favoring the growth of other kinds. In the same paper, in illustration of their excrement acting as a manure, I related having seen a field of many acres of sweet potatoes in Barbados that was laid bare in one night by an invasion of caterpillars. The proprietor, who lived near the scene of the inroad, told me that he had warning of the devastation in progress, by an unusual sound which he heard when lying in his bed, a sound produced by the action of the jaws of the myriads of caterpillars. The following morning he found hardly a leaf left of an abundant crop. His enemies had disappeared, leaving the bare surface darkened by their excrement. Shortly he had the pleasure to see the plants vegetate afresh, and become more vigorous than ever, making ultimately an excellent return. The illustration of the other benefit, that of promoting the growth of various species of plants commingled, I have added, is best witnessed in flower beds and in wild nature, especially within the tropics, where, under favorable circumstances, the powers of vegetation are so great, and where, without some check, such as the one alluded to, the plants of most rapid and vigorous growth would deprive all others of feeble growth near them of nourishment, and so starve them to death.

XVII. ON THE ACRID FLUID OF THE TOAD (*BUFO VULGARIS*).

The fluid of the secretion of the cutaneous glandular structure of the toad is a subject on which many different opinions have been entertained. The popular notion handed down from a remote period has always been that it is a poison. That notion some years ago was held to be a mere vulgar prejudice, and was pointed out as altogether an error by so high an authority as Baron Cuvier.* In 1826 my attention was given to it, and the result of the inquiry was that the fluid is simply acrid, an irritant, offensive as such, but not a poison.† Subsequently it was examined by the French physiologists, MM. Gratiolet and S. Cloez: the conclusion they arrived at is that the secretion is a powerful poison, in some instances occasioning almost instant death.‡ Later still, Mr. George Rainey has engaged in the inquiry, and his results,§ it would appear, are opposed to those of MM. Gratiolet and Cloez, agreeing perfectly with those I had before obtained.

Recently, viz., in August 1860, I made further trials of the fluid. The animals experimented on were a kitten two days old, a fowl, and a slug (*Limax ater*). The toad from which the fluid was collected was of ordinary size. It had been in confinement three days.‖ The fluid was obtained by scraping with a knife the glandular surface secreting it. In each trial it was applied to a small wound in the cutis; and not merely applied by simple contact, but inserted and rubbed in. The animals were watched for twenty-four hours: the results were entirely negative. Neither the kitten, the fowl, nor the slug appeared to suffer in

* Le Règne Animal, ii., p. 94.

† Philos. Trans. for 1826.

‡ A notice of these experiments is to be found in the *Spectator* newspaper of 28th August, 1852.

§ Quarterly Journal of Microscop. Science, No. XII., for July, 1855.

‖ During that time it had voided a good deal of excrement. The fæcal part consisted chiefly of the wings of insects; the liquid, the urinary part, contained a little urea.

the least. The kitten did not refuse milk, although the part chosen for the experiment was the nose; nor had the fowl its appetite impaired—its head was the part selected for the trial.

These results, agreeing with my former, and with those obtained by Mr. Rainey, I shall not attempt to reconcile with those so opposite of the French physiologists.

I do not think that any material difference of quality of the fluid could have arisen from difference of climate or of season. My first experiments were made in the Ionian Islands, and the viscid fluid used, at the time copiously secreted, was acrid and irritant. The fluid last employed had the same bitter taste, and applied to the tongue excited the same kind of sensation as before experienced, an acrid one, but not severe, though of some hours' duration.

I have stated in my early account of the fluid that it was neither alkaline nor acid, it having had no effect in changing the color of litmus or turmeric paper. MM. Gratiolet and Cloez say that they found it alkaline. The fluid which I have recently examined was also neutral, similarly tested. They mention having kept it twelve months without its losing its activity. If kept in a damp place, might not its alkaline reaction have been owing to a little ammonia developed? A small portion of the acrid matter collected from a toad in Barbados in 1847, now in 1860 shews a decided acid reaction. It was melted when collected at a low temperature, just sufficient for its fusion, after being dried. It has been kept since in a dry place, and it seems unaltered in its properties. It is bitter, and, judging from its effect when applied to the lip, it is even more acrid than the fresh fluid of our English toad. Moreover, mixed with water, its character under the microscope is similar, minutely granular; and I have not found it different in its effects when applied to a fowl in the manner before described: the result was equally negative.

The toad of Barbados, it may be mentioned, if not identical in species, has a great resemblance to our toad. It differs chiefly in being of a lighter color, of a somewhat more slender make, and more active in its habits. Incidentally, I may remark, that though introduced from Dominica only a few years ago, it has now spread over all the island; also that it is considered poisonous by the natives. It is said that dogs that ate it became mad. Perhaps the acrid fluid made them wild, and their disturbed state was called madness. It is also asserted that now the dogs have gained experience of its effects, they leave the toads unmolested, carefully avoiding them. For the accuracy of these reports of course I cannot vouch. It would be an interesting circumstance, were it sufficiently well authenticated, that the experience of one dog, or generation of dogs, had been transmitted, and the acquired aversion become hereditary, as is known to be the case with some of the habits of brute animals.

I have stated that the microscopical character of the acrid matter of the toad of Barbados is similar to that of the toad of this country. Both consist of a fluid in which are suspended an infinity of granules of an extremely minute size, so much so as not to be discernible under a quarter of an inch object glass, and yet when seen with the one-eighth power, though excessively small, they are well defined. In one instance* I estimated their diameter to be about $\frac{1}{20000}$ of an inch.

Relative to the use of the acrid fluid, I have before expressed the opinion that, as it is so offensive to the taste and irritating, it is well adapted to secure the sluggish and otherwise defenceless animal from being devoured. I may mention in confirmation of this view, that the secretion seems to be in some measure under the will of the animal. When I have seized one of its limbs with a forceps, or pinched its skin, the acrid matter has been immediately poured out, not only in the exact spot where the pressure was made, but also from the adjoining surface, and so conspicuously that there could be no doubt about it.

If so protected, a question may arise, how is it that this animal is not more abundant? One reason may be that the very young toad, after its final metamorphosis from the tadpole state, is, I believe, in a manner defenceless. The expression of belief is used, the conclusion being founded on the examination

* The fluid mentioned above was from a male, in the month of April, iu the neighbourhood of Edinburgh, caught when on the back of a female *in coitu*. It may be worthy of remark, if a singlo observation may bo trusted, that tho larger female differs from the smaller malo in having her skin apparently destitute of the acrid secretion, at least during the breeding season. In the male, at the samo time, it was unusually abundant. The latter, at this season, perhaps, may afford sufficient protection to tho former whilst the ova are passing from the oviduct; and in her, tho growth of the ova may determine from the skin, for a time, and render the glandular structure inactive.

of a single specimen, and consequently requiring confirmation.*

Another circumstance may be the little power possessed by the toad of resisting cold. I have found a temperature two or three degrees below the freezing point fatal to it, for instance when it has been exposed unprotected by any covering to the open air for a few hours during a frosty night towards the end of autumn. Now, as the toad in the early winter, or shortly before the setting in of winter, seeks its hybernating abode and there becomes torpid, should the season be unusually severe, not having the power of quitting its selected spot, it may be frozen and die. The rapid manner in which the toad has multiplied and spread in Barbados, and the greater abundance of, and the larger size attained by, this animal in a southern climate, such as that of the Ionian Islands, seems favorable to this conclusion.

* The more careful examination of another specimen, a little more advanced in size than the first, has not been confirmatory of the above conclusion. Its size may be inferred from its weight: it was 44 grs. Like the first, its skin applied to the tongue (the only way in which the first was tested) produced no sensation; but when a cutaneous gland, one of the two just behind the head (these were larger than any others), was punctured, the peculiar cream-like secretion exuded. This was bitter and acrid; the sensation it occasioned to the lips and tongue lasted several hours. It may be worth remarking that this toad, which was vigorous when taken, was found dead three days after, and its weight reduced to 17 grs.; thoroughly desiccated, it was further diminished to 9·2 grs. The time was the first week of June. It had been kept in a small earthenware jar out of doors, but not to the exclusion of air: the weather then for most part was rainy; during a few hours, however, a strong drying wind prevailed. The seeming delicacy of life thus indicated may be one of the causes that this creature is not more abundant in our climate. As it feeds chiefly on worms and insects, it is deserving of protection in our gardens.

XVIII. ON THE URINARY SECRETION OF CERTAIN ANIMALS CONSIDERED IN CONNECTION WITH THEIR TEMPERATURE, FOOD, ETC.

1. It is well known that the urinary secretion in the instance of many birds—animals exceeding all others in temperature—consists chiefly of lithate of ammonia. So far as my inquiries have extended I have not met with a single exception; even in the case of birds, as the parrot and dove living in confinement, restricted to a diet entirely vegetable, I have found it the same. The dove in the particular instance to which I allude was fed on Guinea and Indian corn, the parrot on bread and fruit, chiefly the plantain; and other parrots of several kinds, the urine of which I examined many years ago in Ceylon, and was found to be similar, were fed chiefly on rice and plantains.

2. Insects with a variable temperature, varying it would appear with the degree of their excitement, or energy of action, or of respiration, whether living on vegetable or animal matter, or on a mixture of the two, have according to my experience a urinary secretion like that of birds, composed chiefly of lithate of ammonia and lithic acid.

3. Spiders, of low temperature, but of considerable activity, living entirely on insects, secrete a urine of a different kind—guanine, a compound nearly allied to xanthic oxide.*

4. Serpents, of low temperature, a few degrees only above that of the atmosphere in which they live, occasionally, like the spiders, making great muscular exertions, and like them capable of living a long time without food, and their food being entirely animal, have their urinary secretion composed chiefly of lithate of ammonia.

5. Lizards, with a temperature like that of serpents, and like them living entirely on animal matter, resemble them also, so far as my experience allows me to speak, in the composition and quality of their urine. The trials I have made of it have been limited to that of three or four different species.

* The excretion of all the spiders I have examined since my return from the West Indies, I found similar to that of the several kinds which I tested in Barbados.

6. The frog and toad, both of very low temperature, living entirely on animal matter, and capable of long continued fasts, have a urinary secretion different from that of any of the preceding. In all the preceding instances, this secretion would appear to pass from the urinary organs, the kidneys, in a semifluid state (granules of lithate of ammonia mixed with a little watery fluid), and to become solid before it is voided, or shortly after, in consequence of the absorption of the aqueous part in the cloaca serving as a urinary receptacle, or its loss by evaporation in the open air. But, in the instance of these batrachian animals, it is secreted by the kidneys in a liquid state and very dilute, and is commonly after passing into the cloaca received into a very thin, dilatable, and contractile bladder, which communicates by a large longitudinal opening provided with a valve* with the lower part of the intestinal canal, the cloaca, in which the ureters terminate.† The dilute liquid urine of these animals consists chiefly of water holding in solution urea and a little saline matter: in its composition it may be considered an approach to the human urinary secretion. Many years ago I found this to be the nature of the urine of a large species of frog and of a toad which I examined in Ceylon: lately I have found the urine of the toad of Barbados of like composition.

What are the bearings of these facts in their physiological relations? Do they not prove that neither the temperature of the animal, nor its activity, nor even its food (that is, the physiological conditions connected with respiration, muscular action, digestion, etc.), affect materially the nature, as to composition, of the urinary secretion? And does it not follow that the quality of this secretion, therefore, must depend chiefly on the intimate structure of the urinary organs? This is a view which for a

* I am not aware that the valvular structure alluded to above has yet been described: it is a semilunar fold of the delicate lining membrane of the cloaca, extending across, not unlike the valve of a vein in its form, and, in position in regard to the opening into the bladder, not unlike that of the epiglottis in relation to the glottis. When pressed down, as by the pressure of a probe moved towards the anal opening, it completely covers the aperture into the bladder. It is perfectly well adapted to allow a fluid descending from the ureter to pass into the bladder when the sphincter ani is closed, and to prevent fæcal matter from entering into the bladder in its descent through the cloaca, where it never seems to lodge, a sphincter above keeping it in the large intestine.

† As difference of opinion exists amongst high authorities in comparative anatomy relative to the termination of the ureters in these animals, I may mention that I have passed a fine leaden probe through the ureter of the toad into its cloaca.

long time has appeared to me most consistent with established facts; though I believe it has never been generally adopted, and recently other views have been taken, theoretically very different, which have been supported by much ingenuity of reasoning, but not, I apprehend, equally by facts.*

I have spoken of the toad and frog as having a urinary bladder. The function of the organ I allude to and its proper denomination, have for a long while been a subject of difference of opinion—some inquirers holding it to be a receptacle for urine, others the receptacle of a fluid not derived from the kidneys, but rather the product of cutaneous absorption. Even recently I find that two English physiologists maintain this latter opinion. One of them seems to ground it chiefly on the anatomical argument that the vesicle, the bladder of these animals, is the "unobliterated remains of the allantois of the embryo."† Granted that it is so, yet if it be found to have increased in size with the growth of the animal, and to be so modified as to answer the purpose of a urinary bladder, fitted to receive this fluid and to let it escape when necessary, and that the fluid which is found in it is actually of the nature of urine as regards chemical composition, must it not be considered, having the functions of a urinary bladder, to be such in reality? Moreover, apart from the chemical composition of the fluid, it appears to me difficult to conceive how, on the supposition of absorption (the ground on which the other physiologist supports his opinion‡), it can find

* By some physiologists the kidneys are viewed as strainers, separating from the blood substances existing ready formed in it. I have endeavoured to test this experimentally, but with no success. I have examined the blood of the fowl for lithic acid, but in vain; not a trace of it could be discovered: the serum after the separation of the clot was the part tried. With the same view, I have examined the blood of the snake—of two snakes (*Vipera communis*) killed in September, when both of them were gravid—and with the same negative result. Lithate of ammonia was found in the cloaca of one of them, and in the rectum of the other. The ureters, I believe, terminate at the verge of the rectum. In one of these snakes nine young ones were found; in the other eleven. One female was 20 inches in length; the other $20\frac{1}{2}$ inches. Each of the nine was about $5\frac{1}{2}$ inches in length; the smallest of the eleven was 5 inches, the largest 6 inches in length; most of them $5\frac{3}{4}$ inches. Each young one was contained in its own separate vascular sack or membrane, and to each a yolk was attached. They were all colored very like their parents, and seemed fully formed; and their blood corpuscles were very similar, elliptical, and about the same size. The poison fangs were best seen after the head had been dried, the soft parts shrinking and exposing them to view. The yolk in its proper sac attached to the abdomen abounded in oil-globules, many of them large, in the midst of a very fine granular matter with an adhesive tendency, so as to allow of being drawn out in threads.

† Mr. Wharton Jones—General Outlines of the Animal Kingdom, p. 585.

‡ Mr. Thomas Bell—Hist. of Brit. Reptiles, p. 84.

its way into the bladder, inasmuch as this organ is provided with few blood-vessels from which an exhalation can take place; nor does it appear to possess the property of imbibition or endosmosis by which fluid could be drawn in from the cavity of the abdomen; for when immersed in water, empty and collapsed *in situ,* immediately after decapitation of the animal, it does not become distended.

Before concluding, I would make one remark, which is, that though I believe the quality of the secretion of the kidneys to depend chiefly on structure, I am also of opinion that it is affected in a minor degree by circumstances of diet and of atmospheric temperature, especially in man. In a cold or cool climate, using a diet chiefly of animal food, lithic acid and lithate of ammonia are found commonly in a notable proportion in the human urine, but not so in a hot climate, not even when the diet is the same; and, in consequence, within the tropics, where least oxygen is consumed in respiration, the ailments depending on the formation of gravel and calculi are almost unknown, and the same remark applies to gout.

XIX. MISCELLANEOUS OBSERVATIONS ON SOME TROPICAL PLANTS.

The following observations, extracted from note-books kept whilst I was in the West Indies, were made in the Island of Barbados, between 1845 and 1848 :—

1. *On the Juice of the Star-Apple* (*Chrysophyllum Cainito*, Linn.).

The juice of this luscious fruit has, I have found, the property of coagulating on exposure to the air very like coagulable lymph of blood.

I have a note of one trial only. After dividing the fruit with a knife (the knife was much blackened, chiefly, it would appear, by the cortical part), the inner mucilaginous portion with the seeds was scooped out, well mixed with about an equal bulk of water, and pressed through a coarse linen cloth. What was thus obtained was semifluid (its coagulation had commenced), of a creamy appearance, and uniform consistence. Under the microscope it appeared to be composed chiefly of exceedingly minute granules, the largest not exceeding the $\frac{1}{10000}$ of an inch in diameter, which were rendered brown by tincture of iodine, with the exception of a very few that were tinged blue. After two hours a pretty firm coagulum had formed with the separation of a transparent fluid, and so great was the contraction as to be equal to about one-third of the diameter of the containing vessel, a circular glass one; and so coherent and firm was the coagulum itself, that it admitted of being lifted out without breaking or change of form. On the following day, the coagulum was found still more contracted, and in all its dimensions.* The transparent fluid surrounding the coagulum was sweet. It was not rendered turbid by nitric acid. In a few days it began to ferment, and in a few more it became acid. The contraction of the coagulum continued increasing

* Since the above was first published in 1856, Mr. Gullivor has detected fibrin spontaneously coagulable in the juices of many plants, going far to provo, with what I have observed, that fibrin, like that of blood, is one of the proximate principles of vegetables.

during several days. After about a fortnight it softened, became pultaceous, and emitted an offensive smell not unlike that of chyme as met with in the cadaver. Under the microscope, as before, it exhibited a granular texture, without the admixture of any fibres.

2. *On the supposed Influence of the Papaw* (*Carica Papaya,* Linn.) *on Meat.*

It is commonly believed, both in the East and West Indies, that this tree has the property of rendering meat of any kind that is brought near it tender. In Ceylon the opinion is, that the effect is secured merely by suspending the meat beneath the foliage of the tree during the night. In Barbados greater reliance is placed in wrapping the meat in its leaves for a few hours with a portion of the young fruit.

The trials I had made afforded negative results, tending to prove that the effect on the meat was owing to other and incidental circumstances, rather than to any special power possessed by the plant. I shall mention one in illustration.

Of two fowls killed at the same time, one was wrapped in the leaves of the papaw by my cook in the most approved manner, not neglecting the introduction of a piece of the young fruit; the other was similarly treated, substituting the leaves and fruit of the squasse (*Cucurbita Pepo,* Linn.). Both roasted were found equally tender. Other trials, using the leaves of other plants, gave like results.

The juice of the leaf, to which the supposed effect on the meat is by some attributed, appeared, as well as I could judge, to possess very little activity. It is milky, almost insipid, or only in the slightest degree acrid, and only after many hours promotes fermentation, and that in a very slight degree when added to a solution of sugar in water.

As to the incidental circumstances alluded to—these the suspending of the meat under the leaves of a succulent plant exhaling moisture, or the wrapping it in the same leaves—are they not sufficient to account for the softening effect at a temperature such as that of Ceylon or the West Indies, so favorable to rapid change, that change on which tenderness in meat depends, without reference to any occult virtue in the plant?

3. *On the Growth of the Bamboo-cane (Bambusa Arundinacea), and of the Horse-radish Tree (Moringa Pterygosperma).*

These plants afford good examples of the powers of vegetation within the tropics in their rapid growth.

I have been assured on good authority that the first, the bamboo, has been known to shoot fourteen inches in the twenty-four hours. I measured one six days successively, one that was about four feet from the stoale from which it sprung; during the first twenty-four hours it increased in height 6·75 inches; during the second, 5·25; during the third, 4·5; during the fifth, the same; during the sixth, 4·5 inches. The growth appeared to be in part terminal, and in part interstitial, the space between the joints in the new shoot having lengthened. These observations were made between the 22nd and 29th September, and on a plant in a comparatively poor and dry soil.

A horse-radish tree close to my house, that had sprung from seed, had, in nine months from the sowing of the seed, attained a height of at least twenty-four feet. Its trunk then exceeded in thickness a man's arm, and its branches, proportionally large, were at this time bent from the weight of its pods, some of which were ripe. It had received no culture or manure, and the soil on which it grew was stony, and nowise a fertile one.

I find amongst my notes another instance of the activity of tropical vegetation, in the rapid manner in which plants right themselves on change of position. Thus, in a flower-box in which weeds had taken the place of the flowers, placed on its end at six o'clock in the morning, I found in the short interval of twelve hours—viz., at six in the evening—that they, the weeds, had become bent at right angles to the soil in which they were rooted, so that the upper portion of their stems had recovered their perpendicular position.

The extraordinary productiveness of a tropical climate is by many considered an inestimable advantage, forgetful of the more than counterbalancing evil arising from the astonishing growth of exhausting and often smothering weeds. The poet may sing of the

* * * * * "redundant growth
Of vines and maize, and bower, and brake,
Which Nature, kind to sloth,
And scarce solicited by human toil,
Pours from the riches of the teeming soil;"

but the planter knows to his cost that in no part of the earth's surface is more care and industry required than within the tropics to make agriculture profitable.

4. *The Purification of Sugar by Ants.*

If the juice of the sugar-cane—the common syrup as expressed by the mill—be exposed to the air, it gradually evaporates, yielding a light-brown residue, like the ordinary muscovado sugar of the best quality. If not protected, it is presently attacked by ants, and in a short time is, as it were, converted into white crystalline sugar, the ants having refined it by removing the darker portion, probably preferring that part from its containing azotized matter. The negroes, I may remark, prefer brown sugar to white; they say its sweetening power is greater; no doubt its nourishing quality is greater, and therefore as an article of diet deserving of preference. In refining sugar, as in refining salt (coarse bay salt containing a little iodine), an error may be committed in abstracting matter designed by nature for a useful purpose.

5. *Leaf of the Pigeon-Pea Tree* (*Cajanus indicus*).

The leaf of this tree on its upper surface is covered with a fine down. When incinerated, it yields a large proportion of fixed matter, derived from the soil, consisting chiefly of the vegetable alkali, of phosphate of lime, of carbonate of lime, of magnesia, and silica. The silica is derived from the down. Under the microscope it exhibits the same form as the down, viz., that of spiculæ, in shape not unlike the poison-fang of the serpent, tubular to a certain extent, and slightly curved, from about $\frac{1}{300}$ to $\frac{1}{200}$ of an inch in length, and in width at the base about $\frac{1}{3000}$. The substance of these spiculæ—that is, what remains after incineration—I infer to be silica, from its being infusible before the blow-pipe, and insoluble in the mineral acids.

Were the soil in which the plant grows to be examined, probably after a few years, these spiculæ might be found deprived of their vegetable organic portion, the residual silicious matter preserving their forms, and they might be mistaken for the skeletons of infusoria.

The leaves examined were from a plant growing in a volcanic soil, that of St. Kitts, where it is much used as a green-dressing to the cane-fields, and is considered very fertilizing. As its roots penetrate deeply, and the roots of the cane spread near the surface, it seems well adapted to counteract the exhausting influence of the cane.

6. *Peculiarities of the Sweet Potato* (*Batatas edulis*).

In its raw state this vegetable has a slightly acrid taste; on boiling it becomes sweet. In its sound state it is almost without odour; when it is worm-eaten, it acquires a perfume very like that of the hair-powder formerly in fashion called "The Marshall's," or that of the Vanilla-bean. The water in which it has been boiled has the taste of a weak animal broth.

Besides starch, its principal ingredient, it contains a certain quantity of matter resembling gluten or fibrin. When the potato is cut, this matter exudes in a liquid milky state, viscid at first, diffusible through water, but soon becoming solid on exposure to the air. Under the microscope, when suspended in water, it appears in the form of granules of about $\frac{1}{20000}$ of an inch in diameter, which were rendered brown by tincture of iodine.

Probably this matter is concerned in the production of the changes to which the sweet potato is subject, as well as conducing to its highly nutritious quality as an article of diet.

7. *Composition of the Ground-Nut* (*Arachis hypogæa*, Linn.).

This singular nut, becoming now of so much importance in connection with the industry and civilization of the western coast of Africa, not only abounds in oil, to which it owes principally its commercial value, but also contains a considerable quantity of starch—rather an unusual alliance—and in addition a large proportion of albuminous matter. The starch particles are about $\frac{1}{1000}$ of an inch in diameter. In no other instance have I seen so much starch associated with oil.

8. *The Coco-nut* (*Cocos nucifera*).

Of all the gifts which bountiful nature has bestowed on the inhabitants of the tropics, this perhaps is the most valuable, and certainly the one most fitting for a paradisiacal state of idleness. What other fruit is there in which, as in the coco-nut, we find a cool refreshing beverage contained in a nutritious pulp of the consistence of blanc-mange, and as agreeable to the taste!

In a young nut, the lining pulp of which was thin and almost of gelatinous softness, the quantity of contained fluid exceeded rather half a pint. It was quite clear, as much so as spring water, pleasantly, slightly sweet, of specific gravity 10,183. The pulp was rendered brown by the tincture of iodine. No starch particles could be detected in it under the microscope, nor oil globules.

The water of a ripe coco-nut, much less in quantity and nearly transparent, was of the specific gravity 10,203. It did not become turbid on boiling, or by the addition of acetic or nitric acid. Sugar, it may be inferred, was its principal ingredient.

The lining pulp was found to consist of 36 per cent. solid matter and of 64 water, as determined by thorough drying. As is well known, it abounded in oil. I could detect in it no starch particles. In composition I believe it to be very like the ripe almond. The emulsion it makes is equal to that of the almond, and is an excellent substitute for milk for tea.

The coco-nut palm, I may add, thrives best by the sea-shore; it thrives even within high-water mark. Viewed in this light, may it not be considered as designed by a kind Providence to yield a drink in situations in which springs of fresh and wholesome water are often not to be found. It is only the traveller in such regions who can justly appreciate its value, and be sufficiently thankful for such a blessing. In Ceylon, the natives are in the habit of putting a portion of salt into the ground when they plant the nut, so convinced are they that salt is required for its successful growth.

9. *Cassava* (*Manihot*).

Two varieties or species of this plant are cultivated in the West Indies, the so-called bitter and sweet (*Manihot utilissima* and *Janipha*); I say so-called, because neither of them is bitter or sweet, the words probably having been applied by the negroes, who, with a limited vocabulary, are nowise exact in the use of terms. The tuberous roots—the parts used—do not differ in a very marked manner. That of the first-named has a more decided pungent, acrid taste than that of the second; and from the few comparative trials I have made, appears to contain a larger proportion of glutinous matter and of hydrocyanic acid.

When a section of the root is made, three parts are distinguishable—an epidermis, very thin and tasteless, an inner laminated and fibrous layer, which is easily separated, the principal seat of the hydrocyanic acid and gluten; and innermost, the body or main portion, abounding in starch contained in a cellular structure. On the division of the root, the glutinous matter exudes as a milky fluid, like that from the sweet potato, and with the same microscopic character. Its granules are about $\frac{1}{10000}$ of an inch in diameter, and they are colored brown by iodine. The starch particles contained in the substance of the root vary in size from $\frac{1}{5000}$ to about $\frac{1}{800}$ of an inch in diameter.

In the mode of preparing the root as an article of diet, viz., by steeping for a short time in water, grating and pressure, a portion of the glutinous matter is separated, and in the dressing, whether by roasting or baking, the volatile poison, the hydrocyanic acid, is dissipated. To the gluten which remains, probably the highly nutritious quality of the cassava is owing. We learn from Southey's "History of the Brazils," that the Dutch "soldiers preferred mandioc to wheat, thinking it a stronger food;" and I have been assured by a gentleman who travelled in the wilds of South America in company with native Indians, that he lived for many days on no other food than cassava bread, undergoing a great deal of fatigue, and found it to agree with him well and support his strength.

10. *Seed of the Cotton Plant* (*Gossypium herbaceum*).

The cotton plant, once so largely cultivated in the West Indies, offers this advantage, that it succeeds in poor soils; indeed it is said to succeed best in the poorest, and without manure. Another advantage is, that the old, infirm, and children can be employed in collecting its produce.

The uses of the plant are many. Its cuttings are good for fuel; its seeds contain a good deal of nutritive matter, and are eaten by cattle and sheep, but not, I have been told, by horses, only by ruminating animals, and it is said they are even fatal to hogs; but whether truly or not I am ignorant. The plant, as cultivated in Barbados, is of three years' duration.

One self-sown in my garden in that island yielded the first year 192 pods; in each pod or capsule there were 20 seeds, together weighing in their dry state 43·3 grs.; the lining-wool —the cotton-wool—detached weighed 23·7 grs. The shell or epidermis of the seed is black, thin, hard, and tough. The substance of the seed inclosed is of a light yellow color, of an oily taste, followed by a slightly acrid one. Under the microscope it is found to consist of oil globules, which are abundant, and of a fine granular matter. The seeds are broken (for instance when crushed in a mortar) without much difficulty, and with water on trituration yield a yellow emulsion. Thrown in a filter, the liquid which passes through is turbid and yellowish. It is not apparently altered by boiling; but on the addition of acetic acid flocculi separate, and on cooling subside. Now filtered, the fluid is clear and colorless. The precipitate, it may be inferred, is in part at least casein. The larger portion of the washed kernel, that which is retained in the filter with the oil, soon acquires an unpleasant smell; kept a fortnight and then mixed with lime, it gave off a distinct odour of ammonia. The oil is of a yellow color, not volatile, and is fluid at 80° Fahr.

The seed incinerated without the pellicle, after burning—it burns with much flame—leaves a coal that is easily reduced to ash, inconsiderable in quantity, composed chiefly of carbonate of potash, phosphate of lime, and magnesia. The same were found in the ash of the epidermis with some silica. Though growing in calcareous marl, no carbonate of lime or free lime could be detected in either.

XX. ON THE BLOOD OR RED FLUID OF THE COMMON EARTHWORM (*LUMBRICUS TERRESTRIS*).*

The following experiments on the blood or red fluid of the common earthworm were undertaken when this anellid was in its greatest vigor, having been begun in the latter end of June and finished early in July. Professor Rolleston, of Oxford, being with me at the time, was so kind as to assist me, especially in laying bare the cardiac organs and in procuring their contents—indeed it was at his request that the inquiry was entered upon.

The method of obtaining the fluid was the following:—The earthworm was killed by immersion for the space of four or five minutes in hot water.† The great blood-vessel being then exposed by careful dissection, its coats were penetrated by the sharp end of a delicate pipette of the kind formerly and still perhaps employed in collecting vaccine lymph, and which differs from the ordinary pipette chiefly in having at one end of it a very thin bulb.

* Read at the meeting of the British Association for the Advancement of Science, held in 1861.

† The following trials were made to ascertain the degree of temperature fatal to the earthworm under water :—

1. A worm immersed in water of 110° eighteen minutes, the temperature falling to 101°, was found dead: it shewed no marks of vitality when taken out and plunged into water of the temperature of the air.

2. A worm left in water of 106° twelve minutes, the water falling to 100°, shewed when taken out and transferred to cold water, feeble signs of vitality, contracting feebly when punctured: it soon died.

Three worms were put into water of 104°:—

3. One, taken out after three minutes, the water having fallen to 103°, was alive.

4. Another, taken out after ten minutes, the water fallen to 101°, was still alive.

5. The third, taken out after forty-six minutes, the water fallen to 89°, shewed feeble signs of vitality. These worms, put into damp moss, were all found dead the following morning.

Next, three worms were put into water of 101°:—

6. One, taken out after five minutes, was alive.

7. Another, taken out after ten minutes, was alive.

8. The third, taken out after fifteen minutes, the water fallen to 95°, was alive. All these worms, with the exception of No. 6, were found dead the following morning.

In each instance the warm water rendered the worm torpid, and the higher the temperature the more rapid was the effect.

A temperature of water of 95° did not prove fatal; some worms subject to this degree, falling to 84°, were not rendered torpid, and bore it without loss of life.

Before puncturing the vessel the bulb was warmed by contact of the warm hand; cooling rapidly from its thinness, a partial vacuum was thus formed, and the fluid was drawn in; and by the reapplication of the hand to the bulb it was expelled. The quantity thus obtained at once amounted to one or two small drops varying with the size of the anellid and its condition. And that it was tolerably pure may, I think, be inferred from the precautions used: the dissection was conducted under water, and before the cardiac organ was opened, which was done out of water, the fluid which bathed the vessel was carefully washed away, and the surface of the vessel was wiped with bibulous paper.*

Before describing the chemical properties of the fluid, its other qualities may be briefly noticed. It is limpid, transparent, and of a bright red color. Such was its appearance when seen in a fine glass tube by transmitted light. Viewed under the microscope with a one-eighth-inch power, granules or minute corpuscles were seen scattered through it. These varied a little in size; their average size was about $\frac{1}{14000}$ of an inch, or about one-fourth that of the blood-corpuscle of man. Each corpuscle had a luminous centre and well-defined outline, faintly colored red or yellowish red. The coloring matter, it was pretty clear, was contained within the cell. After some doubt and many trials, this was the conclusion we arrived at—that the fluid owed its color either altogether or in great part to these corpuscles.

The experiments made to determine its chemical qualities were limited to the following:—By test-papers it was found to have a slight alkaline reaction. It was coagulated by alcohol, by nitric acid, and by heat. The degree of temperature at which its coagulation took place was about the same as that which coagulates serum.† The coagulum it yielded was slightly colored, shewing that the serous coagulable portion greatly exceeded

* Mr. Wharton Jones, who has described the forms of the blood corpuscles of the earthworm in a valuable paper published in the Transactions of the Royal Society for 1856, Part I., appears to have used in his trials a mixture of the cardiac and perivisceral fluid, which may account for my results not agreeing entirely with his.

† An approximate temperature at which the coagulation took place was ascertained by immersing worms in hot water. In water of 164°, the fluid was coagulated firmly; in water of 160° it was coagulated, but less firmly; in water of 154°, it was not coagulated. The coagulum formed at 164° when covered with a drop of water, hardly sensibly colored the water: that formed at 154° sensibly colored it. The water under the microscope was seen to owe its color to the granules or corpuscles already described.

that of the colored granules; and it was of such a consistence as to indicate that the proportion of coagulable matter was not inconsiderable. By distilled vinegar, aided by heat, it was dissolved with the exception of the included corpuscles. The solution was rendered turbid by ferrocyanide of potassium. Nitric acid dissolved it, with the production of nitrous gas. The solution was yellow, and was not rendered turbid by dilution with water. It was dissolved also by muriatic acid and sulphuric acid. The muriatic solution was of a reddish hue; it was rendered turbid by the addition of a small quantity of water; the precipitated matter was redissolved on being largely diluted. The sulphuric solution was of a brown color; dilution did not render it turbid. When largely diluted a few white flakes were seen, denoting that the whole was not dissolved, and that the coloring matter was most readily acted upon by the acid. Both the liquor potassæ and aqua ammoniæ dissolved the coagulum. The solutions were of a brownish color; no precipitation followed their dilution. The fixed alkaline solution was rendered turbid by acetic acid added in sufficient quantity to neutralize the mixture. Added in excess, a resolution was effected, and the dissolved matter was again precipitated, using ferrocyanide of potassium. The colored coagulum, like the crassamentum of the blood of the vertebrate, was deprived of its color by the action of chlorine. When dried and heated before the blow-pipe on a platina support, it burnt with a bright flame, leaving a little coal, which was readily reduced to ash. The ash, proportionally minute, had a just perceptible ochre-hue, and was soluble in muriatic acid. Tested by ferrocyanide of potassium, it afforded a trace of peroxide of iron: the blue tint acquired was unmistakeable. Tested by ammonia, it yielded a trace of phosphate of lime; there was a just perceptible precipitate of a faint brownish hue, such as might be imparted by the presence of a minute quantity of peroxide of iron. The ash the subject of this last experiment was obtained from the blood of nine large earthworms; thoroughly dried it weighed only the third of a grain. In some of the foregoing experiments the red coagulum extracted from the ventral vessel after coagulation by immersion in water of about 170° was used, being in this state more easily obtained than when liquid. The trials with the acids were made chiefly with this coagulum, and also those, the last mentioned, on the ash.

I have stated that the quantity of fluid procurable from the ventral cardiac vessel varied not only according to the size, but also the condition of the worm. The latter inference was arrived at from finding extremely little in earthworms that had been kept many days in wet moss, and were to a certain extent deprived of food.

Of the two hypotheses which have been formed regarding the nature of this colored fluid, I am inclined to adopt the one in which it is considered to be blood, and, I would add, with the double function of aerating, that is, of receiving and distributing air, and of nourishing. The circumstance that it contains red granules, or corpuscles, and that the fluid owes its color mainly to them, and that iron enters into their composition, may be adduced in favor of the first inference, its relation to air, merely on chemical grounds; and the fact that it contains albumen, and in no small proportion, may be mentioned in support of its having a nourishing quality; and further, in favor of this inference, reference may be made to the fact of the wasting of the fluid in consequence of fasting.* Another circumstance may be adverted to, tending, I think, to confirm the idea that the red fluid performs the part of true blood, that, namely, of the vessels containing it being of largest size close to the more important organs, such as the digestive and sexual, and of there ramifying most abundantly. And seeing how largely supplied the stomach is with vessels carrying this red fluid, it is difficult to avoid the conclusion that it is not concerned in administering to the secretion of the gastric juice, a fluid which in this anellid, as in animals of higher organization, has an acid reaction. Whether the nourishing power of the red fluid is equal to that of the perivisceral fluid is open to question. Probably it is less nutritive, and is subordinate or auxiliary. The circumstance that the latter is more widely diffused is favorable to the inference coupled with what is known of its composition, it being coagulable by heat, and containing nucleated cells.

Taking into account the small quantity of the red fluid, and the comparatively small amount of the corpuscles or granules

* Since the above was written, I find that Mr. Dyster has come to a similar conclusion relative to the red fluid of *Phoronis hippocrepia*, the blood corpuscles of which he describes as varying in size from $\frac{1}{3200}$ to $\frac{1}{1700}$ of an inch, and in form from circular to elliptical.—Trans. Lin. Soc. for 1858, p. 251.

suspended in it, and also the constantly moistened surface of the worm favoring cooling by evaporation, it could hardly be expected that its temperature would be appreciably higher than than that of the medium in which it is found. The results of the thermometrical trials which I have made have been in accordance. In a very limited portion of air saturated with moisture I have not found the temperature of several earthworms collected in a group higher than that of water similarly situated. When exposed to the air, the temperature of the same worms was a degree or more lower, an effect no doubt owing to evaporation. Moreover, in accordance, the quantity of oxygen that is consumed by this anellid would appear to be very small. One worm, the volume of which was equal to ·20 cubic inch, during nine hours that it was confined in a tube containing 1·07 cubic inch of atmospheric air, consumed only ·10 cubic inch of oxygen; at the end of the time mentioned, the following was found to be the composition of the air, as tested by milk of lime and phosphorus: carbonic acid, ·10; oxygen, ·10; azote, ·87. In another experiment, in which an earthworm in bulk equal to ·19 cubic inch, was confined in a volume of air equal to 1·02 cubic inch for twenty-four hours, all the oxygen was found to be consumed with the exception of ·04 cubic inch. In this instance, a partial death of the *lumbricus* had taken place. When taken out, the whole of the worm below the region of the stomach was pale and flaccid, whilst the cephalic extremity was unusually red and turgid, and was still contractile. It is worthy, moreover, of remark, that a rupture had taken place just below the stomach; and on examination the intestine there was found perforated, and the parieties adjoining had a ragged appearance as if from the solvent action of the gastric and intestinal fluid.

XXI. ADDITIONAL OBSERVATIONS ON THE COLORED FLUID OR BLOOD OF THE COMMON EARTHWORM.*

In a paper read at the last meeting of the British Association for the Advancement of Science, after describing some of the properties of the red fluid or blood of the earthworm, I offered a suggestion as to its use—viz., that it is concerned both in the nutrition of the anellid and its aëration or respiration. It is on the latter function that I shall now offer a few observations, which I have since then made, and which appear to be of a confirmatory kind. They are the results of the following experiments:—

1. A vigorous earthworm was placed under a receiver, in a small cup resting on a vessel holding water, standing on the plate of an air-pump, and the air was exhausted. The worm after twelve hours was not rendered torpid; it had left the cup, and was resting on the plate of the pump. It was replaced. After other twelve hours it was still alive, contracting when roughly touched. On examination some hours later, it was found to be dead. No part of it now, not even the caudal, contracted when punctured. A rupture of the intestine had taken place, and of the integuments corresponding, and a considerable quantity of reddish fluid was effused, which proved to be serous, colored by the coloring matter of the blood; it became turbid when heated, yielding some coagulum. The rupture was midway between the stomach and anus.

2. Another vigorous earthworm was immersed in water which had been deprived of air by the air-pump. Thus situated, it was placed under the receiver, and the air exhausted: after six hours it was alive. Two hours later, a red sediment, the color of blood, and which proved to be the blood of the anellid, was seen at the bottom of the water. The following morning, when the worm was taken out, a rupture was detected in it of the intestine and integument near the stomach, with extravasation of blood. The worm seemed dead; the upper portion shewed no

* Read at the meeting of the British Association in 1862.

signs of life; the lower portion towards the anal extremity, after a few minutes, contracted feebly. This portion, cut off from the superior dead moiety, lived several days, the wound healing; and seven days after it was found to have divided into three, each of which gave signs of vitality, contracting when punctured. The experiment just described was twice repeated, and with the like results: in each instance a rupture occurred, with loss of blood and the death of the cephalic portion.

I may remark that in these experiments the exhaustion of the air was as complete as could be effected by a good air-pump in perfect order, so as to exclude air, as ascertained by the gauge, during the many hours occupied in the trial.*

3. A vigorous worm was immersed in water saturated with carbonic acid gas. It continued active from 1·30 p.m. to 4 p.m. The following day it appeared to be torpid; taken out, it was found to be still alive; it contracted when roughly pressed. Three days after, the inferior moiety was alive; the superior, including the stomach and other important organs, was dead and putrid.

In another trial, in which carbonic acid was gradually admitted into water holding an earthworm, the worm at first was very restless, endeavoring to escape. As the water became saturated with the gas, the activity of the anellid diminished, becoming motionless, as it were torpid. Taken out on the following morning it was found to be dead, and the intestine close to the stomach ruptured, as was also the integument corresponding.

4. A vigorous earthworm was put into a bottle full of carbonic acid gas under water, and there confined by a glass stopper. In about five minutes it appeared to be torpid; it was motionless, and its color had darkened; thus it continued. When taken out on the following morning it was found to be altogether dead; opened, the intestine close to the stomach was found ruptured and partially dissolved, as if from the action of the gastric juice. The experiment was repeated, and with a like result.

5. A vigorous earthworm was put into hydrogen gas, made by means of zinc and dilute sulphuric acid. It immediately became very restless, its color not darkening as when immersed in carbonic acid gas. After nine hours it was seen to move.

* It was the same pump as that described in the second volume of my Researches Anat. and Physiol., employed in the experiments on the air in the blood.

After twenty-five hours and a-half it was taken out: it was still alive, contracting when punctured. There was a slight, a very slight, increase of volume of the air, owing, as was ascertained, to a minute quantity of carbonic acid gas disengaged.

What are the inferences to be drawn from these results? Are they not favorable to the idea that the blood of the anellid is in reality a receptacle of oxygen, in which it is stored up to supply the small wants of the worm? The experiments with the air-pump, equally with those with carbonic acid gas, and especially those with hydrogen, seem to harmonise with this conclusion. That the worm bore better exhaustion of air out of water than in water, may have been owing to its integuments being more pervious under the latter circumstance than the former. That it was not killed so soon by water impregnated with carbonic acid gas as by the gas acting directly on it, might be attributed to the retentive power exercised by the water on the gas. The length of time that the worm lived in hydrogen, would seem to indicate that this gas exercised no positively injurious effect, permitting the continuance of life until all the oxygen contained in the blood was expended—converted at least in part into carbonic acid. It is remarkable in the earthworm, as shewn in these experiments, how the part containing the organs by which it approaches most to the higher orders of animals, dies first; whilst the retention of vitality by that portion having the opposite character, seems as it were to bring it near to the vegetable; and the more so in the circumstance of this portion being capable of breaking up, of becoming disjointed, each fragment retaining life.*

Even in the manner in which it is affected by the air-pump, some analogy may be traced between it and vegetable organisms: thus, there are some seeds which do not vegetate *in vacuo*, such as the smaller, cress and mustard, from which the air they contain, owing to their smallness perhaps, is easily exhausted; whilst there are others which do partially vegetate, such as the larger, for instance the pea, from which it is difficult to exhaust the whole of the air stored up in it. Its growth *in vacuo* is limited, I find, to a first effort, and stops before the plumula is developed.

* The medicinal leech exhibits the same peculiarity. When divided, I have found the inferior portion to retain its vitality; in one instance the superior moiety lived six days, the inferior fifteen days.

That the earthworm should have such a reservoir of oxygen seems as admirably adapted to its habits and habitat, as that the seal and mole and some other animals should have a similar provision, they as much needing it, in the vast size and tortuous direction of their intercostal arteries.

The earthworm, however considered, seems to be in the most perfect harmony with its position in the scale of organization, and the function it seems to perform in the economy of Nature. Living chiefly under ground, and feeding chiefly on decayed vegetable matter, which it swallows mixed with soil, its excreta, comminuted soil enriched with animal juices, act as a manure. The carbonic acid it evolves in its feeble respiration, promotes the growth of plants. The necessity it seems to be under to change its place underground to prevent being poisoned by its own exhalation—the gas just mentioned—helps in a small way to drain the ground and improve its porosity. And happily for this lowly creature, and for the benefit of the soil, its nervous system is of the simplest kind, its vitality of a very enduring kind, so that we may be sure that it suffers little from the wounds inflicted on it by the husbandman and gardener in their farming and horticultural operations ; and we know that though divided into many parts death does not necessarily result, and that each section may retain life and exist independently for a limited time.

Physiologically viewed, it may be considered, as before said, as a link between vegetables and animals. Without striped muscles, with merely ganglia, its consciousness of pain may even be questioned, and its motions may be referred chiefly, if not entirely, to reflex nervous action, and the *vis insita* of its muscles.

XXII. SOME OBSERVATIONS ON THE FISHES OF THE LAKE DISTRICT.*

From the very nature of the district, implied in its name, it might be expected to be a favorite habitat of the finny race; and that, as the physical conditions of its waters—such as depth, extent of surface, stillness and motion, elevation above the level of the sea, etc.—are so various, so likewise would be the varieties or species of fish inhabiting them—an inference tolerably according with the facts.

I may commence with giving a list of the fish—of such as have come to my knowledge, belonging to the district, occurring in lake, river, and estuary—not doubting that, on further and more careful inquiry, other species may be discovered, and the catalogue somewhat extended. Belonging to the Salmonidæ are the following:—The salmon, sea trout, common trout, charr, vendace, skelley, and smelt. The remaining species, as to classification, are miscellaneous; they are the pike, perch, tench, roach, chub, barbel, loach, eel, miller's thumb, thornback, minnow.

It would be superfluous and out of place here to enter into minute particulars respecting any of the fish named. I shall confine myself to some general remarks, chiefly regarding points which are not altogether settled, and are open to inquiry, or to some few others which I hope are settled, but which need to be dwelt on and enforced to make them generally known and popular.

1. *Of the Habitats of the Species.*

Of the Salmonidæ, the common trout is the most widely diffused. There are few streams and no lakes in which this fish is not to be found. Its being so widely spread indicates a hardihood of nature, an absence of that delicacy which seems to mark the next I shall mention—the charr. Of its hardihood, indeed, we have the strongest proof, if we extend our view from a dis-

* Read at the meeting of the British Association in 1858.

trict to a country, or continent, and consider its wide range, reaching from the extreme north of Europe almost to the extreme south, and in Asia found even so far south as Palestine—if I may rely on the assurance of an intelligent traveller, well acquainted with that country, who informs me that it occurs in the Jordan, and that he has partaken of it caught in that river, and of excellent quality and goodly size. And this may easily be credited, as we know for certain that the trout flourishes in some of the streams of the south of France, the Sorgue, for instance, even at its source, at Vaucluse,* and in some of the rivers of Portugal, Sardinia, and Corsica—and even of the Atlas.

The charr, I have said, is a more delicate fish. It is confined to certain of the many lakes of the district—viz., Windermere, Coniston-water, Hawswater, Buttermere, Crummock-water, Wast-water, and Ennerdale Lake. The sensitiveness of this fish to noxious influences is indicated by the circumstance, that since the mines have been opened in the vicinity of Ulswater, the charr, before abundant in that fine lake, have gradually diminished in number, and have now entirely disappeared; and the same effect of mine-water, though in a less degree, has been witnessed in Coniston-water, where, before the copper mines in that neighbourhood had been opened, charr were far more plentiful than they have been since. The delicacy, too, of this fish is shewn by the failure of attempts to introduce it into other lakes, such as Derwent-water, in which the trial has been repeatedly made without success. I am disposed to infer, that in all the instances of these abortive attempts, the failure has been chiefly owing to the water not being sufficiently pure, and not to the circumstance of insufficient depth of water. That this fish is intolerant of heat seems indeed certain, and that in summer it retreats to the deepest parts of the lakes it inhabits; yet mere want of great depth of water seems hardly to be an adequate cause, as it is met with in other countries, in Ireland, for example, where it occurs in comparatively shallow water, and also in the Highlands of Scotland, in certain lakes where I believe the water is not deep. Of the latter I speak from what I have heard related; of the former from my own experience. I may

* On the 10th of April, 1830, I found the temperature of the water at Vaucluse, just where the stream gushes from the rock, 54°.

add, in confirmation, the difficulty of keeping it in stews or wells in which trout can be kept in health for a long while: so confined, I am assured, the charr rapidly gets out of condition, and often becomes blind and infested with a parasitical growth.

Even less diffused than the charr are the vendace and its congener the skelley, both of the coregonus genus, and allied to the grayling—this last a fish unknown in the district, though many of its rivers are well adapted for it. Both the vendace and the skelley are restricted to a very few localities. The skelley has been long known as occurring, and that abundantly, in Ulswater, Hawswater, Brother's-water, and Red Tarn. The vendace, which was supposed to have been peculiar to the lochs in the neighbourhood of Lochmaben in Dumfriesshire, I have recently ascertained to be an inhabitant of Derwent-water and Bassenthwaite lake, the two connected by a short reach of the river Derwent.*

Of the rivers frequented by the more interesting and valuable fish, the migratory species of the Salmonidæ, the principal are the Derwent, Duddon, Leven, and Irt; but besides these there are many smaller streams, which are the resort either of the salmon or sea-trout for spawning purposes, as is proved by the presence of their fry, though the parent fish are seldom seen by the honest angler, these commonly falling a prey to the poacher.

Of the other fish, as regards their habitats, having paid less attention to them, I have little information of any value to offer. The pike, perch, and eel are of very common occurrence. The eel, indeed, is found in almost every lake and river in the district, and even in the mountain tarns. The minnow and thornback are also widely diffused in both lake and river. The tench and roach are less frequently met with, and are confined chiefly to ponds into which they have been introduced, brought from other counties. The barbel and loach, too, are of rare occurrence as river fish. And the same remark applies to the smelt, an estuary migratory fish; hitherto I have heard of it as taken only in the estuary of the Kent.

* I satisfied myself of tho fact in 1856, by comparing a specimen of the vendace of the Lochmaben lakes with one from Derwent-water. According to Sir Wm. Jardino, the gwyniad of the Bala lake in Wales is also a vendace.—(See Ed. New Phil. Jour. for April, 1857, p. 347 and 349). The fario (*C. fera*) of the lake of Geneva, if not identical with, is nearly allied to the vendace. The character of each is the same; tho larger average size of the latter (about half a pound) may be owing to better feed. I havo found in the stomach of one I examined at Geneva fragments of a minute crustacean and a small bivalve. The coats of its stomach are thick.

2. *Of the Causes affecting the Dispersion or Limitation of the Species.*

The preceding brief notice of the habitats of the fishes of the district naturally leads to the inquiry, how is it that some are so widely distributed—such as the pike, the trout, the perch; and others are so limited—to be found in so few waters—such as the vendace, the skelley, and charr? The causes operating must be of two kinds, either natural or artificial; the latter of course implying the interference of man. Which of these have been most concerned it may not be easy to determine in the majority of cases. I shall request attention chiefly to those fish of very limited distribution, and which, considering the habits of the fish, and the places in which they are found, it is difficult to suppose could owe their introduction to other than natural means. The vendace and skelley are the best examples. As these fish are rarely taken by the angle, or by any method except the net, and are comparatively of little value, we can hardly infer that where they occur they were originally placed by man, especially in the instance of the skelley, in a tarn such as the Red Tarn, situated under the brow of Helvellyn, many hundred feet above Ulswater, and so difficult of access. Much the same reflection presents itself as regards the vendace in Derwent-water and Bassenthwaite lake, taking into account the distance of these lakes from Lochmaben. If, as more easy of credence, we have recourse to natural causes, I fear we can indulge only in conjectures as to their nature, founded on probabilities. The first conjecture I would venture to offer is, that their ova, after impregnation, may have been conveyed by birds (water-fowl), either adhering to the feet, or retained in their bills. The circumstances which seem to favor this view are, that the impregnated ova (at least of the salmon) preserve their vitality for many days in a moist air, and are capable of resisting a degree of cold sufficient to freeze water, so as to be included in ice. And the fact of the spawning season of the skelley and vendace being in the winter season, as in the instance of the other Salmonidæ, is favorable to this view. The only other conjecture I can presume to offer is, that the ova, having the power of resisting cold, might possibly have found their way originally to the places where the fish now exist by means of glaciers. Of the two, the first, which we owe to a distinguished naturalist, Mr. Charles Darwin, seems the most probable, especially in the instance of the skelley of Red Tarn. As regards the vendace, another conjecture may be proposed, and which would become probable could it be proved that this fish is capable of enduring the sea. I mention it, arising out of an observation of Sir John Richardson, that he has known a vendace to have been taken in the brackish water of the Solway. If, in the way of objection to either of these conjectures, it be asked—Granted that any one of them has had effect, how is it that the fish in question are not more widely spread? May it not be answered, that the presumed causes must be held to be only occasional ones, the favoring circumstances rarely occurring together; moreover, that a limit may arise, from the quality of water, comprising the feed it yields, not being suitable to the species. The charr affords an instance; so do the salmon and sea-trout; the former, it would appear, as regards impurity of water; the latter principally as regards its temperature, a comparatively low temperature being essential to the health and well-being of these fish. Were it not so, it can hardly be doubted that they would be found in the Mediterranean and the Indian Ocean. I have known a stray salmon taken in the sea, off the coast of Malta. Had that sea suited its habits, and could it have met another stray wanderer from the Atlantic of a different sex, the breed might have been propagated; that is, if they could enter the same river in company, and the temperature of its water, and other circumstances belonging to it, were such as would allow of the hatching of the ova.* The subject, in its generality, is well adapted for experiment, and on that account mostly I have ventured thus to bring it under your notice, keeping clear from all speculations relative to the habitats *ab origine* of species—a matter even more obscure than the preceding, and truly transcendental.

3. *Of the Growth of Fish.*

This, I am disposed to think, has hardly received the attention it deserves, whether we consider the effect, the growth itself, or the peculiarities of organization with which it is connected. It

* In the Italian lakes Lugano, Como, Lago Maggiore, the trout is most commonly found in its coolest parts, those namely which receive the cold streams descending from the adjoining mountains, and which are the resort of the fish in the breeding season.

may be difficult to assign what are the limits of each species as to size, and still more as to progress of growth. This is certain, that some species, such as the minnow and thornback, always remain of comparatively diminutive size; whilst others, the majority of the other species, under favoring circumstances, are capable of attaining a great size. Appropriate food, and abundance of it, seem to be most concerned. The Salmonidæ afford striking examples. It is now well ascertained, that the young salmon, which, as a smolt, enters the sea only two or three ounces in weight, in five or six weeks may return to its native river augmented in weight to four or five pounds. It is equally well known, that the same fish may remain in fresh water several months without gaining weight—on the contrary, losing in its fasting or low feeding in lake or river that fat which it acquired in the sea from high feeding, and losing also the rich color of muscle characteristic of the fresh run fish.* The common trout is scarcely a less striking instance. How small is the brook trout, especially in hungry streams, such as mountain torrents in a country of primary rock formation! Rarely is it taken exceeding a very few ounces. Yet, change its position; put it into water where it can satisfy its appetite—where there is abundance of rich food—and in a short time it will start into active growth, and soon become a large fish, and this chiefly from increase of muscle and fat. Very recently a remarkable instance of such rapid growth has come to my knowledge. About two years ago, I am informed, some small brook trout were put into the ponds which have recently been formed at Rivington as reservoirs for securing a supply of water for Liverpool. So soon as July last (1857)—that is, in about two years—these fish had attained a goodly size, and had become of excellent condition, like the finest lake trout; and one was taken by an angler that weighed seven pounds. Other instances of the like kind might be mentioned of rapid growth of brook trout

* Even the quality and amount of food appears to determine the conversion of the parr into the smolt; *i.e.*, the acquiring a fresh crop of silvery scales, and its migration seaward. When well fed, about twelve months, it would seem, suffice; when scantily, nearly, or more than, double that time. I have knowledge of parrs artificially bred in the Isle of Lewis which after twelve months were still parrs; the experience obtained in the great experiment on artificial breeding made at Stormontfield on the Tay, has given the same result—a portion only of the parrs becoming smolts at the end of the first year, and quitting the pond in which they were bred for the sea.

put into made pieces of water, well authenticated, though not quite so remarkable as the foregoing.

By some it is supposed that, as regards the size of fish, there is some relation between it and the volume of water—a relation of dependency. It is true that large fish of any kind are seldom taken in very small streams or ponds, and that the largest are to be found rarely excepting in water, either rivers or lakes, of ample dimensions: but is not this owing to the larger pieces of water affording more ample food, and also a better chance of escaping capture? For the mere purpose of respiration—in the performance of which function, water, owing to the air it contains, is to the gills of the fish what the atmosphere is to the lungs of the higher classes of animals—no great quantity is required. The water of a small swiftly-flowing brook might answer, almost irrespective of size; and, accordingly, sometimes a trout of two or three pounds is caught in a neglected pool of such a stream; and I have it on good authority that a charr of the extraordinary weight of four pounds was once captured in a tarn (Lillytarn, the water of which is remarkable for its purity) a few miles from Kendal, into which some charr had been placed as an experiment; the exact time it had been there I could not learn. No charr, I believe, of the same size was ever known to have been taken in Windermere, an ocean, as it were, in comparison, but severely fished and poached, whilst the tarn was carefully preserved.

Passing from the main cause, that which is external, the abundance and good quality of food, I would beg to advert briefly to the other, the internal, that connected with organization and function. These, I am led to infer, consist chiefly in a stomach possessed of great power of active digestion, with associated parts conducive to a rapid assimilation, unchecked or little abstracted from either by the kidneys or gills;—these two organs acting in harmony, the one excreting chiefly azote,—*i.e.*, matter abounding in azote,—the other exclusively carbon. We know for certain, and it has been long known, that in accordance with the low temperature of most fishes, the quantity of oxygen they consume respiring by their gills is very small; and such experiments as I have made have brought me to a similar conclusion as regards their urinary secretion; and hence, whilst feeding largely, losing little by excretion, their increase in size, it is easy to understand, must of necessity be rapid, no internal cause, limiting size, preventing.

4. *Of Varieties of Species.*

In the instance of the Salmonidæ, we have striking and instructive examples of varieties of the several species depending on external causes, and probably chiefly on the quantity and quality of food, the quality of water, and the nature of the bottom in relation to light. How different is the well-fed lake trout, or the well-fed beautiful river trout, and the trout of the mountain brook or of the peaty stream! even in the same lake or river, how great is sometimes the difference of color and quality of the fish!—of so rich and bright a hue, and so brilliantly spotted, where there is full exposure to light, and much reflected light from a clear gravelly or sandy bottom, and the reverse where there is much shade and a dark bottom absorbing the rays of light.* In the instance of the charr we have similar examples; so various are they, indeed, that in no two lakes do they perfectly agree, either in their average size, form, and coloring, or even in their habits. Compare the charr of Windermere and Hawswater; were it not for their scales and other distinctive features, there would be little hesitation in saying they are different species, the charr of Hawswater is so much smaller, and thinner, and differently spotted; the one taking the artificial fly of the angler freely, the other—that of Windermere—rarely so tempted, and seldom caught except by trolling with the minnow. Much the same remarks are applicable to the salmon and sea trout, those of each river having commonly some peculiarities by which they may be known.

Age and sex also, the latter especially, at the spawning season, have a great modifying influence. Throughout the year, independent of season, the male of the salmon is distinguishable by the form of its head, narrower, and more pointed anteriorly than that of the female. In all the Salmonidæ, as is well known, the

* Even in a few hours a change of color may be witnessed in the trout, by transferring it from a vessel of a light color to one of a dark, and *vice versâ*, owing probably to the color-granules being moveable, subject to concentration and diffusion, as from the researches of Mr. Lister they appear to be in the frog, an animal liable to similar changes of color according to the degree of light to which it is exposed. It may be a question whether the human complexion may not be affected somewhat in the same manner.

under jaw of the male fish becomes more or less elongated and hooked towards the breeding season; and the color of its belly acquires a distinctive bright red hue, by which it can be known from the female of the same kind. These changes are observable in all the species, but most remarkably in the salmon and charr, and the larger varieties of trout. The changes which accompany growth with age are well seen in the young of the salmon, whilst in fresh water, in passing from the parr stage into the smolt, and again, after quitting the river, in becoming from the smolt the grilse. So remarkable is the difference of aspect of the parr and smolt, that the two, as you know, were for a long period held to be different species, and are still so regarded in the eye of the law and by the watchers of our salmon rivers; the capture of the former being tolerated to the great destruction of the salmon fry, whilst the taking of the smolt is finable, subjecting the angler to a penalty of five pounds.* One of the peculiarities of the parr, now well established, is the full development of its testes, and its power, in consequence, of impregnating the ova of the adult fish.† It might be interesting to ascertain experimentally whether the male of the other species, of those not migratory, as the common trout and charr, in its earliest stage, is similarly gifted.

An opinion has been entertained by one or two naturalists that there is a parr, truly a distinct species, founded on the belief that it is somewhat different in form, and that it is found in rivers, which, owing to inaccessible falls in their course, salmon cannot reach. The difference of form, as it may be affected by difference of food, seems deserving of little attention. The other circumstance, if established as a fact, would be of weight in the argument; but I believe it to be fallacious. Lately, in Sutherlandshire, I examined with care a stream, one of those said to have parr, *sui generis,* above such a reputed fall. I found the fall one that a salmon could easily surmount; and the parr which I caught for examination above and below the said fall

* Happily, thanks to the new Salmon Bill, this is only true of the past.

† Externally, there is no distinguishing mark in the male and female: in the female parr, whilst in the male the testes are as just described, the ovaries are very small, merely in a rudimentary state. This extraordinary peculiarity of the male parr, noticed above, was first ascertained by Mr. Shaw. See his excellent paper on the development of the salmon fry in the Philosophical Transactions of Edinburgh for 1840, vol. xiv. The milt of the young fish, so far as I have observed, is always shed before the parr becomes a smolt.

proved alike in all respects. I cannot but conclude, therefore, adopting the view taken by the highest authorities in Icthyology, that a parr, a distinct species, is a creature of the imagination; and that the idea of such a species ought to be opposed, both as founded in error and as affording a pretence to allow of the wasteful, mischievous capture of the salmon and sea-trout fry.

In most salmon rivers fish are occasionally taken of an ambiguous character, having the markings, the transverse bands of the parr, undiminished in intensity, and yet the fish of the size of the smolt, or even larger, sometimes reaching half a pound, or even ten ounces. It has been conjectured that this fish may be a cross between the salmon and common trout, and that, owing to the mixture, the migratory habit has been checked. This is possible, as it has been ascertained that the ova of the salmon can be impregnated by the milt of the common trout. I think it, however, more likely that the fish in question are salmon or sea-trout parr, which have missed their time of passage to the sea, and have owed their growth to their remaining in the river, and the retention of the markings to the want of such food (having other food in plenty) as is essential to effect the change to the smolt state, *i.e.*, the growth of a new crop of scales so amply provided with the white lustrous lining as to entitle them to be called silvery scales. This food, I am inclined to think, is a sufficiency of insects—insects, like the scales and their coloring matter, containing a considerable proportion of phosphate of lime. In confirmation of this view, I may relate that parr, put into a pond from which they could not escape, have grown in two or three years to be about half a pound in weight, and when caught were found to be in excellent condition, and still retaining their original transverse markings. The subject, it must be confessed, is obscure, and requires further and more exact observations for its elucidation.

Under the head of varieties, mention perhaps deserves to be made of a fish of Wastwater, there called "a botling," which is chiefly taken in the fall of the year, in the spawning season, in the streams which flow into the lake. It is always a male, and is distinguished by its large size and thickness of body, and by the marked projection and curvature of the extremity of the lower jaw; in weight it often reaches seven pounds. By the people of the country it is held to be a distinct species. It seems more likely to be an example of the modifying influences of the causes referred to, and merely a lake trout of a certain age, well fed, that for successive seasons has escaped being captured, more fortunate than its less active mate when intent on spawning.*

Before concluding, I must express regret, which I am sure is shared by very many of the inhabitants of the Lake District, that a region in its numerous lakes and rivers so peculiarly fitted for fresh-water fish, and where one might expect to find abundance, should, generally speaking, be so disappointing, affording as it now does but a scanty supply, and this mainly owing to want of due protection from the poacher, and, from the nature of the fishing laws, the difficulty of affording such protection.† Whether we view fish in the light of a source of national wealth as a wholesome diet, or in their capture by the angler as an innocent and healthy recreation, the subject surely is important, and deserving the attention of the legislature. What seems to be the main desiderata are, that a close season should be established by law; *that* regulated by the spawning time, and applicable to the trout and charr and other fish, as well as to the salmon; and as regards the last-named fish and the sea trout,

* The influence of various agencies in the production of varieties is well marked in the salmonidæ: the botling feeding, like the salmo ferox, chiefly on small fish, very much resembles it; the river trout, when feeding chiefly on incased larvæ, acquires a stomach of unusual thickness, like the gillaroo trout of many of the Irish lakes, feeding chiefly on shell fish. From the notes of my brother, the late Sir Humphry Davy, I find that the charr of the lakes of Southern Austria, feeding similarly, have a like thick stomach, thus differing from our English charr, the stomach of which I have never found but of ordinary thinness. Like Mr. Darwin, he was of opinion that the habits of animals, especially as regards their prey, may modify their forms. In a journal kept in 1827, writing at Eisenhartz on the 28th of May, he remarks, "From the similarity of the charr of the Leopoldstein Lake and the ombre chevalier of the Lake of Borguet, I am induced to make some observations on the physical causes which, by changing the habits in many generations may change the forms of fish. The trout, when it feeds principally on fish, must be extremely active and strong, and, from its predatory mobile habits, acquire large teeth, large fleshy fin, thick skin, and great pectoral fins for turning. When it feeds on shell fish, it gains the stomach of the charr, and its colors as in the gillaroo trout, and the charr when it becomes large is extremely like, if not the same, as the ombre chevalier." After some remarks on the effects of light on the color of fish, he adds, "The habits of spawning of fish must be influenced by weather; the charr I got this morning with mature eggs was just about to spawn; yet in England they spawn in winter." If summer is the spawning time of the charr and trout of the lakes of Southern Austria, is it connected with or owing to the water at that time being of the temperature best fitted for the purpose, most of those lakes being fed by mountain streams, frozen in winter and full in summer from the melting of the snow?

† It is to be regretted that in the Bill recently passed for the protection of salmon and sea trout in England, the trout, charr, and grayling are omitted.

that the parr, unquestionably their young, should be protected as well as the smolt. It is at the spawning season that the depredations of the poacher are carried on on the largest scale, when the fish are most easily taken, when they are of most value for keeping up the supply, and of least value as articles of food; and, if I may add another consideration, when the season of the year is least inviting for the angler's sport.

XXIII. OBSERVATIONS ON THE OVA OF THE SALMONIDÆ.

M. Vogt, in his able and elaborate work on the embryology of the Salmonidæ, has pointed out a remarkable property belonging to the ova of these fishes—viz., that of having their fluid contents coagulated by admixture with water. Thus, as he states : " Lorsq'on crève un œuf dans l'eau, ou voit à l'instant même la masse entière du vitellus se transformer en une matière blanchâtre, lactée, opaque et filamenteuse, qui n'a plus aucune ressemblance avec la substance vitellaire de l'œuf intact. Voulant m'assurer si c'était réelement l'effet de l'eau, j'ouvris un œuf au foyer du microscope et j'y mêlai une goutte d'eau, pendant que j'observais le vitellus : partout où les deux liquides entrèrent du contact il en résulta à l'instant même une quantité de petite granules opaques, qui furent affectés pendant long temps d'un mouvement molèculaire très prononcé. Ces granules etaient si petits que sous mon plus fort grossissement, ils ne m'apparurent que comme de petits points foncés et leur nombre considerable me prouva suffisament que ce n'etaient pas des nucléolules devenus libres par l'effet de l'eau qui auraient fait crever les parois des cellules."*

The observations of M. Vogt were made principally on the ova of the palée (*Coregonus palæa*, Cuv.) of the Lake of Neuchâtel. Those which I have to offer have been made in most part on the mature ova of the charr of Windermere. These, it may be right to mention, are commonly spherical, about two-tenths of an inch in diameter, weigh about a grain each (the fluid contents about ·98 of a grain,† the membranous shell about

* Embryologie des Salmones, par C. Vogt, p. 11, in vol. i. of M. Agassiz's Hist. of Fresh-water Fishes, Neuchâtel, 1842.

† Four ova from a charr taken from the spawning-bed on the 14th January, 1863, weighed 3·5 grs. ; each measured ·18 inch. The spermatozoa from her companion measured each about $\frac{1}{5000}$ inch. Four salmon ova, the largest of fifty, weighed 8·3 grs. ; four of the smallest, 6·1 grs. The largest measured ·28 inch ; the smallest ·22 inch. These ova were a portion of some sent me through the kindness of Thomas Ashworth, Esq., from his fishery in Galway : they were taken from a spawning salmon on the 30th December. In the stomach of the male charr above-mentioned, I found two ova of charr, a proof that this fish feeds on the ova of its

·02 of a grain), are of specific gravity 1,095, or thereabout, being suspended in a solution of common salt of this density. The contained fluid, the vitellus, is slightly viscid of a light yellow hue from oil particles of this hue contained in it, and slightly alkaline, as indicated by its effect on test-papers.

Having premised thus much, I shall briefly relate the results of the experiments I have made; and—

1*st*. *On the Action of Water on the Vitelline Fluid.*—When about equal parts of the fluid of the egg and water were mixed together, the result was an immediate coagulation exactly similar to that described by M. Vogt in the instance of the vitellus of the pallée. If the proportion of water was very much less, the two fluids mixed without coagulation, either at the instant or afterwards. The mixture was capable even of dissolving a minute quantity obtained by the action of a large quantity of water. When a puncture was made in the egg under water, the little fluid that issued was instantly covered with a delicate pellicle, and was shortly wholly coagulated, as were also gradually and pretty rapidly the entire contents.

2*nd*. *Of the Action of Heat.*—Contrary to what might have been expected, heat, even a temperature of 212° Fahr., did not coagulate the vitellus. Eggs placed in a dry tube immersed in boiling water, shrunk and became shrivelled from evaporation, but not opaque; and, when evaporation was arrested by the presence of steam generated from accompanying moist cotton, even this change was prevented; after immersion of the tube from five to ten minutes in boiling water, the vitellus remained fluid, coagulable, however, as before, on admixture with water. Heated in water, the effect was strikingly different. At 160° Fahr., the coagulation took place pretty rapidly; at 120°, more slowly; and slower still at lower temperatures: at 100°, the time required for coagulation to take place was about half an hour. The higher the temperature at which the coagulation was effected, the greater was the firmness of the coagulum; at the boiling temperature, continued for a few minutes, it was as firm nearly as the yolk of the egg of the common fowl similarly treated. That in all these instances water penetrated and

own kind—a striking example of antagonistic appetites. In a note-book of my brother, kept at Rome, I find a similar example in the torpedo; in one from the fish-market, which he there examined, he found a small torpedo in the stomach of a larger one.

became mixed with the vitellus can hardly be doubted; it may be mentioned in confirmation that at 100° the coagulation extended gradually, spreading almost from a point. These trials were made with unimpregnated eggs. Repeated on others that had been subjected to the influence of the spermatic fluid by admixture about thirty-six hours previously, the effect of coagulation was decidedly slower in taking place—*i.e.*, the fluid resisted longer incipient coagulation, but, when it commenced, it seemed to proceed as rapidly in one instance as in the other.

3*rd*. *Of the Action of Alkalies and Salts.*—Ammonia or potassa, or the sesquicarbonate of either alkali in solution, added in very minute quantity to the vitellus, did not prevent its coagulation; but, if of moderate strength, the result was different—the addition of water did not occasion coagulation; moreover, the fluid now was capable of dissolving a portion of coagulum obtained from the vitellus by the action of water.

Common salt, muriate of lime, muriate of ammonia, muriate of barytes, nitre, phosphate of soda, sulphate of magnesia, alum, acetate of lead, these in solution acted very similarly, when weak not preventing coagulation, but preventing it when not much diluted. In the instance of common salt, a solution so weak as to be of the sp. gr. 10,045, did not, when added to the vitellus, impair its fluidity; it required to be reduced to 10,029 to effect coagulation. The stronger saline solutions, in the same manner as the alkaline, were found capable of dissolving a certain quantity of the coagulated vitellus.

4*th*. *Of the Action of Acids and some other Agents.*—The fluid of the vitellus was not coagulated by the tartaric, oxalic, or acetic acids, either strong or very much diluted. By strong muriatic acid it was inspissated, the acid and fluid not incorporating. The inspissated mass was transparent; on the addition of water it became opaque, and of a milky whiteness, the color of the ordinary coagulum. The effect of strong sulphuric acid was but little different; whilst the greater portion of the vitellus was inspissated, a very small portion was dissolved, as was indicated by the acid, after having been decanted, becoming milky on the addition of water. Nitric acid, whether strong or weak, coagulated the vitellus. A solution of corrosive sublimate had a like effect, as had also alcohol.

The results of these experiments seem to shew that the fluid the subject of them possesses properties distinct from those of either the albumen or yolk of the eggs of birds, or indeed of any other form of albuminous fluid hitherto described. In consequence, may it not be held to be a species or variety apart, as much so as the albumen of the serum of the blood, or the coagulable lymph of the same fluid?

I could have wished to have extended the inquiry to the ova of the other species of Salmonidæ; but I have not had an opportunity except in an imperfect manner in the instance of those of the trout and salmon and white trout (*S. trutta*). The results obtained, few as they were, as also from the ova of the pike and perch, were similar, leading to the conclusion, so favored by analogy, that the ova of all the several species will be found alike in their properties; and further, that the ova, if not of the cartilaginous, at least of the other species of osseous fishes, will not be found dissimilar. But, however probable this may be, it is desirable to have it determined by exact experiments, especially as in the instances of the ova of several of the cartilaginous fishes, comparing one with the other, there are marked differences, both as regards their component parts, and probably as regards the qualities of those parts. Thus, from such observations as I have made, the eggs of the viviparous fishes of this order appear to be destitute of a white, which those of the oviparous possess. The torpedo and *Squalus squatina* may be mentioned as belonging to the former, the *Squalus catulus* and *acanthias* and the *Raja oxyrinchus, clavata*, and *aquila* to the latter. The yolk of all these fishes, both of those which have and of those which have not a white, seems in its general properties to be very similar to that of birds: I can state confidently that it is not coagulated by water. The white (the glairy fluid corresponding in situation to the albumen ovi of birds) will probably be found to possess properties differing from those of the white of the bird's egg. In the instance of that of the *Squalus catulus*, I found that it was neither coagulated by nitric acid nor by heat. In a note dated Malta, 1832, I have described it "as a transparent viscid fluid unaltered by boiling during two minutes, in which time the yolk had become hard, and also as uncoagulated by the addition of nitric acid."

There is a tendency of the mind to seek an object, some end, in all that we witness, a final cause, in accordance with the maxim that Nature does nothing in vain. Reflecting on the property of the ova of the Salmonidæ—how so long as they retain their vitality they remain transparent—how on losing their vitality they become opaque—it has occurred to me that even this difference may not be without a use. The transparent ova are less easily seen than the opaque white, the living than the dead ; and in consequence the latter may be more attractive, more liable to be preyed on than the former. And the circumstance that the opaque coagulated ova resist change, and keep in water a long time, even several months, without undergoing any perceptible alteration, is in favor of the conclusion that they are specially intended for becoming food, serving as lures, and thereby in a manner protecting the transparent—those retaining vitality and in course of being hatched—from being devoured by birds and fishes.

XXIV. ON THE IMPREGNATION OF THE OVA OF THE SALMONIDÆ.

From time to time it has been asserted that the function of impregnation of the ova of these fish is performed after the manner of that of the cartilaginous, viz., before exclusion. The instances related in proof are commonly of a vague kind, and such that little credit can be attached to them. Recently a more precise example has been adduced—how the ova of the trout, taken from the abdomen of the parent fish, and placed in a "running stream" apart, included in a perforated box, in due time were hatched, producing young fish. The particulars of the experiment, and the result, were published in the spring of last year, 1853, and in more than one of the provincial papers; and Dr. Robertson of Dunkeld was named as the institutor and reporter of the trial.

Considering the manner in which this statement was made and received, and the practical conclusion deduced—that no longer any trouble need be taken in the artificial mode of breeding to obtain the milt to apply to the roe, I have thought it worth while to give the subject some attention, on the supposition that the result, as stated, may have been accurate, being, as it appeared to me to be, within the limits of possibility—though I cannot say, keeping in mind the structure of the male and female fish, and all the information hitherto collected respecting the manner in which the generative process is carried on by them, that it is within the limits of probability.

I shall first briefly notice some trials which have been made, and with a view to determine the question.

Mr. Shaw, in his valuable paper on the "Development and Growth of Salmon Fry," published in 1840 in the Transactions of the Royal Society of Edinburgh, describes how, in two instances, he obtained negative results in experiments on mature ova of the salmon which had not been mixed with milt after exclusion, though in all other respects placed and treated like other ova from the same fish, ova which had been mixed with milt after their exclusion, and were thereby impregnated, and rendered prolific.

Mr. Young, in his Natural History of the Salmon, gives an account of some experiments with a similar negative result. In page 17, he states, "We have often experimented on the ova of fishes, merely to arrive at facts. We have impregnated one part of the ova of the fish with milt, and have left part unimpregnated, and then deposited both parts in the same stream, at the same depth, and in a current of exactly the same velocity. But never, in any one instance, did we find one grain of the unimpregnated part productive, while the other portion that was impregnated with the milt never failed to produce fry in due time." He adds, "This has been frequently tried, and has at all times proved the same."

Mr. Ashworth, by whom the production of salmon on a large scale has been so successfully carried on in Ireland, informs me, in a letter with which he has favored me, of a similar negative result—how Mr. Ramsbottom, in his employ, "took a female fish (a salmon) and extracted a quantity of eggs, then placed them in a box alone, without impregnating them with the milt, and none of them came to life;" and how "he took the remainder of the ova from the same fish, and impregnated them with the milt, and these produced young fish."

The trials I have made have afforded similar negative results. I shall mention three in particular.

On the 10th of last November, from a stream in which there were known to be male fish with mature milt, two female trouts were taken with fully formed ova—ova that were expelled by the application of gentle pressure to the abdomen. These were placed on gravel in a glass vessel with water, which was changed twice daily; they exhibited no marks of development, and one after another became opaque from imbibing water.

On the 25th of the same month, I procured two charr from Windermere, a male and female fish, taken from a shoal in the lake, a breeding bed. On gentle pressure to the abdomen, ova in large quantity were obtained, and abundance of spermatic fluid; each fish at the time was alive. A portion of the ova was placed in three glass vessels with gravel and water, without having been allowed to come in contact with the milt. Another portion of them was mixed with the milt, and similarly distributed. The vessels were kept in a room of pretty equable temperature, which ranged from about 51° Fahr. to 44°, that is, from the commencement to the present time, and the water—spring water—was changed daily once, and no oftener. Now, January 4th, a large number of the eggs which had been mixed with the milt are well advanced, the fœtal fish being visible in the ova with the naked eye, and this in each of the three vessels; but, on the contrary, in the other three vessels, not one egg bears any marks of vital progress; many of them have become opaque; the majority of them, and those which remain transparent, are of uniform appearance, whether seen with the naked eye or under the microscope. Under a one-inch object-glass, in all of them, at one spot, a patch, as it were of cellular tissue, is observable, seemingly adhering to the membrane of the egg, with oil globules entangled in and surrounding it.

On the 2nd December, I procured some eggs from two charr, taken at the same time as the preceding, and from the same breeding shoal, and kept in company with male fish in a well fed by a small stream. The eggs, obtained by pressure on the abdomen, were the few remaining, the greater portion having been previously shed, as was manifest from the lankness of the fish. From this circumstance, they seemed peculiarly favorable for the trial, on the hypothesis of the possible admission of the spermatic fluid *ab externo*. But the result was equally negative with the foregoing. The ova put into water, the same as that used with the impregnated fertile ova, and under the same circumstances, all underwent no change, excepting that denoting loss of vitality.

Many other instances of the like kind I could relate, that have been communicated by friends interested in the subject; but I hardly think them necessary, those I have given appearing to me so conclusive, even on the doctrine of chances.

Next, it may be well to advert to the structure of the male and female organs of the Salmonidæ, to which I have alluded, as seeming to render impregnation from without very improbable.

The female, as it is well known, has no true oviduct, as in the instance of the cartilaginous fish. Her ovaries accordingly are not connected with the anal aperture, through which their exclusion is effected. How ill adapted is this for the required effect, according to the supposition of impregnation of the ova before exclusion! Moreover, as regards the male fish, we see the same inaptitude exhibited in the conformation of its generative organs.

The more carefully the parts are examined, the greater is the difficulty, as it appears to me, to suppose that impregnation in these fishes takes place *ab externo*. In the instance of the female, the aperture communicating with the cavity of the abdomen is situated between the papilla (the urethral clitoris) and the anal opening of the intestine, a delicate fold of the latter overlapping it, acting as a valve; it has also a delicate fimbricated fringe suitable to the same purpose. The papilla, moreover, which is posterior to both openings, is fitted to act as a valve. All these parts are situated in a little pouch or cavity, on each side of which, as if for its defence, is a minute projection of integument with an elongated scale—these together, in a sea-trout of about 2lbs., measuring in length one-tenth of an inch.

Though totally unfit for the reception of a fluid *ab externo*, the abdominal opening is admirably fit for the passage of the ova; and these in passing, coming in contact with the papilla, which they must unavoidably, may excite a pleasurable sensation that in its turn may induce the muscular exertion requisite for their expulsion.

The seminal ducts in the male fish terminate in its rudimentary penis (that a papilla totally unfit for intromission); each opens close to its point, and each has a distinct opening. It may, perhaps, be said, that in birds, such as the gallinæ, the male organs are merely rudimentary—are merely papillæ—yet the ova are fertilized in the oviducts. True; but though there is a certain similarity in the instance of the male organs of birds and of fish—such as the Salmonidæ—the female organs of the two are different, the birds having a vagina opening into the cloaca in a manner and with an action well adapted to receive the impregnating fluid when injected into the common receptacle.

Further, if attention be given to the manner in which the male and female fish behave during the spawning time, I think we shall have confirmation that there is no act of intromission—which indeed, anatomically considered, it may be presumed there cannot be—and also that there is no attempt made favoring the notion that the spermatic fluid is injected (as would be necessary for the impregnation of the ova) into the cavity of the abdomen of the female. That the fish in the act of spawning sometimes come in contact, pressing against each other, and thereby aiding the expulsion of the ova and milt, cannot, I think, be doubted. By many observant fishermen, poachers addicted to the taking of the fish at the time of their spawning, I have been assured of the fact from their own observations; but this is very different from the act of copulation as performed in other classes of animals in which impregnation is effected before the expulsion of the ova; but though so dissimilar, perfectly suitable to the end required, and quite in accordance, as we have proof in the artificial process, with the necessary requirements.

It is an axiom that Nature does nothing in vain; it is not less true that Nature is perfect in her works, as regards the adaptation of means to ends. In no part of the animal economy is this more strongly and happily illustrated than in the generative system of organs, diversiform and varied as they are in the several classes of animals. Consistently, then, were the mode of impregnation that which has been asserted, we may be sure that an organization, an apparatus, would have been provided suitable to it. Also, as I think, consistently with the hypothesis, we might expect occasionally to find ova in the cavity of the abdomen, bearing marks, if they had been impregnated there, of incipient development, according to the analogy of extra uterine fœtal growth sometimes witnessed in the Mammalia; but none have been described, that I am aware of, as ever observed. In spent fish, that is, those which have spawned, in the instance both of the salmon and trout, I have in spring found mature transparent ova detached from their ovaries, so included, when the aperture for the passage of the ova was closed, or almost so; but they were totally destitute of any appearance of vital development.

In conclusion, granting the observations referred to—of the hatching of the ova of the trout in the manner described, viz., without milt, so far as was known, being brought into contact with the expressed ova—to be accurate in their detail, it may be asked, Does the result, as stated, warrant the inference that impregnation was effected before the expulsion of the ova? The box, we are informed, containing them was placed in a stream. What is more likely than that they might have been impregnated, so included but not insulated, by the spermatic granules, the spermatozoa of milt shed by some fish in the adjoining water? The diffusibility of these living granules—not the least remarkable of their qualities—seems to be favorable to this conclusion.

XXV. OBSERVATIONS ON THE CHARR (*SALMO UMBLA*) RELATING CHIEFLY TO ITS GENERATION AND EARLY STAGE OF LIFE.

The natural history of the charr, especially as regards its generation and the early period of its life, is admitted to be very defective, partly, no doubt, arising from the peculiar habits of the fish withdrawing it from observation, and in part, and more, to the circumstance that it is comparatively of rare occurrence, being found only in a limited number of the deepest lakes of this country, and, with few exceptions, seldom taken by the angler, and consequently a good deal removed from the notice of the naturalist.

Residing for several years in the neighbourhood of Windermere —a lake in which this fish, though decreasing in number, is still pretty abundant—I endeavoured to collect information respecting its breeding, the time required for the hatching of its ova, and the peculiarities of the young fish after its exclusion, but in a great measure in vain. The fishermen of the lake were acquainted with its spawning season and the spawning localities; but none of them had ever seen a young charr after its quitting the egg, nor till it had attained a notable size.

Artificial breeding—that process of fecundation which was first tried by Count Golstein in the middle of the last century, and has since been so successfully employed both in propagating some of the more valuable species of the Salmonidæ, and in illustrating their history—occurred to me as the only likely means of affording the information desired.

About the same time, viz., in the autumn of 1850, a gentleman, Morris Reynolds, Esq., living near the lake—through whose garden a small stream of good water descends from the hill above, very favorably circumstanced for carrying on the process of artificial breeding—commenced the attempt, after the manner recommended by Jacobi. This process is now so well known as hardly to require description. I may briefly mention, that two wooden boxes, communicating, were used, through which a small current of water was allowed to pass by a grating of perforated zinc, over a bed of gravel laid on the bottom of each compartment. In these boxes the roe of the fish for trial, after admixture with the fluid milt, was deposited, each obtained from individuals in the act of spawning, or mature for that act, as denoted by both the roe and milt being yielded under gentle pressure applied to the abdomen, soon after the fish were taken from the water—the roe in detached ova, the milt in the state of a milk-like fluid.

It was from these boxes that I obtained, through the kindness of their proprietor, most of the subjects of the following observations; and to him, too, I was indebted for exact particulars, without which the observations would have been almost valueless.

1. *Of the Roe and Milt of the Charr.*

The ova of the charr, at their full time, that is, when they are detached from their ovaries, and are loose in the cavity of the abdomen, ready for expulsion, are, like those of the other Salmonidæ, almost, if not quite, spherical. Those I have examined, I have found to vary in diameter from ·16 to ·18 and ·20 of an inch; and in weight (after the removal of adhering moisture by wiping) from ·7 grain to 1 grain each. Their color is a light yellow, lighter than that of the ova of the salmon or lake trout with which I have compared them, and thus distinguishable, as well as by their smaller size. The matter of which they consist may be described as an almost colorless, transparent, viscid fluid, containing suspended in it very many oil globules of various sizes, hardly distinguishable without the aid of the microscope, of a yellow color, to which the color of the egg is principally owing. This matter may be considered as corresponding to the yolk of the egg of the bird; the chief portion of it, the coagulable part, as described in the preceding paper, may be viewed as a distinct species of albumen.

The shell of the egg of the charr may be briefly noticed. Nearly transparent and colorless, it is of considerable strength, and until thinned and weakened in the process of hatching, is not easily ruptured. Five emptied of their contents, but not deprived of their moisture by drying, weighed one-tenth of a grain; thoroughly dried, so as to expel this moisture, they were reduced from ·10 to ·07 of a grain, thereby denoting a large proportion of solid matter, viz., 70 per cent.

Whether this shell in its sound state, before putrefaction has commenced, is pervious to water, seems to me questionable; and also, whether the internal vitelline membrane, after fecundation, is altogether impermeable by it. M. Vogt holds that the shell is at all times so permeable, but the vitelline membrane, after impregnation, never, so long as the ovum retains its vitality; losing which, the membrane, he infers, no longer resists the transmission of water, and the coagulation of the fluid yolk takes place as an unavoidable consequence. I might assign reasons for the doubts I venture to entertain on these points; but not sure that they would be considered satisfactory, or that the points themselves, though not without interest, require here to be discussed, I shall avoid bringing them forward. That the death of the impregnated ovum, as pointed out by M. Vogt, is clearly indicated by the coagulation of the yolk, from the penetration of water into its substance, is certain. But there is another indication of the event, and not less certain, viz., the adherence of the lighter oil globules to the vitelline membrane, preventing thereby their change of place with a change of position of the ovum, and that tendency to ascend in the heavier yolk fluid which is observable whilst vitality lasts, and which may perhaps be considered as a characteristic of it. The adhesion of the oil globules alluded to not unfrequently takes place in eggs which retain their transparency. In no instance have I observed any traces of fœtal development after these have become fixed, or, if commenced, any further progress. Why these ova do not become opaque, why their membranes should remain impervious to water, I am ignorant; but that they are so must be inferred from the circumstance, that when ruptured, and their contents mixed with water, coagulation is immediately effected.*

Relative to the milt or spermatic fluid of the charr, I have but

* The different degrees of permeability of different ova is remarkable: of 385 salmon ova sent to me from Galway, on the 30th of December, the day they were taken from the fish, and after admixture with milt, 210 became opaque in a few minutes, when transferred from the moist moss in which they were packed to water. This on the 1st of January, the day they arrived by post, and day after day up to the present time, the same change has been taking place in others. Now, February 2, there are only 36 remaining retaining their transparency; and of these only 18 bear marks of retention of life, and are distinctly in progress of development, black spots, the rudimentary eyes, being visible, with vascularity of membrane.

few observations to offer, the examination I have hitherto made of it not having been minute, except very partially. Like that of the Salmonidæ generally, in its mature state when ready to be shed it is a milk-like fluid, slightly viscid, heavier than water, and containing diffused through it (the cause of its milkiness) a vast number of granules (spermatozoa). These minute bodies are nearly spherical in form, are about $\frac{1}{10000}$th of an inch in diameter, and seem to move spontaneously, as seen under the microscope, for a short time after the expulsion of the fluid from the live fish. Though they are of greater specific gravity than water, yet, owing to their minuteness, they are easily diffused and suspended in this fluid. After a rest of two hours, water rendered turbid by the addition of a small quantity of spermatic fluid had not become clear, even towards its surface. A drop placed under the microscope was found to abound in spermatozoa. Another property of the spermatic fluid, not unworthy of mention, is the remarkable manner in which it resists putrefaction. Whether the spermatozoa are capable or not of impregnating the ova after they have lost their power of spontaneous motion, I cannot offer any decided opinion; from the few trials I have made, I am led to believe that the one quality or power is distinctive of the other, and that, ceasing to move, they become inert.

In a charr weighing about half a pound I have found the number of ova to be 1,230, all nearly of full size. As the volume of the mature and distended testes is about the same as that of the ripe ovaries, the number of spermatozoa belonging to them must almost baffle calculation; and if, as there is reason to believe, a single one may suffice to impregnate an ovum, the whole from one male may, it is presumed, be more than adequate to effect the impregnation of the entire eggs of many females, especially taking into account how readily these minute bodies are suspended and diffused in water.

2. *Of the Time required for the Hatching of the Ova; and of the Young Charr in their Early Stage.*

The principal spawning season of the charr in the several lakes of the Lake District in which this fish occurs, is the beginning of winter, from about the first week in November to the first in December, when the water over the spawning-beds has become comparatively cool, reduced from about 60° Fahr. to about 50°. Whether this is the only season is somewhat doubtful; the fishermen of Windermere speak of a later one, in which it is believed by them that fish of the larger size and few in number deposit their spawn, viz., in February and March.* Be this as it may, all the observations I have recorded were made on spawn obtained during the first period mentioned.

From analogy, it might be inferred that the time required for the hatching of the charr would be a variable one, depending on the degree of temperature of the water and on other less appreciable circumstances. In 1850–51, Mr. Reynolds, as he informs me, found none hatched in a shorter period than sixty days; the greater number on the seventieth, and from that to the seventy-fifth day; some few as late as the ninetieth. The average temperature of the water in the breeding boxes was about 40°. At a higher temperature, viz., an average one of about 55°, I have witnessed the completion of the process in the short period of forty-one days. In this instance the milt and the roe were mixed as soon as they were taken from the fish on the 29th of last October; a certain number of the ova were put into a glass vessel and covered with water to the depth of about an inch, which was changed twice daily, and kept in a room the temperature of which was very uniform—seldom below 54° and never above 56°. On the 10th of December two young fish left their shells, and on the following day a third. They were all three feeble, as if their development had been premature; in a few days they died. Some eggs from the same fish which had been placed in Mr. Reynolds' breeding boxes were not hatched till the ninetieth day, or more than double the time.

What the other circumstances are—other than that of mere difference of temperature—which influence the acceleration or retardation of the hatching process, are deserving of being investigated experimentally. Something may, perhaps, depend on the size and quality of the egg; something on the contact of the

* I am disposed to think that the breeding-time of the charr in Windermere is even less limited than is stated above, having found in the latter end of February individuals with the testes nearly of their full size, and this not in large fish; and others with ovaries containing eggs varying in size from a mustard to a millet seed. These fish were all from the lake; I have never heard of one being taken or seen in the Brathay (a river flowing into the lake, to be mentioned hereafter) after December.

spermatozoa, their number and activity; and other conjectures might be offered.

In illustration of the growth of the young fish, after quitting the egg, I shall briefly describe what I witnessed in the instances of three that I observed with some care from the time of their escape from the shell to the attainment nearly of their perfect form. It was on the 17th of January that they were hatched. Some days previously the embryos were very active, frequently changing their position by sudden jerks, effected by the tail and the posterior portion of the body. One I saw in the act of bursting the shell, now become very thin and tender. The rupture took place suddenly at a spot where there was a little prominence—an evident yielding of the shell to the pressure from within—and simultaneously the coiled-up fœtus became liberated; the effort, it may be inferred, made by the tail, by which the opening was made, sufficing to extricate it. The instant the young fish entered the water, it darted about wildly for a few seconds; then rested, lying on its side. It was most easily disturbed; on the slightest touch, even if merely applied to the water near it, it fled from the touching body, moving with wonderful rapidity, and in such an irregular, devious course as was well adapted to promote its escape from a pursuing enemy.

These fish varied in length from about six-tenths to seven-tenths of an inch; the yolk attached was about ·25 of an inch in length, and about ·15 of an inch in depth, of an oval form. They were transparent and almost colorless, allowing the circulation of the blood to be seen distinctly with the microscope, using even a low power, such as a glass of one-inch focal distance. Their eyes appeared to be perfect, the lens visible and apparently prominent, the iris colored; and, in accordance, the vision seemed to be acute, even the approach of a moving body, without coming in contact with the water, exciting alarm, indicated by a sudden change of place.* The pectoral fins were distinct, and almost constantly in action; the single embryonic fin, including the rounded tail, extended inferiorly to the yolk sac, and superiorly a little beyond the spot where the dorsal fin was to be.

On the 30th of January, a very slight increase in their length

* One young fish was without eyes, otherwise apparently well formed. It was as active as the rest.

was observable, about ·02 of an inch. The several fins, the dorsal, the abdominal, and anal, were beginning to appear in the form of slight projections from the single fin, especially the dorsal, in which rays were noticeable. The gill-covers now were somewhat projecting, resembling fins, and were in constant motion over the branchial arches, in which the blood corpuscles were to be seen circulating in looped vessels.*

On the 4th of February, it is noticed that the fish were acquiring color, dark coloring matter being deposited in stellaform specks; that the embryonic fin was diminishing, and that the adipose fin was beginning to appear, marked by a slight elevation.

On the 14th of the same month they were found to have increased to about ·8 of an inch in length, and the yolk to have diminished to ·2 of an inch, and to have become narrower.

On the 22nd, the water in which they were kept was frozen over: they were seen swimming actively under the ice, and restlessly, as if in search of a passage to deeper and less cold water.

On the 12th of March the dorsal fin was almost apart, the other fins advancing, the single one receding from absorption; the tail still rounded; the abdominal integument extending over the diminishing yolk, but not yet entirely covering it.

One died on the 18th of this month; the others on the following day. In these there was an appearance of sooty matter about the gills, which probably was the cause of their death, by obstructing respiration. One of them, weighed, was found to be little more than half the original weight of the egg; merely wiped, it was equal to ·58 of a grain; thoroughly dried, at a temperature of 100°, it was reduced to ·16 of a grain. From the time of their hatching to that of their death, I am not aware that they had taken any food other than that provided for them by nature in the attached yolk, a period of sixty and sixty-one days. Probably, had they been favorably situated, where they could have found suitable food in the water, their growth would have been more rapid. One taken from the breeding boxes on the 22nd of March, hatched about the same time as the preced-

* When first hatched the branchial arches are naked, that is fully exposed to view, the gill covers as yet not being sufficiently developed to hide them. As in the instance of Salmonidæ, and of other osseous fishes, no branchial filaments are known to exist in the fœtus, may not their place be supplied by this naked state of the branchia?

ing, viz., the 17th of January, and when, consequently, about sixty-five days old, may be adduced in proof; premising that, from the manner in which the boxes were supplied with water, and their being shaded with trees, and some aquatic plants having been introduced, brought from the bed of the Brathay—that part of the river where the charr is known to spawn—there was probably no want of the proper food of the young fish, minute insects and infusorial animalcules, traces of which, indeed, were detected in its excrements, when seen under the microscope using a high power. The young fish of the age mentioned was perfect in its form. The embryonic fin had entirely disappeared, with the exception of a slight vestige of it between the anal and the abdominal fins. All the permanent fins had become distinct, even the adipose, though it was rather more extended and less elevated than in the full-grown fish. The caudal had lost its rounded form, and had become not forked, but square. No vestige remained externally of the yolk-vesicle, the abdomen being entirely closed, covered uniformly with a silvery integument. The back and sides, of a light greenish brown, were marked by two rows of spots of a dark hue, almost black, the inferior the largest, reminding one of the bars of the parr and the marking of the young trout. Measured, its length was found to be one inch; its width or depth, where greatest, about ·16 of an inch. It was very active, and disposed to feed, darting often with avidity at any minute body thrown into the water, but only whilst in motion; and often, after taking it into its mouth, casting it out. Fed daily, chiefly with finely grated dried beef, it was kept alive till the 21st of June, when it was increased in length only to 1·06 inch, so inconsiderable had been its growth. The water in which it had been kept, and which was changed daily, was about the temperature of 50°, sometimes two or three degrees higher, seldom lower. The young fish was frequently to be seen in a restless state, as if seeking to escape. Those of the same brood, left in the breeding boxes, effected their escape about the middle of April, when, in consequence of a flood, the water overflowed. They were then from 1·25 to 1·5 inch in length.

In the cartilaginous fishes, the yolk is found in the cavity of the abdomen long after it has disappeared externally. In the torpedo I have detected it there as late as the fifth month from the time of hatching.* That the same happens in the young charr, I cannot entertain a doubt. In one instance—that of a fish hatched six weeks, kept the whole of the time in the breeding-box, and which was nearly perfect in its form—though no trace of the vesicle remained externally, it was visible within, seen through the transparent parieties of the abdomen, distinguishable both by its form and under the microscope by the oil globules belonging to it.

3. *Of some Agencies and Circumstances supposed likely to Influence the Ova and Young Fish.*

These, so far as I have tested them by experiment, I shall briefly notice.

From the best information I have been able to obtain, the charr in the Lake District, with few exceptions, chooses for its breeding-place stony and gravelly shallows in the lakes in which it is found, and never, after the manner of the trout, ascends the small streams towards their source to deposit its spawn. The exceptions alluded to, which have come to my knowledge, are in the instances of the charr of Windermere and that of Ennerdale. The former, it is known, not only breeds in the lake, but also in the river Brathay; but it deserves to be kept in mind, that that part of the river which it chiefly selects for the purpose has a good deal the character of a lake, the water there being expanded, forming a small lake or pool, where, in parts out of the actual current, it is little more disturbed by the wind than the shallows of Windermere itself.† The charr of the lake of Ennerdale—the other exception—I am assured on good authority, that of Dr. Lietch of Keswick, frequents in the spawning season

* See Researches, Physiological and Anatomical, vol. i., p. 73.

† There are, I have since learned, two other brooks in which the charr of Windermere spawn—Holebeck, a small stream near Lowwood-inn, and Troutbeck, a larger stream. The part selected is near to the outlet of each into the lake, where the water is little broken. I have since, too, learned that the spawning ground of the Brathay is more extended than I had supposed, reaching even to the first fall—Colwith Force; but, be it remembered, most part of the river so far is gently flowing. The ova and spermatozoa described in a preceding note, p. 224, were from fish taken in Holebeck. These charr were externally redder than those which spawn earlier; even the abdominal surface of the female was tinged red. The muscles of both were white, as was also the air-bladder of each. This greater redness, according to the fishermen, is distinctive of the late spawning fish. They are said, too, to be larger than the others. Of the two I had, both finely formed, with small heads, the male was rather above the average: its length was 13 inches; its girth 8 inches. The length of the female was 11 inches; her girth 6 inches.

a pool of a little mountain river, called from the circumstance, the "Charr Dub," about 300 yards from the head of the lake; itself (the pool) about 120 yards in length, and about six or seven yards in width, with a sandy, gravelly bottom, and large stones here and there interspersed. In this pool it is said that the fish congregate, with great regularity as to time, about the seventh or eighth of November, and remain there usually about a fortnight, when, having performed the function for which they came, they return to the deep water of the lake.

I make this statement in consequence of some naturalists, guided by the analogy of the best known species of the Salmonidæ, having inferred that, like them, the charr can breed only in running water, and that its being seen in large numbers in the spawning season in shallow water in lakes, was only preparatory to ascending the streams. The weight of evidence against this conclusion is such that I think it cannot be maintained; nevertheless, it appeared to me worth while to make a few experiments for the purpose, if possible, of testing it. With this intent, portions of roe, after having been mixed with liquid milt, were put into vessels, some of earthenware, some of glass, with a limited quantity of water (not changed during the trial): some in the open air, some within doors. This was done on the 4th of November, using the roe that had been obtained on the 30th of October, the same as that from which three ova, as already mentioned, had been hatched in forty-one days. None of these trials were perfectly successful: excepting in one, no progress towards development was observable. This was in the instance of ova contained in a glass bottle of eight ounces capacity, the water about two inches deep, and kept in a room, the temperature of which was commonly about 55°. On the 26th of the same month, marks of progress were observable in one of these ova; the eyes of the embryo were apparent as black specks, and vessels carrying red blood were to be seen ramifying in the vitelline membrane. The development went no farther. Even imperfect as this result is, is it not in favor of the conclusion that running water is not essential to the hatching of the fish?

Mr. Reynolds mixed together the roe of the lake trout and the fluid milt of a charr, which he placed in his breeding boxes in November. In seventy days some of the ova were hatched, and the young fish had a hybrid character, the fish themselves having much the appearance of the charr of the same age, whilst the yolk attached, with its few large richly-colored oil globules, was exactly similar to that of the trout. Is not, I would ask, this fact that the ova of the one species can be fertilized by the spermatic fluid of the other, in favor also of the conclusion that the breeding-places of the two are different? Were they not so, as the breeding season of the two is the same, a constant crossing would be almost unavoidable, and a confusion and loss of species would be an almost necessary consequence.

As a solution of common salt has the property of not only keeping liquid the fluid of the yolk, but also of dissolving its coagulum, it seems well adapted as a medium for the purpose of examining the fœtal structure. Using it thus, I found that an ovum in which the embryo was active on the forty-second day, immersed in a solution of salt of the sp. gr. 1,033, kept therein about half an hour, retained its vitality; and that, excluded by an opening artificially made in the shell, the young fish remaining in the solution, continued active for another half hour. This result led me to try the effect of keeping the ova in solutions of common salt, and also the young fish, to ascertain whether the former would be hatched, and what would be the effects on the latter. One trial was made with the ova, using salt water of the specific gravity mentioned, 1,033; another with water just perceptibly impregnated with salt, confined in glass bottles, and kept in the room of the average temperature of about 55°. In the stronger solution, the ova remained transparent, but no marks of development appeared. In the weaker solution, on the 26th of November (the trial was begun on the 4th), black specks denoting eyes, in the act of forming, were observable in four ova, and vessels carrying red blood in the vitelline membrane. In this stage further progress was arrested by death. The first experiment on a young fish was made on one that had been hatched about twenty-two days. Put into sea water, diluted with spring water so as to be of sp. gr. 1,020, it was found dead three hours after; it was contracted in length from ·68 to ·46 of an inch. The next was on a young charr of the same age: this, immersed in a solution of the specific gravity 10,036, after twenty-four hours, seemed as active as before. More salt was then added, so as to increase the specific gravity to 10,068, but still without marked effect. After other twenty-four hours, the specific gravity, by another addition of salt, was raised to 10,098; now the fish became more restless, as if seeking to escape. After the same interval a fresh portion of salt was introduced, raising the specific gravity to 10,153. The effect now was strongly marked: in about six hours the fish was found motionless, except the lower jaw, which, under the microscope, exhibited a tremulous movement, and, except the heart, which still acted pretty vigorously, and which continued to act, but with decreasing force, for about twenty hours, reckoning from the time that the fish first appeared motionless and moribund.

The next trials I shall mention were made with the intent to endeavor to ascertain how long young charr might be kept alive in the same portion of water, and that a small quantity, such as might be used in conveying the fish from place to place at an early age, when, before the yolk is exhausted, it stands in no need of a supply of food from without. Two experiments were made, one with a portion of pure oxygen over the water, the other with common air. The volume of water and air in each instance was nearly equal—about four ounce measures—the capacity of the containing bottle being about eight ounces. The bottles after the introduction of the young fish, were closed with a glass stopper and inverted in water; they were kept part of the time in the open air, and part of it in the room of equable temperature: each fish had been hatched about six weeks. The one in water, with oxygen, put in on the 28th of January, was very active till about the middle of February; about the 24th of that month it began to appear languid, and it was more so on the 26th, when it was taken out and transferred to a vessel fully exposed to the air, and the water in which was changed daily. Though it lived till the 18th of March it did not recover its activity. Its growth whilst under oxygen was much the same as if it had been kept in water exposed to the air and changed daily. The oxygen used was not tested for carbonic acid; by the test-paper its purity did not appear to be impaired. The trial with common air was commenced on the 7th of February; on the 13th, the young fish was found dead. As there was a small spot of stagnant blood in the vitelline membrane, its death might be owing to disease, unconnected with the peculiarity of circumstances in which it was placed. On the 28th of March I repeated the experiment with a young fish which was vigorous and active. Taken out on the 4th of April, its activity seemed unimpaired; it fed greedily. This fish had been hatched about seven weeks.

The only other trials I have made have been on the effects of temperature—an influence this fish appears to be peculiarly sensitive of, as indicated in all its habits, and in the circumstance that it is only found in those lakes, at least in the Lake District,* in which, in consequence of their great depth, it can find a retreat in summer and winter in water of about 40° Fahr. On the 28th of March I transferred into water, of the temperature 83°, a young charr that had been hatched not quite seven weeks. It rushed about for a second or two, then turned on its back and rose almost inanimate to the surface. The heart and gill-covers being still in motion, it was instantly put back to the water from which it had been taken of 52°. It made one or two efforts as if reviving, swimming for a few seconds in a natural position; but in less than a minute it was dead, the heart having ceased to act: thus, compared with the effects of a solution of common salt, offering a remarkable contrast. On the 29th of the same month, a young charr of about the same age as the preceding was put into water of 75°: it immediately became very restless, its gill-covers moving rapidly. After a quarter of an hour, when the temperature of the water had fallen to 70°, it lay still at the bottom, and not apparently distressed, except that the movement of the gill-covers and the action of the heart were unduly quick. In an hour and a half, when the water was 60°, it was still at rest: some hours later, when the water was 54°, it seemed well; and on the following day, put into fresh water, it appeared as active as before.

I have now to conclude. This I shall do without entering on the embryology of the charr, a vast subject, which, in the instance of one of the family of the Salmonidæ (*Coregonus palæa*), M. Vogt has so ably and elaborately treated of in the work already referred to.

* As before stated, the charr occurs in Ireland, in certain lakes which are comparatively shallow; that of Lough Oured in Connemara is an instance. Such as I have taken there were with the artificial fly. They were smaller than the Westmoreland charr, and though they cut very red when dressed, they were soft and of very inferior quality for the table. The trout of the same lake—the common trout—cut white.

The observations I have described are fewer than I could have wished, and the results more imperfect; I can offer them only in the manner in which I trust they will be received, viz., as a contribution to the history of the charr.

I may notice some of the facts which they seem to establish, and some of the inferences which they appear to me to warrant.

1. That the time required for hatching the ova of the charr is variable, depending on the degree of temperature of the water and other influences: that seventy days may be considered about the average, and forty and ninety about the extremes.

2. That after exclusion from the egg the young fish can live at least sixty days without taking food, deriving the material required for its support and growth from itself, and chiefly from the store that Nature has supplied in its yolk.

3. That under favorable circumstances, it attains its perfect form in about from sixty to seventy days, when it becomes dependent for its subsistence chiefly on food which it has to seek and to procure from without; though even then it is probable the whole of the yolk is not expended, so that external food failing, the privation can be borne and life maintained, and that for no inconsiderable time, by means of the residual yolk contained within the abdominal cavity.

4. That running water is not essential to the hatching of the ova; and, in consequence of its breeding-place being distinct from that of the trout, it is exposed to little risk of being lost as a species by repeated crossings with the trout.

5. That salt water, even of greater saltness than sea-water, is not immediately fatal to the embryo, even when not included in its shell; moreover, that in slightly brackish water a partial development of the ovum may take place; and that the young fish can exist some days in such water, rendering it probable that the adult may be capable of existing in a tidal stream, or even in the sea, for a time, where it is stated that the Welsh charr has been caught.*

6. That in water of small bulk, such as may be used for transporting fish from place to place, with common air, the young charr may endure confinement for several days without impairment of its vigor; and that, substituting oxygen, it may

* See Mr. Yarrell's History of British Fishes, vol. ii., p. 71. First edition.

endure such confinement for a much longer time, at least quadruple that period.

7. That the young fish can bear, without any immediate injury that is apparent, a temperature removed only a degree or two from the freezing-point of water; and also a higher temperature, ranging from 60° to 70°, but not above 83°, which, in the single instance tried, was almost instantly fatal to it.

The application of these facts to the breeding and transporting of the charr hardly requires any comment. Whilst they shew how easily it may be introduced into any lake or body of water, they are of no significancy in relation to the establishing it for a permanency in such water. What appears to be most requisite for the purpose is deep and pure water. In no body of water in the Lake District is the charr found, which is not of this character. The attempts to establish it in some not possessed of the qualities named, have repeatedly failed; and in others, in which the fish once abounded, it has become either entirely or almost extinct, since mines have been opened in their vicinity, by which the purity of the water, it may be inferred, has been impaired. Whether the quality of the food is of much importance, seems to be doubtful in relation to this its maintenance. There are circumstances that seem to warrant the conclusion, that, like the trout, its condition rather than its existence depends on the kind of food, and the quantity it can obtain. This we know, that it is taken with the same baits as the trout, and also that it exhibits varieties like the trout, though hardly so strongly marked, according to, as is believed, its manner of feeding; for instance, the charr of Hawswater, which is known to feed a good deal on insects, is a small and slender fish in comparison with the charr of Windermere, which feeds more at the bottom, and has a less precarious supply, especially of squillæ, which abound in that lake.* These remarks are offered with hesitation. The subject is one that is not without obscurity, and in need, for the better understanding of it, of further and minute inquiry specially directed to it.

P.S.—Reflecting on the effects of sea-water on the ova of the charr and its young, shortly after quitting the egg, as described

* The charr of the Lake District, though occasionally taken with the artificial fly and minnow, like the trout, on the whole, I believe, may be considered a more delicate feeder, and, in consequence, of superior quality for the table; its organization is in accordance with this, viz., its smaller teeth, and smaller stomach and intestines.

in this paper, I venture to offer the conjecture, that the action of sea-water may be similar on the impregnated egg of the salmon and its fry; and that it is on this account (looking to the final cause), rather than for the purpose of seeking water cooler and more aerated, that the salmon, impelled by instinct, quits the sea for the river, preparatory to breeding; and also, that the young remain in fresh water till they have acquired not only a certain size and strength, but also additional scales, fitting them, in their smolt stage, to endure without injury the contact of the saline medium.

I have had no opportunity to try the effect of sea or salt water on the impregnated ova of the salmon. The few experiments I have been able to make on the young fish have given results favorable to the above conjecture. I shall briefly relate them.

On the 10th of April, a young fish, about an inch in length, its permanent fins fully formed, taken from a small pool in the bed of the Leven (the river that flows out of Windermere, and then unusually low) was put into a half-pint of salt water, of the specific gravity 10,277. It lived about thirty-three minutes. Shortly after, a smolt the instant it was taken was put into the same water; it was about seven inches in length, and its head was not constantly under water. It lived about an hour. From comparative experiments with fresh water, I am led to infer that in the same limited quantity of river water it might have lived two hours, the limit being probably the exhaustion of the air. When a stronger solution of salt was used—that in the preceding experiments being nearly the same as sea-water—the effects were far more decided. Thus a fish of the same size as that first mentioned, put into a saturated solution of common salt, died in two minutes; and a parr taken on the 10th of October, measuring about four inches in length, put into a solution of common salt of the specific gravity 1,047, died in a few minutes.

XXVI. ON THE OVA OF THE SALMON, IN RELATION TO THE DISTRIBUTION OF SPECIES; IN A LETTER ADDRESSED TO CHARLES DARWIN, ESQ., M.A., V.P.R.S., ETC.

MY DEAR SIR,

In a letter with which you have favored me, that of the 28th of January, 1855, you did me the honor to ask my aid in an inquiry in which you take an interest, in common, as you remark, with most naturalists—viz., the geographical distribution of species, especially that of fish. At the same time you expressed your opinion that some useful information might be procured by experiments on the impregnated ova of the latter, were they so conducted as to shew what the ova are capable of bearing without loss of vitality, and under exposure to circumstances such as might be compatible with their being conveyed from one river or lake to another, adhering, for instance, to the plumage, beak, or legs of birds. In reply, I acquainted you of my willingness, should I have an opportunity, to accede to your wishes; and, that occurring, having been so fortunate as to procure the means of making some experiments likely to be elucidatory, I have now the pleasure of communicating the results obtained.

All the experiments I have to describe have been made on the ova of the salmon, for which I have been indebted to two gentlemen, John Barker, Esq., of Broughton Lodge in Cartmel, and William Ayrton, Esq., of Chester. By the first, through one of his keepers, I was supplied with a considerable quantity of ova, taken from a breeding-bed in the Leven, a river that flows out of Windermere, and from a part of it near Newby Bridge, about eighteen miles distant from my house. Through the latter I obtained ova from Overton on the Dee, taken from boxes in which they had been placed in the process, as it has been called, of artificial breeding.

Both gentlemen were so good as to desire the keepers, in packing the ova, to attend to the directions I gave in writing, with the intent of commencing the inquiry even in the act of their being sent. Those from the Leven were divided into three portions; one, of 110 ova, was contained in an eight-ounce vial, two-thirds full of water, which was changed more than once on the way; another, of seventy-five ova, was enclosed in wet wool; and the third, of sixty-two ova, in dry wool. The latter two were in a small box, the lid on, which box as also the bottle were carried by hand. These ova reached me in about twenty-four hours from the time they were taken from the river, and were received on the 6th of February. They all appeared healthy and in good progress of development, the eyes of the embryos being visible, and the blood-corpuscles distinct in the vessels of the vitelline membrane, when placed under the microscope, using a glass of one-inch focal distance. Without loss of time they were variously distributed; some in shallow earthenware pans, some in finger glasses used at table, and with water in all little more than sufficed to cover them. No gravel was added. The water employed was well-water of considerable purity, of about 50° Fahr., and was changed once daily, and once only. The vessels were kept in a room, the temperature of which seldom exceeded 50°, and was rarely below 46°. Most of these eggs proved productive, and have yielded young and vigorous fish. The first which broke their shell appeared on the 15th of February, the last on the 17th of March: of the total number not more than three or four aborted.

The ova from the Dee were received on the 7th of February, conveyed by rail, and had been sent off the preceding day. One portion of them was in a two-ounce vial, two-thirds full of water; another, in a vial of the same size, full of water; a third, in dry sand; a fourth in wet sand; a fifth in wet cotton wadding; and a sixth, in dry wadding: all enclosed in a covered box. These ova on arrival exhibited no signs of organic development. They were distributed immediately, much in the same manner as the preceding, and were treated in the same way, but with a different result. All of them in succession became opaque from imbibing water, and not in a single instance were there any indications afforded of vital progress; leading to the inference that they were dead when they reached me. From Mr. Ayrton I have recently been informed that the ova remaining in the box from which those had been taken were doing well; and hence, necessarily, the conclusion that the journey had been fatal to those I received. This may have been owing to their having been sent at so early a stage; and I may mention in confirmation, that a second supply which was forwarded to me later—three weeks later—sent by post in moist wool, in a more advanced stage, nearly as much advanced as those from the Leven, arrived alive and are now hatched. It may perhaps be said, that the treatment of the unsuccessful ova after I received them, especially as to the manner in which the water was supplied, was the cause of their failure: but this does not appear to me probable, having found it to succeed with the ova of the delicate charr—ova taken by myself from the parent-fish, and impregnated forthwith and immediately distributed in the same kind of vessels as those now used, and the water in which, of the same quality, was changed once only daily.

Having premised thus much, I shall now describe the several experiments which I have made for the purpose of testing the power of endurance of the ova. Unless otherwise specified, it is to be understood that the ova in each instance used were of those from the Leven.

1. *Of Exposure to the Atmosphere.*

1. An ovum exposed for an hour on a slip of glass to the air of a room at 64°, placed near a fire, became dry superficially without its circulation being stopped. Returned to water, its circulation was distinct at the end of forty-eight hours. Nine days after it was in a dying state.

2. An ovum on a slip of glass was exposed to the air of a room at 52° for two hours. The shell then had become at one spot indented as from shrinking, the effect of evaporation, yet the circulation seemed unimpaired; but transferred to water, the circulation presently stopped, the egg becoming opaque from the absorption of water, and of course dead.

3. An ovum exposed to the air of a room on a watch-glass from noon till 4 p.m., the thermometer rising from 49° to 51°, had become dry and shrivelled. From the state of the shell, the blood-vessels were indistinct under the microscope. Put into water, in one or two minutes a rupture of the shell took place and the young fish escaped. It was very languid, only the

slightest indications of life being perceptible; yet the heart did not cease its feeble action till the eighth day, counting from the rupture of the shell.

4. An ovum on a support of glass was exposed to the air of a room at 49° for an hour and ten minutes: its shell was slightly indented. Returned to water, on the second day the young fish burst its shell, was vigorous, and so continued.

5. An ovum was exposed to the air of a room during the night for about ten hours, the thermometer under 50°. The following morning it was found shrivelled; put into water, the shell presently burst; the young fish, excepting for a slight motion of its pectoral fins, appeared lifeless, and it soon died.

6. An ovum placed on a rock in the open air in the shade at 38°, after two hours was slightly shrivelled and its circulation had become languid. The following morning its circulation had ceased, and it shortly became opaque.

7. An ovum placed on snow during a thaw with occasional gentle rain, the air about 34°, and kept there from half-past nine in the morning till four in the afternoon, did not appear to be shrunk, nor was its circulation interrupted. Replaced in water, its circulation the following day was active.

2. *Of Exposure to Moist Air.*

To ascertain the effect of exposure to moist air, I have made many experiments, as by placing the wet ova in watch-glasses covered with other glasses of the same size; keeping them in moist wool, from which water had been wrung out; and in vials slightly wet within; in each instance taking the precaution to allow of the admission of air. The trials have been made at temperatures varying from 34° to 50°. The results have been so uniform that I do not think it necessary to enter into minute details. The ova in no instance appear to have materially suffered, whether the exposure has been for an hour or for several days. Thus, in one experiment, nine ova were kept in a vial, one of six-ounce capacity, eleven days; examined then under the microscope, the circulation in each of them appeared to be vigorous, as vigorous as before; and, replaced in water, they all produced healthy fish, and sooner on an average than those constantly kept in water. These ova were from the Leven. In another experiment, with ova last received from the Dee, four were kept in a vial of the same capacity and merely moist within, fourteen days without apparently suffering; they were all hatched on being replaced in water. And, in a third trial, two ova, also from the Dee, have been kept in moist wool twelve days, also without any appearance of injury, these too having been hatched after having been put into water.

3. *Of Exposure in Air and Water to a Temperature at or below the Freezing-point.*

1. An ovum exposed on a watch-glass to the open air from 4 p.m. one day to 10 a.m. the following, the thermometer at 30° at the commencement and termination of the trial, had become slightly shrivelled, and its circulation was stopped; put into water with snow, so as to be gradually thawed if frozen, it did not revive; its death was denoted by its yolk becoming opaque.

2. An ovum exposed to the open air at about 30° for an hour, was found adhering to the slip of glass on which it rested by a frozen drop of water, so that it could be carried inverted without falling off. Under the microscope, still attached to the glass by ice, the blood-corpuscles were seen in slow motion in the vessels; in one vessel they were moving backward and forward. Where adhering to the glass the ovum was slightly flattened. Removed to water, the following day the embryo was seen active and the circulation vigorous; thirteen days later the young fish burst its shell, and was to all appearance uninjured.

3. Another ovum, exposed to the open air of 29° for an hour and twenty minutes, was found frozen to the glass, but without loss of vitality. The result was the same as that of the preceding experiment.

4. An ovum exposed in water in a watch glass to the open air during the night, the thermometer so low as 20°, was found in the morning included in ice and dead; the yolk had become opaque, and was probably frozen.

5. Exposed an egg in a wine glass to the open air from 3 p.m. one day to 10 a.m. the next, the thermometer as low as 22°. The whole of the water was frozen; when thawed no circulation was visible in the ovum; two days after a feeble circulation was

detected, which ceased the following day, and the yolk became opaque.

6. An ovum exposed in water to the open air, about 31°, in an hour was covered with a pellicle of ice; the circulation had become languid. An hour and a half later, the thermometer at 30°, the ovum was included in ice; the circulation much the same. The experiment was continued about eighteen hours longer, the ovum included in ice at about the same temperature; the circulation was now languid but distinct, and the ovum was nowise altered in appearance.

7. An ovum was exposed in water in a wine glass to the open air below the freezing point. When a pellicle of ice had formed on the water, the glass was surrounded with wool in a little box, and left in the open air. During the night the thermometer fell to 9°. The ovum in the morning was found adhering to the bottom of the glass by ice, and the inside of the glass was coated with ice, the greater portion of the egg, however, remaining in water. The ice thawed, the circulation was seen going on, and it soon became active. Twelve days after, the young fish burst its shell, was, and has continued vigorous.

8. An ovum was exposed in water to the open air at 28°. In about two hours the water was frozen at the surface, and spicula of ice had formed round the ovum, as it were shooting from it. Thawed on the following day, the circulation was found to be vigorous, and in eleven days a young active fish was produced.

9. An ovum, one of those last received from the Dee, was exposed to the open air, placed on green moss, and left so exposed during three entire days and nights. It was then returned to water: in six hours after it was hatched. The young fish was languid, and in point of size comparatively diminutive, as if prematurely produced, yet the action of the heart was vigorous, and the circulation as seen under the microscope normal. It may be right to notice the kind of weather that prevailed during the exposure of the ovum. During the first twenty-four hours the thermometer by day was between 36° and 38°; there was some rain, ·39 inch was the quantity, and partial sunshine; during the night the thermometer on the grass fell so low as 29·5°; there was a little rain, ·02 inch. During the second twenty-four hours the thermometer by day varied from 39° to 33°; the air

most of the time was misty, but without rain; at night the thermometer fell to 28°, yet, as there was no frost in the morning, it was probably so low only for a short time. During the last twenty-four hours the state of atmosphere and the temperature differed but little from what they were in the preceding. Part of the time, especially during the latter third, the ovum was a good deal protected by the leaves of the moss, between which it had sunk. At the end of the three days it was neither dry nor shrivelled, and only very slightly indented, and that on the point on which it rested. It is worthy of remark, that it was the first hatched of the ova last received from the Dee; and that the young fish, now six days old, is alive and thriving.

4. *Of Exposure in Water to a Temperature of or above* 70°.

In these trials ova were employed and young fish, and chiefly the latter, as better adapted to shew the effect of the high temperature. In each instance the ovum or young fish was put into a thin glass vessel of the capacity of about four ounce measure, nearly full of water, and this vessel was placed in a water-bath of the temperature required. The temperature given in each following instance was that of the water in which the subject of the experiment was immersed.

1. An ovum kept two hours and a-half in water at 70°, placed under the microscope, was found to have its circulation somewhat impaired, rendered more languid; kept in two hours more, the temperature rising to 80°, no further injurious effect was produced, at least that was apparent. The vessel was now withdrawn from the bath and allowed to cool gradually. When next seen, ten hours later, a young fish had burst its shell and was vigorous.

2. An ovum and a young fish were kept in water between 68° and 72° about eight hours. The ovum, one of those from the Dee, was then found hatched, and the young fish produced was tolerably active. The following day both were exposed about nine hours to a temperature between 70° and 80°, rarely reaching 80°. At the end of this time they appeared languid, and when in motion disposed to irregular movements. Removed from the water-bath, on the following day they were active, and exhibited no peculiarity appreciable that could be attributed to

the higher temperature to which they had been subjected. The ovum in its hatching in this instance preceded all the others from the Dee, with the exception of the one already mentioned —that exposed three days to the open air.

3. A young fish and an ovum were put into water which in the bath presently acquired the temperature of 82°, and in an hour rose to 85°. Now taken out and allowed to cool gradually, the circulation in the young fish was found to be very languid, the heart contracting feebly. The following day it was found dead. The ovum did not appear to suffer materially; three days after it was hatched, and a vigorous young fish was produced.

4. An ovum kept in water for two hours, at a temperature from 90° to 95°, lost its translucency, and opened under water was found to be dead.

5. An ovum, one of the last from the Dee, kept half an hour in water at 100°, afforded the same result.

6. A young fish was kept in water three hours, the temperature of which at the commencement was 70°; it rose to 85°, and when taken from the bath it had fallen to 82°. The heart then was acting with tolerable vigor, and the day following the fish appeared to be nearly in its usual state: five days later it was alive and tolerably active, but less vigorous than those which had not been so exposed.

7. A young fish kept in water an hour at 84° was found dead. No action of the heart was perceptible nor of any of the muscles when it was taken out. Another young fish was put into the water when cold without experiencing any bad effect. This trial was made to be certain that the fatal effect was not owing to want of air in the water.

8. A young fish was kept in water rising in temperature from 78° to 81° three hours and a-half without any permanent bad effect that was appreciable. When taken out it appeared torpid, but the heart was acting well. Two days after the fish was as active as before.

9. A young fish, kept two hours in water between 88° and 90°, was, when taken out, dead.

10. A young fish, kept only a few minutes in water at 92°, appeared to be dying when taken out; the circulation in its tail was stopped and the heart was acting feebly; in about a quarter

of an hour it ceased to act. The following morning the fish had a sodden appearance, and its disintegration had commenced.

11. A young fish was put into water at 80°; after three hours, when the temperature had risen to 85°, it appeared to be dead; its body was bent and it had become pallid. Under the microscope the heart was seen acting feebly, and the circulation was proportionably languid. On the following day the body had become unbent; the circulation in the tail had ceased, but the heart was still acting feebly. Two days later the heart's action had ceased, and the only vestige of life was indicated by a just perceptible motion of the lower jaw, which was protracted three days longer.

12. A young fish was kept in water gradually rising from 78° to 88°, for three hours. At 85°, the heart acting, no circulation was perceptible in the tail; at 88° the body had become bent and pale, and the heart's action arrested.

5. *Of the Effect of Salt and Brackish Water.*

1. An active young fish, and an ovum in which the circulation was vigorous, were put into a solution of common salt of the specific gravity 1,026, which it may be conjectured is nearly the degree of saltness of the sea at the estuaries of our salmon rivers. The fish immediately became restless, and the heart's action accelerated. At the end of five hours it appeared to be dying; the heart's action had become so languid as not to suffice for the circulation; notwithstanding, life was not entirely extinct, as was indicated by a feeble motion of the lower jaw, till about forty-eight hours from the commencement of the experiment. The dead fish was colorless and contracted in all its dimensions, and shortened at least one-third of its length.

The effect of the salt water on the ovum was equally fatal, but, judging from the circulation, life was protracted in it a few hours longer.

2. An active young fish was put into a solution of common salt of the specific gravity 1,016. It lived about four days, the heart's action gradually becoming feebler till the circulation ceased. When dead there was an accumulation of blood in the large vessels, and, as in the former instance, a diminution of the bulk of the fish, as if from contraction. The saline solution, it

may be remarked, was changed daily, so as to be sure that death was not owing to, nor had been hastened by, deficiency of air.

3. A young fish was put into a solution of salt reduced to the specific gravity, 1,007, so as to be only slightly brackish. Immediately on immersion it shewed great restlessness and increased activity, which continued with little abatement for several days. It has now been in the solution ten days. During the two last its activity has diminished, and at times it has appeared to be dying. It is rather more changed in form than the fish of the same age left in spring water, and the vitelline sack is decidedly more diminished, as if from increased vascular action produced by the stimulus imparted by the solution.

4. An ovum from the Dee, the circulation in which was active, was put into saline water of the same specific gravity as the last. It was hatched at the end of about forty-eight hours. The young fish was at first languid; now, on the fourth day, it is little altered; it is seen commonly lying on its side, and is restless only by fits and starts.

Besides the experiments above detailed, I have made others, but differing so little in their results, that I do not think it necessary to describe them even in confirmation.

6. *Concluding Remarks.*

On the conclusions which may be drawn from the experiments as bearing on the subject under consideration, I shall be very brief; for the sake of order I shall advert to each section.

From the experiments detailed in the first section, it would appear that the ova of the salmon in an advanced stage can be exposed to the open air, if dry, but a short time, at ordinary temperatures, without loss of vitality; but for a considerable time, if the temperature be low and if the air be moist; the limit in the former case not having exceeded an hour, whilst in the latter it has exceeded many hours.

From the experiments in the second section, it would appear that the vitality of the ova was as well preserved in air saturated with moisture, as it would have been had they been in water.

From the experiments in the third section, it would appear that the ova might be included in ice without losing their vitality; but that if exposed to a temperature many degrees below the freezing-point, probably effecting their congelation, they were deprived of their vitality.

From the experiments in the fourth section, it would appear that both the ova and the young fish were capable of bearing a temperature of about 80° or 82° in water for a moderate time with impunity, but not without loss of life at a higher temperature, any exceeding 84° or 85°.

From the experiments in the fifth section, it would appear that a degree of saltness of water equal, or nearly equal, to that of sea-water, is pretty speedily fatal both to the ovum of the salmon and to the young fish; that the same effect is produced on the young fish by brackish water of specific gravity 1,016, but in a longer time; and that, when the solution is so diluted as to be reduced to the specific gravity 1,007, the advanced ovum may be hatched in it, and the life of the young fish may be sustained in it for many days, but with diminishing power.*

Finally, in reference to the distribution of species, do not many of the preceding results render it probable in the instances of fish of the salmon-kind, and by analogy of other kinds, that it may be effected in the manner you have suggested in proposing the inquiry, viz., by means of impregnated ova conveyed by animals, whether birds or quadrupeds, adhering to some part of their body, such as their feathers or hair, feet or

* In confirmation of the above, I may mention the result of an experiment on the ova of the salmon made in the island of Lewis by the direction of Sir James Mathason, in the winter of 1860-61, of which he has been so good as to send me an aceonnt. Two portions of impregnated ova were used, one for trial in brackish water of specific gravity 1,015, the other in fresh water. They were held on a wire cloth in a glass vase with a tap at its bottom, and the water was changed daily. During the first ten days, the ova in the brackish water did not appear to snffer, and no longer; no fœtal development was observed in them, and they all died, whilst those in the fresh water made progress, and in dnc time were hatched. It is asserted that salmon are known to spawn within the reach of the tide in tidal rivers, tending to prove that salt water is not fatal to their ova. In the isle of Lewis at the month of the Greamster, a salmon river, there is a spawning bed eorresponding to this deseription, where it is pretty well aseertained that ova are laid and hatched. But what are the eircnmstances? I am informed that the spot is eovered with "brackish water" only for about two hours at eaeh high tide, and that it is never reached by "braekish water" during the week of neap tides; moreover, and this is very important, that what is called "brackish water," is so dilute as to differ but little from fresh water in specific gravity, the tide serving as a dam to the river water and by obstructing its free outflow, oeeasioning its aeeumulation and overflow. I have witnessed the same phenomenon at Gairloch in Ross-shire, whete, close to the inn, a small stream enters the sea, and at high tide overflows its banks, the water there, as well as I eould judge from its taste, remaining fresh, and its inmates, the common brook trout, not forsaking it.

mouth—by the latter provided the temperature do not exceed 84° or 85°? And, during rain or snow, are we not warranted in concluding that an ovum may be taken from one river to another without loss of vitality by an otter, or heron or other aquatic bird, if lodged in the mouth of the one—with the proviso mentioned above—or in the bill of the other; or during a time of frost or snow if adhering to the feet of either of the animals mentioned?

When my attention was first given to the subject, which was before I was favored with your letter, I imagined that the impregnated ova might be conveyed in the stomach of birds, taken up from one river, and, it might be, disgorged in another, without loss of vitality, inasmuch as the ova of the salmon found in the stomach of a trout have been known to be productive when returned to water. For an authenticated instance of the kind, I may refer to a report by Mr. Halliday, the agent of Messrs. Edmund and Thomas Ashworth, on the artificial process of breeding salmon carried on at Oughterard in Galway.* It was to test this conjecture that the experiments in the fourth section were made; and, I may add, with negative results, knowing, as we do, that the temperature of the stomach of birds is usually above 100° of Fahr.

Besides the main and express object for which the preceding experiments were made, I trust the results may be of some use in aiding to solve the question as to the period, the age, at which the impregnated ova of fish are most retentive of life, and, consequently, are in the state best fitted for transport without loss of life; and that those in the two last sections may help to explain the absence of the Salmonidæ in tropical seas and in those approaching to them in temperature, such as the Mediterranean; and may also throw a little light on some of the peculiar habits as well as on the localities of their migratory species.

I am, my dear Sir,

Yours very truly,

JOHN DAVY.

* The report is attached to "A Treatise on the Propagation of Salmon and other Fish," by Edmund and Thomas Ashworth: London, Simpkin and Marshall, 1853.

XXVII. ON THE VITALITY OF THE OVA OF THE SALMONIDÆ OF DIFFERENT AGES; IN A LETTER ADDRESSED TO CHARLES DARWIN, ESQ., V.P.R.S., ETC.

MY DEAR SIR,

In a letter which I had the honor to address to you last year, "On the Ova of the Salmon in Relation to the Distribution of Species," I have expressed the hope that some of the results of the observations therein described may aid in solving the question as to the period, the age, at which the impregnated ova of the fish are most retentive of life, and consequently are in the state best fitted for transport without loss of life.

Joining with you in considering the subject in need of and deserving further inquiry, I have taken the earliest opportunity that has offered of resuming it. The experiments which I have made, and which I shall now describe, have been more limited than I could have wished, having been confined to the ova of the charr, as I was not able to obtain the ova of the salmon or any of its congeners in a state fit for the trials required.

The ova of the charr which have been the subject of my experiments, were from living fish, brought to me on the 9th November, from the river Brathay, a tributary of Windermere. They were obtained by the pressure of the hand on the abdomen of the females under water, and immediately after their expulsion a portion of liquid milt, procured in the same way from a male, was mixed with them for the purpose of impregnation.

The ova thus treated, 654 in number, procured from two fish, were transferred, after little more than an hour, to water in a shallow glazed earthenware pan, of a circular form, about a foot in diameter, without gravel. The vessel was kept in a room of a temperature fluctuating from 55° Fahr., when highest, to about 40° when lowest. The water used was well-water of considerable purity, and before used it was allowed to acquire the temperature of the room; it was changed daily once, and never more frequently.

Two modes occurred to me as likely to afford the means of testing the vital power of the ova, or their power of endurance without loss of vitality: viz., one by subjecting them for a limited time to a temperature above the ordinary temperature; the other by having them conveyed to a considerable distance.

For the trials first proposed, the ova were put into a thin glass vessel half full of water, which was placed in a water-bath and heated to the temperature desired.

The first experiment was made on ova taken from the general stock one day after their expulsion. Six, for two hours, were exposed to a temperature varying from 79° to 80° of Fahr. The result was that they became opaque in the course of twenty-four hours, all but one, and that, some days after, underwent the same change, denoting loss of vitality.

The second experiment was made on the 10th of November. Six ova were similarly exposed for two hours to a temperature rising gradually from 70° to 78°. The result was nearly similar: on the following day they were all found opaque.

The third experiment was made on the 11th of November. The same number of eggs were exposed for an hour to a temperature falling from 70° to 69°. Two shortly became opaque. Four retained their transparency during a month, though in reality dead, which was denoted by their bearing no marks of development when seen under the microscope, those ova which retained their vitality being then well advanced.

The fourth experiment was made on the 1st of December. The ova, the same number, were exposed to a temperature rising from 75° to 78° for an hour and twenty-two minutes. Three became opaque; other three retained their transparency and vitality and in due time were hatched—the first on the 31st of December, the last on the 7th of January.

The fifth experiment was made on the 13th of December. Six ova were exposed for an hour and twenty-five minutes to a temperature falling from 82°, which it was at the beginning, to 78°, which it was at the end. Two became opaque. In these no marks of progress could be seen of development; thus indicating that they were dead at the time of trial. Four remained transparent. In these, under the microscope, embryo-fish were seen with an active circulation of the blood-corpuscles. One of them was hatched on the 31st of December; one, the last, on the 6th of January.

The sixth experiment was made on the 20th of December, on six ova containing living embryos. They were exposed for an hour and twenty minutes to a temperature of about 98°, and this during the whole time. When taken out they had not lost their transparency, but in each the heart's action was arrested, and death was the result: they all sooner or later became opaque, from the common cause, the imbibition of water.

The seventh experiment was made on the 21st of December, on six ova, in which the circulation was distinct in the fœtal fish. After an exposure for an hour and five minutes to a temperature of 70°, rising to 82°, on cooling the circulation was found active in five; in one stopped—this was dead. Two were hatched on the 5th of January; three, the remainder, on the 7th of the same month.

The eighth experiment was made on the 23rd of December, on six ova, each containing a living fœtus. They were exposed to a temperature falling from 84° to 82°, during an hour and twenty minutes. Examined after the water had cooled, in one the circulation was seen pretty distinct; in two very feeble; in three the blood-corpuscles appeared to be stagnant. Examined the following day, the circulation was active in all. One was hatched on the 5th of January, the other five in the two following days.

The ninth experiment was made on the 24th of December. Six ova were exposed for two hours and four minutes to a temperature falling from 72° to 70°. Examined in a quarter of an hour, before the water was cold, the circulation was found vigorous in all. One was hatched on the 2nd of January, the remainder between the 5th and 8th.

The tenth experiment, and the last of its kind that I have to describe, was made on the 2nd of January. Six ova, in each of which the circulation was distinct, were exposed for four hours to a temperature varying from 70° to 72°—the greater part of the time 72°. Examined immediately after being taken out, the circulation was seen uninterrupted in three, arrested in the other three. In three-quarters of an hour, when the water had cooled nearly to the temperature of the room, 55°, the circulation was found to be renewed in the latter. In the interval one of the former was hatched, and a vigorous fish produced. On the following morning four more had come forth, and in the one remaining egg the fœtal circulation was vigorous: it was hatched on the 4th of January.

I beg now to pass to the other series of experiments referred to: those in which the trial of the vitality of the ova was made by sending them to a distance. The method was briefly the following:—The ova were lightly packed in wet wool, contained in a tin-plate box perforated at its bottom to admit air, and covered with a wooden cover that had been soaked in water, with the intent of preserving moisture. The box was wrapped in tow, loosely covered with oiled paper, and the whole, in an envelope of common writing-paper, was well secured by a binding of thread. Thus prepared the ova were sent by post to Penzance in Cornwall, a distance exceeding 500 miles, with a request which had preceded them, that they should be sent back by return of post unopened.

The first experiment was made on the 9th of November. The number of ova was thirty, taken from the common stock without selection. They were received on their return on the 14th of the same month. On taking them out, all were found transparent; but, with the exception of one, all became opaque on being put into water, and that one, after a few days, also underwent the same change.

The second experiment was made on the 14th of November. Twenty ova then sent were returned on the 18th. All became opaque on being put into water.

The third experiment was made on the 1st of December. Twenty ova sent then were returned on the 5th. Put into water, eleven became opaque within a minute; most of these were slightly shrivelled. After three hours two more became opaque; after forty-eight hours four only remained transparent. In these, under the microscope, the circulation was found active in two; in the other two it could not be detected. Of the former, one was hatched on the 31st of December; the other died before hatching.

The fourth experiment was made on the 13th of December. Twenty-two ova then sent came back on the 17th. During the interval there was a severe frost; the thermometer here in the open air was constantly below the freezing point, and it would appear to have been much the same throughout England. When examined, eleven of the ova immediately became opaque on immersion in water. In the other eleven there was no loss of transparency, and in these, under the microscope, the circulation was found active. Those which had become opaque were placed in a pretty strong solution of common salt, by which their transparency was restored, the saline solution dissolving the coagulum. Now examined, no traces of development could be detected under the microscope in any one of them, shewing that they had been dead before they were sent away.

On the following day, the 18th of December, the eleven transparent ova were re-packed, and again sent the same distance. They came back on the 22nd. They retained their transparency. Placed in water, a feeble circulation was to be seen in two; in nine the blood corpuscles had ceased to flow: these became opaque. Of the two in which the circulation was visible under the microscope, one was hatched on the 28th of December. In the other it would appear that the young fish died in the act of breaking the membrane, its head, on the 29th, having been found protruding, but the heart's action stopped.

The fifth experiment was made on the 26th of December. Ten ova, in which the circulation was active, and the fœtus in each well advanced, were sent off on the day mentioned, and returned on the 31st. The weather during the whole time was mild, the frost having ceased. When opened, the ova were all found hatched, and the young fish dead, as might have been expected. When put into water, not one of them shewed any signs of remaining vitality: they were all examined under the microscope.

The sixth and last experiment was made on the 6th of January. Six ova, in each of which the circulation was vigorous, were put into a glass tube of one cubic inch and a half capacity, with water to the height of about 1·4 cubic inch, the remaining space, after closure by a cork, being filled with air. The intention was to try the effects of conveyance to a distance on these ova in water with a small quantity of air. Owing to a mistake, they were not forwarded. Examined on the following day, five ova were found hatched, the young fish dead. In the one ovum remaining unhatched, the fœtus was alive, the circulation active. On the 9th it burst its shell; the young fish was vigorous.

As I could not with any certainty determine, at the time the experiments were commenced, what eggs were impregnated and alive, and what were not, I had at the beginning thirty ova taken indiscriminately from the common stock, and put apart in a glass vessel, the water in which was also changed daily. Of this number, seven were found in progress of development on the 14th of December, or 23 per cent.; the rest had become opaque. One of the seven was hatched on the 31st of December, the others in succession; the last on the 8th of January.

Further, to arrive at a proximate average of the proportion of impregnated and unimpregnated ova, or living and dead, on the 14th of December, when in the living ova the circulation was distinct under the microscope, and the embryos were visible even to the unaided eye, I examined the whole number then remaining, viz., 405, thus reduced, 67 having been removed one after another, on account of having become opaque, and 152 having been taken out for the purpose of experiments. Of these 405 remaining, 138 were found alive, each containing a well formed embryo, and 267, though still transparent, without life, no marks of organization being to be seen in them, either with the naked eye or under the microscope. Hence, irrespective of the 152 experimented on, the proportion of living to dead on the 14th of December would appear to be as 138 to 364, or about 25 per cent. And with the exception of two, which died after the 14th, all those then alive were hatched, the first on the 31st of the same month, the last on the 9th of January.

What are the conclusions to be drawn from these results? From those of the first series of experiments, may it not be considered as proved that the power of resisting an undue increase of temperature is possessed in a higher degree by the ova in an advanced, than in an early stage of development, the degree probably being in the ratio of the age? From those of the second series, is it not as manifest that the power of bearing distant transport, and of retaining life in moist air, is in like degree increasing with age? And from both may not the general conclusion be drawn, that the strength of vitality of the impregnated ovum, or its power of resisting agencies unfavorable to its life, gradually increases with age and the progress of fœtal development? And, as the charr is one of the most delicate of the family of fishes to which it belongs, may it not further be inferred, with tolerable confidence, that the ova of the other and more hardy species of the Salmonidæ, were they similarly experimented upon, would afford like results, confirmatory of those obtained last year in some trials on the ova of the salmon, and mentioned in my former letter to you.

The practical application of these results and of the conclusions deducible from them, is obvious, and need not at present be dwelt on.

I am, my dear Sir,
Yours very truly,
JOHN DAVY.

XXVIII. MISCELLANEOUS OBSERVATIONS ON THE SALMONIDÆ.

1. *Of the Air-bladder of the Salmonidæ and its contained Air.*

I HAVE examined this organ and the air contained in it in the salmon, white trout (*S. trutta*), charr, and common trout. In each instance I have found it very similar: opening into the œsophagus by a minute aperture,* of a cylindrical form, tapering at each extremity, composed of a transparent membrane, very faintly and partially vascular, extending the whole length of the abdomen, and, with one exception, without color. The exception is that of the charr, in which, when the fish are strongly colored red (their muscular substance), it, the air-bladder, is with few exceptions of a rose hue.

The proportional size of the organ, or the quantity of air distending it, on which its size no doubt very much depends, varies, it would appear, in different fish of the same species, and still more in those of different species. Commonly, I believe, the bladder of the migratory kinds, as the salmon and white trout, is smaller or less distended than that of the common trout or charr. In a fresh-run salmon of about 12lbs., taken with the net in the first week in August at Ballyshannon, opened half an hour after its capture, I found in its bladder only about a quarter of a cubic inch of air. And one of the men, an intelligent person belonging to the fishing establishment of that place, who, this last season, had, he said, opened about two hundred

* The angler is sometimes startled in handling the trout he has taken to hear a sound from it, as it were an utterance of pain. It is occasioned by air passing rapidly through the œsophagial aperture, owing to the pressure made by the hand on the abdomen. As the air-bladder is considered a rudimentary lung, so the aperture may be viewed as a rudimentary glottis. It has, however, no rudimentary epiglottis, or valvular structure. Its structure is such as to allow air to enter as well as to be expelled through it. I am disposed to infer that the air of the air-bladder is in part, if not altogether, obtained from the atmosphere. If we suppose that the "leaping fish" takes air into its mouth, and makes an effort to swallow, the air may be forced into the air-bladder, through the aperture in question. This remark of course does not apply to the air-bladder of certain fishes which has no communication with the œsophagus, and is perfectly closed; the air of which is evidently a secretion, derived from the blood.

fresh-run fish, varying in weight from about 8lbs. to 28lbs., assured me that, as well as he could remember, the quantity of air was "oftener under than over half a wine glass full."

For the purpose of chemical examination, of course, it was necessary to open the fish under water. The contained air, extricated on puncturing the bladder, was collected in a graduated tube. For carbonic acid, it was tested by agitation with lime-water with excess of lime. The proportion of this acid gas was in every instance small, barely a trace. For oxygen, the test used was a stick of phosphorus, left exposed to the action of the air some time after it had ceased to fume, or the air to suffer diminution. In the instance of the salmon mentioned, the diminution from the action of the phosphorus was hardly appreciable. The same remark applies to the air of the bladder of the white trout: it was tried in two instances—fish of about half a pound caught in the Claudy river in Donegal. In the trout, river trout, fish of about a quarter of a pound (two were examined), the proportion of oxygen was greater; it amounted to about 10 per cent. of the whole volume of air.

As these trials were mostly conducted when on fishing excursions, and under circumstances nowise favorable for minuteness of research, the experiments I have made on the air were chiefly limited to those above described, which sufficed to convince me that the air of the air-bladder was principally azote, and to allow the inference that the trace of carbonic acid was most likely rather accidental than essential, owing probably to the secondary action of the minute proportion of oxygen present on the organ itself.

These results, I may remark, accord with those obtained by former inquirers on the air of the air-bladder of several other fresh-water fish, whilst they differ so greatly from others—those afforded in like trials on deep-sea fish—the air of the air-bladder of which was found to be principally oxygen.

That the same organ should secrete two gases so very different in their nature, appears anomalous, and deserving of further inquiry. Indeed, does not the entire subject need more minute inquiry? At present the facts relating to it are few, and seem far from adequate to allow of any satisfactory conclusions being drawn as to the use of the bladder and its secretion in the animal economy, except of a mechanical kind, as affecting the specific gravity of the fish. Were the gas uniformly of one kind, were it constantly azote, it might be easy to assign it a plausible end; the function of the air-bladder might be inferred to be auxiliary to that of the kidneys. The secretion of oxygen is the anomalous fact, so contrary is it to the ordinary course of changes in living animals, in which the general tendency is to the consumption of oxygen. *A priori,* one might almost as much expect oxygen to be exhaled from the lungs in respiration, as to be separated from the blood by secretion by the air-bladder; and, had we not the authority of so accurate an observer as M. Biot, we might be led to suspect that the statement of its being so was founded in error.

2. *Of the Breeding Localities of the Salmonidæ.*

It is commonly believed that the several species of the genus, at least the more distinguished, such as the salmon, white trout, bull trout, common trout, and charr, require their ova to be exposed to the action of running water for their fecundation and hatching; that for this purpose, the migratory kinds quit the sea for the river, the lake fish the still water for the streams, and the river fish their ordinary places of abode for the smaller tributaries.

From such information as I have been able to collect, I am led to infer that, though this belief is commonly well founded, there are exceptions; and that the conditions to successful breeding are not quite so restricted as has been generally supposed.

In a former paper, that on the generation and early stage of life of the charr, proof was adduced that this fish more commonly avoids than seeks running water for the purpose of breeding, and that the gravelly and rocky shoals of the lakes it inhabits are its favorite breeding localities, rather than the bed of a river or brook, where the water is in rapid motion.

In artificial breeding, not only have the ova of the charr, but also of the common trout and salmon, been successfully hatched without the use of running water, merely by changing the water daily. And in accordance with this, I have been well assured of similar instances in nature, that is, of the ova of the trout and of the salmon having been laid, like those of the charr, on beds of gravel in lakes, and where it is believed they have been hatched. I shall mention the few instances which I believe are worthy of credit. In Connemara, County Galway, Ireland, there is a lake, about five miles from Clifden, called Lough Anaspick (the Lake of Contention), abounding in good trout, and which, from its situation in a flat part of the country, is fed more by the rain that falls into it than by the stream which enters it—a stream so small as to be unfit for a breeding-place—the same remark applying to the little outflowing stream. On a gravelly shoal of this lake I have been assured by fishermen residing in the neighbourhood, that the trout deposit their spawn, that they have been seen in the act, and that the roe has been found there.

In Blea-tarn, in our Lake District, observations of the same kind have been made. I have been informed that the roe of the trout has been detected in plenty near the shore. The person from whom the information was obtained remarked that such a laying of the spawn was unavoidable, inasmuch as, from obstructing obstacles, the fish could neither run up nor down the inflowing or outflowing stream. In this instance it was stated, that the favorite place of spawning was near to the fall of the little stream into the tarn, its principal feeder.

At Lough Melvin, in Ireland, a lake in which the gillaroo trout is plentiful, I learned, whilst there, that this trout never enters the tributaries with the other, the common trout, in the spawning season, and that it had never been seen in them; from whence the fishermen, my informants, who watched the streams, inferred, with confidence, that it bred exclusively in the lake. But though confident of this, they had never, they said, found the ova—which, indeed, is not surprising, considering the great extent of the lake, and that they had never made search for them.

The only instance I can mention as appearing to be well authenticated, of a salmon spawning in a lake, I learned from an old Irish fisherman residing in the neighbourhood of Lough Erne. He assured me that he had seen a pair of salmon preparing their spawning-bed in a shoal of the lake off an islet known by the name of Rabbit Island.

If what I have stated should be received as satisfactory, I hope it may prove not without use, as tending to shew that the Salmonidæ may be bred in lakes and ponds, and thus encourage attention to these as breeding-places: for instance, in ponds, by providing beds of gravel, and in lakes, where fish are known to spawn, by affording protection from the depredation of the poacher, which, in the instance of the charr, we know from experience to be much needed in some of our Westmoreland lakes.*

3. *Of the Variable Time of the Hatching of the Ova.*

In a paper already referred to, that "On the Impregnation of the Ova of the Salmonidæ," mention is made of the successful result of impregnation in the instance of the mixing together of the roe and liquid milt of the charr obtained from the living fish.

The roe, after having been thus brought into contact with the milt, was divided into three portions. One, the largest, was placed in an earthenware pan, about two feet in diameter, in which was a stratum of gravel taken from an adjoining brook, and water, soft spring water, to the depth of about three inches, which for the most part, about two-thirds, was changed daily. The ova were laid on the gravel, and the vessel was placed on the floor of a room without a fire, where the temperature was liable to little fluctuation.

Smaller portions were put into two water or finger glasses, such as are used at table, about four inches in diameter, in which also gravel of the same kind was laid, with water from the same spring to the depth of about two inches, and which, as in the first mentioned, was in great part changed daily. Both glasses were placed on a stand about three feet from the ground, and within a few feet of the pan, and all three were exposed to about the same degree of light.

The experiments were commenced on the 25th of November. On the 8th of January, in each of the glasses, three young fish were found in the morning at large, excluded during the preceding night. On the 9th, more had made their appearance in the upper vessels, but none in the one below. The temperature

* Shallow water appears to be essential to the hatching of the ova of fish generally, and especially of the Salmonidæ; and may it not be because shallow water, from exposure to the atmosphere and the action of the winds, must be better aerated than deep water. A depth of four or five inches seems most suitable to the successful hatching of the ova of all the Salmonidæ.

of the water in the glasses was 50° Fahr., that of the water in the pan on the floor being 48°. On the 10th, many more young fish were produced in the glasses, but one ovum only was found hatched in the pan. On the 13th, whilst no more had appeared in the lower, all but one egg were hatched in the upper, and that one proved to be dead. On the 20th, and not till then, another egg, the second, was hatched in the pan. On the 22nd, it was noted that many more were hatched during the twenty-four hours, the temperature of the water being about 52°. From the 22nd to the 31st, the hatching process continued, a few young fish appearing daily. Those last produced, it was observed, were smaller and feebler than the first, and died in larger proportion.

It is worthy of note—such was the remark made at the time—how, under the same circumstances, as in the water-glasses, or under slightly different circumstances, as in the earthenware vessel on the floor, ova from the same fish, impregnated with the same milt, at the same time, differ so much as to the time of being hatched, and the size of the fœtal fish, and their vitality both before and after exclusion, some embryos dying in the egg, some in an advanced stage in the act of extricating themselves, other young fish at intervals of hours or days.

Now, what was witnessed in these experiments it can hardly be doubted occurs in the natural process of hatching, in which the circumstances commonly must be very much more varied, and the results, it may be presumed, equally so, and not without advantage as regards the preservation of the species.

4. *Of the Circumstances and Agencies likely to Exercise an Influence on the Young Fish.*

In the paper before referred to, "Observations on the Charr," a section was given to the subject above named. The inquiry was, as it appeared to me, an interesting one; and it was my intention to prosecute it farther, but hitherto I have done little for want of opportunity, and that is comprised in two trials, one on keeping the young fish excluded from light; the other on placing them in a very small quantity of water, barely sufficient to cover them.

The first trial was commenced on the 31st of January. On that day one of the water-glasses, with its brood of young charr, was placed in a dark cupboard, from which light was entirely excluded. Here they were kept till the first of April; during which time the vessel was taken out only once daily, for the purpose of changing the water and giving food, which occupied no more than a minute or two. Now, comparing them with the brood in the other water-glass, which had been daily exposed to the light, I could perceive no well-marked difference in the appearance of the fish as to form, color, progress of growth, or activity. The only difference noticeable was, that those kept in the dark were much shyer than those exposed to light, which was indicated, on their being brought to light, by their endeavouring to hide themselves; this they did by thrusting the head under the gravel.

The other trial was made on the 13th and 17th February, with healthy young charr, hatched between the 8th and 10th January, and on the 10th March on a young salmon hatched about the 26th February. On the first-mentioned day a young charr was put into a platina capsule about 1¾ inch across its brim, with just sufficient water to cover it. It remained active fifty-two hours; and it may have been active longer, as it was found dead in the morning, the night hours not being included in the time specified. In the second experiment, the young fish similarly treated lived rather more than seventy-four hours. The capsule, it may be mentioned, was covered with a slide of glass, to prevent rapid evaporation and yet not exclude air. In the third experiment, the young salmon was put into a small liquor-glass, with a little water, little more than sufficed to cover it and enable it to move freely, in weight no more than 47 grs. Left till the 22nd; during these nine days it did not appear to have suffered from being so limited and confined. Returned on this day to the vessel from which it had been taken, it seemed unimpaired in vigor and activity. The temperature of the water, in each instance, did not exceed 52° Fahr., and was never below 48°.

These experiments, I need hardly point out, were made with the intent of testing the power of endurance of young fish during periods of drought, when they are liable, from the lowering of the streams, to be left almost dry.

The result of the last experiment, I may remark, was somewhat different from what I had expected, supposing, as I previously did, that the air necessary for supporting life would soon be exhausted in so small a quantity of water; not taking into account that the motion of the fish (and it was very restless) might promote the absorption of air; and that even the small volume of water, so fully exposed and almost constantly agitated, would conduce to the same.

5. *Of the Food of the Young Fish.*

In consequence of the attention that is now being paid to the artificial process, as it has been called, of breeding fish, the question, what kind of food is most suitable to the young fish when it has to provide for itself, after the consumption of the store laid up in the vitelline sack, becomes one of considerable interest.

My own experience as regards the attempt to feed the young fish is very limited. The first trial I made was with finely-grated boiled beef; the second with dried charr—the muscular part dried rapidly and pounded very finely. A small portion of each was given daily to a different brood. The young fish fed on the particles greedily, seizing them chiefly when in motion. Those which had the charr seemed to thrive somewhat better than those fed on beef; more of the former attaining their perfect shape, as denoted by the development of the fins, than of the latter. In a third trial no food was given. This was in the instance of the brood hatched in the larger vessel, the earthenware pan, in which was a considerable quantity of gravel and small stones, with the addition of some aquatic plants from the river Brathay—that part of it which is one of the breeding-places of the charr. In this instance the young fish, after the disappearance of the yolk, throve well, and, as I believe, chiefly on the infusoria present. This inference was made partly in consequence of finding, on microscopic examination, infusoria on the plants and stones and sides of the vessel, and vestiges of them in the excrements of the young fish, and in the intestine of some that were opened, and partly from observing how the young fish, when in pursuit, as it was supposed, of food, seemed to confine themselves to the spots where the infusoria were in greatest numbers. Another reason (were we to reason *a priori* on the subject) might be assigned, viz., the presumed fitness of these microscopic animalcules for the food of creatures of so small a size as the young of the Salmonidæ in their early stage, and the fitness of the latter, with their microscopic eyes, to see and make the infusoria their prey.

The fish, the subjects of these trials, were all young charr. Those from the eggs impregnated on the 25th of November were so advanced by the second week in April as to be considered fit to be set at liberty. Some were taken to a lake in the Highlands of Perthshire; others to a mountain tarn in this neighbourhood. Both sets were from the earthenware pan, to which no food had been given.

I may offer as a suggestion, that where minnows abound, their eggs and young, it is probable, may be employed as a useful aid for the support of the young of the Salmonidæ. The time of breeding observed by the minnow, early in May, seems suitable, and especially so the minuteness of the ova of this little fish, and of their fry when hatched. Their eggs at maturity I have found ·06 of an inch in diameter; the fœtal fish on quitting the egg was about ·20 of an inch in length, and no more than ·35 inch when of its perfect form, as denoted by the growth of its permanent fins, which it acquired in a few days. In this stage its weight (moist) did not exceed ·03 grain, and when dried was reduced to ·01 grain?*

6. *Of the Parr.*

As discussion, wherever doubt exists, is always useful so that it be temperately conducted and lead to further inquiry, I venture to bring under the notice of the Society the question, the vexed question, whether there be or not a fish of the family of the Salmonidæ a parr, having so close a resemblance to the

* The above results were obtained in the month specified. The eggs were from minnows from the river Rothay, a tributary of Windermere. On the 6th of May, they were impregnated by the artificial process, and placed in water, varying in temperature from 50° to 54°, which was changed daily. One fœtal fish burst its shell on the 11th of May; the next on the night of the 12th; the majority on the following day; some did not appear till the 15th. On the 31st of the same month, most of them had acquired their permanent fins. In substance, the eggs of the minnow were found similar to those of the Salmonidæ, being composed of oil globules, and of an albuminous fluid, coagulable by admixture with water. Some of the ova to which no milt was added—the omission intentional—died without showing the slightest appearance of organic development.

salmon fry as to be with difficulty distinguished, and yet a distinct species.

So far as the weight of authority is concerned amongst naturalists who have given their attention to the subject, I believe it is in favor of the negative; and on the ground, first, that the parr, according to their experience, has never been found in a river inaccessible by the salmon or sea trout; and, secondly, that no reliance apart from this is to be placed on slight difference of form, or of color, or spots, as these vary in the known fry of the salmon, according to the quality of the water, food, and other influential circumstances, and in themselves, therefore, are insufficient to constitute a species.

The few who are opposed to this view, and who maintain that there is a parr a distinct species, state that though rare, yet such a fish is to be met with in streams where the salmon and sea trout have never been seen, and in some in which they assert it is impossible that either could resort, inasmuch as the access to them is prevented by impassable falls. Two rivers I have heard named as coming under this category, in which it is said that parr have been caught above falls that no salmon or white trout could by any possibility surmount—one a tributary of the Shin, in Sutherlandshire, which I have found to be accessible (see p. 220), the other the Kirkiag, in the same county, on its west coast.

Granting even the fact that in each of these rivers there is an impassable fall, is the inference drawn by the advocates of the distinct species quite conclusive? May not the parts of the river above or below the fall have some communication by a channel through which a sea trout might be able to pass, either subterraneous, or by some collateral branch formed during a period of flood? or, if nothing of the kind be discoverable on careful examination, may not the parr found in the upper stream derive their origin from impregnated ova of the salmon or sea trout conveyed by birds, such as the water ouzel, adhering to its feet or plumage, or loose in its bill? Thus conveyed, it is presumed they would retain their vitality, and in due time be hatched. There is a well-authenticated instance of impregnated ova of the salmon, taken even from the stomach of the common trout, having produced salmon fry.

But waiving these arguments pro and con, what is the evidence that all parties would probably hold to be satisfactory or conclusive? Is it not the shewing that the parr, the asserted distinct species, propagates its kind, and that in due season, and at the same time, the male and female fish are to be found with roe and milt mature—the one of its maximum size, loose in the cavity of the abdomen, fit for exclusion; the other in its liquid milky state, ready for expulsion?

So far as I can learn, such a coincidence has never been observed. I have examined hundreds of parrs, and a large number from a river where the salmon is rare, and is never known to be taken but by the poacher in the fall of the year, after a flood. Male parrs I have frequently found with mature milt, but never a female with roe correspondently developed; on the contrary, in the female fish, without exception, the ovaries have been so small that, had they not been sought after carefully, they would have escaped notice, and the ova contained in them so minute as to be mere granules. The mature milt, it is worthy of remark, I have found only in the young salmon in the autumn, when its parr marks are distinct; never in the following spring, when these marks have become concealed by a new coat of silvery scales in the more advanced smolt on its way to the sea.

In conclusion, is it not reasonable to hold that, till such evidence as that referred to be adduced and clearly substantiated, the existence of a parr as a distinct species must be considered as not proved.

XXIX. ON THE URINARY SECRETION OF FISHES, WITH SOME REMARKS ON THIS SECRETION IN OTHER CLASSES OF ANIMALS.

Notwithstanding the progress made of late years in animal chemistry in connection with comparative anatomy, I am not aware of any observations that have yet been published on the urinary secretion of fishes. The neglect of this inquiry probably has arisen from several circumstances—the nature of the element inhabited, the peculiarities of the urinary organs, the difficulty of collecting the matter voided, and its having no well-marked distinctive qualities obvious to the senses.

For some years, as leisure and opportunities offered, I have given attention to the subject. Few and imperfect as my observations are, they are given mainly with the hope of attracting notice to the inquiry and of inducing others more favorably situated to engage in its prosecution.

The fishes I have examined in search of their urinary secretion have been the following: the salmon, sea trout, charr, common trout, pike, and perch; the skate, ling, conger, cod, pollack, haddock, turbot, bream, mackerel, and a species of uranoscopus.

Of these the salmonidæ, pike, perch, ling, ray, and uranoscopus, have a small urinary bladder; and in all but the ray communicating directly with the kidneys. In the ray and uranoscopus the communication appears to be indirect, after the manner observable in some of the batrachians, in which the ureters terminate in the cloaca.

The other fishes named seem to be destitute of a urinary bladder, or, if possessed of one, it was so small as to have escaped observation. The ureter in these, when distinct, was found to terminate near the verge of the anal aperture; in several instances it was so large and dilated as to serve the place of a bladder.

In the small urinary bladder of the salmonidæ (so small as to be little more than rudimentary), I have never found any fluid collected. In the bladder of a trout (*Salmo fario*) taken in

June, in Windermere, when in the highest condition, there was seen a little whitish mucus-like matter. Tested by nitric acid and heat properly graduated, it became yellow, without the slightest purplish tinge, indicative of the presence of lithic acid.

The urinary bladder of the perch (*Perca fluviatilis*) is larger, and internally plicated and spongy, and has been found to contain a fluid. In that of one—a fish weighing about a pound and a half, taken in the same lake, and in the same month as the trout—there was a little mucus-like matter suspended in its fluid contents. The fluid was rendered turbid by admixture with alcohol. It cleared on rest, from the subsidence of the precipitated matter. The clear solution, decanted and evaporated gently, yielded crystals approaching in form those afforded by a weak solution of muriate of ammonia similarly treated. Redissolved on the addition of a minute portion of nitric acid, and again evaporated, crystalline plates were obtained very like those of the nitrate of urea. Subjected to the temperature required for detecting the presence of lithic acid, the result was negative: the hue produced was yellow, without the slightest tinge of purple; and the mucus-like matter similarly tested afforded a like result.

The urinary bladder of the pike (*Esox lucius*) is very small. I have always found it empty. In the ureter* of one of about two pounds, taken in Windermere in May, a few delicate yellowish flakes were detected. These, under the microscope, exhibited no characteristic appearance; acted on by dilute nitric acid, however, they were in great part dissolved; and when evaporated with a graduated heat to dryness on a support of thin glass, the purple stain distinctive of lithic acid was produced, and it was so strong, that it colored a proportionally large quantity of water.

The ling (*Lota molva*) has a comparatively large urinary bladder. From the bladder of one—a fish of about four feet long, taken in the Mount's Bay, in Cornwall, in the month of June—a small quantity, about a drachm, of nearly colorless fluid was obtained, in which a few flakes resembling lymph were

* Professor Owen, in his Lectures on the Comparative Anatomy of the Vertebrate Animals (Part I., p. 223), describes the bladder of the pike as communicating with the kidneys by a single common ureter; in most instances I have found the communication such, but in one fish, one of six pounds, it was by two.

suspended. These flakes were tested for lithic acid, but with a negative result. The fluid was coagulated by heat, by nitric acid, and by alcohol, indicating the presence of a notable proportion of albumen. The alcoholic solution, after the separation of the precipitated albumen, evaporated to dryness at a low temperature, yielded, after the addition of a minute portion of nitric acid, crystals which, seen under the microscope (they were too small to be seen without this aid), resembled so closely those of nitrate of urea, that I had little hesitation in coming to the conclusion that they were this compound.

The common ray (*Raia batis*) is provided with two small bladders, each distinct, and neither of them communicating directly with the kidneys. In a male, examined in November, they were found distended with a nearly colorless limpid fluid, in which, placed under the microscope, were seen many small globules, and a few spermatozoa. This fluid, evaporated at a low temperature, yielded a colorless residue, in which were minute crystals of common salt; and, acted on by alcohol and nitric acid, indications were afforded of the presence also of a little albumen and urea, but without any trace of lithic acid.

A species of uranoscopus occasionally but rarely met with in the Indian Ocean, is provided with pretty large lobulated kidneys, which are connected by a common ureter with a small oval urinary bladder, and the bladder by a urethra with the cloaca. In the only specimen that I ever examined, one taken off the coast of Ceylon, when I was in that island in 1819, I found in its bladder a small quantity of fluid, little exceeding a scruple. From the few trials I made of it, it appeared to consist of water, a little lithic acid and animal matter. The presence of the acid was pretty clearly shewn by the rosy hue the minute extract exhibited when gradually heated, after the addition of nitric acid.

Of the fishes before named, destitute of a urinary bladder, the ureter, in the instance of the haddock (*Morrhua æglefinus*), of the cod (*Morrhua vulgaris*), of the pollack (*Merlangus pollachius*), of the turbot (*Rhombus maximus*), was found so capacious, that it might answer the purpose of a receptacle or bladder. In each its inner surface was wet; but only in one, that of the turbot, was there any fluid collected. The quantity obtained, by cutting out the duct, after a ligature had been passed above and below, was about ten drops. It was colorless, not quite clear, and had suspended in it a few white flakes. These were not dissolved by nitric acid, nor did they, when the acid was evaporated by heat, afford the slightest indications of lithic acid. The residue was yellow; nor could urea be detected in the minute portion of fluid.

Of the bream (*Pagellus centrodontus*), the ureter is narrow, and of little capacity; as is also that of the conger (*Conger vulgaris*), and that of the mackerel (*Scomber scombrus*). Of all three the ureter was found merely moist, wet, as if a fluid had passed; in neither could any solid matter be detected. At the termination of the ureter of the bream a minute portion of whitish matter was seen adhering, suggesting lithate of soda or ammonia, but not confirmed when tested; for, when acted on by nitric acid and heat, the color acquired was yellow without the slightest tinge of purple.

I may mention, generally, that in most of the fishes, the names of which have been given, I did not omit examining the cloaca, but with results so unsatisfactory that they might be said to have been negative. Often there was an appearance as if of the presence of an alkaline lithate; but when tested it was found to be different, and the matter chiefly intestinal excrement. In the instance of one only, and that a sea trout (*Salmo trutta*), was a trace of urea indicated, judging from the form of the minute microscopic crystals obtained on evaporation, after treatment with alcohol and nitric acid.

I may also mention, generally, that in each fish I carefully inspected the structure of the kidneys, but without success as to the finding of any matter conspicuous to the eye, such as is commonly seen in the same organs in the instance of serpents and lizards, viz., the opaque lithate.

In one instance only, that of the haddock, have I examined these organs chemically. The result, too, was negative. The trial was made, first by digesting the kidneys in alcohol, decanting the clear spirit, evaporating it at a low temperature, and to the concentrated extract obtained adding nitric acid; secondly, by digesting the organs with aqua ammoniæ, filtering the solution, and testing the little extract obtained by nitric acid and heat.

If any conclusions are permissible from the preceding few and imperfect observations, I would venture to submit the following:—1st, that the urinary secretion of fishes is very limited as to quantity; 2ndly, that it is commonly liquid; 3rdly, that the nitrogenous compound eliminated is variable, either urea or a lithate (the latter probably very seldom), or some nearly allied compound of azote; 4thly, that in the instance of sea fish, it always contains sea salt, and in a notable proportion.

A brief glance at this secretion in other classes of animals may here not be out of place, as bearing on these conclusions. I need not dwell on the importance of the urinary secretion, denoted by its generality, and how, in all the great divisions of the animal kingdom in which it has hitherto been examined, viz., the mammalia, birds, reptiles, insects, spiders, the mollusca, it has been found to consist chiefly of compounds abounding in nitrogen, authorizing the commonly received conclusion that the secerning organs are depurating in their function, and the main channel by which the excess of this element (nitrogen) is removed from the system.

The differences, however compatible with this intent—differences in the nature of the secretion—are not a little remarkable. I allude merely to the quality, to the chemical ingredient; and they seem to be regulated more by the structure of the urinary apparatus, or secerning vessels, than by any other circumstance, not even excepting the kind of diet, whether animal or vegetable, or admixture of the two.

In the mammalia, provided with an ample urinary bladder, the normal secretion is seen to be entirely liquid, and the principal ingredient, so far as it has yet been determined, always soluble urea: such it has been found to be in man; such in the carnivorous animals; such in the herbivorous; with the addition in that of some of them of the hippuric acid.

In birds, on the contrary, and in those reptiles which, like them, are destitute of a urinary bladder, viz., snakes and lizards, invariably the secretion, judging from my own pretty extended experience, is chiefly solid—a soft, plastic one, owing its consistence to admixture with water, and composed principally of lithate of ammonia and lithic acid. Yet in others of the latter class, which have a receptacle corresponding to the urinary bladder, and destined to hold the secretion—the secretion is fluid, as in the instance of the toads and frogs, and the nitrogenous matter eliminated is again the soluble urea. The same remark applies to the tortoises, with this difference, that sometimes, though their food be vegetable, solid matter, flakes of a lithate are occasionally found suspended in the fluid contents of their urinary bladder.

In insects, also in spiders and scorpions, all which, it is presumed, have no receptacle for the secretion but the cloaca, we find it in consistence analogous to that of birds, snakes, and lizards, a soft solid; in insects, as far as my observations have extended, it is composed chiefly of an alkaline lithate; but in others, the spiders and scorpions, of guanine.*

Of the secretion in the mollusca, also without a urinary bladder, I can venture to say little. In two instances, as already described, I have found it to be lithic acid.

Of animals lower in the organic scale, the only ones the urine of which I have examined with any positive result have been two of the Myriapoda—the common centipede of the West Indies and our millipede—the one voracious, feeding on insects, the other feeding on vegetable matter. In the mixed excrement of the scolopendra, as already stated, lithate of ammonia in abundance was detected; but in that of the millipede, merely a trace of lithic acid.

In this brief notice of the urinary secretion in the several classes of animals mentioned, I have, as I premised, taken notice only of its principal ingredient; I would further beg to remark, that in stating that the quality of the secretion is independent of the quality of the food, I would wish to be understood as not holding the opinion that it is not in some measure modified by the kind of food—especially as regards the quantity of matter eliminated. As might be expected, the larger the proportion of nitrogen in the food consumed, the larger, *cæteris paribus*, seems to be the quantity of the nitrogenous compound excreted, and *vice versâ*. Moreover, when the food is entirely vegetable, there

* When I first examined the excrement of spiders and scorpions in 1847–1848, operating on minute quantities, I inferred that it consisted chiefly of xanthic oxide: guanine was not then known. Since its discovery by Bodo Unger, I have re-examined portions of the excrement of each, which I brought from the West Indies, and have satisfied myself that the principal ingredient of both is this compound; I have also found it, in accordance with the researches of Will and Gorup-Besancz. to form the chief portion of the excrement of our spiders. The very low degree in which this excrement is soluble in *cold* muriatic acid may account for its having been first confounded with the xanthic oxide.

seems to be in some instances a tendency towards the production of the hippuric acid rather than of the lithic. MM. Magnon and Lehmann have found this compound in the urine of the tortoise feeding on lettuce;* and have found it mixed with lithic acid in the urine of caterpillars feeding exclusively on vegetables —a result which accords with my own experience.

In the animal economy we see commonly, amongst the different classes of animals, a certain relation and accordance of functions conducive in action to the elaboration and wellbeing of each individual structure. Such a relation is manifest between the kidneys and the lungs; the former the depurator of nitrogen, as much as the latter is of carbonic acid. How strongly is this exemplified in birds;—of high temperature, consuming much atmospheric air, evolving much carbonic acid—their urinary secretion, also, is remarkably abundant, and abounding in nitrogen.† And in other classes of animals, such as insects in their several stages, such as serpents and lizards, and the hybernating ones of different classes, whether active or torpid, a like accordance, though perhaps not so strongly shewn, is yet clearly observable.

Reasoning hence, guided by analogy, might it not be expected that in the instance of fishes, inasmuch as their temperature is low, and the quantity of carbonic acid evolved small, that their urinary secretion also would be small—proportionally small? And, granted that it is so, as the results of the experiments described would seem to indicate, does it not lead to another conclusion, viz., that subsisting, with few exceptions, exclusively on animal food, this their food, under the influence of a high digestive power, is almost entirely assimilated, and that no more is expended on the urinary secretion than is requisite to balance

* Lehmann's Physiological Chemistry, vol. ii., p. 458.

† I may mention as an instance the swallow, feeding like the trout, when the food of the latter is chiefly insects, and, as regards the secretion in question, shewing a remarkable difference. From the nest of a pair I had an opportunity of observing, the young of which were only a few days old, the droppings on a flag-stone beneath, in one day, were as many as forty-five; those collected and dried thoroughly weighed 78·3 grains; the following day, the droppings were seventy. They consisted chiefly of lithate of ammonia with a little urea, and of the indigestible remains of insects, the urinous portion by far the largest. The excrement, it may be inferred, was chiefly from the young birds, as the parent birds were almost constantly on the wing providing food. How large in quantity was this excrement in comparison with the bulk of the birds! I have found an old swallow to weigh only about 300 grains, and when thoroughly dried no more than 105 grains, so that the amount of excrement in two days exceeded considerably in weight one of the old birds!

the small amount consumed in carrying on the aerating process? And if this be admitted, does it not help to explain some of their peculiarities—their remarkable rapidity of growth when supplied with abundance of food—their little waste of substance when sparingly supplied, and their long endurance without loss of life, under a total, or nearly total, privation of aliment?

The history of the salmon and its congeners, which of late years has been so carefully and successfully studied, might be adduced in illustration—exemplifying, 1st, The great activity and power of the organs carrying on the digestive functions—the stomach itself of the captured fish,* with the parieties adjoining, being found more or less dissolved by the action of the gastric juice in the short space of a few hours, and in being always found empty in the migrating fish; 2ndly, The extraordinary increase in weight during the short sojourn of the young salmon in the sea, when, without stint of food, it passes from the smolt stage of growth to that of the grilse; and, 3rdly, The comparatively very slow growth of the young salmon in its parr stage, during the months of winter and early spring, when its food is scarce.

Reasoning also on the circumstance that the urine of sea-fish contains common salt, and that in a notable proportion, does it not help to account for the freshness of the salmon on its leaving the sea, and of other sea-fish, which in the act of feeding must swallow a certain quantity of sea-water, and which, if not thus eliminated—*i.e.*, the chloride of sodium—it might be expected, would be found accumulated in their solid parts.

* This remark applies to the young of the salmon and sea trout in their parr and smolt stages and to the common trout, not to the salmon on its return from the sea.

XXX. OBSERVATIONS ON FISH IN RELATION TO DIET.

What are the nutritive qualities of fish, compared with other kinds of animal food? Do different species of fish differ materially in degree in nutritive power? Have fish, as food, any peculiar or special properties? These are questions, amongst many others, which may be asked, but which, in the present state of our knowledge, I apprehend it would be difficult to answer in a manner at all satisfactory.

Now I shall attempt little more than an opening of the inquiry, and that directed to a few points—chiefly those alluded to in the foregoing queries.

1. *Of the Nutritive Power of Fish.*

The proposition probably will be admitted, that the nutritive power of all the ordinary articles of animal food, at least of those composed principally of muscular fibre, or of muscle and fat, to whatever class belonging, is approximately denoted by their several specific gravities, and by the amount of solid matter which each contains, as determined by thorough drying, or the expulsion of the aqueous part, at a temperature such as that of boiling water, not sufficiently high to effect any well-marked chemical change.

In the trials I have made, founded on this proposition, the specific gravity has been ascertained in the ordinary hydrostatical way—the portions subjected to trial, in the instance of fish, have been taken from the thicker part of the back, freed from skin and bone, composed chiefly of muscle. And the same or similar portions have been used for the purpose of determining their solid contents, dried in platina or glass capsules of known weight, and exposed to the process of drying till they ceased to diminish in weight.

The trials on the other articles of diet, made for the sake of comparison, both as regards specific gravity (excepting the liquids), and the abstraction of the hygroscopic water, or water capable of being dissipated by the degree of temperature mentioned, have been conducted in a similar manner.

The balance used was one of great delicacy when at home, or a small portable one when from home, of less delicacy yet turning readily with one-tenth of a grain.

The results obtained are given in the following tables. In the first, on some different species of fish; in the second, on some other articles of animal food.

I have thought it right, whenever it was in my power, to notice not only the time when the fish were taken, but also the place where they were procured—not always so precise as I could wish—as both season and locality may have an influence on their quality individually. When the place mentioned is inland, it must be understood that, in the instance of sea-fish, they were from the nearest seaport.

TABLE I.

Species of Fish.	Spec. Grav.	Solid Matter per cent.	Place where got, and Time.
Turbot, *Rhombus maximus*	1,062	20·3	Mar. Liverpool.
Brill, *R. vulgaris*	1,061	20·2	Oct. Penzance.
Haddock, *Gadus æglefinus*	1,056	20·2	Aug. Ambleside.
Hake, *G. merlucius*	1,054	17·4	Oct. Penzance.
Pollack, *G. Pollachius*	1,060	19·3	Oct. Penzance.
Whiting, *Merlangus vulgaris*	1,062	21·5	Mar. Chester.
Common cod, *Morrhua vulgaris*	1,059	19·2	April. Ambleside.
Red gurnard, *Trigla cuculus*	1,069	23·6	Oct. Penzance.
Dory, *Zeus faber*	1,070	22·9	Oct. Penzance.
Mackerel, *Scomber scombrus*	1,043	37·9	Oct. Penzance.
Sole, *Solea vulgaris*	1,065	23·0	Feb. Ambleside.
Ditto, *ditto*	1,064	21·1	Feb. Ambleside.
Thornback, *Raia clavata*	1,061	22·2	Oct. Penzance.
Salmon, *Salmo salar*	1,071	29·4	Mar. River Boyne, Ireland. Fresh run from the sea.
Sea trout, *S. criox*	...	41·2	June. Ambleside.
Charr, *S. umbla*	1,056	22·2	Nov. Windermere.
Trout, *S. fario*	1,053	22·5	Mar. Lough Corrib, Ireland. Weight about ½lb., in good condition.
Ditto, *ditto*	1,050	13·7	Oct. River Brathay. A small fish of about 2 oz.
Smelt, *S. eperlanus*	1,060	19·3	Mar. Liverpool.
Eel, *Anguilla latirostris*	1,034	33·6	June. Ambleside.

TABLE II.

Kinds of Food.	Spec. Grav.	Solid Matter per cent.	Time and Place.
Beef, sirloin	1,078	26·9	Mar. Ambleside.
Veal, loin	1,076	27·2	Nov. Ambleside.
Mutton, leg	1,069	26·5	Nov. Ambleside.
Pork, loin	1,080	30·5	Jan. Ambleside.
Pemican, composed of beef and suet	...	86·25	Victualling-yard, Portsmouth.
Common fowl, breast	1,075	27·2	Nov. Ambleside.
Grey plover, breast	1,072	30·1	Nov. Ambleside.
Cow's milk, new, before the cream had separated	1,031	11·2	Nov.
White of hen's egg	1,044	13·9	
Yolk of the same	1,032	45·1	

These results I would wish to have considered merely as I have proposed in introducing them, viz., as approximate ones. Some of them may not be perfectly correct, owing to circumstances of a vitiating kind, especially the time of keeping. Thus, in the case of the whiting, which was brought from Chester, its specific gravity, and its proportion of solid matter may be given a little too high, owing to some loss of moisture before the trials on it were made. Casting the eye over the first table, it will be seen that the range of nutritive power, as denoted by the specific gravity and the proportion of solid matter, is pretty equable, except in a very few instances, and chiefly those of the salmon and mackerel; the one exhibiting a high specific gravity, with a large proportion of solid matter; the other, a low specific gravity, with a still larger proportion of matter, viz., muscle and oil, and, in consequence of the latter, the inferior specific gravity. A portion of the mackerel, I may remark, merely by drying and pressure between folds of blotting paper, lost 15·52 per cent. of oil. Oil also abounded in the sea trout and eel, and hence the large amount of residue they afforded.

Comparing *seriatim* the first table with the second, the degree of difference of nutritive power of those articles standing highest in each, appears to be inconsiderable, and not great in the majority of the others, exclusive of the liquids — hardly in accordance with popular and long received notions.

2. *Of the Peculiar Qualities of Fish as Articles of Diet.*

I am not prepared to enter into any minute detail on this important subject, from want of sufficient data.

That fish generally are easy of digestion, excepting such as have oil interfused in their muscular tissue, appears to be commonly admitted, as the result of experience, a result that agrees well with the greater degree of softness of their muscular fibre, comparing it with that either of birds or of the mammalia, such as are used for food.

A more interesting consideration is, whether fish as a diet is more conducive to health than the flesh of the animals just mentioned, and especially to the prevention of scrofulous and tubercular disease.

From such information as I have been able to collect, I am disposed to think that it is. It is well known that fishermen and their families, living principally on fish, are commonly healthy, and may I not say above the average? and I think it is pretty certain that they are less subject to the diseases referred to than any other class, without exception. At Plymouth, at the Public Dispensary, a good opportunity is afforded of arriving at some positive conclusion, some exact knowledge of the comparative prevalency of these diseases in the several classes of the community. The able physician of that institution, Dr. Cookworthy, at my request, has had the goodness to consult its records, and from a communication with which he has favored me, it appears that of 654 cases of "confirmed phthisis and of hæmoptysis, the probable result of tuberculosis," entered in the register of the dispensary, 234 males, 376 females, whose ages and occupations are given individually, the small number of four only were of fishermen's families—one male and three females—which is in the ratio of 1 to 163·2; and of watermen, "who fish with hook and line, when other work is scarce, generally very poor, and of habits generally by no means temperate or regular," the number, including their families, did not exceed eleven, of whom ten were males, one a female, which is in the ratio of 1 to 58·8. The entries from which the 654 cases are extracted, Dr. Cookworthy states, exceed 20,000. He assures me that had he taken scrofula in all its forms, the result would, he believes, have been more conclusive.

Such a degree of exemption as this return indicates in the instances of fishermen and boatmen, is certainly very remarkable and deserving of attention, especially considering the prevalency of tubercular consumption, not only in the working classes generally throughout the United Kingdom, but also amongst the regular troops, whether serving at home or abroad, and having an allowance of meat daily, but rarely tasting fish.*

If the exemption be mainly owing to diet, and that a fish diet, it may be presumed that there enters into the composition of fish, some element not common to other kinds of food, whether animal or vegetable. This I believe is the case, and that the peculiar element is iodine.

I may briefly mention, that in every instance in which I have sought for this substance in sea fish, I have found distinct traces of it, and also, though not so strongly marked, in the migratory fish, but not in the fresh-water fish. The trials I have hitherto made have been limited to the following, viz., the red gurnet, mackerel, haddock, common cod, whiting, sole, ling, herring, pilchard, salmon, sea trout, smelt, and trout. In each instance, from about a quarter of a pound to a pound of fish was dried and charred, lixiviated, and reduced to ashes, which were again washed. From the sea fish, the washings of the charcoal afforded a good deal of saline matter on evaporation; the washings of the ash less. The saline matter from both consisted principally of common salt, had a pretty strong alkaline reaction, and with starch and aqua regia, afforded, by the blue hue produced, clear proof of the presence of iodine. In the instance of the fresh-run salmon, sea trout, and smelt, a slight trace of iodine was thus detected; in the spent salmon descending to the sea, only a just perceptible trace of it was observable, and not a trace of it either in the parr or in the trout.

That iodine should enter into the composition of sea fish, is no more perhaps than might be expected, considering that it forms a part of so many of the inhabitants of the sea on which fish feed; to mention only what I have ascertained myself, in the common shrimp I have detected it in an unmistakeable manner,

* In 1,205 fatal cases, not selected, in which the lungs were examined at the General Hospital, Fort Pitt, Chatham, tubercles were found to exist in 734 (61·7 per cent.). See the author's work "On Some of the more Important Diseases of the Army," p. 159, for details.

and also in the lobster and crab; and likewise in the common cockle, mussel, and oyster.

The medicinal effects of cod-liver oil, in mitigating, if not in curing pulmonary consumption, appear to be well established. And as this oil contains iodine, the analogy seems to strengthen the inference that sea fish generally may be alike beneficial.

Should further inquiry confirm this conclusion, the practical application of it is obvious; and fortunately, should fish ever come into greater request as articles of food, the facility with which they may be preserved, even without salt, by thorough drying, would be much in favor of their use. I lay stress on thorough drying, as that seems essential; for preservation, I believe even hygroscopic water should be excluded. Even in the instance of those articles of food which can be preserved in their ordinary dry state, the expulsion of this water would be advantageous under certain circumstances, were it merely on account of diminution of weight. Thus, referring to the second table, it will be seen that the pemican, carefully prepared in the Portsmouth Victualling Office, lost by thorough drying 13·75 per cent., so much being the water it contained in a hygroscopic state—a lightening of weight that, to the Arctic land explorer, could not fail to be welcome and useful.

The inference regarding the salutary effects of fish depending on the presence of iodine in the prevention of tubercular disease, might be extended to some other diseases, especially to that formidable malady goître, the mitigation or cure of which has in so many instances been effected by iodine; and which, so far as I am aware, is entirely unknown amongst the inhabitants of seaports and sea coasts, who, from their situation, cannot fail to make more or less use of fish.

Amongst the many questions that may be asked in addition to those I have proposed, I shall notice one more only, and that in conclusion. It is, whether the different parts of the same fish are likely to be equally beneficial in the manner inferred—the beneficial effect, it is presumed, depending on the presence of iodine. From the few experiments I have yet made, I am led to infer, reasoning as before, that the effects of different parts will not be the same, inasmuch as their inorganic elements are not the same. I may instance liver, muscle, and roe or milt. In the ash of the liver and muscle of sea fish I have always found a large proportion of saline matter, common salt abounding, with a minute portion of iodine—rather more in the liver than in the muscle—and free alkali, or alkali in a state to occasion an alkaline reaction, as denoted by test paper ;* whilst in their roe and milt I have detected very little saline matter, no trace of iodine, or of free alkali ; on the contrary, a free acid, the phosphoric, analogous to what occurs in the ash of the yolk of the domestic fowl, and in consequence of which the complete incineration of the roe of the fish and its milt, like that of the yolk of the egg, is very difficult.

The same conclusion, on the same ground, viz., the absence of iodine, is applicable to fresh-water fish, a conclusion that can hardly be tested by experience, nor is it of practical importance, since fish of this kind enters so sparingly into the ordinary diet of the people.

I have mentioned briefly the test employed to detect iodine. To prevent obscurity, may I be permitted to add a few particulars relative to the mode of proceeding? On a portion of starch in fine powder, that is, in its granular state, aqua regia is poured, or about equal parts of nitric and muriatic acid, in a platina capsule, and then well mixed, using a glass rod. The salt to be tested, either in solution or solid, is then added. The blue tint due to the presence of iodine is immediately produced, if any of this substance, or a sufficiency of it to take effect, be present. The delicacy of this test is, I believe, well known. I have by means of it detected iodine, when $\frac{1}{10}$ of a grain of the iodide of potassium was dissolved in 16,775 grains of water. Relative to this method, I may further remark, that by well mixing the acid and starch, not only is the starch reduced to a gelatinous state favorable for being acted on by the iodine as liberated by the action of the chlorine, but also that the excess of chlorine is, to a great extent, got rid of. The platina capsule has appeared preferable to one of glass, as shewing the effect of color by reflected light more readily and distinctly; and also, I am disposed to think, from some peculiar influence which the metal exercises, favoring the combination of the starch and iodine, similar, it may be, to that of spongy platinum in effecting the union of oxygen and hydrogen.

* This quality, I believe, belongs to the muscle of fish generally: I have witnessed it in all I have tried, and in the trout the instant it has been killed.

In seeking for iodine in animal substances by incineration, it may be well to keep in mind that, experimentally considered, the liability to error lies in underrating, rather than in overrating the result by the methods employed, and that mainly in consequence of more or less of loss of iodine being sustained in the process of combustion, incineration, and evaporation used. To illustrate this by a simple experiment, I may mention that a portion of water, equivalent to about 1,525 grs., in which were dissolved 10 grs. of common salt, and ·09 gr. of iodide of potassium, was quickly evaporated to dryness by boiling. Previously, the iodine could be detected in the mixture by the test I have used; but not afterwards, when the residual salt was dissolved in the same quantity of water, proving how there had been a loss of the iodine in the operation of boiling, a loss chemists are familiar with, of substances in themselves not volatile, carried off suspended in aqueous vapor.

In stating the comparative exemption of fishermen and their families from pulmonary consumption, as indicated by the Plymouth Dispensary return, I have not given the total number of this class of persons. This deficiency I am now able to supply. From information which I have received, for which I am indebted to the Registrar-general, it would appear, that of the total male population of Plymouth (24,605), the number of fishermen is 726, exclusive of 37 pilots. This large proportional number renders the fact of their exemption the more remarkable, and especially comparing them with a class of the population altogether different in their habits, and, it may be presumed, in their diet, using fish only occasionally when abundant and cheap —these are the cordwainers or shoemakers, whose number altogether (males) is 608. Now, on consulting the Dispensary return, I find that the total number of this class that have died of the disease under consideration has been 37, viz., 19 males and 18 females!

Reflecting on the fact that iodine has been detected in all the trials I have hitherto made on sea fish, it seemed probable that guano, considering its origin, would not be destitute of this substance, and the result of experiments has been confirmatory; using the test-method noticed above, a distinct indication of its presence was obtained, both in the instance of the Peruvian and African guano, the only two I have yet tried.

XXXI. MISCELLANEOUS OBSERVATIONS ON THE VITALITY OF FISHES.

The experiments which I have to describe were made at different times and places, as opportunities offered. I shall be under the necessity of entering into some minuteness of detail for the sake of accuracy, which can hardly be otherwise than tedious; the subject, however, I trust may be considered sufficiently interesting to warrant such minutiæ.

1. *Of the Degree of Temperature Fatal to Fishes.**

In making the trials certain precautions were necessary, and were carefully attended to, especially the following. The warm water used had never been boiled, so as not to be deprived of any of the air which it contained; and in every instance the quantity of water used was so large as to render it certain that none of the effect attributable to elevation of temperature was owing to exhaustion of air. I may further premise, that each fish the subject of experiment had been recently taken, and was in a vigorous state.

1. *Of the Common Trout* (*Salmo erox*).—On the 7th August, a trout of about a quarter of a pound, just taken from the river Rothay near Ambleside, was put into water, the temperature of which, from 62° Fahr., was pretty rapidly raised to 75° by additions of warm water. The fish, from a state of indolent rest, became restlessly active, and the more so as the temperature reached 75°, when it made by violent leaps repeated attempts to escape. In an hour the temperature was increased gradually from 75° to 80°; and in a few minutes more, by the further addition of warm water, to 85°. Now the motions of the fish became convulsive; it presently turned on its side, and the gill-covers soon ceased to act, except at short intervals spasmodically: it was now transferred to cool water, but it did not recover.

* This first part was read at the meeting of the British Association for the Advancement of Science, in 1862.

A smaller trout was put into the same water, after it had fallen to 70°. It did not appear to be distressed. Seen after four hours, it was alive and had not apparently suffered. The following morning, however, it was found dead; the water had fallen to 67°. The volume of water was considerable; a minnow put into it, did well.

2. *Of the Parr* (*S. salar*).—A young fish, about four inches long, taken on the 30th June in the Greta, was kept in cool water until the following day, when the temperature of the water was gradually raised from 60° in about half an hour to 70°. Before quiet, it now swam about rapidly, as if it wished to escape. More warm water was gradually added until the temperature had risen in about half an hour to 80°. Now the parr seemed little disposed to move, as if becoming torpid; a tremulous, slightly convulsive motion was seen in its pectoral fins; its tail was slightly bent; its opercula were in rapid and feeble motion; it rested on its pectoral fins, and did not stir till touched. Now the temperature of the water was raised to 84°; the fish in a few seconds lost its balancing power, turned on its side, and in a few seconds more the opercula ceased to act. Transferred to cool water, there was a feeble renewal of the gill-covers, but only for a very short time. When all motion had ceased, the gills examined were found of a healthy florid red.

3. *Of the Charr* (*S. umbla*).—A small charr, of about a quarter of a pound, taken in Windermere, on the 10th May, at 5·30 a.m., at 11 a.m. was put into water gradually raised by successive additions of warm water to 68°:—

	A.M.	DEG.
At	11·8	70.
	11·30	72.
	11·45	76.
	12·10	80.

At 76° it became very restless, rising to the surface and swimming about rapidly, as if seeking to escape; before it had kept at the bottom of the water. As soon as the temperature had risen to 80°, it turned on its side, all the fins immediately ceased to act except the caudal, and that so feebly as to have scarcely any propelling power; its gill-covers at the same time moved feebly and rapidly; at 12·30 their motion was only perceptible at short intervals; at 12·35 their action ceased altogether. Now taken out and opened (this about two or three minutes after the opercula had become motionless) and the heart exposed, this organ was seen without motion; it contracted feebly when punctured, but only for a few seconds.

It may be worthy of note that this fish, compared with the trout, shewed a marked difference in its actions. Whilst the trout endeavoured to escape by leaping out of the water, when distressed swimming from the first towards the surface, the charr kept to the bottom, and till almost the last moment had its head directed downwards, as if intent on seeking, according to its wont, deeper and cooler water.

4. *Of the Perch* (*Perca fluviatalis*).—On the 29th July, a perch just taken from Rydal-mere was put into water at 86°, in a quarter of an hour falling to 84°. It soon became motionless, floating on its back: this was in less than a minute, after some violent efforts made to escape.

Another perch from the same lake, put into water at 83°, falling in fifty minutes to 80°, gradually became distressed; turned on its back, its fins acting irregularly. Now taken out and placed in cool water, its opercula recovered their action, and it was so far restored that it was able to move its fins and resume its natural swimming position. Two hours after it was found dead.

Another, a small one, was put into water at 80°. At first it appeared to be little affected, swimming about in its natural manner; but in little more than half an hour it seemed distressed, swimming hurriedly. In an hour and twenty minutes it had turned on its back, its fins acting irregularly. The water had fallen to 74°. Two hours later it was found dead; the water 70°.

5. *Of the Minnow* (*Leuciscus phoxinus*).—A full grown one taken from an aquarium of the temperature 63°, and put into water at 99°, almost immediately appeared to be rendered lifeless; it rose to the surface and there lay motionless. Transferred to cool water, it did not revive. The experiment was repeated on another with the same result.

Another minnow was put into water at 92°; it instantly became languid; in less than two minutes it had turned on its side, its gill-covers acting feebly. The water falling to 90°, it recovered, resuming its natural mode of swimming.

Another put into water at 92°, in which a perch had been immersed, exhibited a like result, soon becoming languid and swimming on its side: it recovered as the water fell to 88°.

Another put into water at 90°, falling to 85°, did not appear to suffer.

Another put into water at 94° instantly became motionless, and though directly transferred to cool water, it did not revive.

6. *Of the Gold-fish* (*Cyprinus auratus*).—One of average size taken from an aquarium and put into water at 96°, immediately became restless, swimming about hurriedly and making violent leaps, as if attempting to escape. Gradually it became languid, swimming on its side, the caudal fin seldom acting. After a few minutes, when the water had fallen to 94°, it appeared to be motionless; the pectoral fins and the opercula were the last that ceased to act. Now transferred to water of 70°, it rapidly revived, the gills first acting. After an interval of about an hour, it was put into water at 93°. This temperature it bore pretty well at first; gradually it became languid, swimming on its side. As the water cooled, its languor abated; and when the temperature had fallen to 88°, it had resumed its natural position.

7. *Of the Common Carp* (*Cyprinus gibelio*).—A small fish from an aquarium, on the 26th of July, was put into water at 60°, raised in about five minutes to 78°, and in twelve more to 80°. Even at 78° it was sensibly affected, its balancing power becoming impaired. After another half hour, the temperature of the water was raised to 85°, and this suddenly. It now for a few seconds swam about rapidly, presently turning on its side. After about twenty minutes, the water fallen to 80°, it seemed recovering, swimming less on its side. The temperature was raised now to 95°; it immediately fell on its side motionless, as if dead. In less than a minute it was taken out and put into cool water. In a few seconds it shewed signs of returning animation: it very slowly recovered its power; even after twenty-four hours, in swimming there was a slight lateral inclination, and for two or three days it seemed languid, and its balancing power was defective.

8. *Of the Loach* (*Cobitis barbatula*).—One taken in a small stream in the neighbourhood of Ambleside in August was put into water at 80°, falling to 75°. This temperature seemed to

affect it but little: warm water was added, raising the temperature to 85°. Now it swam rapidly about, and seemed distressed; once it turned on its side. The experiment was begun at 11·14 a.m.; at 11·30 a little more warm water was added, so as to raise the temperature to 86°. It instantly became much distressed, and was convulsed; presently it turned on its side, and seemed dying. It was now transferred to cool water, where it presently resumed its normal position, and soon completely recovered.

9. *Of the Eel* (*Anguilla vulgaris*).—One of average size, taken in a stream near Llandovery in Wales, on the 25th of September, was put into water at 92°; it instantly shewed signs of distress, swimming about rapidly; in a few minutes it turned on its back and was convulsed, ejecting a small, partially digested fish. It now lay on its back coiled up; when stirred, it swam about in a hurried irregular manner, its abdomen mostly upwards. As the water cooled it became more composed, and in about half an hour, when the water had fallen to 86°, it had assumed its natural attitude. Two hours and a half later, when the water was 72°, it was active and seemingly well. Now, put into water at 100°, it instantly became very restless, and shewed convulsive movements; in about three minutes it became motionless, and was apparently dead. Transferred to water at 60°, and watched for a quarter of an hour, its recovery seemed doubtful. Three hours later, it was found quite recovered; and the following morning it seemed well and active.

The remaining experiments which I have to describe were made at Oxford, with the kind assistance of Mr. Robertson, and were witnessed for most part by Professor Rolleston, in whose laboratory in the University Museum they were conducted.

An eel of the same kind as that before tried, but somewhat larger, was put into water at 96°; immediately it became extremely active; in about three minutes, the temperature falling to 95°, it turned on its back. After about another minute it made violent efforts to escape, and twice leaped out of the vessel; after about fifteen minutes, when the water had fallen to 92°, it had become almost torpid, lying on its back. At 11·30 it was apparently dead: the heart had ceased to act, and no muscular contraction was effected by the pinching pressure of the forceps.

Left in the water gradually cooling, it did not revive. Its gills exposed, shewed no signs of sanguineous congestion.

Another eel put into water at 98°, soon died. No particulars of this experiment were noted down at the time.

10. *Of the Ruffe* (*Perca cernua*). One of a good size, a gravid female, taken on the 23rd of April, was on the same day put into water, the temperature of which was gradually raised to 83° in twelve minutes. It now appeared to be much distressed; it had lost its balancing power, and was affected with convulsive tremors; presently its gill-covers ceased to act; transferred to cool water, it did not revive.

11. *Of the Tench* (*Tincta vulgaris*).—A fish of about half a pound, caught the day before, was put into water at 60°, the temperature of which was gradually raised. At 76° it became very active, moving about hurriedly, as if trying to escape. The experiment was begun at 11·40; at 12·17, when in the state just described, it was transferred to water at 78°; its restlessness increased. At 12·25 the water was again changed; it was put into a fresh portion at 83°. At 12·40 it was transferred to water at 90°; after two minutes it lost its balancing power, turned on its back, no fins acting but the pectoral, the opercula moving rapidly and feebly. It was now returned to water at 82. At 12·55, the water fallen to 80°, it was swimming in its natural position; and at 2·40, remaining in the same water, which had fallen to 76°, it appeared quite recovered.

Another trial was made with this fish, the notes of which have been lost. At a temperature of water of about 90° (it might have been 2° or 3° higher), it seemed lifeless, and was supposed to be dead. In this state Mr. Robertson exposed its gills to the action of a stream of cool water, which had the effect of reviving it. On the following day it was as active as when first taken, affording thus another example of the great tenacity of life possessed by this fish.

12. *Of the Pike* (*Esox lucius*).—One of about a pound, on the 22nd April, at 11·40, was put into water at 98°; after a short struggle, swimming about rapidly, it turned on its back and became motionless. Transferred to water at 54°, and although a jet of cool water was directed to its gills, it did not revive. The vessels of these organs were found to be congested.

Conclusions.

From these experiments it would appear that the enduring power of each fish in relation to variation of temperature differs in degree; that the charr was one of the most intolerant of warmth, and the gold fish least so; but that even the last mentioned could not endure without loss of life a temperature of about 93°; and if we consider how rapidly in several instances the fatal effect took place, it may be inferred that the temperature of the water, as denoted by the thermometer immersed in it, was only in part communicated to the fish—that is to say, that the deep-seated parts were of a lower temperature than the water; and this I have found to be the case: thus, a gold-fish, after having been twenty-five minutes in water at 75°, was suddenly plunged into water at 90°; taken out after two minutes, a thermometer of great delicacy introduced into its pharynx, and the mouth closed on it, fell to 86°.

In works on ichthyology, and in narratives of travel, statements are to be found little according with these results—statements the accuracy of which it is difficult not to question. Thus, we are told by Bosc that many species of river fish experimented upon by Broussonnet, "lived for several days in water which was so hot that he could not bear his hand in it for a single minute," and that these were fish (species not given) of French rivers. Now, I find that a temperature of water of 112° is disagreeable, of 115° hardly bearable, and of 120° not endurable by my hand.

Sonnerat states that in the Island of Lugon, one of the Manillas, he saw fish in a hot spring, the temperature of which was 60° of Reaumer, equal to 187·25 Fahr., and yet were not apparently incommoded by the heat. Were this correct, such endurance would be most marvellous, as implying a different organization from that of fishes generally, keeping in mind the simple fact that the serum of their blood is coagulated by a temperature of about 160°.

These instances I find given in a note by M. A. R. Dusgate to the second chapter of Dr. William Edwards' work, "On the Influences of the Physical Agents on Life," the English edition, translated by Drs. Hodgkin and Fisher. Dr. Edwards' own results, so far as they are applicable, are as much opposed to them as are mine.

The water of highest temperature in which I have seen living fish was that of the hot springs of Cannea in Ceylon. These springs, when I visited them in 1819, ranged from 91° Fahr. to 107; but it was only in the one of 91° that I saw small fish swimming about.* These fish, according to M. Renaud, are the *Ambasis thermalis*. He states the temperature of the water in which they were found as 115·25° Fahr. at the time of his visit. Now, irrespective of temperature, as azote was disengaged from the springs of greatest heat, it is hardly credible that a fish could exist in them. It is easy, however, to understand how seeing a fish in one of the springs, if its temperature was not tried, an error might have been made.

Relative to the rationale of the effects in those instances which I have described, it is more easy to speculate than to assign a satisfactory cause. I am disposed to think that the high temperature of the water may have a paralysing influence on the heart and muscles; and I am led to this inference both from the irregular and diminished action of the fins and opercula before death in the fish experimented upon, and from observing in very young fish, the circulation of the blood of which could be seen under the microscope, how in the distant parts, under the influence of heat, the flow of blood became arrested. In adverting to this difficulty of the subject, I had in recollection a fact I had noticed regarding the torpedo, how it died almost instantly, on being transferred from sea water to fresh water, both of the same temperature, and both there is reason to believe differing little in the quantity of air they contain. There are other facts, shewing how easily fish are deprived of their vitality; one I may mention, which is familiar to most anglers who have fished in brooks in which there are small trout, and who may have witnessed the instant death that has followed the fall of a fish, on its capture, on hard ground, or against a stone.

The great sensitiveness of fish in relation to the temperature of the water, their peculiar element, cannot but have an influence on their habitats; and as it varies in different species, this circumstance alone may well account for the manner in which limits seem to be put to their diffusion; and, in accordance with

* See my Account of the Interior of Ceylon, p. 44–45.

this, the results may be quoted of the experiments of M. Quatrefages on their spermatozoa, shewing how a difference of a very few degrees above or below the temperature of the individual fish was fatal to them.*

2. *On the Influence of Moist compared with that of Dry Air on the Vitality of Fishes.*

Collateral with the preceding experiments some were made to ascertain whether the life of fish would endure longer in air saturated with moisture than in air of ordinary dryness.

1. *Of the Minnow.*—One minnow was placed in an ounce vial just emptied of water, and the vial was closed with a glass stopper; another minnow was taken out of water and exposed to the open air at 65°. After forty minutes, the one in the vial had become motionless, and in a few more it shewed the *rigor mortis.* In the other, after the same lapse of time, the heart was seen slightly acting. Both minnows were replaced in water: the first did not revive; the second soon gave signs of revival, the gill-covers acting feebly. For at least ten minutes it seemed in a very feeble precarious state, occasionally turning on its back. Seen again, after an hour and a-half, it had quite recovered.

The experiment was repeated on other two: one was put into a pint bottle, immediately after the water with which it had been filled was poured out; the other was exposed to the open air. In somewhat less than twenty minutes the first became motionless; put into water, it did not revive. In the other, which had been exposed freely to the air on a surface of paper, after the same interval of time the heart continued to act; put into water, in the short space of two or three minutes it recovered its activity.

In a third trial, one placed in the same bottle with moist confined air, after eight minutes seemed moribund; its heart had ceased to act. Put into water, there was seen a slight movement of its lips, and no more; it did not revive. The other, exposed to the open air, after fifteen minutes had not altogether

* See Annales des Sciences Nat., xix., 348. He found that a variation of from 4° to 5° above or below a determined temperature had the effect of shortening the time of their vitality on admixture with water (that of extreme brevity) more than one-half.

lost its activity; its heart was still acting, and transferred to water it speedily recovered.

The activity of the minnow, and the display which it made of muscular power when taken out of water were remarkable. One an inch in length sprang a distance of five inches, and this even some minutes after exposure to the air. Like the trout, as soon as it has been forcibly taken from its native element, it makes a succession of bounds, as if from an instinctive impulse to return; and, as the banks of a stream are commonly shelving, often, if not stopped, with success. I have witnessed the exercise of the same impulse in the salmon and other Salmonidæ, nor is it, I believe, peculiar to them.

Experiments similar to the preceding on the minnow I have made on the trout and perch, with, on the whole, like results, but less strongly marked; in one or two instances, indeed, with the latter fish, life was sustained as long in the confined air saturated with moisture as in the open air of ordinary dryness.

Reasoning on these results, they are not what might have been expected. I was induced to institute the comparative trials from finding that Dr. William Edwards, in his very interesting work "On the Influence of Physical Agents on Life," considered the main cause of the death of fish out of water to depend on the loss they sustained from evaporation, or, as he expresses it, from "perspiration." Now, as there could be little loss of this kind in air nearly saturated with moisture, as was shewn by the slight difference of temperature of the two, tested by a delicate thermometer introduced into the gullet of each, his explanation cannot be considered satisfactory. Perhaps the drier air may have a more stimulating effect than the moist air, and may be more readily absorbed. But this is mere conjecture. An influence, we know not of, in these cases, may be operative, as obscure as that which occasions, as already mentioned, the sudden death of the torpedo when transferred to fresh water; or the extinction of the light of phosphorus in slow combustion in atmospheric air by the addition of a minute portion of carburetted hydrogen.

3. *On the Influence of Exhaustion of Air on the Vitality of Fishes.*

The experiments I have made on this subject have been few, chiefly the following:—

1. *Of the Loach.*—The same fish that was employed in the preeeding experiments was used in this, having recovered its aetivity after having been in eool water for a day or two.

It was put into spring water from whieh the air had been previously well exhausted by the air-pump. Placed under a reeeiver no larger than was neeessary to eover the vessel which contained it, the pump was worked for a few seeonds to remove the incumbent and adhering air. After some brief rapid movements the fish became stationary, as if all but dead, the gill-covers aeting feebly. The working of the pump was renewed and eontinued. Air was disengaged in bubbles from the surface, and in larger quantity per anum. Now, from time to time, it moved quickly about, projeeting its head out of water, its gill-eovers at rest. Air-bubbles did not eease to be diseharged per anum and from the skin, and last of all it was extrieated from the fins, which were seen to be distended with air, and the anal fin most remarkably. Death took plaee in about fifteen minutes: it was not easy to fix the exaet moment of time, as the loach moved bodily after the opereula had ceased to act. Opened immediately, the heart was motionless; the blood was fluid.

2. *Of the Trout.*—A trout six and a half inehes long, was submitted to the same trial as the loach. As soon as it was put into the water exhausted of air, it struggled very much; working the air-pump, the fish beeame motionless in about eleven minutes, the opereula acting at inereasing intervals. Air was given off in minute bubbles from the gills, and also more sparingly from the skin. Taken out and opened, the heart contraeted when touehed; the gills were pale; the air-bladder collapsed.

The experiment was repeated on a trout seven and a-half inehes long. After the fish had been put into water deprived of air, the pump was worked to extraet the air in the receiver, merely for this purpose, and not eontinuously as before. In about fifteen minutes, the trout beeame motionless, and proved to be dead. No air had been disengaged from its surfaee; opened, the air-bladder was found not quite eollapsed; the gills were redder than those of the former.

3. *Of the Minnow.*—A small minnow was put into a vial full of water well exhausted of air; another, a larger one, was put into another vial, containing an equal quantity of water from

which the air had not been exhausted; both were closed with glass stoppers. Each minnow became motionless and dead in about the same time—an hour. They were immediately opened. The heart of each had ceased to act. The air-bladder of the one which had been in the water exhausted of air was collapsed; that of the other was in its ordinary state of distension. The gills of both were congested with dark blood.

A minnow was placed on the plate of the air-pump, and after having been covered with a small receiver, the contained air was rapidly exhausted. In ten minutes, not before, the fish was motionless. Taken out and put into water, the opercula renewed their action two or three times very feebly, and then ceased altogether, death occurring. Opened, the air-bladder was found collapsed.

Another minnow was put into water, about five cubic inches and a-half in volume, from which the air was rapidly exhausted by working the pump continuously. Not until thirty-five minutes had elapsed was the fish motionless; taken out, it proved to be dead. Opened, the heart had ceased to act; the air-bladder was quite empty, and of course collapsed.

Another minnow was put into water exhausted of air, and after being placed on the plate of the pump and covered with a small receiver, the included air was exhausted and no more, not working the air-pump after the vacuum was effected. In an hour and twenty minutes, and not till then, had the fish become motionless, and life extinguished. Opened, the air-bladder was found collapsed.

These results seem noteworthy, as shewing:—

1st. How long a fish can live without air.

2ndly. In the instances of fish which have an air-bladder, as the minnow and trout, how life is probably prolonged in consequence of the air in that receptacle—*i.e.*, on the supposition, from its collapsed state, that the air it contained was gradually expended. The duration of life, which somewhat varied in the trials, might perhaps have been owing to the air-bladder in the several instances containing a different quantity of air, or different proportions of oxygen.

3rdly. How, in the comparative trials, life was sustained at least as long, if not longer (in the instance of the trout), in equal quantities of water—one containing its ordinary quantity of air, the other deprived of air by the pump; as if, in the former, the carbonic acid gas formed in respiration had a noxious effect equal at least to the total privation of air.

Lastly, in the instance of the loach (a fish without an air-bladder), and in a less degree in that of the trout, it is remarkable how, on exhaustion, air was extricated from the fish, and from so many parts of the body, especially from the fins, in which, owing to their transparency, it might be seen, distending them, and at the same time passing out. The source of this disengaged air may be a question—whether derived from the blood, or the alimentary canal, or from both.

All the experiments described, and some more affording similar results, were made within two or three days of each other, towards the end of August. The air-pump used was in excellent working order.

XXXII. FRAGMENTARY NOTES ON THE GENERATIVE ORGANS OF SOME CARTILAGINOUS FISHES.

These notes have been made at different intervals of time, and in different places—some, and the majority of them, at Malta, in 1832-33, some at Constantinople in 1839-40, and a few at a still earlier period, viz., in 1816, when on a voyage to Ceylon.

Imperfect and brief as many of them are, I am induced to give them, thinking they may be of some use as conveying the results of unbiassed observation, and that as such they may prove a small contribution to a difficult branch of ichthyology—difficult, not indeed so much from the nature of the subject as from the comparatively few opportunities enjoyed by naturalists of obtaining specimens.

In accordance with the heading, I may premise that, in the details to be given, I shall do little more than transcribe the account of the particulars observed, and nearly in the words employed at the time of noting them down, and this, though the terms may not always be of the most approved and correct kind.

The only general remarks I shall have to offer will be a few in conclusion.

1. *Of the Squalus Squatina.*—The notes I have on this fish were all made in Malta. The subjects of them were two females in a gravid state, and the generative organs detached of other eight, which were procured from the fishmarket of Valetta—the Squatina being a fish there in some request amongst the lower classes as an article of diet. I shall give them nearly in the order in which they were made, submitting a brief notice of the organs in question, conveying the idea I have been able to form of their general structure.

In most respects they are very similar to the same organs in the torpedo.* Like them, they may be said to consist chiefly of three parts: the ovaries, situated high up above the liver; of oviducts, with a common infundibulum, and of two uterine cavities, expansions as it were of the oviducts. Each oviduct has

* See Physiol. and Anat. Res., vol. i., p. 55, for an account of these organs.

two glandular bodies, one above the other, not unlike the one belonging to the oviduct of the torpedo, but somewhat larger, and its transverse striæ more strongly marked. The uterine cavities differ from those of the torpedo, in being smooth and entirely destitute of villi. During gestation they seem to be virtually closed, so that though a probe can be passed, both in the direction of the ovaries upwards, and in that of the cloaca downwards, yet they are capable of holding a fluid, of which a certain quantity has always been found present associated with the contained ova and their embryos. These cavities, at least in the early period of gestation, have been found to communicate with the cloaca by two openings close to the papilla, in which is the common passage from two urinary bladders. Whether these openings do not become one at a more advanced period, I am doubtful.

The first specimen examined was procured on the 30th of August. Each uterine cavity was found to contain two ova with an embryo attached to each by an umbilical chord,* in the midst of much transparent colorless fluid, without any traces of a common enveloping membrane. The ova were large; each weighed about 2¼ oz. They consisted entirely of yolk, and, like those of the torpedo, they had two membranes, one internal, very delicate and transparent, of little more consistence than that of the albumen ovi of the fowl, but thickening towards and in the chord, the other internal and vascular. The embryos were all small, and of about the same size. The branchial filaments were very short, and of a bright red color; the eyes large and projecting, the mouth and gullet very large, the stomach very small, the intestine large and empty, the liver large.

On the 31st of the same month another specimen was procured. Three ova, with embryos, were found in one uterine cavity; two in the other. They were nearly in the same stage of development as the preceding. One egg with its embryo weighed 3½ oz.; the embryo 22 grs.; another embryo 17 grs. The former measured 1·7 inch in length, ·45 inch in width.

* I use this term for the sake of convenience in its ordinary sense, and not being aware of any sufficient reason for discontinuing it, seeing that the part performs the same office as the umbilical chord in the mammalia, connecting the embryo with its source of nourishment: moreover, a mark remains of it, denoted by a depression, after its removal by absorption, which may be called an umbilicus. This at least I have seen in the young torpedo. See my Res. Physiol. and Anat., plate vii., fig. 1, in which it is shewn.

Some yolk was found in its intestine. The branchial filaments of both were about the same length as the preceding. The other eggs were not weighed; their membranes were so delicate that they broke in the attempt. The oviducts entire, including the uterine cavities and their contents, weighed 22½ oz.; emptied of their contents, their weight was 1¾ oz. and 29 grs. Now, supposing the weight of all the five eggs to be nearly the same, the weight of the fluid in both cavities would be about 4¼ oz. The cavities differed from the preceding, in being distinctly vascular.

On the 12th of September, a female fish was examined that weighed 6 lbs. An embryo was found in each uterine cavity, and attached to one of the ovaries a large egg. Its membranes had been broken; but from what remained, it might be inferred to be of its full, or nearly full size, and ready, or nearly so, to be detached, and to pass into the infundibulum. Many small ova were contained in the ovaries. Both embryos were very small, with short branchial filaments. Some small tortuous vessels, conveying fluid blood, were seen on the inner surface of the uterine cavities.

On the 13th of September another specimen was obtained. In each uterine cavity were two ova, with an embryo attached to each. One egg weighed 4½ oz. and 40 grs.; the embryo 28 grs. An egg from the other cavity weighed 4 oz. 5 drs.; the embryo 26 grs.: the other egg 4½ oz. 40 grs.; its embryo 30 grs. This embryo was 2¼ inches in length. Its branchiæ were beginning to be covered; its branchial filaments were red and very short. No yolk substance was found in its intestine.

On the 30th of September, a specimen then procured contained an unusual number of ova, four in one cavity, three in the other. The eggs weighed with their embryos attached, and the latter after their separation, gave the following results:—

	Eggs and Embryo. oz.	drs.	Embryo. drs.	grs.
1	3	5	1	46
2	3	5	1	57
3	3	6½	1	46
4	3	4¼	1	57
1	3	6¼	1	50
2	3	3⅛	1	50
3	3	6	1	55

Four of the embryos were females, three were males. In most of them the branchiæ were no longer naked, and the branchial filaments had disappeared. Put into fresh water, some of them shewed signs of life—a movement of their gills was perceived. The internal yolk-membrane was stronger than that of any of the preceding at an earlier stage, allowing the egg to be lifted without breaking, the thickness increasing towards the end. The internal membrane was beautifully vascular. There appeared to be two orders of vessels, their branches anastomosing, one conveying a brighter blood than the other; the vessels conveying the former smaller than those conveying the latter. The intestine of two embryos was examined; a little greenish matter, but no yolk, was found in it; yet, using the blow-pipe, air passed pretty freely into it through the vitello-intestinal canal. The gills were similar to the preceding. The brain was distinctly formed; the kidneys were comparatively large; all the fins were distinct. The quantity of fluid in the uterine cavity was considerable.

In a specimen procured on the 2nd of November, an embryo was found in each uterine cavity. The ovum of one was broken; the other, which was entire, weighed, with its embryo, 3 oz. 6⅛ drs.; the weight of the embryo detached was 3 drs. and 11 grs. The gills were no longer naked, and they were without filaments. One embryo was opened; the substance of the egg was found passing into the intestine through a straight canal; the intestine was distended with a greenish matter, colored by bile. The ovum presented a beautiful vascular appearance.

On the 9th of the same month another specimen was obtained. Two ova, with their embryos, were found in one uterine cavity, one only in the other. All the eggs were broken but one. It, with its embryo, weighed 3 oz. 3⅓ drs.; the embryo alone weighed 1 dr. The branchial filaments were short, but distinct. The stomach was very small and empty. A little greenish matter was found in the intestine. A considerable quantity of fluid, as usual, was found in each uterine cavity, and of its ordinary appearance, clear and transparent, and colorless, and slightly saline to the taste; a portion of it evaporated yielded a considerable quantity of coagulated albumen; washed with alcohol, the solution obtained slowly evaporated, frothed at a temperature considerably below the boiling point, giving the idea of the presence of urea. When evaporated to a moderate degree of consistence, a drop of strong nitric acid was added, and an immediate formation of white matter took place: this at the time I supposed to be nitrate of urea, as it dissolved on the addition of a little water, and as, when evaporated in its turn, a solid matter appeared in minute white scales, here and there giving off gas from decomposition: with these scales were intermixed a few minute prismatic crystals.

On the 8th of February, the last specimen of which I have to make mention was obtained. This was a fish of about two feet in length. The oviducts, with the uterine cavities, formed a complete circle. The uterine cavities were thin, distended, and vascular, and were lined with much thick mucus or mucus-like matter. They contained each a single fœtus. Each fœtus was about six inches long, and appeared pretty perfect in form. The eggs were still large. One fœtus was opened; a yolk-sac was found in the cavity of the abdomen, freely communicating with the outer yolk, and with the upper part of the intestine. The intestine was distended with the substance of the yolk, which in its lower portion was of an orange hue. One of the young fish was in part corroded, as if by the action of the fluid with which it was in contact: it shewed no signs of putridity.

2. *Of the Squalus Galeus.*—The only notes of the generative organs of this species which I have were made in Malta, and are very brief. On the 25th of January a female was procured from the market. Its uterine cavities were semi-transparent, and lined with a very vascular chorion. Each cavity contained three young fish; and each of these was included in a very delicate membrane, together with some gelatinous fluid. There was no appearance of an internal yolk; but to each fœtus was still attached the residue of the umbilical chord, still vascular; its floating extremity its thickest part. When the fœtal fish were opened, an internal yolk-sac was seen in each, communicating with the upper portion of the intestine. The sac was distended with the substance of the egg, and a substance of the same kind was found in the intestine.

From one of the ureters of the parent fish a fawn-colored matter was pressed out, semi-fluid, not unlike lithate of ammonia. With equal parts of nitrate acid and water it effervesced, and frothed when heated; but it became brown, not purple. There was not sufficient for further examination. It seemed to resemble the urine of the torpedo. May it not be a peculiar kind of animal matter?

3. *Of the Squalus Acanthias.*—The notes on this fish were made in part in Malta, and in part at Constantinople. The specimens examined were eight, one only of which was a male.

This male fish was procured from the fish-market of Galata. It was about two feet and a half long. Its anal appendages were of moderate size, composed of muscles and cartilages. Each organ communicated by a canal with an abdominal sac. These sacs, situated immediately under the common integuments, one on each side of the mesial line, were lined with a smooth, very vascular membrane, and contained a little opaque fluid, consisting, as seen under the microscope, of minute granules. The canal or duct of each terminated on the inner surface of its corresponding appendage. The appendages were without the glandular body met with in these organs of the rays, but each contained what I believe to be an auxiliary heart, such as I have described as occurring in the *Raia batis.** The proper generative organs were well developed. The testes, situated high up under the liver, were of a large size, about 3½ inches long by ¾ inch broad, rounded at their extremities, of a pale hue, and indistinctly mammillated, as if composed of no well-marked lobules. Each was bordered by a milt-like appendix, similar to that belonging to the testes of the *Raia clavata,* of which a figure is to be found in the work just quoted. This latter part was connected with the epididymis, or commencement of the vas deferens, by several straight tubes passing across, and included, in a delicate peritoneal fold. The epididymis superiorly was small, the vas deferens there composing it being very slender and convoluted, but not collected in a mass as in the instance of *Raia clavata.* As it descended, still tortuous, till about 2½ inches from its termination, it suddenly enlarged and became very capacious, continuing so till it terminated in the common receptacle of the spermatic and urinary fluid—*i.e.*, that receptacle in which the ureters end as well as the vasa deferentia. The capacity of each vas deferens, when expanded, was at least equal to that of the common receptacle; each contained about half a cubic inch of a creamy yellowish fluid. The same kind of fluid was contained in the common

* See Physiol. and Anat. Res., vol. ii., p. 451.

receptacle, but of rather thicker consistence. This fluid, microscopically examined, was found to abound in spermatozoa. They were seen also in the fluid of the vas deferens, but not in the epididymis, testis, or in its milt-like appendix. The fluid obtained from these exhibited only granules similar to those found in the like parts in the *R. clavata,* suggestive of a growth, in transition, from granules into spermatozoa. The common receptacle or bladder terminated in a rudimentary penis, projecting about one-third of an inch into the cloaca, and about half an inch from the verge of the anus. The spermatozoa, of a spiral form, extremely fine at each extremity, were very long in proportion to their breadth—at least thirty-two times longer. Their length was about $\frac{1}{500}$ of an inch.

The female generative organs of the *Acanthias* have a considerable resemblance to those of the torpedo, especially in the circumstance that the uterine cavity, when gravid (and it does not appear to exist except in this stage), has a distinctly villous structure.

All the female fish examined, with the exception of one, were procured in Malta.

The fish obtained at Constantinople was got from the market on the 17th February. It was shorter than the male fish already noticed, being about two feet long, but proportionally thicker. I expected to have found it gravid, but it was not; the generative organs were little developed. The ovaries, situated high up under the liver, were each about the size of a sixpence, and each only a few lines thick. They contained a small number of ova, the largest not bigger than a peppercorn. These, cut open, yielded a little glairy fluid, which, under the microscope, exhibited globules and granules. The oviducts were very small—so much so as to be traced with difficulty. To each of them, near the ovary, a glandular body was attached, of about half an inch in length.

On the 3rd of March, in a female about a foot and a half long, an egg was found in each uterine cavity. It was of a long oval form, within a delicate transparent membrane, containing a little clear fluid. There was no appearance of fœtal development. The uterine cavity was of a bright vermilion color, and covered with villi. The ovaries were situated nearly as in the torpedo; each, containing a small cluster of ova, was attached to the peritoneum. The oviducts joined the infundibulum over the superior margin of the liver. In each, a little higher than the uterine cavity, was a glandular structure.

On the 10th of March two uterine cavities, which had been found gravid on opening the fish in the market at Malta, were brought to me. In each were two embryos with the ova to which they were attached. They were free—that is, without any including shell or membrane. The lining coat of the cavity was strongly villous; the villi projected two or three lines; the surface, moreover, was beautifully vascular, and of a bright vermilion hue. The villi appeared to be formed of looped blood-vessels. The yolks belonging to the two embryos in each cavity were somewhat different, comparing the external and internal portion; in one instance the outer yolk bag was reduced very small—the inner had become pretty large; in another the case was the reverse—the outer was the largest. As might have been expected, there was a correspondence in the size of the young fish: the development of that to which the smaller internal yolk belonged was farthest advanced.

It may be mentioned that one of the uterine cavities, which made a very beautiful appearance from its vessels being distended with vermilion blood, was put into distilled vinegar; and that, when examined two days after, the vessels were found to contain air, as if extricated from the blood during its partial solution.

On the 1st April two small fœtal fish, each about two inches in length, attached to their ova, were procured from the market. On extraction from the parent fish they had been put into water, according to instructions given to the Maltese fisherman, and which were observed in other instances. The branchial filaments were nearly an inch long, and were numerous. The head was large, the eyes very large; distinct marks of spines were apparent anterior to the dorsal fins.

On the 15th April a fish was obtained in which several eggs were found, nearly of their full size, attached to the ovaries. In one uterine cavity there was a single fœtus; in the other, two. No membrane enveloped them; they seemed nearly fully formed, and were in immediate contact with the villous surface. This was very vascular—its colour bright red. Two of the young fish were opened. The yolk of each egg was in part internal;

but the inner yolk was small in comparison with the outer, and the sac containing it was even less in size than the intestine, which was distended with yolk substance. The stomach was empty. The communication between the gut and the inner yolk sac—the vitello-intestinal canal—was sufficiently large to allow of the free passage of an ordinary surgeon's probe. The contents of the intestine were of a brighter yellow than the yolk in either the inner or outer sac.

On the 1st October, in a small fish then examined, a single fœtus tolerably advanced, destitute of branchial filaments, was found in each uterine cavity. In one of the ovaries were minute ova about the size of a millet seed; attached to the other were some that were pretty large—about the size of a boy's playing marble; their enveloping membrane was highly vascular.

On the 22nd of the same month a fish was obtained about two feet long. An ovum was found in each of its uterine cavities, contained in a delicate transparent capsule, which, towards its ends, had a light olive hue and a slight horny appearance. It contained a considerable quantity of white, a pretty large yolk, and an embryo about 1⅓ inch long. Large vessels passed from the yolk by the chord to the embryo. About one-half of the yolk's proper membrane, at each side of the chord, at its margin, was beautifully vascular. The eyes of the embryo were large; the fins very small—only just appearing on the back, the pectoral more distinct. The branchial filaments were long, and of a bright red. Besides these filaments there were others similar, proceeding from the head, its back part, and also from each side of the abdomen, in a line extending from the pectoral fin. Each filament—the branchial are specially mentioned—on careful examination with a lens, was found to contain four blood-vessels terminating in loops.

In a fish procured on the 15th of May, of about the same size as the last, two eggs were found in each uterine cavity, with an embryo attached to each. The ova and embryos were contained in one common, very delicate, and transparent shell. Each embryo was in the same stage of development; each about three-quarters of an inch long. Viewed with a magnifying glass, its eyes were distinct and proportionally large; they were almost colorless, with hardly a trace of a pupil. The mouth was proportionally large, and apparently expanded wide.

It gave the idea of the jaws being formed rather than the mouth itself. The branchial cartilages were distinct. Two or three short filaments were pendant from them on each side. Close to where the umbilical chord entered was a red spot—the heart. A vessel carrying red blood extended from it to the tail, and returned. The pectoral fins were small, the dorsal only just appearing, the tail gradually tapering. An attempt was made to lay open the cavity of the abdomen, but it failed, though using a very delicate and sharp scalpel, owing to the great tenderness of the parts—it was torn rather than cut. The cartilaginous skeleton throughout seemed to be formed. The ovaries contained ova of different sizes; the largest were about the size of large cherries, the smallest about the size of a millet seed.

Another fish, also of about the same size as the two preceding, was obtained on the 13th of June. Two eggs were found in each uterine cavity, with an embryo attached to each, in about the same stage of development as the last mentioned. Besides the branchial filaments, there were two filaments proceeding from the head, just behind the spiracula—the water passages.

4. *Of the Squalus Carcharias.*—On a voyage to Ceylon, when within the tropics, an opportunity occurred of making a hasty examination of two gravid fish of this kind. In the uterine cavity of one, designated a small shark, taken in lat. 8° 23′ N., four fœtal fish were found, each about a foot long, with "a placenta" attached to each. From three the placenta was immediately removed—cut or torn off. These fish died almost instantly. The one from which it was not removed lived in the open air at least three hours after its extraction. Its stomach and intestines were both found empty; no yolk was detected internally. No mention is made of any including capsule, seeming to warrant the inference that no membrane of the kind remained, and that the young fish were in contact with the walls of the uterine cavity.

The other shark, which was called a large one, was taken in lat. 2° 34′ N. In its uterine cavities nine fœtal fish were found, five in one cavity, four in the other. Each was contained in its own membrane, full of "liquor amnii," and each was connected with "a placenta" by a long "umbilical chord." All of them were about the same size, about two feet long. When extracted and thrown on deck, they were active and vigorous. Though without advanced teeth, two or three of them were seen to make an effort to bite a stick thrust against them.*

The so-called "liquor amnii" was very salt to the taste, was slightly viscid, not quite transparent, and of a light grey color. A few white flocculi were suspended in it. When boiled, it did not coagulate or undergo any apparent change. Evaporated, it thickened, became brown, and ultimately black from charring, when it emitted much smoke and a strong ammoniacal odour.

In the stomach and intestines of the parent fish four different kinds of parasitical worms were observed, two in the former, two in the latter.

5. *Of the Squalus Centrina.*—In the month of March, when at Constantinople, I procured two fish of this kind, which had been taken in the Sea of Marmora, and, it is worthy of remark, by the same cast of the net: they were male and female.

The male fish was about 2½ feet long, and rather slender. The testes were pretty large, each nearly of the form of a date, its surface vascular, smooth, and equal; its substance soft; when cut into it yielded some opaque fluid, which, under the microscope, was seen to abound in globules. The milt-like part superiorly was thin and small; cut into, it yielded a milky fluid, in which, under the microscope, numerous globules were seen, and one spermatozoon. The epididymis, itself small, was connected with the milt-like part by four or five delicate tubuli; these, divided under water, yielded a little milky fluid, also abounding in globules similar to those of the milt-like part. A milky fluid was also obtained from the epididymis, from its superior portion. This was rich in spermatozoa; it contained besides a few globular particles. The vas deferens and the vesicle in which it terminated yielded a cream-like fluid, rich also in spermatozoa. The vesicle in which probably the ureters also terminated (it was not ascertained by dissection), communicated with the cloaca through a papilla, the rudimentary penis.

* Other instances of a like kind might be mentioned, shewing how provident nature is in giving instincts and organs to young animals, suitable to their protection when in their feeblest state, and their lives, in consequence, most in danger. The fœtus of the torpedo, even before birth, I have found capable of giving a shock. In the fœtus of the viper (*Coluber berus*) I have found the poison fangs developed. The young alligator I have seen, as soon as it left the egg—and that prematurely, from the egg being broken—make to the adjoining water, and, if stopt, attempt to bite the arresting object.

The spermatozoa were all similar. Many of them were collected in a cluster. They were all motionless in fresh water and in brine; but in salt water—that of the Bosphorus*—many of them were active. They were found to vary in length from about $\frac{1}{400}$ to $\frac{1}{200}$ of an inch. The diameter of the rounded extremity was about $\frac{1}{4000}$ of an inch. The anal appendages were large, and proportionally thick. Each communicated with a subcutaneous sac similar to that of *S. Acanthias.* The cavity of each was about an inch long, and follicular. The appendages, in their general structure, were "similar to those of the rays and squali."

In my "Researches Physiological and Anatomical," when treating of the male organs of cartilaginous fishes, I ventured to offer the conjecture—an old opinion—that the anal appendages, the characteristic of the male fish, are designed for the purpose of intromission in the performance of the generative act, and I then quoted a passage from Aristotle to the same effect: "Sunt qui se vidisse confirmant non nulla in cartilagineis aversa modo canum terrestrium cohærere." In examining these two fishes, I found what appeared to me to be circumstances favorable to the above supposition. In the instance of the male, the generative organs, as described, were clearly in the condition required at the breeding season. Those of the female were found to be so also. The female, about one-third larger than the male, exclusive of the anal appendages, was similar to it in form and appearance. The cloaca, the common opening—that in which the intestine and uterine cavities terminated—was sufficiently large to admit the appendages; and it is worthy of remark, that the part was slightly lacerated at its superior commissure; also that the mouths of the uterine cavities were protruding, and were very red and vascular. Within the cloaca, between the two uterine openings, above the opening into the intestine, was a clitoris, if I may so call a vascular conical projection, of about one-eighth of an inch in length, through which was a passage from the urinary bladder. The bladder was of a globular form, and pretty large; two ureters terminated in it, at its upper end. The ovaries contained ova of different sizes, the largest about the

* This water is less salt than that of the sea—the Mediterranean and ocean—nearly the same as that of the Euxine. I have found it of specific gravity 1,012, that of the Euxine being 1,011.

size of a boy's playing marble. They were enveloped in a delicate vascular membrane. Their contents were of a soft consistence, like the yolk of the egg of the common fowl, and of a light cream color. Above the liver was situated the infundibulum of the oviducts. These ducts were thin and plicated. About two inches above the uterine cavities, on each side, was a glandular body, forming a part of the oviduct. The uterine cavities were long, wide, and capacious. Their superior opening was small, their inferior large. Their inner surface was red, and covered with villi—these about a quarter of an inch long; they were well displayed by immersion of the part in water. Both the oviducts were empty, as were also the uterine cavities. A little fluid lubricating the latter was scraped off and subjected to the microscope; some blood corpuscles were seen in it, and minute globules, and also two spermatozoa, respecting which it is said in my notes, "I think there can be no mistake, their form being so peculiar."

6. *Of the Raia Aquila.*—Of this fish I have notes of two specimens only, both procured in Malta, and both females.

The first was procured on the 12th of April. In each of its oviducts was a large membranous shell, which, independent of its horns, was about five inches long. One of them opened, was found to contain a yolk about the size of the yolk of a hen's egg; a considerable quantity of glairy white enveloped it. The shell externally was nearly black, rough, tough, and very strong: internally, it was lined with a very delicate white glistening membrane. Above that part of the oviduct in which the egg was contained was a large globular body surrounding the duct, in appearance, as to structure, more like a testis than any other that at the instant I could call to mind. The lower end of the oviduct terminated in the cloaca; it was so contracted that the little finger was introduced with difficulty. There was no appearance of development in the ova.

The second fish was obtained on the 22nd of September. Though quite fresh, its ovaries and oviducts were for the most part reduced to a pulp, as if by a process like that which sometimes destroys the stomach, and has been referred to the action of the gastric juice. In this instance, however, the stomach was quite sound, without any traces of softening. It was of moderate size, full of *broken* food, suggesting the idea that this fish masticates its food, for which its strong, laminated molar teeth are so well adapted.

7. *Of the Squalus Canicula.*—Of this species I have notes of four, one of them a male. All were procured in Malta.

I shall first make mention of the male. It was obtained on the 11th of September. Its anal appendages are merely stated to be similar in structure to those of the torpedo, the lateral anal fins uniting behind them and partly covering them. The testes were very distinct, and situated high up in the abdominal cavity; the spermatic tubes large and tortuous, terminating in vesiculæ seminales, and these in a single papilla situated in the cloaca, close to the anus. When the vesiculæ were pressed, a thick creamy fluid was discharged, flowing from the papilla.

Of the other fishes, females, the first was obtained on the 22nd of April. A large cluster of eggs was situated over the spine: these of various sizes, the largest about the size of large cherries; their membrane was vascular, including a yolk. The upper part of each oviduct was also very vascular. The infundibulum was small. In the oviduct there were no ova and no enlargement. Each was provided with a glandular structure. Below the gland, where it is presumed the egg would rest and acquire its shell, the oviduct was very small and pale—not thicker than a crow quill—its sides in contact.

On the 15th of May another fish was obtained, of about a foot long. An egg was found in each of its oviducts, enclosed in a firm, hard shell, so hard as not to be easily cut. The ovum was of a brownish hue, and was surrounded by a glairy white. No traces of development could be detected in it.

On the 28th of August two ova, each in its shell, were got from the market, said to have been extracted from the left oviduct of a cat-fish. Each shell was about two inches long, and about half an inch wide, tough, and yet transparent, pointed at its extremities, from both of which a strong fibre proceeded. The fibres, drawn straight, that from one end measured about a foot in length, that from the other about half a foot. The largest was in part divided into several delicate filaments. The contained ovum, seen through the transparent shell, was situated midway. In one shell that was opened, a yellow yolk was found in a small quantity of colorless, transparent, and "very viscid white." This "white" did not mix readily with water, and was not coagulated by nitric acid.

On the 5th of September two fish were obtained, both of them gravid. The condition of the generative organs of each was similar. In each oviduct an egg was found, surrounded by white, in a semi-transparent shell. One was immersed in boiling water. The yolk became hard after having been boiled about two minutes; the white did not coagulate, nor undergo any apparent change—it remained transparent and viscid. On each side, above that part of the oviduct holding the egg, and about an equal distance from the infundibulum, was a glandular body surrounding the tube. The infundibulum was large, and very vascular. The ovaries, joined together, lay in the direction of the spine, about half way between the oviducts, and about the same distance from the infundibulum. In them was a cluster of ova of different sizes, connected by a loose cellular tissue. The smallest of the cluster were about the size of mustard seed, hard, and opaque; the largest were nearly the size of the mature yolk, and spherical in form. Between the largest and the smallest there were many of intermediate grades. The largest were situated lowest, and consequently most distant from the infundibulum.

8. *Of the Scyllium Melanostomum.*—Of this fish I have notes of two, both procured at Constantinople—a male and a female—and both in the same month, February.

The male was about two feet long, and slender. Its testes were proportionally large, one on each side of the spine, not distinctly divided throughout. They were of a light fawn color and soft consistence, more resembling the testes of the osseous fishes than those of the majority of the cartilaginous kind. They tapered towards the cloaca, where it may be supposed their ducts terminated. The whole seemed homogeneous. Under the microscope, their soft substance seemed to be composed of globules, nearly transparent, of from about $\frac{1\frac{1}{2}}{4000}$ to $\frac{1}{2000}$ of an inch in diameter. Different parts of the organ were examined, without any difference of result; no capillary spermatozoa could be detected, and the globules, except when moving in currents, were motionless. The anal appendages were small. They were not specially examined as to their structure.

The female was also about two feet long, but thicker in proportion to its length than the male. At the time it was obtained, it was not quite dead.* The ovaries were large and long, extending nearly the whole length of the abdomen. Their upper portion abounded in ova, from the size of a grape seed to that of a mustard seed, and smaller. The larger were not perfectly transparent; they contained a turbid fluid, which, under the microscope, exhibited globules of about $\frac{1}{4000}$ of an inch in diameter, and smaller. The confining membrane was thick and strong. The lower portion—more than one-half of the whole—had a milt-like appearance. Under the microscope it exhibited globular nucleated particles, of about $\frac{1}{2000}$ of an inch in diameter. The oviducts were large—their infundibulum above the liver. To each oviduct a glandular body was annexed, just below which was a little enlargement of the tube; and towards the termination of each duct in the cloaca there was also an enlargement of it.

9. *Of the Raia Oxyrhynchus.*—Of this fish I have notes of two specimens, both examined at Malta, and both females. In one, opened on the 2nd of April, an egg was found in each oviduct, below its gland. The shell inclosing it was not perfectly formed; its lower moiety, which was perfect, was of a greenish brown, tough and strong; its upper portion was greyish, tender,

* The following experiments were made almost immediately after receiving the fish, when its muscular system was so active, that there was difficulty in keeping it on the narrow board on which it was placed on the table.

1. Divided the brain longitudinally with a strong knife: no marked effect.

2. Destroyed the brain by breaking it up with a probe: the head was quieter, but the rest of the body did not seem to be affected.

3. Passed a wire down the spinal canal, and destroyed, I believe, the larger portion of spinal chord: now in the upper part of the body there were no active movements, but they were undiminished in the lower.

4. Cut off the tail by a transverse incision just below the anus. The cut off part moved with undiminished activity; the spinal chord in it had not been touched or injured. When the tail-fin was pricked, the whole tail was moved, much in the same manner, as well as I could judge, as if there had been communication with the cerebrum. The experiment was repeatedly tried with the same result.

5. Divided the spine in the caudal portion, carrying the incision through the vertebræ, a little on one side so as to avoid dividing the spinal chord itself. Now on pricking the two sides the results were very different. On the side through which the incision had been carried, pricking had no effect; on the other side it had the same effect as before.

6. Cut out the spine of the caudal portion: puncture of the fin now had no effect in producing contraction of the muscles so as to occasion a movement of the part.

7. The heart cut out continued to contract for a considerable time, although not punctured, and with some regularity, and instantly when punctured.

8. So did the muscles also of that part of the body in which the chord had been broken up above the tail; the contraction was limited almost to the punctured place. When a mass of muscle was cut out, on puncturing it, the same effect was produced, though in a somewhat less degree.

and very easily broken. Much thick, tenacious, mucus-like matter enveloped it. There was no appearance of an embryo.

The other fish was obtained about the same time. The precise date is not given. In this instance, also, an egg was found in each oviduct. Each egg was contained in a horny shell, the horns of which were short, as if not fully formed. A tough glutinous matter, of the color of the shell, was found covering it, seemingly the material of which it was formed. The oviducts were red; their glandular structure large. The ovaries contained many ova; and there were several eggs loosely attached to the ovaries. They were of a spherical form—the largest about the size of the yolk of a pigeon's egg.

In conclusion, recurring to the preceding notes, it is worthy of remark, that whilst there is a certain resemblance to be seen in the generative organs of the several species, there are also well marked differences—differences which, it may be inferred, have relation to fœtal development. Under this head, do not the observations justify at least three divisions? 1st, The viviparous fish, of which the *Squatina* is an example, an instance, like that of the torpedo, of the ovum passing into the uterine cavity, and there undergoing its full development uninclosed in any shell or membrane. 2ndly, The ovo-viviparous fish, such as the *S. acanthias*, *S. galeus*, and probably *S. carcharias*, the ova of which, enveloped in a glairy white and contained in a delicate membrane, undergo their development in the same cavity. 3rdly, The oviparous fish, such as the *S. canicula*, *R. aquila*, the ova of which, provided with a horny shell the matter of which is secreted by one or more glands, are expelled from the oviducts before their development begins, and are hatched in the sea.

As regards the first division, are not the ova fully formed in the ovaries, and undergo no further increase of size after entering the oviducts? Also, as regards the fœtus, is not its growth in the uterine cavity not solely due to matter derived from the yolk, but in part to matter absorbed from the cavity itself? I am induced to suppose that this is the fact, from the analogy of the fœtal torpedo, which at its full time of birth I have found to be very much heavier than the egg;* and also from the circumstance that the uterine cavity, as I have seen both in the instance

* See Physiol. and Anat. Res., vol. i., p. 65.

of the torpedo and of the squatina, has become much thinner as the period of gestation advanced and approached its maturity, comparing it with the average of the organ earlier.

As regards the second, is not the common including membrane or capsule of the ovum and embryo found in the uterine cavity as a temporary provisional membrane? and is it not absorbed, in part or in whole, before the young fish quit the uterine cavity? Some of the appearances described under the head of *S. acanthias* and *S. carcharias* seem difficult of explanation except on this idea. The absorption of the membrane, whilst it may conduce to the exit of the young, may aid also their growth.

Further, are not what I have called "placentæ"—the cotyledons of Müller—residual masses of vitelline vessels—residuary after the absorption of the yolk—the view long ago entertained by a distinguished naturalist?† and, though differing from the true placenta, yet do they not exercise a similar function, supposing, as I believe was the case, that in the instance of the young of the *Carcharias* there was an active circulation in the mass, owing to which the fœtus that had not the vascular mass detached from it lived so long?

As regards the third, are not the ova of these fish all hatched in the sea, their development altogether taking place after being laid? That they are, I have been led to believe, not so much from my own limited observations of a negative kind, never having, in the examination of the eggs whilst in the oviducts, seen any traces of embryonic growth, as from the experience of the Maltese fishermen, who, in opening hundreds of the species, I have been assured, have never found a young fish included.

The branchial filaments of the embryo of the cartilaginous fish have commonly been considered as concerned solely in aërating the blood of the young fish. Have they not another use also?—are they not concerned, in a formative way, in promoting the growth of the part to which they belong? The circumstances that they are absorbed about the time that the gills

* See Hist. Nat. des Poissons, par MM. Cuvier et Valenciennes. The remains of the vitellus is described by the former (inferring that the first volume was written by Cuvier) as adhering to the uterus almost as firmly as a placenta. This I have never witnessed; nor have I ever witnessed, till at an advanced period, the interior lobe of the vitellus, which is described by him as always existing in the fœtus—"comme un appendice de l'intestin." See *loc. cit.*

become covered—*i.e.*, cease to be naked—and that they are not always restricted to the branchia, seem to favor an affirmative answer.

As to the use of the anal appendages of the male cartilaginous fishes, respecting which there has been so much difference of opinion amongst naturalists—some, as Rondelet, Willoughby, Ray, Artedi, Macri, De Blainville, following Aristotle in the opinion that they are penes, organs of intromission; others, as Bloche, Home, Cuvier, and most recent writers, maintaining that they are merely holders, "claspers," and in the generative act employed solely to embrace and retain the female—I have been led to prefer the older view mainly from the consideration of the structure of the parts, seemingly so ill adapted for the use last referred to, especially keeping in mind the glands with which they are furnished. Which of the two hypotheses is the correct one, can only be determined by further and careful observation. The fact I have mentioned under the head of *S. centrina* must be admitted, I think, to favor most the old opinion. Theoretical arguments might be used in support of the same; but these, at best, cannot compel conviction.*

* Macri, in Atti della Reale Accademia Scienza (of Naples), vol. i., uses a very ingenious argument of the kind above alluded to: "In natura osservi una legge costante ed invariabile, stabilata dall' omnipotente, che quando gli animali maschi son corredati d'una sola verga, le lor femmine hanno eziando una sola vulva ed un sol utero. E all' opposto, dove le medesime son provvedute di due vulve, o d'una bifurca, e die due uteri, o d'un uteri bifido, posseggno i maschi o una verga bifida o un doppio membro generatore" (p. 83).

XXXIII. MISCELLANEOUS FRAGMENTARY NOTES.

These notes, like the preceding, were made at different times and various places, chiefly in Ceylon, the Ionian Islands, Malta, Constantinople, and at home. I shall give them with little or no comment, excepting such as may have been written at the moment they were made.

1. *On the Eggs of the Turtle* (*Chelonia midas*).

A large one, a female, taken at the Isle of Ascension, was a present from the master of a whaler, which communicated with our ship on her voyage to Ceylon in 1816. Here I shall extract from my notes only the following relating to the ova, a subject —the ova of animals generally—which, as regards their qualities, has hardly received the attention it deserves.

When the turtle was opened, 200 fully formed eggs were found in the abdominal cavity, each about the size of a billiard ball, and apparently as perfectly spherical. Their shell was tough membrane, without any calcareous incrustation. When boiled, their white coagulated imperfectly, acquiring the consistence of soft jelly; the yolk was hardened more, but a degree or so less than the yolk of the egg of the barn-door fowl. As many as 1,360 eggs in addition were counted attached to the ovaries, these consisting merely of yolk; and besides them there were very many which were not counted, varying in size from a boy's playing marble to a pin's head, affording thus a wonderful example of fertility.

2. *On the Heart of the Shark* (*Squalus carcharias*).

A female, taken on the same voyage, on the 15th of March, in lat. 4° 9′, with hook and line, was seven feet long. It was a considerable time before it was drawn on board. Its stomach was found distended with water. The only part of the fish which I examined with some care was its heart. The ventricle had very thick parieties, formed chiefly of muscular bands by their intersection, producing a delicate retiform appearance, without columnæ carneæ. The auricle was capacious, its color of a paler red than that of the ventricle, and its retiform muscular structure not so strongly marked. At its entrance, between it and the venous trunk, was a circular valve of a still paler hue, about a quarter of an inch wide, its margin loose, projecting inwards, so as effectually to prevent on contraction any reflux of blood. Between the auricle and the ventricle there were four valves, two large and two small, alternately placed, nearly semi-lunar, thinly membranous, with fine tendonous insertions attached to the muscular substructure. The bulb of the pulmonary artery rising from the ventricle was provided with three rows of valves, —the upper row the largest and deepest; the lower the smallest; the upper most acuminated. Their form, especially of the upper, was an approach to the pyramidal; in substance they were thick, of a reddish white, and of a fibrous structure. Like the other valves, they were admirably adapted to prevent the reflux of blood into the ventricle.

The same morning a smaller shark of the same kind was caught. In its arterial bulb only two rows of valves were found. Its pericardium, like that of the former, was a strong unyielding membrane.

3. *On the Python* (*P. reticulatus*).

This snake in Ceylon bears the name of pimbera. The following are notes taken in 1819 of one I examined in that island, seventeen feet long, which was killed four months after its capture, during which time it ate nothing, refusing the different kinds of food which were offered to it.* During the period it shed its skin once, and was in the act of shedding it again.†

* In the stomach of a smaller snake of this kind, which had devoured a chicken three days before being killed, nothing was found but some bones, these partially digested, and the claws and feathers. The upper portion of the intestine contained a dark mucous fluid; the lower, what was inferred to be the contents of the crop of the chicken, grains of paddy unaltered, gravel, etc. These particulars seem to prove that the python's digestion of animal food is pretty rapid, as well as its inability to digest vegetable food, such as rice. It is commonly supposed that snakes feed on nothing dead. This is not strictly correct. I find by a note made in Ceylon, that a cobra-capelle which had refused food for about three months devoured a large frog which was given to it when dead: about twenty-four hours after the frog was disgorged in a soft, almost pultaceous state.

† I found the cuticle of another snake of this kind, the one referred to in the preceding note, composed chiefly of animal matter and phosphate of lime, the latter to the amount of 7·5 per cent.

The cuticle was loose, as was also the cornea of each eye; the cutis was very vascular. The moulting did not appear to extend to any of the mucous membranes. It had a gland very like a lachrymal gland, and six muscles to each eye.

The rings of its long larynx were bony anteriorly, membranous posteriorly where in contact with the œsophagus.

Its long sacculated lungs had a beautiful reticulated structure, increasing in delicacy from above downwards; a transverse section displayed spiral fibres, which contracted in spirit of wine, and were probably irritable. The lungs descended nearly to the pylorus; between them and the abdominal cavity there was a membranous septum.

In the pericardium there was a little transparent serum. The heart, pretty large, was composed of two auricles which had no direct communication, and of two ventricles which communicated freely. The right auricle was large, its parieties thick. The venæ cavæ terminated in a sinus, which, at its junction with the auricle was furnished with two semilunar valves, closing perfectly. This sinus appeared to have muscular fibres. Where the auricle opened into the ventricle there was a large semilunar valve, capable of closing the passage completely. The left auricle was smaller than the right, and not quite so thick. One pulmonary vein, and only one, terminated in it; before its entrance it seemed a little dilated, and there muscular. At its entrance there was a narrow transverse semilunar valve, formed of muscular fibres, which must act as a valve. The auricular-ventricular opening was large, had the same kind of valve as the right, but more delicate. The right ventricle may be described as formed of two compartments, divided partially by a muscular septum. From the right compartment the pulmonary artery took its rise, with two strong semilunar valves. A little above its origin it sent off a branch to each lung. From the left compartment two great arteries sprung, each furnished with two strong semilunar valves. One, about two inches from its origin, sent off a considerable branch to supply the head and neck with blood. A little lower the two main arteries united to form the descending aorta. The left ventricle was very small, about half the size of the left compartment of the right; its parieties very thick and strong. It communicated with this compartment by a large passage. No artery rose from it. Supposing both sides of the heart to contract at the same time, venous blood would be sent into the lungs, and a mixture of venous and aerated into the system.

The stomach was long and, as seen in its empty state, plicated; the pylorus very small. The intestine was straight; its mucous membrane reticulated. The cloaca was separated from the intestine by a strong sphincter muscle, and there was a sphincter between the latter and the anal aperture. The ureters did not terminate in the cloaca, but close to the anus, so that the urinary excretion must flow back into that receptacle. When their papillæ, which were close together, were pressed, there was a considerable flow of white semi-fluid lithate of ammonia, and in the cloaca there was a large quantity of the same, in part of butteraceous consistence, in part as coarse gravel. The intestine contained a good deal of dark green fæcal matter, in which were found included fragments of corroded bone, with the claws of some animal, hair still adhering to them.

This snake, as is well known, is remarkable for what may be designated rudimentary posterior extremities. In another python, this a male, which I examined (its dimensions not given), I found them situated just before the anus, one on each side, and projecting about a quarter of an inch. This projecting part, not unlike a spur, and, like it, horny, was connected beneath the skin with a minute bone, the head of which was received into the glenoid cavity of a thin, long bone, itself terminating in a tapering cartilaginous process, which was only slightly connected with the surrounding cellular membrane.

4. *On the Rat Snake of Ceylon.*

This is one of the most common snakes of Ceylon; it may be considered almost a domestic one, being protected on account of its hostility to rats.

The specimen I examined was about five feet long. Its skin was loose on its body, being only slightly attached by cellular membrane, and in consequence admitting of a good deal of motion; it is provided with muscles of its own: each scutum had two pair of small muscles.

The lung is a single lengthened sac, opening into the cavity of the abdomen, and above communicating immediately with the trachea without the intervention of bronchia. Its inner surface is beautifully reticulated; its outer surface is formed of condensed cellular tissue. The fibres entering into the former are very elastic and contractile.

The stomach is of an oval form; its inner coat wonderfully plicated; its pyloric opening very small. The intestine is straight; its upper portion delicately plicated; its lower exhibits slight longitudinal folds.

The ureters of the two kidneys terminate in a single papilla in the cloaca; when pressed, abundance of white lithate of ammonia mixed with dilute mucus issued from them.

At the extremity of each penis there are several bony points, two of which in particular were about a quarter of an inch long and very sharp. These and the others were found to consist of phosphate and carbonate of lime and of animal matter—the carbonate, judging from the degree of effervescence of the part in an acid, in a larger proportion than in the bones of the mammalia.

The heart, nearly of an oval form, was contained in a very delicate pericardium. It consisted of two auricles and one ventricle. The three arteries which rose from the ventricle had each two delicate semilunar valves. The two auricles communicated with the ventricle by a common passage provided with a single valve, which was fleshy and globular, attached at two points, and moving up and down, as it were, on an axis, so that when one moiety was depressed the other was elevated.

5. *On the Heart of the Alligator of Ceylon* (*Crocodilus biporcatus*).

Through the kindness of a friend I procured the heart of an alligator from Chilau, where this animal is common, of sufficient size for satisfactory examination.

The descending vena cava, where it entered a common sinus, was furnished with a semilunar valve; owing to the manner in which the ascending was detached I could not ascertain whether it was similarly provided. The passage from the common sinus into the right auricle was large. As in the instance of the python it had two semilunar valves. This auricle was pretty capacious, its columnæ carneæ numerous and strong. Two arteries took their origin from it: that nearest the auricular-ventricular passage had one semilunar valve; the other two. About an inch above the origin of the latter it divided into two; the largest had a semilunar valve. The communication of this ventricle with the left was not very distinct. The upper part of the dividing septum was formed of columnæ carneæ and of muscular fibres interlaced. On minute examination, several small openings were found in the septum, but, with the exception of one or two, not large enough to admit the passage of a common probe: they were largest on the right side; on the left they were not easily perceived. Were it not for these foramina the septum would have been complete—there would have been no communication between the ventricles.

The left ventricle was much smaller than the right; its substance thicker; its columnæ carneæ smaller and less numerous. A single aorta rose from it, provided with two semilunar valves. It presently divided into three branches. The left auricular-ventricular passage, of moderate size, had two semilunar valves—one large, one small. The left auricle was pretty capacious, but less so than the right; a single vein, the pulmonary, terminated in it, which had no distinct valve. There was no direct communication between the two auricles; the septum was membranous and diaphonous. The arteries rising from the heart, with their divisions, six in number, were bound firmly together by cellular substance, and included in an extension of the outer membrane of the heart. The whole was cylindrical, resembling a single aorta, of enormous dimensions compared with the small heart. The arteries near their origin were much sacculated and tortuous, as if to aid in preventing a reflux of the blood.

6. *On the Heart of the Kabara-goya* (*Monitor exanthematicus*).

This lizard is common in Ceylon; the specimen which I examined was about three feet long.

Its heart more resembled in structure that of the python than that of the alligator, the two ventricles having nearly the same kind of communication immediately under the contiguous valves of the two auricles. The venæ cavæ terminated in a long sinus, which, as in the two preceding instances, was lined with a muscular coat. Its muscular fibres were distinct; when examined, the heart was still in action; the contraction of the sinus was synchronous with the expansion of the auricles and ventricles, or, in other words, the systole of the one was synchronous with the diastole of the other. Where the descending vena cava entered the sinus, there there was a delicate semilunar valve. The ascending vena cava, passing through the liver, terminated in the sinus immediately after its exit from that viscus: it had no valve that I could detect. At the opening of the sinus into the auricle there were two semilunar valves. The right auricle was a little larger than the left. Both were very muscular; they did not communicate. Both the right and the left auricular-ventricular opening had a large valve; and the two were so situated and united over the passage from one ventricle into the other, that they might be considered as one, of a boat-like shape. The passage of communication under them was pretty large. Two pulmonary veins terminated in the left auricle, without valves. The left ventricle was tolerably capacious; its walls very thick; no vessel rose from it. The right ventricle was much more capacious. A pulmonary artery and two aortæ had their origin from it. The three were contained in the same sheath, in length about an inch. Each had two semilunar valves. The aortæ anastomosed in the abdomen, and again separated. That one from which the carotids were given off, rose nearest the left ventricle; next followed the other, which in its course gave off an artery to each upper extremity before its junction with the first; next in order rose the pulmonary artery: this as far as possible from the left ventricle. The muscularity of the venous sinus seemed to insure the propulsion of the blood into the right auricle, and the valve of the right auricular-ventricular passage was so formed and situated as to direct the venous blood towards the pulmonary artery.

7. *On the Talla-goya* (*Monitor dracæna?*)

The heart which I examined of this lizard was from a specimen about two feet long. It closely resembled in structure the heart of the kabara-goya. The great vessels rising from the heart were all included in one sheath, which was so transparent as to allow them to be seen through it.

Grasshopppers and small shells were found in its stomach. This organ in its structure approached the gizzard of the gallinæ; it was embraced by powerful muscles; its mucous coat had a puckered appearance towards its cardiac extremity, but smooth towards its pyloric.

8. *On the Heart of the Green Turtle of Ceylon* (*Chelonia virgata ?*).

The heart was examined before its action had ceased. The common venous sinus had the appearance of a third auricle, and action in accordance. Where it communicated with the right auricle—the opening was large—there were two thin semilunar valves. I neglected to ascertain whether the entrance of either vena cava into the sinus was provided with a valve. The right auricle was much larger than the left, and pretty muscular. The passage from it into the ventricle had a large thick semilunar valve, as had also the left auricular-ventricular passage. The left auricle, though comparatively small, was rather more muscular than the right; it received one pulmonary vein without a valve. The left ventricle, too, was much smaller than the right, and its parieties thicker. No vessel took its rise from it. The right ventricle was pretty capacious. Three great vessels sprung from it, as in the instance of the kabara-goya—viz., one pulmonary artery and two aortæ. These vessels were connected side by side, but were not enveloped in a common sheath. Each had two semilunar valves. Besides the communication between the two ventricles close to and beneath the valves of the auricular-ventricular passages, there was a very delicate network of fibres, apparently proceeding from, and a continuation of, the inner lining of the ventricles forming part of the septum.

9. *On the Tiger Spider of Ceylon* (*Mygale fasciata*).*

This large spider seems to be of the same species as the *Mygale avicularia* of the West Indies, of which the temperature has been given in a preceding page. A specimen brought to me tied by a string afforded a striking example of its tenacity of hold by means of its "air-pump feet." In attempting to put it under a wine glass, three of its feet came in contact with the side of the glass, and such was their hold that, when the string was raised, the glass was lifted from the table. The weight of the spider

* For a very good figure and an interesting account of this spider, see Sir James Emmerson Tennent's Sketches of the Natural History of Ceylon. London, 1861.

was 344 grs., of the wine glass, 1,995 grs.; so this spider sustained by the adhesion of three of its feet to a smooth surface a weight 5·8 times that of its own.

Each foot was armed with two small sharp claws, by means of which probably it can walk against gravity on a rough surface; but from such a surface it is easily withdrawn, proving, if proof be needed, that these are not concerned in its powerful hold of a polished hard surface, such as that of glass.

10. *On the Dog* (*Canis familiaris*).

In September, on the 7th, at noon, in Malta, a pound of beef was given to a lean cur in good health, and another portion of the same weight at 9 p.m. Like the common dogs of that island, it had not been accustomed to meat, and, till he tasted it, seemed averse to it; then he swallowed it greedily, without attempting its mastication. On the following morning, at 8 a.m., another pound of beef was given, which he began to eat with seeming reluctance, but swallowed speedily.

At 11.35 a.m. he was killed by a blow on the occiput, penetrating to the brain and rupturing one or more blood-vessels, from which there was a considerable hæmorrhage. The blow stunned him, the respiration continuing some time after. The cavity of the abdomen was immediately opened; blood flowed in a jet from the vessels divided. The temperature amongst the intestines was about 100°; under the stomach and liver, 103°; two or three minutes later, it was the same, 103°, under the heart.

The lacteals, distended with white chyle, presented a very beautiful appearance. I could discover none proceeding from the stomach, and a few only from the duodenum; they were most numerous from the upper part of the jejunum, gradually becoming fewer towards the end of the ileum; none were seen coming from the large intestines, but this part was not examined with care. Never before had I seen the lacteals so beautifully displayed, so large, so full; yet their branches in the mesentery were not very numerous. The thoracic duct was also distended. Punctured, the chyle sprang out in a little jet, and continued flowing some time. Pretty much of it was collected. As it flowed, it appeared quite white; in the glass in which it was collected it had a light vermilion hue, but whether this was its true color, or owing to a few particles of blood mixed with it, was doubtful, it is so difficult to obtain it absolutely free for a certainty from blood. Presently it coagulated, a whitish fluid like milk separated; the coagulum, of soft consistence, retained the red particles, and was slightly colored by them.

The milky fluid, held between plates of glass before a candle in a dark place, had no distinct effect in coloring light. A portion of the coagulum was similarly tried; the light transmitted was certainly yellower.

Some chyle, about half a drachm, was mixed with hydrate of lime; no ammoniacal odour was produced.

In a few minutes after exposure to the air, the lacteals on the intestines and in the mesentery became quite empty.

Attention was next paid to the blood. The great vessels close to the heart were secured by ligature, and this organ was cut out. The blood from the right side and from the left was collected in separate vessels. The quantity from the former was about twice that of the latter. Placed side by side, the former looked the more florid of the two. Each presently coagulated. Chyle soonest made its appearance, and not ambiguously, on the blood from the right side—indeed, even when liquid there were whitish streaks in it, indicating chyle; streaks were seen too, but fewer, in the blood from the left side. Blood which flowed from the wound in the brain also shewed the presence of chyle; it appeared, too, in the blood of the aorta; after the coagulation of the blood there, it was conspicuous on its surface.

Neither the blood from the right or left cavities of the heart, mixed with lime, gave off a perceptible ammoniacal odour; nor did a portion taken from the ascending vena cava, between the liver and sinus venosus, nor from a portion from the emulgent vein. The result was alike negative with the bile, the substance of the spleen and of the kidney—the two latter broken up and triturated with lime.

The action of the heart continued feeble perhaps a minute after respiration had ceased. Before it had ceased, an opening was made into the cavity of the chest, and the heart was laid bare. Grasped by the hand, so forcible was its action that it overcame the pressure in diastole.

The urinary bladder was very much distended; as soon as an opening was made into the cavity of the abdomen it rose suddenly, as if inflated with air. Opened an hour after, when it was tense from the urine it contained, it was very red, as if inflamed, of which there was no proof. As soon as emptied, it contracted, so as to occupy very little space. The urine was bright yellow; mixed with lime, it gave off a strong ammoniacal odour.

The stomach contained the pieces of meat swallowed, some hair, and a little thickish white chyme—this at the pyloric end. With lime, the chyme gave off a distinct ammoniacal odour. The pieces of beef were changed from a red to a leaden hue; they were a little softer, and but very little. With lime they gave off a slight ammoniacal odour. They were acid by the test of litmus paper, as was the surface of the stomach generally. A portion of the meat put into milk coagulated it slightly; a portion of the pyloric extremity of the stomach acted more powerfully and rapidly in coagulating milk. Another portion, after having been washed with lime water, had no effect on milk; nor had a portion of the ileum washed with water any effect.

In the duodenum and jejunum there was a small quantity of thickish whitish chyme and many round worms.* It had an acid reaction tested by litmus paper. It gave off a distinct ammoniacal odour with lime. The ileum contained a small quantity of yellowish fluid, which was neither acid nor alkaline: it too gave off ammonia with lime.

Four days after, a portion of the meat which had been taken from the stomach had no putrid odour, did not appear to be changed, as if protected by what it had absorbed in that organ.

11. *On the Kid* (*Capra hircus*).

On the 14th of February, in Malta, a goat brought forth four young ones. I saw them about two hours after their birth. The first born was pretty strong and able to walk; the others

* This term implies that the parasites so named were similar to the round worms (*Ascaris lumbricoides*) infesting the intestines of man, and in Malta in so remarkable a manner, that in the *post mortem* examinations made in the large civil hospital of Valetta the absence of this worm was of rare occurrence. See for remarks on the subject "Some of the more important Diseases of the Army," p. 423.

were feeble and tottering on their feet; the mother seemed feeble, and was indisposed to stand. When the young were removed from her a few feet, they tottered towards her, going straight to the udder, seeking the nipples, guided either by the sense of smell or instinctively. The umbilical chord of each had been detached from the placenta by the bite of the mother; from those last born, it was hanging red and moist several inches long: there was no appearance of bleeding.

The day following two of them were drowned. The chord of each was dry and shrunk. They struggled under water three or four minutes. Opened immediately on taking them out, the heart was found contracting.

In the stomach of each of them there was a good deal of milk: in the first compartment it was coagulated; in the second it was changed into chyme; in the third it was of increased liquidity and more uniform. The lacteals in both the kids were fully distended with chyle, and made a beautiful appearance in the mesentery, milk-white as it was, and contrasted with the vermilion blood in the blood-vessels. Their size, that of the first order, was great, and their number amazingly great. They passed from the intestine to a chord of continuous glands—the pancreas of Aselli. A vast number of minute lacteals entered it from the intestine; and several large lacteals proceeded from it towards the thoracic duct. The fluid was like milk. A ligature passed round the root of the mesentery and drawn close, the thoracic duct could not be detected, it so soon emptied itself. When the duct was tied near its termination in the vein, then it was well displayed, distended as it was with chyle; but, even with a ligature on it, its contents were not retained, owing, it may be inferred, to some inosculating branch.

The foramen ovale and ductus arteriosus were open; the latter was thickening, and its calibre in consequence diminishing. The umbilical arteries within the abdomen were hard and chord-like, and until very near their termination were obstructed by coagulated blood.

There were some scybula in the large intestines.

The thymus gland of each was comparatively small. The cœcum was distended with mucus colored by bile.

12. *On the Kitten* (*Felis domestica*).

At Corfu, in July, a kitten twenty-four hours old was immersed in water several minutes; when taken out, it respired at short intervals, and would probably have revived had it not been again submerged.

When the chest was opened, the heart was motionless; but when exposed to the air by laying open the pericardium, the right auricle—that alone—contracted for a few seconds.

The eyelids were found glued together; or, rather, their edges were strongly adhering, so that there was difficulty in separating them. The cornea was not quite transparent, but opalescent.

The outer passage of the ear was nearly closed. The tympanum was full of a reddish fluid.

The apertures of the nostril were exceedingly small.

The mouth, tongue, larynx, primæ viæ, were well developed and well formed; the tongue, long and large, was well adapted for suction.

The jaws were in a cartilaginous state, beginning to ossify.

The brain was a soft greyish pulp.

The ductus arteriosus was open, and very large in proportion to the branches of the pulmonary artery, and the foramen ovale was large.

The umbilical arteries were very large; they contained a very little coagulated blood, and this in their superior part. The iliac arteries were very small.

The urinary bladder was large. The urachus was distinct, but closed.

The lungs, liver, spleen, kidneys, were well developed.

Putrefaction took place slowly.

What a close connection is there between the organization and powers of this young animal! All the senses imperfect: their organs unfinished, except that of taste. All the muscles of voluntary motion excessively feeble, except those concerned in the function of sucking and the act of crying. The fœtal state of the heart, and the fœtal communication between the pulmonary artery and the aorta persistant, whence less danger of obstructed circulation and suffocation.

Man at birth has been described as the most helpless of animals. He is not strictly so; those of the cat-kind are more so. What a contrast there is between the kitten and the kid—between the carnivorous and herbivorous animal! Is there not a gradation from the one to the other, through man, the monkey, bear, hog, etc.? And may not the same gradation be observed between the young of the carnivorous and herbivorous birds? This is certain, that according to the necessities and peculiar circumstances of the animal, so is its organization, it would appear, developed in all its details at its birth; indeed, were it otherwise the species must become extinct. How well is this adaptation illustrated in fishes and reptiles, and the young of animals still lower in the scale of organization, which at birth are left dependent on their own resources!

13. *On the Chick of the Common Fowl* (*Gallus domesticus*).

On the twenty-first day of incubation the process of hatching had commenced in one of twelve eggs; the big end was slightly fractured, and the chick was heard chirping and tapping within. In this state it was brought to me wrapped in flannel. This was about 10 a.m. It was put thus wrapped on a sand bath, the temperature of which was little above 80°. At this lower temperature, compared with that under the hen, its activity ceased; when warmed by being brought near the fire, its feeble voice was again heard, and the tapping. After about two hours, no appreciable progress having been made, I carefully broke the big end of the shell, and took out the chick; some hæmorrhage of fluid blood followed the rupturing of the umbilical vessels connecting the chick with the allantois.

The chick was wet; the abdominal aperture of a considerable size, at least half an inch in diameter, allowing so much of the included yolk to be seen, its surface beautifully marked, almost covered with vasa lutea.

After the extraction of the chick there remained adhering to the shell, and this most closely, the allantois, crowded with ramifying vessels full of fluid blood. A delicate sac (the amnion?) was loosely connected with it, not vascular, within which a little glairy fluid like albumen was seen, a blood-clot, and a large quantity of white opaque matter (lithate of ammonia), partly aggregated, forming little lumps, and in part scattered in streaks like vessels, suggestive of tubuli, also a little dark fæcal matter. The sac containing these several matters was easily detached from the vascular lining of the shell, the allantois.

The chick as soon as extracted was wrapped in warm flannel, and replaced on the sand bath, and to aid in drying it and keeping up its warmth, cotton-wool well warmed was repeatedly put over it under its wrapper. It was interesting to see how with each accession of warmth its activity increased, and *vice versâ*. When about 80°, its eyes closed, it seemed asleep or torpid, the heart acting; warmed, there was an immediate revival, with an opening of its eyes. After a little while there was a movement of the lower extremities, and an attempt made to stand. Gradually it became dry, its feathers not adhering together. At 5 p.m. it seemed in vigorous life. The abdominal aperture had been gradually contracting, and now it was nearly closed. Even so soon it began to eat, pecking at some softened crumbs of bread which were put before it. It was now taken back to the nest and put under the hen. On the morning following, with the other chicken hatched the same day, the 21st, it was running about and seeking its food, seemingly as strong and well as if its hatching had not been interfered with.

The day previous I examined a chick of the same brood that had been taken from its egg under water when just ready to leave the shell; and which, though alive when extracted, presently died, no attempt having been made by warmth to keep it alive.

It was noteworthy how admirably its organization was fitted for its early state of independent life; its lower extremities fully developed; the long bones hard and strong; the claws well formed and firm; the wings, on the contrary, very small and feeble; the frontal bones firm; the eye-sockets of hard bone, well fitted to protect the fully-formed eyes, each provided with its complete membrana nicktans; the gizzard large and fully formed, as was also the proventricle, the former containing pretty much curd-like matter, which had a strong acid reaction. The intestines were empty, with the exception of a little fæcal matter of a dark color near their termination in the cloaca. The liver was of a light yellow. The gall-bladder was distended with dark green, almost black, bile, which was intensely bitter.

Though there was a large quantity of lithate of ammonia* in the membrane adjoining the allantois; none was found either in the ureter or cloaca; these were empty. The blood from the vessels of the allantois and from the heart was rich in elliptical nucleated red corpuscles, which, indicating little firmness, became acuminated in flowing between two plates of thin glass, recovering their form on rest. Though the muscles of the lower extremities were large and powerful, yet, even with the aid of acetic acid it was difficult to find in them any striped fibres.†

14. *On the Torpedo* (*T. oculata*).

The heart, compared with the size of this fish, is unusually large; when I examined it at Rome in April, 1828, it appeared larger proportionally than that of any other fish with which I was then acquainted. I dissected at this time many specimens; in the pericardium of each a little fluid was always found. The heart consists of a strong, thick ventricle; its cavity, forming almost a circle, is partially divided by a muscular septum, and is a little contracted on each side in a line with the inferior portion of the septum, so as to constitute, as it were, two compartments. On one side, the arterial bulb, of a cylindrical, not of a conical form, rises from the ventricle; it is moderately thick, and apparently but not certainly muscular—certainly elastic. This bulb has at its base a series of valves, three rows, one over the other, and in each row three small valves, red, and terminating in a point. Three branchial arteries are given off from it on each side above the valves; it gives off also a large ascending branch, which subdivides into two. On the other side of the septum the auricle communicates with the ventricle. The auricular-ventricular passage is provided with six small valves, three on each side, not unlike the mitral and tricuspid of the mammalia. The auricle is very thin, and capable of much dis-

* Dried, it weighed 2 grs. Under the microscope before desiccation it appeared finely granular; this, and its giving off much ammonia when acted on by hydrate of lime, and assuming a rich purple when heated carefully with nitric acid, proved it to be lithate of ammonia.

† Even in the muscles of the thighs of the full-grown cock, and in the pectoral muscles of the Merlin hawk, I have not been able to detect markings of the kind. Mr. Gulliver relates nearly the same when speaking of the great pectoral muscle of the swift and of various small birds. The transverse streaks in them he describes as "very indistinct, and often difficult to be seen, although they are very plain in the muscles of the leg." Proc. Zool. Soc. for June, 1842, p. 67.

tension. It receives blood from two large very thin veins, having a common orifice at their junction into the auricle: they may be considered as the ascending and descending vena cava. The orifice is smaller than either of them; it has two thin semilunar valves. These veins pass through the cartilaginous parieties of the sternum, where they are like the cerebral sinuses; and are there joined by veins bringing blood from different parts of the body. The upper margin of the vein on the left side—the inferior cava—overlaps or extends a little way over the entrance into the auricle, directing the blood towards the right vein; in consequence, a probe introduced into the left, and passed on, glides over the orifice, but introduced into the right, and passed on, enters the auricle. The aorta receives its blood from the gills by three branches on each side; after giving off branches in its descent to different organs, it enters in the tail into a cartilaginous case.

At a later period, when passing through Malta, on my way to Constantinople in November, 1840, I had an opportunity of examining the torpedo—it was an oculata—with the aid of the microscope.

In a former work I had stated that its electrical organs are destitute of muscular fibres. This conclusion was now confirmed, using a high power. The blood taken from the heart (it had only just ceased to act) was still liquid. It contained comparatively few red corpuscles. Their long diameter was about $\frac{1}{800}$ of an inch; their short diameter about $\frac{1}{1000}$; their central nucleus, which was distinct, about $\frac{1}{4000}$. They were exceedingly thin, and also very soft and yielding, so much so that their forms were altered by contact with each other, as when observed flowing between two surfaces of glass not completely touching. The change of form was seen to occur at the instant of contact; after passing on, many recovered their original form. Some globular corpuscles were mixed with the elliptical; they were about $\frac{1}{4000}$ of an inch in diameter.

15. *On the Muræna Helena.*

The specimen examined was from the fish market, Malta. It was about a foot and a half long. It is sometimes taken of a large size, weighing more than 100 lbs.

It has teeth in its palate and fauces; a large stomach extending nearly to the anus; the intestine straight; the liver large, composed of two lobes; neither spleen nor pancreas could be detected; large kidneys were hid in the muscles posterior to the anus—they extended upwards, tapering narrower and narrower; a small bladder was also posterior to the anus. Its air-bladder, of an oval form, situated above the liver, consisted of a very strong external coat, not unlike the human albuginea testis, and of a minute red globular body firmly united to the inner surface, this a very delicate vascular membrane, somewhat like the pia mater. A small quantity of air procured from the air-bladder was not diminished by lime water, and very slightly indeed by phosphorus, thus proving it to be chiefly azote.

16. *Murœna Anguilla.*

This also was from the fish market, Malta, and was taken in the sea. It was about half a foot long: it occasionally attains there a great size.

Its stomach, intestine, liver, and kidneys are very like those of the helena. No urinary bladder was found. A long cylindrical air-bladder lay close to the spine, formed externally of a very delicate membrane of a pearly lustre. The air it contained was found to be azote; it was neither diminished by lime water nor by phosphorus. The blood corpuscles of one from the fish market at Constantinople, taken in salt water, were about $\frac{1}{2000}$ of an inch in diameter, by $\frac{1}{2666}$; their nuclei also were elliptical, with the exception of a few that were circular.

17. *On the Cuttle-fish* (*Sepia officinalis*).

The eggs of this cephalopode (Malta, June 2, 1832) were like a bundle of vesicular sea-weed; each was attached by a peduncle to a stalk, and at the opposite end was acuminated to a point. They were all nearly spherical, of different sizes, the largest about $\frac{6}{10}$ of an inch in diameter. They were of a dark brown color, almost black, and of a leathery texture. The whole mass of eggs—this irregularly globular—exceeded in bulk the two fists joined, and consisted of two or three bunches. One aggregate was composed of small vesicles, very little larger than large peas; the other of vesicles of different sizes. No embryos were detected in the former, merely a pulp, not unlike the vitreous humour of the eye, and were lined inside with a kind of pigmentum nigrum. In the other vesicles there were embryos in different stages of growth. On dividing the outer strong, resisting, leather-like coat of the most advanced, a transparent sac was brought into view; this also possessed of considerable strength. It contained a transparent fluid, in which a fœtus was found, swimming free, with a small transparent gelatinous mass attached to its mouth, on which it fed. This little mass or vesicular body was easily shaken off. The younger the fœtus, the larger was this attached substance. In the stomach matter of the same kind was detected.

Later, at Constantinople, I examined the colorless blood of the cuttle-fish, procured from the fish market of Galata. The fluid was from one of its hearts. The corpuscles it contained were circular; their diameter about $\frac{1}{2000}$ of an inch. There was a faint appearance in them of nucleus.

18. *On the Tunny* (*Thynus vulgaris*).

Examined yesterday (Malta, September 22, 1833) the heart and gills of a tunny that weighed about 80 lbs. The pulmonary artery rises from a thick conical bulb, fibrous, white, and very elastic.* The artery divides higher up, and a branch from it enters a cavity in each principal branchial cartilage, where it is associated with a large nerve and vein. Five large nerves are distributed on each side through the gills; they enter where the arterial branches come out, branches which united form the aorta. Are not these large nerves connected with the high temperature of this fish? Its branchia are very elaborate; the blood very abundant; the muscles red like those of the mammalia.†

Whilst at Constantinople I examined the blood of a tunny that weighed between two and three hundred pounds, and the heart of another of about the same weight. The red corpuscles

* In no instance, whether of osseous or cartilaginous fishes, have I found striated fibre in the structure of the bulb; nor in the young fish, such as the charr, the transparency of which allows the circulation of the blood to be distinctly seen under the microscope, and likewise the heart's action, does the bulb appear to have any contractile power independent of its elasticity.

† It is stated that during the last siege of Malta by the Turks, human flesh from the bodies of the slain was salted and exported as pickled tunny.

were about $\frac{1}{1000}$ by $\frac{1}{2000}$ of an inch in diameter; some were a little larger, some a little smaller. They all contained a nucleus, commonly elliptical: they seemed to be soft. Mixed with a solution of salt and water, many of them became bent. The corpuscles retained their form when dried. The specific gravity of the blood was 1,070.

The heart was of a triangular, pyramidal form, and was covered with a delicate transparent membrane, which was easily peeled off. The cavity of the ventricle was very small; its muscular parieties exceedingly thick. Between the auricle and the ventricle there were three strong valves. Between the ventricle and the elastic bulb there were two strong capacious valves. The organ was so cut when brought to me, as to be deficient of part of the bulb, and also of part of the ventricle. I could not detect in the heart striated muscle. The fish had been out of the water twenty-seven hours: the blood corpuscles were still distinct.

19. *On the Heart of the Sword Fish* (*Xiphias gladius*).

The heart of this fish, which I examined when at Constantinople, is very like that of the tunny. It has an auricle of moderate size, more muscular than that of fishes generally, a large and very powerful dark red ventricle, of very small capacity, and a large arterial bulb of a pale hue. Between the auricle and the ventricle the reflux of the blood is prevented by two valves. Between the ventricle and the pulmonary artery there are three semi-lunar valves, very like those of the mammalia.

20. *On the Ornithorynchus Paradoxus.*

The specimen, a male, was from New South Wales, and had been in spirits two or three years in the museum at Fort Pitt. The spur which distinguishes the male is attached to the hind feet, much in the situation of the spur of the common cock, and is analogous to the poison fang of the serpent; differing, however, from that of the serpent in not being of bone, but horny. In the fang of the serpent the aperture near the apex is in the side, in this it is at the point. When immersed in spirit, the capillary tube is distinct, and I have introduced a very fine probe both at the base and the opposite extremity. The spur is moveable; its base has a joint which moves in a capsule, into which a duct terminates from the poison gland.* The gland is comparatively small, nearly oval, situated high up amongst the muscles of the thigh by the action of which it must be compressed. The duct is long and slender.

Inside the mouth of the animal there are two large pouches, at the entrance of which are the teeth, one in each jaw—four altogether. The teeth from their form seem best adapted for grinding. In these lateral pouches there was a good deal of gravel of a hard kind, which might aid in the grinding process. The teeth were so loosely attached, that in examining them they fell out of their bony sockets: they were found to consist of a matter like that of horn.† The pouches were lined with a dark brown cuticle. There was a well formed velum palati; when down it forms a partition between the mouth and pharynx. The nostrils communicate with the mouth and pharynx by two small apertures in the palate. There is a delicate epiglottis, a small rima glottidis, long larynx, small lungs, very small stomach, large intestines—almost straight, a very long spleen, a lobulated liver, a very large gall bladder, a urinary bladder of moderate size, a long urethra, a cloaca. The meatus auditorius externus is very long; it passes round the angle of the inferior jaw.

About the same time I examined two portions of blood sent from New South Wales expressly for the purpose; and the result was, that its blood corpuscles are circular discs.

21. *On the Menobranchus Lateralis.*

The specimen was from Canada, about seven inches in length, and thick in proportion; it was preserved in spirits in the Museum at Fort Pitt. It had on each side three branchial tufts and two branchial apertures. The glottis was low down,

* The natives, we are told, hold the spur to be as described above, yet its poisonous quality has been called in question by Europeans from the circumstance, it is stated, of no bad effect having been experienced from its puncture. Is it not probable that the fluid, the product of the gland, is only secreted and poured out when the animal is irritated? Another conjecture may be offered, viz., that as the spur is peculiar to the male, the gland is active only during the breeding season, and that the poison may be designed to render the male more formidable in protecting the young.

† Before the blow-pipe it intumesced like horn, burnt with a bright flame, leaving only a very minute quantity of ash. A tooth weighing 2·5 grs. left about ·1 gr. of light colored ash, composed chiefly of phosphate and carbonate of lime.

opening into the œsophagus, and extremely small; I attempted to blow air into it, but did not succeed; a fine probe was introduced. It had two very narrow, slender lungs. Its heart was small. There appeared to be an opening in the septum of the auricles by which they freely communicated; but the part was so delicate and the heart so small that I could not be sure of the fact, though I examined it in other two specimens, each about half a foot long. It had an aortic bulb, partially divided by a septum with a loose border. The testes were large, and each was connected with its epididymis by horizontal ducts. The epididymis seemed to terminate in its corresponding ureter: I could not trace it farther. The ureters seemed to enter the cloaca. There was a small urinary bladder behind the pubis, which I believe communicated with the cloaca by regurgitation, after the manner of the batrachians. The liver, of a dark hue, was slightly lobulated or fissured. Imbedded in it was a pretty large gall-bladder. The spleen was of moderate size. A small pancreas was attached to the upper part of the intestine. The intestine was pretty large, and had a somewhat tortuous course. The stomach was moderately large, and internally plicated.

22. *On the Lophius Piscatorius.*

The specimen, a large one, a female, was sent from Aberdeen to the Museum at Fort Pitt, after all its viscera had been removed with the exception of the heart and kidneys. The kidneys were high up, soft, nearly globular, and not lobulated. The heart was large. Between the capacious auricle and ventricle there were two large semilunar valves. Likewise between the ventricle and the aortic bulb there were two semilunar valves, like the preceding, perfectly membranous. There were no valves in the bulb; it had fibres like delicate columnæ carneæ. A portion of the bulb and of the ventricle was examined under the microscope; in neither could striped structure be seen.

23. *On the Aorta of the Elephant.*

A specimen of the aorta of this animal in the Museum at Fort Pitt very much resembled the aorta of the ox. The ascending portion, compared with the descending, was very large; and the latter towards its bifurcation to form the iliacs rapidly diminished in size. The varying thickness of the middle coat was still more remarkable; where thickest, as about half way between its origin and the innominata, it measured ·60 inch; where thinnest, only ·16 inch, and the outer coat was about the same—this increasing in thickness as the middle coat diminished. The inner coat throughout appeared to be equally fine, as much so as in the human subject, certainly not more than a line in thickness; it was difficult to detach it, and consequently to measure it.

24. *On the Raia Aquila.*

From the fish-market, Malta. Its tail was cut off; otherwise it is not allowed to be sold there, on the idea that its puncture is poisonous. Its water-passages were very large, admitting the fore-finger with ease, though the specimen weighed only about a pound. Its teeth were laminated masses of bone admirably adapted for masticating. At the back of the mouth there was a delicate velum pendulum. The stomach, of moderate size, was full of broken food. There was no softening of its coats; and yet the ovaries and oviducts were reduced to a pulp, as if by a process like that which sometimes dissolves the stomach, and is referred to the action of the gastric juice. The intestine as well as the stomach was firm; there were no marks of putrefaction even in the pulpy oviducts. The liver, large and brown, had two lobes. The spleen, also large and of the same color, was close to the under margin of the stomach. The pancreas, white and small, was in the curvature formed by the pylorus and intestine.

25. *On the Squalus Acanthias.*

The specimen was from the fish-market of Galata (Constantinople). Its heart did not fill its rigid pericardium. It consisted of a capacious thin auricle, of a large pretty thick ventricle, and of a double arterial bulb. Between the auricle and great venous sinus there were two valves: these semicircular and opposed. The auricular-ventricular opening was large; there were no valves that I could find to prevent the reflux of the blood from the ventricle into the auricle. The ventricle was amply provided with columnæ carneæ and chordæ tendiniæ. The arterial bulb, consisting of an inferior and superior compartment, had in the former three rows of small valves, three in each row; in the latter, just below the origin of the pulmonary artery, three narrow well formed valves. Microscopically examined, striped muscular fibre was found only in the columnæ carneæ.

The specific gravity of the blood was 1,030; of the serum, 1,027. The serum was proportionally abundant; the fibrin small in quantity; the red corpuscles not numerous, but regular in form and uniform. Their long diameter was about $\frac{1}{1421}$ of an inch; their short about $\frac{1}{1600}$. Mixed with them were circular, or rather globular particles, as I believe, of about $\frac{1}{4000}$ of an inch in diameter, which was nearly that of the nuclei of the red corpuscles. When mixed with water the corpuscles from elliptical became globular, the nuclei remaining distinct. The latter in several instances were seen to change their place in their envelope. On the addition of a strong solution of common salt some of the corpuscles became again flat and elliptical.

The kidneys of this fish were large and lobulated. Its spleen was pretty large, close to the pylorus. The pancreas was composed, as it were, of three portions, connected together by pancreatic substance. The liver was large, and formed of four lobes. A distinct glandular body, cylindrical, about half an inch long, in substance like the spleen, was attached to the lower part of the intestine; within it was a cavity which appeared to open into the intestine.

About the same time I examined a female acanthias—the preceding was a male. This fish was somewhat shorter, little more than two feet, but proportionally thicker. The liver of this differed in form from that of the male; it was large and very long, reaching from one end of the cavity of the abdomen to the other, and composed of two lobes, with a longitudinal depression in each; they were connected by a broad isthmus. It differed also in having two spleens; these were nearly of the same size: one was attached to the pyloric end of the stomach; the other was lower, contiguous to the pancreas. The pancreas in each was similar. Its blood corpuscles were of about the same size. The effect of sunshine on them may be worth noting. When dried in the serum, in which they were examined, exposed to the direct rays of the sun, they acquired a star-like form, many of them exhibiting four rays, some six, a few three, and from elliptical becoming circular. When dried by heat, and in about the same time, there was little change of form, and still less if rapidly dried by being moved to and fro in the air. In this last instance, on exposure to sunshine, the same change as that before described took place, though in a somewhat less marked manner. In each instance, after twenty-four hours, the stellate form became less distinct. Probably the phenomenon was owing to saline matter in the blood. May it not be adduced in favor of the inference that the central portion contains a large share of saline matter, as in that part the ray-like lines were most distinct.*

26. *Squalus Centrina.*

The specimens examined were those from which the description of the several organs was given in the preceding paper. Though the fish exceeded two feet in length, the heart of each was exceedingly small—less than that of a large toad. The quantity, too, of blood was very small, as was also the proportion of the red corpuscles. Some of these were $\frac{1}{666}$ by $\frac{1}{888}$ inch in diameter; many were $\frac{1}{880}$ by $\frac{1}{1000}$; their nuclei varied from about $\frac{1}{2666}$ to $\frac{1}{4000}$. In some of the corpuscles the nuclei were circular, or rather globular; for, viewed with a strong light, the sun shining into the room, they had a spherical appearance, and were slightly colored.

27. *On the Mackerel* (*Scomber scomber*).

The specimen, of a large size, was from the fish-market of Constantinople in February. Its blood corpuscles were from $\frac{1}{2666}$ inch in diameter to $\frac{1}{2000}$. They were subject to the same kind of change of form from contact as those of the torpedo.

* It may be useful sometimes to narrate mistakes: the following is a note of one made in examining the eye of this fish. In a portion of the iris prepared for the microscope there was the appearance of a minute thread-like worm, an appearance that was confirmed on a first inspection; indeed so much so that I had little doubt of it, and set about examining it more minutely; now, on taking it up with a forceps, it seemed firmer than I could have expected; then it occurred to me that it might be a bit of hair, accidentally attached to the glass, which in truth I believe it was, for when dry its appearance corresponded to that of fine hair, and also its strength.

When flowing in currents between two plates of glass none were seen to turn over. May not their softness have prevented this? When immersed in a strong solution of salt, their nuclei, before faintly seen, became distinct and clearly oval. Now, in flowing they were seen to turn over, shewing their lateral surface. Some of them were bent.

28. *On the Red Mullet* (*Mullus barbatus*).

This specimen was from the same fish-market. Its blood corpuscles were about $\frac{1}{2285}$ by $\frac{1}{3200}$.

29. *On the Frog* (*Rana temporaria*).

In the neighbourhood of Constantinople, as late as the 13th April, frogs of the common kind were found preparing to spawn. Two males were brought to me on that day which had been taken when *in coitu*, and a male and a female the following day. The testes were each about the size of a barley-corn, nearly of the same form, and of a bright rich yellow color. Filaments (tubuli?) passed from the testes to the adjoining kidneys. The substance of either testis, cut into, yielded a somewhat opaque whitish fluid, which, when diluted with water, seen under the microscope with a one-eighth-inch power, was found to abound in spermatozoa in a state of great activity. They were nearly of a spindle shape, with a delicate filament at one extremity, commonly bent on itself in a whirl. They moved with a wriggling motion, and seemed to have the power of elongating and contracting themselves. Two were seen apparently united, suggesting the idea of male and female, and of the generative act.

According to MM. Prevost and Dumas, the spermatic fluid of the frog can be deprived of its spermatozoa by filtration—the filtered fluid in consequence losing its fertilizing power. Repeating the experiment, using good common filtering paper, many of the spermatozoa were found to have passed through.

The blood corpuscles of this frog were about $\frac{1}{1000}$ of an inch in diameter by $\frac{1}{1666}$.

30. *On the Esculent Frog* (*Rana esculenta*).

The blood of a frog of this kind, a male, taken on the 13th March in the neighbourhood of Constantinople, contained corpuscles which seemed to be of two orders—one of a large size, about $\frac{1}{1000}$ by $\frac{1}{2000}$ inch in diameter, the other about $\frac{1}{2000}$; the former had a nucleus corresponding in size to the latter. The blood coagulated feebly. The clot was broken up the following day; corpuscles of the larger kind were found in the serum, but none of the smaller. A portion of the clot under the microscope had the usual appearance of being formed of fibres; some of it, however, it is noteworthy, seemed to consist of elongated elliptical particles adhering endwise, giving the idea that the smaller corpuscles, numerous in the fresh blood, might be lymph particles, which, cohering, might have formed the fibres in question.

The testes of this frog were about the same size as those of the preceding; its spermatozoa also were similar, with this exception, that instead of the filamentous portion ending in a coil or whirl, it was tipped by a little roundish mass, which was in rapid tremulous motion. The length of the spindle-like portion was about $\frac{1}{2000}$ of an inch, its thickness about $\frac{1}{12000}$ of an inch.

31. *On the Turbot* (*Rhombus maximus*).

This and the following specimens of fish were from our own fish-markets. The stomach of the turbot is large; its epithelium thick; the cardiac orifice very large; the pyloric very small. The gall-bladder, not in contact with the liver, is connected with it merely by its ducts. There are two appendices pyloricæ opening into the intestine close to the pylorus. Between the upper and lower intestine there is a single valve, very like the human valvula coli. The lower intestine has long large villi, without valvulæ connivantes, such as are in the upper intestine: the villi there too are large.

32. *On the Pike* (*Esox lucius*).

Its pericardium large; a large auricle; two valves nicely fitting between it and the venous sinus. The opening from the auricle into the ventricle about midway in the septum, a thin valve attached helping to prevent the reflux of blood into the former; at the origin of the pulmonary artery two thin semilunar valves; the ventricle very thick—its cavity proportionally small; a large arterial bulb without any valves.*

* In a pike taken in Loughrigg tarn in March, its weight 3½ lbs., a single delicate valve was found between the sinus venosus and the auricle. The abdomen was dis-

33. *Red Gurnard* (*Trigla cuculus*).

In the œsophagus of this fish, just before its termination in the stomach, there is on each side a kind of tooth, circular and rough. In each of three specimens brought from Whitehaven the same was found. The stomach contained crustacea; its reaction was slightly alkaline. The fluid from the appendices pyloricæ, contrary to what is commonly observed, had an acid reaction.

34. *On the Perca Marina* (?)

Of this fish I have examined several specimens taken in the sea at the mouth of the river Duddon, varying in size from half a pound to three pounds and a-half, all in excellent condition, and were delicate eating. There were oil globules in the blood, and also, it seemed, in the blood corpuscles. In the abdomen there were masses of white fat very like milts, suggestive of such masses possibly having been mistaken for testes.* In one of them there was an ovary on each side, each about an inch long and about one-eighth of an inch wide; the ova they contained were distinct. The time was the middle of August. In their general appearance and color these fish much resembled, apart from their fins, the fario of the Lake of Geneva. The pupil was large and angular, iris yellow, cheeks light pink, teeth very small, back bluish grey, sides white, belly silvery white. Rays in first dorsal fin in one specimen, 8; in another, 9; in second, 14; pectoral, 15; ventral, 6; anal, 14; caudal, 16.† Stomach large: in each specimen shrimps were found in it, and other crustacea; its reaction alkaline. The form of stomach was peculiar; its narrow, long, pendulous portion was plicated; the shorter portion, the pyloric, rounded and smooth. The intestine of one was in part dissolved, and its contents had escaped. There was a large spleen; no pancreas that I could find; no appen-

tended with roe, the ova nearly mature, each about one-tenth of an inch in diameter; they were connected by vascular cellular tissue; their total number was calculated to be 54,875; the weight of a portion containing 304 ova was 34·6 grs.; of the whole, 6,250 grs. The stomach was empty, with the exception of a little glairy mucus and a few small fish-bones; it had neither an acid nor alkaline reaction.

* Cavolani states that the serran, or perche de mer, is an hermaphrodite, and has constantly the male and female organs united.

† These particulars are given, as, from consulting Yarrell's British Fishes, the species seems doubtful.

dices pyloricæ; no urinary bladder; a large closed air-bladder. Striated muscle in the ventricle of the heart, but not visible without the aid of acetic acid. Upper surface of tongue horny; rough in patches; under surface muscular; could not find any striated fibres in it.

It was remarked at the time that, though feeding like the salmon, the flesh of this fish was totally different in color, being quite white: it had neither an acid nor alkaline reaction.

XXXIV. MISCELLANEOUS OBSERVATIONS ON THE SERUM OF THE BLOOD—ITS FIBRIN AND ITS RED CORPUSCLES.

1. *On the Relation of Serum to Carbonic Acid and Oxygen.*

When engaged in an inquiry on the blood in connection with respiration and animal heat in the winter of 1837-38, the serum of the blood naturally had my attention. It was subjected to various experiments, the results of which were given in that section of my Researches, contained in the second volume, relating to the inquiry just mentioned, but without any detail of the trials. This omission, considering their physiological bearing, was an oversight; and as I am not aware of any of the like kind that have since been made, I hope I may be justified, even after the lapse of so many years, in describing them.

Experiment 1.—The serum used in this and the following experiments was from the mixed venous and arterial blood of a sheep, killed the preceding day: 100 measures of it, agitated with carbonic acid gas over mercury, absorbed 150 measures. Thus saturated, the serum had lost the little viscidity which it had before, and ceased to froth.

Experiment 2.—To a portion of this saturated serum, muriatic acid was added in excess—the undiluted acid; the serum coagulated; no gas was disengaged immediately, but after a while some was liberated.

Experiment 3.—Another portion of it was immersed in water and heated, but not sufficiently high to effect its coagulation; as the temperature rose, gas was given off in minute bubbles, and for a considerable time, the serum not becoming turbid.

Experiment 4.—To nine cubic inches of serum, added one cubic inch, which had absorbed a cubic inch of carbonic acid gas over mercury. The mixture of the two was poured into a double mouth pneumatic bottle, and agitated with 20 cubic inches of common air, the capacity of the bottle being 30 cubic inches.

No air was disengaged on turning the stop-cock, to which a bent glass tube was attached, communicating with water.

Experiment 5.—Now another cubic inch of serum, containing also its own volume of the acid gas, was added through the tube with which the other mouth of the bottle was provided, that also having a stop-cock. Agitation was again employed; after which, immediately on turning the stop-cock, from which the bent tube rose, there was a disengagement of air equal to one cubic inch.

Experiment 6.—32 measures of venous blood from the jugular vein of a sheep, agitated with 45 measures of carbonic acid gas, there was an absorption of 35.

Experiment 7.—32 measures of serum of arterial blood from the carotid artery of the same sheep, agitated with the same quantity of gas as the last, the absorption was the same.

Each experiment was repeated, and with the same result.

Experiment 8.—To 78 measures of serum of the mixed blood of a sheep, killed the day before, 12 measures of pure oxygen, obtained from the decomposition of chlorate of potash, were added over mercury, and agitated; there was no appreciable absorption. Examined the following day, the volume of the two was just 90 measures.

Experiment 9.—Blood from the jugular vein of a sheep, killed yesterday, its fibrin, separated by agitation with pieces of lead in a stoppered bottle, completely filled, was kept under water; not a single air-bubble was disengaged. Ten cubic inches of it were introduced into the pneumatic bottle previously filled with oxygen. After a few seconds the red corpuscles subsided, remaining unchanged in color, a stratum of nearly colorless serum being between them and the oxygen. Now agitated the blood in the oxygen, it acquired a bright vermilion hue; and turning the stop-cock of the bent tube, water rushed in, filling the bent tube, and denoting consequently a certain absorption of oxygen. A portion of the gas was expelled and tested for carbonic acid; not a trace of it could be detected.

There were many more experiments of the same kind as the preceding made at the time. Those given may suffice. The results of them, I need hardly remark, may be considered in a manner fundamental in relation to animal heat, in connection with respiration. They shew that, whilst the serum of the blood is capable of absorbing and retaining a large quantity of carbonic acid gas,* it is incapable of absorbing an appreciable quantity of oxygen. Further, they shew that the blood deprived of its fibrin has the property of absorbing oxygen gas, and that the absorption is effected by the red corpuscles. Besides this, an experiment was made, which was described in my Researches (vol. ii., p. 168), proving that in the act of the absorption of the oxygen by the red corpuscles, there is a disengagement of heat. The theoretical consequences I shall not here revert to, excepting to offer one suggestion, viz., that as the red corpuscles are the bearers of oxygen, so serum is of carbonic acid; and that the latter, in relation to the red corpuscles, may be as azote is to oxygen in the atmosphere; the serum in the blood having, it may be conjectured, a moderating influence, and a protective one—the first on the red corpuscles, the second on the tissues generally.

2. *Of the Coagulation of Serum by Heat.*

The following experiments were made with three objects in view: 1st, To endeavour to ascertain the effects of different degrees of heat on serum, minutely observed; 2ndly, Whether there is any marked difference of coagulability of the serum of different animals; and, 3rdly, The effect of various degrees of dilution with water on this property of the serum.

The animals from the blood of which the serum, the subject of the three following experiments, was obtained were the sheep, the common fowl, and the salmon. The serum was in each instance fresh. In each instance the trial was made by subjecting a certain quantity of serum in a thin glass tube to heated water; marking the temperature when first immersed and again when taken out, the water having been removed from the fire at the instant of immersion, and then gradually cooled. The thermometer used was one of Negretti and Zambra's patent kind, and

* Probably serum, whether in the blood, or when effused into cavities, contains carbonic acid. The following note of an experiment made in July, 1839, is in favor of the inference. "A portion of serum, drawn off by paracentesis from an invalid laboring under ascites, was received into a phial, from which distilled water, deprived of air by the air pump, had just before been emptied. Allowed to cool, a moiety was poured out (the phial had been filled and closed by a glass stopper), the remainder subjected to exhaustion by the air pump, gave off air; stopping, suddenly, some aqua potassæ deprived of air was added; again subjected to the pump, a little air was disengaged, but very much less than before, giving the idea that the serum contained carbonic acid and azote, the former in largest proportion. The serum was of specific gravity, 1,014.

compared with a more delicate one of the same makers of verified accuracy, appeared to be pretty correct.

1. *The Sheep.*—The sheep from the blood of which the serum was obtained was eighteen months old, and was killed for the market in November. The serum was of specific gravity 10,249.

At 142° falling to 123°, no change seen, except a change of color from a reddish tinge to a yellowish.

At 147° falling to 130°, no further change.

At 149° falling to 140°, no further change.

At 160° falling to 150°, no further change.

At 170° falling to 150°, its liquidity and transparency slightly diminished.

At 180° falling to 160°, coagulated, bears inversion without flowing; also, when broken up; is of an opaque white.

At 194° falling to 155°, coagulum firmer and of increased opacity. Boiled in water for a few minutes, its opacity and firmness were still more increased.

2. *The Common Barn-door Fowl.*—A hen, three years old, killed for the table; the blood was of specific gravity 10,543; the serum of specific gravity 10,229.

At 147° falling to 130°, no change perceptible.

At 160° falling to 150°, its fluidity perhaps a very little impaired.

At 170° falling to 160°, its fluidity and transparency diminished; the tube inclined, the serum still fluent.

At 180° falling to 165°, it barely flows slowly on inclining the tube; continued in the hot water till the latter had fallen to 135°, the coagulum bore inversion; but when broken up it flowed.

At 194° falling to 155°, is more firm; bears inversion even when broken, but is still soft; slightly opaque.*

3. *The Salmon.*—A grilse, weighing 5·5 lbs. (that is, the salmon on its first return from the sea), taken with the fly in the river Crede in the Isle of Lewis, on the 9th of September, was immediately blooded by cutting its gills; the yield of blood was only about an ounce, though the fish when bleeding was

* Similar trials were made on the serum of the blood of a goose, six months old, weighing 11·5 lbs.; its blood was of specific gravity 10,589; the serum of specific gravity 10,195. The results were nearly alike. At 167° it slightly jellied; at 173° it ceased to flow, the tube being inclined; at 192°, falling to 150°, it was somewhat and but little firmer; at 206° its firmness was much increased, but still it was only slightly opaque.

suspended with the head pendant. The serum was decanted from the clot, when moderately contracted on the following day. Its specific gravity was not ascertained on account of the smallness of the quantity. Serum from the blood of three salmon taken about the same time was of specific gravity 10,176; the blood of one of these was of specific gravity 10,366.

At 150° falling to 140°, no perceptible change.

At 160° falling to 150°, it became slightly gelatinous without becoming opaque.

At 168° falling to 160°, still semifluid; its transparency diminished.

At 170° falling to 160°, a slight increase of opacity; still semifluid.

At 180° falling to 170°, still in a slight degree semifluid, moving slowly when the tube was inclined, with but little increase of opacity. Again subjected to 180°, it became a soft jelly-like coagulum, bearing inversion without flowing; not opaque, but diaphonous. After standing two hours, a little fluid exuded, which, exposed to a boiling temperature, became milky; it had a fishy but not a saline taste; its reaction was slightly alkaline.

3. *Of the Effect of Aqueous Dilution.*

The serum of the blood of the same sheep, that already noticed, was used. When diluted with an equal volume of water, the specific gravity of the mixture was 10,139.

1. One water, one serum, at 165° falling to 154°, no change. At 178° falling to 160°, a slight opacity was apparent. At 195° falling to 180°, a slight increase of milky opacity. At 205° falling to 165°, an increase of opacity and impairment of fluidity; it was not rendered more firm by boiling.

2. Two water, one serum, at 178° became slightly milky. At 195° a slight increase of opacity; it flowed freely; no further marked effect from boiling.

3. Three water, one serum, at 195° became slightly opaque; boiled without further effect.

4. One water, two serum, at 178° falling to 168°, it became milky. At 198° falling to 168°, its fluidity was slightly impaired. At 201° falling to 78°, it became much thicker, still fluent; boiled, its thickness increased and fluidity diminished.

5. One water, three serum, at 180° falling to 168°, its fluidity was considerably impaired, yet flowing, the tube being inclined; it was rendered opaquely white. At 200° falling to 170°, its fluidity was a little more diminished, yet it still flows freely on inclining the tube.

Trials were made on the dilution of the serum of the blood of the duck; the serum was of the low specific gravity 10,147. The effect of dilution, as perhaps might be expected, was even more strongly marked. I shall give only one result: a mixture of one of water and one of serum, at 195° had its transparency a little diminished, but not its fluidity.

What are the conclusions to be drawn from the foregoing results? Few as they are, do they not appear to prove—1st. As regards the specific gravity of the serum of the blood of different animals, that there is no regular ratio between that of the entire blood and the serum which it yields. 2ndly. That though commonly the lower the specific gravity of the serum the higher is the degree of temperature required for its coagulation,* it is not unexceptionably so. 3rdly. That aqueous dilution, in the same proportion that it lowers the specific gravity of the serum, diminishes its coagulable property.

As an exception to the second conclusion, the following is an example, afforded by the serum of the blood of a pig killed for the market. It was of the low specific gravity 10,207.

At 156° falling to 140°, no change.

At 160° falling to 145°, no change.

At 162° falling to 145°, it coagulated feebly, flowing when inverted.

At 165° falling to 140°, it coagulated so as to bear being inverted without flowing, thus differing from the sheep's and bird's serum.

As in the instance of the fibrin of the blood, so I believe in its serum, the whole is not of uniform quality, but is a mixture as it were of serums of different qualities, of different degrees of coagulability; some coagulating at a comparatively low temperature, some at a comparatively high. An example will illustrate my meaning.

The serum of the blood of a duck, the same as that already

* The serosity, the fluid found in serous membranes, whether sound or morbid, is generally admitted, and I believe truly, to come under the above rule.

mentioned, of specific gravity 10,147,* tried in the same manner as the preceding :—

At 167° falling to 150°, became slightly turbid; filtered, at 171° falling to 145°, again turbid; filtered, at 197° falling to 140°, a soft coagulum formed. This thrown on a filter, a little clear fluid exuded from it, which, exposed to the boiling temperature, had its transparency impaired, a slight degree of turbidness appearing. Thus shewing that in this serum there was a part not resisting a temperature of 167°, another yielding to 171°, another to 197°. The little fluid that exuded from the coagulum, after having been subjected to a boiling temperature, had a slight alkaline reaction and a slight taste of common salt.†

Whether this difference as to degree of coagulability depends on the substance dissolved being not of precisely the same quality, or to the influence of saline or other ingredient on the albumen, I have some hesitation in offering an opinion. On the whole, I am inclined to think that the latter is less concerned than the former, and this also in accordance with the analogy of fibrin. In young animals both the fibrin and the serum shew less contractile force in coagulating, than in the same animals in their adult stage. In a note of a trial I made on the serum of the blood of a calf, it is stated that even at 212° the coagulum was soft and gelatinous.

4. *On the Action of the Serum on the Blood Corpuscles.*

I am not acquainted with any account hitherto published of the effect of the serum of the blood on the blood corpuscles.

I find that when a minute quantity of the red corpuscles has been kept for a few days, at a temperature of about 55°, in serum

* The blood was of the specific gravity 10,488. The temperature in ano 107; the weight of the duck after being blooded was 5·5 lbs. The quantity of blood obtained weighed 1,577 grs.

† The serum from the blood of another duck eleven weeks old, killed for the table, and in excellent condition, was of a milky hue and yellow color, from oil globules of excessive fineness suspended in it, which did not subside after several days. It did not coagulate at 176°; at 193° it became only slightly jellied, but did not bear inversion without flowing; at 207°, falling to 85°, it bore inversion, but was still soft. Its specific gravity was not ascertained. A portion of this serum dried, acted on by alcohol, imparted its color to the spirit. On evaporation of the alcohol, at the temperature of the air, a proportionally large residue was obtained, consisting of oil with crystals, as seen under the microscope, of two or three different kinds, some in the form of plates resembling cholesterine.

of the blood from which the corpuscles were obtained, the serum has become of a darker hue, and has acquired a peculiar smell, not in the least putrid, and the subsided corpuscles have become white, having entirely lost their color. In the instance of those of the common fowl, from elliptical they have acquired a globular form; and intermixed with them, using the microscope, numerous granules have been seen, and minute prisms, reminding one of those of phosphate of lime.

If the quantity of red corpuscles be at all considerable in relation to the volume of serum, and the latter of moderate depth, the progress of the change from the red to the colorless corpuscles may distinctly be seen. As the color of the corpuscles fades, that of the serum immediately incumbent increases, spreading from below upwards, passing in gradation from a deep red into orange red. The odour of the serum thus discolored, after having been kept a few days, is very like that which proceeds from the cadaver of a person of drunken habits.

Whether in the circulation of the blood during life the red corpuscles are liable to be affected at all in a similar manner, is a question perhaps deserving of consideration. In the instance of a gentleman ætat. 66, who labored under a lingering disease eventually fatal, cancer of stomach and other organic lesions interfering with nutrition, productive of extreme emaciation, I found the blood corpuscles unusually small, about half their normal diameter, as if wasted. The quantity of blood remaining in the cadaver was very small; it was not destitute of fibrin, judging from a small quantity of fibrinous concretion contained in the heart.

That the coloring matter of the corpuscles admits of solution by the serum during life I have been led to think probable, from occasionally finding the blood of the fowl on its first separation from the clot tinged red, and also that of the fresh-run salmon, and this when the serum in each instance was transparent and free from red corpuscles; so marked was it in the instance of the salmon, that I was disposed at the time of making the observation to infer that the fish (they were red, as the males always are about the spawning time) may have owed their peculiar color to their red serum. The fowl, it may be mentioned, as well as the salmon, both abounding in blood, in excellent condition, had been killed fasting—a circumstance which may have promoted the absorption in part of the white matter of the corpuscles.

5. *On a certain Change which takes place in Serum from Keeping.*

If perfectly clear and transparent serum,* such as that of the blood of the common fowl, within a few hours after the contraction of the clot, be placed over mercury with a measured quantity of atmospheric air, for a certain time, as for two or three days, about the temperature 55°, I have observed, as in the instance described of the serum of the sheep's blood, no change of volume of the air; and the air examined has been found to contain no appreciable quantity of carbonic acid.

Should the experiment, however, be continued for many days, an absorption slowly takes place, and the air examined, though still destitute of carbonic acid, is found to have lost a portion of its oxygen, which it may be inferred has been expended in the production of carbonic acid gas, which gas has been absorbed by the serum.

If the experiment be still longer continued there is an increase of the volume of air, owing to carbonic acid produced more than the serum can absorb, and this with the disappearance of the whole of the oxygen. The serum is now found to have acquired an offensive putrid odour, to have become turbid, and to have deposited a whitish matter.

If the serum be allowed to be acted on by atmospheric air freely, just sufficiently covered to check the evaporation of its aqueous part, then in the course of a few weeks, varying according to the temperature, and the degree of purity of the fluid, or the proportion of its heterogeneous contents, a whitish deposit will appear, the serum will become in a manner dilute,† affording less coagulum when heated, will acquire a disagreeable odour, and if the keeping be protracted, a putrid, sickening one.

* If the serum be not clear, then I find that even during the first twenty-four hours thero is an absorption of oxygen; and this, whether the fluid is discolored by the coloring matter of the blood, or by matter of the nature of chyle. In the latter case, after two or three days, I have found a sediment to take place and the serum to become clear. If this experiment be repeated with the clear serum, no absorption is presently produced, no more than in the instance of the originally transparent serum.

† I have found tho specific gravity of the serum reduced from 1,027 to 1,022; and though having a strong ammoniacal as well as putrid odour, its coagulability by heat was but little, if at all, diminished. At 166°, falling to 140°, the effect was produced. Tho coagulum, after having been washed, mixed with lime, emitted a strong ammoniacal odour.

I shall here do no more than notice the deposit. By repeated washings with water it is deprived of its disagreeable smell. The finer part of it is readily suspended in water; another part in the form of flakes rapidly subsides. The former may be described as loose granules, more or less free; the latter as granules connected together. Such is their appearance under the microscope, as seen with a one-eighth-inch power. The granules are readily soluble in liquor potassæ, very slightly by acetic acid, or by a solution of nitre. The flakes are rendered more transparent by the liquor potassæ, and are slowly dissolved. It is worthy of remark that neither the granular nor flaky matter exhibits change of color on admixture with this fluid.*

In fatal cases of empyema complicated with pneumathorax, a deposition of soft white matter on the pleura is a common occurrence. May it not be derived from the serous portion of the effusion from the slow action of atmospheric air? If so, like the deposit mentioned, it should contain no sulphur; but whether it does contain any I have not had an opportunity to ascertain.

6. *On the Red Corpuscles of the Blood as Affected by Heat and Congelation.*

This is a subject like that of the action of serum on the red corpuscles, which has not, so far as my reading extends, received any attention. The following are a few of the experiments which I have made to endeavour to illustrate it.

1. *On the Effect of a Temperature Below the Coagulating Point.*—Serum from the fresh blood of the common fowl slightly colored by a small quantity of red corpuscles, was subjected by immersion in water in a thin glass tube to a temperature of 126°. Examined with the microscope, the corpuscles appeared unaltered in form, with the exception perhaps that their edges were a little thickened.

At 132° falling to 92° during an hour, and again from 133° to 55° during the night, the corpuscles in the morning were found subsided, and of a bright scarlet, the serum perfectly clear and transparent, of a light yellow, without a tinge of red. Under the microscope some of the corpuscles were seen contracted mid-

* The liquor potassæ used, that of the Pharmacopœia, though colorless, contained some iron in solution, the peroxide, as was indicated by the black precipitate it yielded from sulphuretted hydrogen, and consequently may be considered as a test of this gas, and, as I believe of nascent sulphur.

way, the majority retaining their original shape, but somewhat diminished in size, and their margin a little thickened.

At 140° falling to 138°, besides diminution of size, many of the corpuscles had acquired the dumb-bell form. After some minutes they became rounded, and their nuclei less distinct. After a while, on cooling, most of them recovered their normal elliptical shape.

At 147° falling to 130°, the corpuscles became reduced in size, rounder and less distinct, as if wasted, from partial solution.

At 150° falling to 110° in an hour, the corpuscles had become circular. Using a strong light with careful adjustment, their nuclei were distinct, the including part barely visible, as if wasted. The serum was now bright red, and it continued so on rest, when it became clear and transparent. The altered corpuscles which had subsided, after having been washed with fresh serum, were colorless. During the following twenty-four hours they underwent no further appreciable change. The diameter of the nuclei was about $\frac{1}{8000}$ of an inch.

At 170° falling to 150°, the corpuscles lost their form for most part; the few visible under the microscope were very small. In the place of the former there was an amorphous appearance approaching the hyoloid.

In all these trials fresh serum and blood corpuscles were used.

Similar experiments were made on the red corpuscles of the sheep, the ox, and of man. The results were so similar that I need not give the details of them. In all of them, with the contraction of the corpuscle, the bright vermilion hue appeared to be intensified; and at the higher temperatures, accompanied with a wasting, the serum acquired the same color.

2. *On the Effect of Higher Degrees of Temperature on the Color of the Blood.*—It is commonly stated that the coloring matter of blood is rendered insoluble at 150° Fahr. Using a solution of it obtained from the blood of the common fowl, I have observed no change at 156° falling to 120°. At 165° falling to 140°, it became turbid, but still without change of color; 181° falling to 160°, there was a change to brown, and the coloring matter was coagulated. In a like trial with the coloring matter of the blood of the ox, the aqueous solution, before clear, became slightly turbid at 155°; at 160° there was an increase of turbidity and a change of color to brown. Filtered,

a brown matter was collected; the fluid that had passed through was of a light yellowish brown.

The results were very similar when serum was used, colored by red corpuscles. Here is an instance. The fresh serum of the blood of a cow killed for the market, and in good health at the time, was loaded with red corpuscles, was of a bright red, and of specific gravity 1,031. At 152° falling to 135°, it still retained its fluidity; but with impairment of brightness of hue. At 161° falling to 125°, a soft coagulum formed; the color now was completely changed; by reflected light it was almost black; by transmitted, seen through a thin layer, it was light brown. At 180° falling to 145°, the coagulum had become firm. The color, as seen by reflected light, was now dark dull brown. Broken up, after the addition of water, the water was not immediately colored; but gradually in a few hours it acquired a greenish brown hue.

The results, it may be remarked, were much the same when a portion of crassamentum was added to the serum, with, however, this difference, that either a higher temperature was required for a rapid effect, or a longer continuance at a temperature between 170° and 180°, owing, no doubt, to the crassamentum being a bad conductor of heat.

3. *On the Effect of the Congelation of the Blood-corpuscles.*—In some experiments made many years ago on the freezing of blood,* the results of which confirmed the earlier ones of Hewson, I found though the blood coagulated after liquefaction, that the coagulum was more tender, softer, than that from the same blood which had not been frozen. The following are the results of experiments which I have recently made on the red corpuscles, using ether for the purpose of congelation. The manner of employing it was the following. The bottom and inferior portion of a thin glass tube, into which a minute quantity of serum was introduced holding the corpuscles in suspension, were surrounded by loose cotton-wool; on this the ether was poured, and renewed till the fluid was frozen.

The first trial was on the corpuscles of the blood of a duck. Thawed after having been completely frozen and for several minutes, the corpuscles, as seen under the microscope, had a shrunken appearance; many of them were variously contracted;

* Research. Physiol. and Anat., vol. ii., p. 77.

a few had acquired the dumb-bell form; some had become circular, some more pointed in the direction of their long diameter and at one end.

A like trial on the corpuscles of the blood of the common fowl afforded very similar results; besides a visible contraction, some of them were puckered at their margin.

In another trial, this in the open air, when a register thermometer placed by the side of a tube containing the corpuscles of the blood of a fowl suspended in its serum fell to 29° during the night, the result was negative; the following morning, when the thermometer was several degrees above the freezing-point, the corpuscles were found unaltered in form.

The foregoing results, I am inclined to think, may be turned to some account, and may be significant pathologically. May not the first described be worth keeping in mind in investigating the effects of sun-stroke? It is easy to imagine that under exposure to powerful sunshine at a high atmospheric temperature, if the head be not well protected, the temperature of the surface of the brain may be raised nearly, if not quite, to that point which the blood corpuscles cannot bear unaltered; and the circumstance that a sudden cooling tends to restore the form of the corpuscles when only slightly altered by heat, is seemingly in accordance with the effect of cooling applications to the head in the disease in question.

May not they and those that follow them be of service too in medical jurisprudence? If a body has been immersed in water of a temperature from 150° to 160°, the blood, at least in the superficial vessels, should afford indications of it in the altered form of the blood corpuscles; and, if it has remained in water at a somewhat higher temperature for some hours, so as to be permeated throughout by the heat, the blood should be coagulated in the vessels and its color altered.

Further, in severe cases of burn—cases always dangerous—may not some of the danger arise from the effect of heat on the corpuscles and serum? The altered blood corpuscles may act injuriously, and the coagula, besides the local lesion, should any of them be loose and pass into the circulating blood current, may have a more seriously bad effect in giving rise to obstructions in the brain and other important parts.

In the *Medical News* of the 3rd January, 1863, there is a notice of six children who had perished by fire in Soho, whose bodies were seen and reported on by Dr. Buzzard. In one of these, of which he made the autopsy, whose body had suffered least, he found the blood no where of the ordinary venous color, but bright red. He inferred that the burns, which were very extensive but not deep, had been inflicted before the suspension of vital action, and that the deaths were not owing to suffocation. Have we not confirmation of this in some of the preceding results—those shewing that at a temperature below that required for the coagulation of the serum, the red corpuscles, either *per se*, owing to slight contraction, or by their coloring matter being dissolved in the serum, occasioned an intensification of the vermilion hue.

In frost-bite have we not an example, as it were, of the poetical expression of the "burning frore," as used by our great poet? And may not some of the injurious effects be mainly owing to the freezing of the blood, and the changes in consequence in the corpuscles, and in a less degree in the fibrin?

7. *On the Contraction of the Fibrin of the Crassamentum.*

Though it is well known that the fibrin of the blood as contained in the crassamentum continues contracting for a considerable time, and that the amount of its contraction varies according to a variety of circumstances, such as the species of animal, its age, condition, and state of health, yet I am not aware of any exact account having been given of it as determined by measurement.

Amongst my notes is the following, in which the attempt is described. It bears the date of Fort Pitt, 1839.

The blood of a rheumatic patient, abstracted on the 30th of December, was collected in a circular cup; measured before any contraction had taken place, its diameter at the surface was equal to 3·1 inches.

At midnight on the 31st, thirty-two hours later, it had contracted to 2·2 inches.

On the 1st of January, twenty-two hours later, it measured 1·9 inch.

On the 2nd, sixteen hours later, it was reduced to 1·7 inch.

It was now remarked that the depth of the entire blood was about 1·4 inch, of the buffy coat ·3 inch, of the crassamentum 1·1 inch.

On January 3rd, sixteen hours later, in place of further contraction, there was a slight expansion, viz., from 1·9 inch to 2 inches. The serum was still colorless.*

I shall give two other examples, these of the blood of the sheep and common fowl, both killed when in a healthy state. The trials were made in November, and in a room, the temperature of which during the time ranged from about 50° to 55°.

The crassamentum of sheep's blood, chiefly arterial, measured a few minutes after coagulation took place, at its surface was 4 inches in diameter. The vessel in which it was received was a circular cup. Two hours later there was a contraction to 3·8 inches. On the following day, viz., seventeen hours later, it was diminished to 3·5 inches; on the next day to 3·3 inches. On the following day there was no farther contraction; on the next there was a slight expansion. The diameter now was 3·4 inches; the day after, 3·45; the day after that, 3·5.

Together with the expansion, the serum had become reddish, from the solution of some of the coloring matter of the corpuscles; and the crassamentum was of diminished firmness, and shewed a tendency to putrefaction.

The blood of the fowl† was from the divided cervical vessels, and was collected in a wine glass: it coagulated in less than half a minute. The diameter of the crassamentum then was

* In the same note the structure of the buffy coat is described, followed by a pathological observation, both of which, on account of the interest connected with the subject, I venture to give:—

The buffy coat was enveloped in this instance in a membrane very like a serous membrane, both as seen with the naked eye and the microscope. Under the latter, when carefully dissected off, it was very like the omentum, having the appearance of a most delicate tissue. It is added: It may help to illustrate the formation of cysts, especially the sacs of aneurisms, which, it may be conjectured, are similarly formed, and consist merely of condensed coagulable lymph. After a portion of the buffy coat had been macerated in water a few days, its resemblance to the sac of an aneurism was even increased. Further on the subject will be recurred to.

The pathological observation was the following:—In the serum of the blood of the same rheumatic patient, taken the day before—the blood much buffed and cupped—a few whitish specks were seen. Under the microscope, besides a small number of blood corpuscles, many corpuscles were seen four or five times as large, confusedly granular and radiated, as if formed from prismatic crystals radiating from a granular base or centre, with some particles not unlike those of lithate of ammonia. Touched with nitric acid on thin glass, and exposed to a graduated heat, a distinct pink color was produced, which was heightened by ammonia, and was imparted to water, thus denoting the presence of lithic acid.

† A hen, fifteen weeks old, weight 3 lbs., temperature in ano 109°.

1·9 inch; in half an hour, when serum had been pressed out, it was 1·85 inch; two hours later it was diminished to 1·7 inch; the following day to 1·69; the next its diameter was hardly appreciably altered; the following there was a slight expansion, which continued for two or three succeeding days; and, as in the instance of the sheep's blood, the serum became reddish and the clot softened.

Amongst the admirable effects resulting from the contractile quality of fibrin, not the least remarkable and salutary is that of its promoting first the closure of incised wounds, and next the adhesion of the divided part, as witnessed in the process of healing by "the first intention." Thus, when a minute incision has been made with a lancet to obtain a few drops of blood for a microscopical purpose, if, after the flow of blood has ceased, the last exudation is not wiped away, shortly, as the fibrin which it contains contracts, the skin adjoining will have a puckered appearance, evidently owing to the contraction of the film of fibrin. And the agency, thus minutely illustrated, is it not of universal application? Is it not a fine example of the preservative powers of nature, and *vice versâ*, of the failure of those powers in disease? In healthy animals generally, the more energetic the heart's action and the greater the danger from hæmorrhage from a wound, the sooner the blood coagulates; and the contrary, the feebler the action of the heart, the feebler and commonly the slower is the contraction of the fibrin: birds ranking highest in the scale, reptiles and fishes lowest.

8. *On the Effects of the Boiling of Fibrin.*

In the second volume of my Researches I have given an account of the change produced in the textures of the human body by the action of boiling water, including a trial of the same kind on the fibrin of the blood. The result was nowise well marked, and was very different from that obtained by the coction of the same substance at a temperature of blood heat. Amongst my notes I find an account of the repetition of the experiments, made with some care and confirmatory of the preceding. I give it, reflecting on the importance of the substance, and for the sake of brevity, without comment.

A portion of moist fibrin (65 grs.) from the blood of an ox, prepared by washing in running water until all the coloring matter was removed, was boiled in 5 oz. of water for eight hours.

The decoction reduced to 2 oz. was semi-transparent, of a light brown color, which it retained after filtration. It had no smell, no taste, but it imparted a soft feel to the mouth, not unlike that from rice water. It was slightly precipitated by corrosive sublimate, distinctly by tincture of galls and by acetate of lead. Evaporated to dryness, it yielded ·2 gr. of transparent matter of a fawn color, in the form of an exceedingly thin pellicle, only slightly adhering to the platina capsule. This matter digested in about half an ounce of water dissolved for most part; the residue was very small, too minute for examination. The solution was affected as before by the agents mentioned. Again evaporated to dryness, it yielded the same kind of transparent pellicle, which had the pleasant taste of the gravy of roast meat, ozmazome.

The fibrin was boiled a second time in water for eight hours. The decoction was clear. It was rendered slightly turbid by corrosive sublimate, tincture of galls, and acetate of lead. Evaporated to dryness, it yielded ·1 gr. of a light brown color and transparent, like that first obtained.

The fibrin after these two boilings was of a light brown color; its elasticity but little impaired. Thoroughly dried, it weighed 14·4 grs.

XXXV. MISCELLANEOUS OBSERVATIONS ON BLOOD AND MILK.

1. *On the State of Combination of the Alkali in the Blood.*

The condition of the alkali in the blood—of that portion on which its alkaline reaction depends—has been the subject of much speculation, and of many experiments. Enderling is one of the latest inquirers who has given it his attention. After having made an analysis of the ashes of the blood, he has come to the conclusion, that the alkali in it is in combination with phosphoric acid, the former predominating in the form of the tribasic phosphate of soda.*

Granting the accuracy of Enderling's analytical results on the ashes, does it follow that his inference must be correct relative to the condition of the alkali in the liquid blood? It appeared to me doubtful *à priori*; and the doubt I entertained was confirmed by experiment. The doubt arose from considering the tendency of the alkaline carbonates, when strongly heated with charcoal, to be reduced; and when heated with phosphate of lime in excess, to exchange their carbonic acid for a portion of the phosphoric—the acid gas of course escaping, and compounds of lime and alkali remaining, each with excess of base. In accordance with this, when I have added carbonated alkali to the coal obtained from blood, and have reduced the coal to ashes, I have not been able to detect in the lixivium obtained from them any trace of carbonic acid. Moreover, I find that the carbonate of soda is liable to loss when heated strongly, exposed to the air; and, consequently, when it exists in a small quantity in a bulky coal, the whole of it may be dissipated—carried over much in the same manner as boracic acid is in combination with water as a hydrate, when it is subjected to heat.

If Enderling's view were correct, the blood, after having been acted on by the air-pump, ought not in its fresh state to yield

* See Mr. Paget's Report on the Progress of Human Anatomy and Physiology, in the British and Foreign Medical Review for January, 1845.

any carbonic acid on the addition of an acid. This is the experiment alluded to, which confirmed my doubt. I find that blood or its serum, after having been so acted on until perfectly tranquil, has effervesced strongly, when mixed either with dilute sulphuric or muriatic acid purged of air, or with a solution of cream of tartar. And, in accordance with this, I have also found that serum, after having been subjected to the air-pump, gives on coagulation, by immersion in boiling water, a different result, whether immersed unmixed, or after admixture with a little acid. In the one instance no air bubbles are disengaged; in the other very many.

Some years ago, when engaged in experiments on the blood, especially in relation to the present question—the condition of the alkali in it—I noticed the effect of cream of tartar in expelling carbonic acid, and that both from venous and arterial blood and from serum; an effect which, with other considerations, induced me then to conclude, that the soda in the blood exists in the form of the sesqui-carbonate; an inference which appears to me still to be most in harmony with the facts.*

In opposition to this view, perhaps it may be said that farther proof of its correctness ought to be afforded by the effect of a solution of muriate of lime on the serum—that, if the latter contain the alkali as stated, a precipitate of carbonate of lime ought to be the result. This experiment I have tried, with the aid of the air-pump, sometimes with a doubtful result, sometimes with a negative one, especially in the instance of serum from venous blood. But in these instances I have also found the result the same, even on the addition of a portion of sesqui-carbonate of soda, as much as ·2 of a grain to 316 grs. of serum—a quantity of the alkali, which, when dissolved in the same bulk of water, is more than sufficient to give a precipitate with muriate of lime. Would not this seem to indicate that in the blood and its serum the carbonated alkali is in a peculiar state of combination with the animal matter? And the same remark is applicable to the phosphates or their elements.

The trials referred to have been made on the blood and serum of the ox and sheep, at a favorable time of the year, during the winter season, when the temperature of the air has been little above the freezing point.

* Physiological and Anatomical Researches, ii., p. 152.

2. *On the Viscid Quality of the Blood Corpuscles.*

That the corpuscles of the venous blood of the mammalia, when quite fresh, and in the act of coagulating, collect together in piles, as it were by a kind of attraction, is well known. The viscid adhesive quality I am about to notice is distinct from this, and, indeed, is best seen when the aggregation in piles ceases to be witnessed, as in cruor, procured by breaking up the crassamentum, and separating the fibrin by straining through linen.

The cruor thus obtained is essentially a semifluid, the particles loosely adhering forming a mass in some respects not unlike honey or molasses. I shall notice some appearances connected with and indicating the condition referred to.

When poured into a fluid, such as water or serum, it rapidly falls to the bottom; and, in the instance of serum, if not agitated, remains as a connected mass. If now a glass rod be put into it, and withdrawn through the supernatant serum, it will come out not sensibly colored by the red particles; the surface of the cruor round the rod will be seen to be raised a little in the act from adhering to it, and then to return to its former level, shewing that the corpuscles adhered to each other in the mass more strongly than to the glass; and if the serum through which the rod has been drawn is examined with the microscope, a small number only of blood corpuscles will be detected in it.

If, instead of allowing the cruor to remain undisturbed, it be broken up by agitation with the serum, it will be found to be divided into clusters of corpuscles and detached particles. When one of these clusters is placed under the microscope, between two plates of glass, the adhering corpuscles forming the group are seen to be attached, not by their broad or concave surfaces, as in the instance of aggregation by piles, but by their narrow rims. Now, if graduated pressure be employed, so as to break up the cluster, just before separating, the adhering corpuscles will be seen to be elongated, as if drawn out almost to a fibre, and yet when detached, the adhesion being overcome, recovering, and that suddenly, their circular form; and, on relaxing the pressure, many of them will be seen to reunite, sticking to each other even when in motion.

This adhesive quality of the blood corpuscles is exercised, not only on each other, but also on other substances, though, perhaps, in a less degree. Proof of this is afforded when cruor has been allowed to remain, even but a short time, in a glass tube, or any other vessel. The portion in contact with the bottom of the tube is found to adhere to it, and is not easily detached; whilst any that may adhere to the sides commonly appears in streaks, the blood corpuscles being attached to each other, and so producing a linear arrangement.

This viscid property of the blood corpuscles must, I apprehend, be considered as specially belonging to them, quite distinct from the fibrin, which appears to be viscid only in its transition state, in the act of coagulating—previously even more liquid than serum attenuating the blood, and subsequently, as soon as coagulated, constituting the firmest and the cementing part of the crassamentum. The blood corpuscles, as regards this quality of viscidity, are far more constant; it belongs to them when fresh, probably when circulating in the vessels—it is exhibited in them long after removal from the living body, and is not even lost with incipient putrefaction, and, connected with that, the change of the particles to a globular form.

3. *On the Tendency of Fibrin in Coagulating to a certain Arrangement of its Particles.*

Amongst the many remarkable properties of coagulable lymph I am not aware that a tendency of its particles to arrange themselves in a certain manner out of the body, representing, as it were, what takes place in the body in the process of growth and of reparation, has hitherto come under observation, or, at least, has been the subject of commentary.

A striking instance of the kind I have witnessed in the buffy coat. When the buffy coat is well marked, as in cases of acute rheümatism, when it is thick and cupped, the blood abstracted having been slow in coagulating, it is easily detached from the soft crassamentum, and this is best done under water. Thus separated, it may be described as a fibrous mass loaded with serum, enveloped in a pellicle or membrane performing the part of a sac. This pellicle or containing membrane is very thin, yet of considerable strength, and with care may be dissected off, especially after maceration in water for two or three days, at a low temperature. It is very like a serous membrane, both as seen with the naked eye and under the microscope. Under the latter, it bears a strong resemblance to the arachnoid, appears as a tissue of extreme delicacy, hyoloid, without any visible pores or fibres, with a few particles like blood corpuscles, or their remains (according to the method used of separating it), scattered through it.* When a force is applied to it, it breaks less readily in one direction than another, and exhibits, when drawn in one direction, more elasticity than in the opposite. When the blood, as is usual, has been received in a circular vessel, and the buffy coat, of course, is of the same form, tearing the membrane with a forceps towards the margin, shreds of it several lines in length are easily detached in a line from the centre to the circumference, but not in a line at right angles to this; and in the same direction small portions of the membrane exhibit considerable elasticity, which they do not in the opposite direction.

I may mention another example, also well marked. If the blood, in the act of coagulating, is stirred with a glass rod, or a wooden skewer, or the like, the fibrin, as it is well known, will adhere, with which blood corpuscles will be mixed. The adhering clot, consisting of the two, the fibrin in excess, when pulled off, which it easily is, exhibits a canal with a smooth inner surface. If it be well washed to deprive it of coloring matter and slit open, it will be found to bear a close resemblance to an artery, especially to its middle coat, being composed of fibres arranged seemingly transversely, that is, at right angles to the axis of the tube. This is to be inferred from the effect of a force applied. If applied in that direction, transverse shreds pretty readily separate; but if in the opposite direction, using a forceps, only small bits. And in the one, the transverse direction, the tube is far more elastic than the other, after the manner of the middle arterial coat.

Other instances might be given, tending to shew the same disposition on the part of coagulable lymph to a certain regular

* The following note was made in 1842, at a time I was making experiments on fibrin:—"When fibrin is immersed in pyroligneous acid, it swells up, becomes like a semi-transparent gelatinous mass, and under the microscope exhibits many circular or globular particles, some very small, some as large or larger than blood corpuscles; but this not uniformly: there are parts *where there are no globules*, only diaphanous substance, giving the idea that where they are found they are not essential to the fibrin but accidental or adventitious."

arrangement of its parts, as it were, of a *nisus formativus*, in the act of coagulation. In examining the buffy coat, or the fibrinous masses which are so commonly met with after death in the right cavities of the heart, it is not uncommon to find in them, when divided, cavities containing serum resembling cysts. And in the ventricles of the heart, and the aorta and principal veins, especially the iliac and femoral, fibrinous concretions, as it is well known, are often found after death from lingering diseases, in which a puriloid matter is contained, as in a sac, a matter which has been imitated by Mr. Gulliver, by the coction of lymph, at about the temperature of the human body, and which, previous to his experiments, had been considered as pus, and erroneously as the product of inflammation.

I would ask in conclusion, is not this disposition of coagulable lymph called into play in other occasions during life, and may it not serve to explain certain appearances which are commonly accounted for in a different manner, such as the cysts which so rapidly form in the instance of aneurisms,* the consequence of wounds, and the lining membrane of the sacs of false aneurisms, which is hardly in appearance distinguishable from the inner coat of the artery with which it is continuous?

4. *On the Effect of Serum in Promoting the Coagulation of Milk.*

There is a marked difference, as is well known, between the albuminous part of the serum of the blood and that of milk—ordinary cow's milk—viz., that whilst the former is coagulated by a temperature below the boiling point of water, the latter in its fresh state is not so affected, even by ebullition, but, on the contrary, has its natural tendency to coagulate, connected with the absorption of oxygen and the formation of an acid, retarded. *A priori*, perhaps, it would hardly be expected, as regards the property of coagulation, that the one fluid mixed with the other would have any material effect. But that it is not so, I have

* The late Mr. Liston expressed his astonishment at the rapid manner in which an aneurismal sac is sometimes formed. He writes (Lancet, No. xiii., 1844):—"I have a preparation of an aneurism of two or three days' growth, with as regular and beautiful cyst as you would wish to see in an aneurism." Now, on the idea that the sac is formed as I have ventured to conjecture above, such rapidity of formation is easily accounted for. In a recent work "On Some of the more Important Diseases of the Army," in the chapter on aneurisms, there are appearances described well according with this mode of production of the aneurismal cyst—see p. 393.

found on trial. Milk, I find, when mixed with serum in certain proportions, is coagulated by heat. I shall notice some results obtained, using mixtures of the serum of the blood of the sheep, which coagulated at about 170° Fahr., and cow's milk.

Equal parts of the two remained liquid at 170°, and coagulated about 175°. The coagulum was of an opaque white, very little softér than the coagulum of the serum alone. Mixed with water, it did not render it milky ; and the watery infusion was not rendered turbid by acetic acid, and only in a very slight degree by the nitric acid.

One part of serum and three parts of milk in mixture, coagulated at about 190°, forming a soft tremulous mass, which, by boiling, was rendered firmer. Broken up and mixed with water and filtered, the fluid had the properties of weak whey, and was not rendered turbid by acetic acid.

A mixture of one part of serum and five of milk did not coagulate after several minutes' boiling ; but, keeping it on the fire, in about a quarter of an hour the effect was produced. The coagulum formed was very soft. Mixed with water, it rendered the water turbid at first ; but after a while, the finer particles subsiding, left the water clear, and it was not affected by acetic acid.

A mixture of one part of serum and five of milk, immersed in boiling water at least an hour, was found coagulated. The coagulum was of the consistence of very soft custard. A like result was obtained, using a mixture of one of serum and seven parts of milk.

Lastly, a mixture of one part of serum and ten of milk was liquid after more than an hour's boiling ; but after about three hours' boiling, when a portion of its water was expelled, it was found coagulated.

As in these instances the serum promotes the coagulation of the milk, so also the latter may be considered as favoring the coagulation of the former, that is, viewing the milk as a diluent of the serum, having the effect of removing further apart its albuminous particles, and comparing it as a diluent with water ; two parts of which, I find, with one of serum, prevent the coagulation of the latter, even at the boiling point, and thus heated, for many minutes.

The property exhibited in the foregoing experiments is to be witnessed in many other instances; it may be, probably, in every instance in which liquid casein and albumen are heated together in certain proportions. Mixtures of white of egg and milk exhibit it even more strongly than serum and milk. With equal parts of milk and white of egg, a firm coagulum is formed on boiling, the fluid exuding from which is almost transparent, and is not sensibly affected either by acetic acid or by the nitric acid. With one of white of egg and five of milk a coagulum is formed of moderate consistence; and with one of the former and ten of the latter, a very soft coagulum, after immersion in boiling water for about a quarter of an hour. I give these results, because in no work on chemistry with which I am acquainted, have I met with any account of the effect of white of egg on milk, even in culinary processes. Using the yolk of the egg in place of the white, the effect on milk has been, as might be expected, very similar, the chief difference in the coagulum being, that it has been somewhat softer.

From similarity of composition, it might be inferred that the roe of fish mixed and boiled with milk would have the effect of coagulating it; and this I find is the case. The only other animal substance I have tried has been muscle; it, triturated and boiled with milk, did not coagulate it; the muscular fibres, it may be noticed, were found collected together in a mass in a singular manner.

Extending the analogy to vegetable substances, it seemed likely that all those which contain albuminous matter, similar to casein, may be affected when boiled with serum or white of egg, in the same manner as milk; and the result of a trial, with a mixture of serum and a strong emulsion of sweet almonds, has confirmed the inference. After boiling, the fluid expressed from the coagulum was not precipitated by acetic acid.

As the serum of the blood of even the same species of animal is liable to slight variations, affecting the degree of temperature at which it coagulates, and as milk is subject to some variation in regard to the same quality, as indicated by the effect of rennet—a variation, perhaps, most of all depending on season of the year, and the time that the milk has been exposed to the atmosphere—should the experiments I have described be repeated, some little difference in the results may perhaps be perceived, depending on the circumstances just referred to.*

The action of one animal fluid on another, and those so similar as regards their albuminous part, as milk and serum, offers curious matter for speculation, and may be deserving of special attention, not only in relation to the culinary art, and processes of manufacture in which vegetable juices are concerned, but also in connection with physiology. It may be found that the principle of rennet exists in the blood, and that the analogous power of both, as regards the coagulation of milk, depends on the same cause; and if so, then it may be farther deserving of attention, in connection with pathology and processes of morbid softening.

* In Barbados, during a period of drought, when cattle were greatly underfed, I have found milk as taken from the cow, to be sure that it was not adulterated with water, of the low specific gravity of 1,020.

XXXV. OBSERVATIONS ON THE COAGULATION OF THE BLOOD.

In a recent work* of an elaborate kind, displaying much ability, its author, Dr. Richardson, has endeavoured to prove that the cause of the coagulation of the blood is of a chemical nature, and referable to the escape of a volatile matter, and that the volatile matter is ammonia.

It is not my intention at present to consider the various circumstances which he brings forward in favor of his conclusion. I shall restrict myself to a few observations which I have made, the results chiefly of trials instituted for the purpose of testing his speculations, and of satisfying myself, if possible, on the subject.

If the coagulation of blood depend on the escape of ammonia in any form, that is, however combined, and is purely a chemical phenomenon, it follows that, the escape of the volatile matter being prevented, the blood should remain liquid.

To determine this by experiment does not seem to be a difficult matter, whether as regards the means required or the results to be obtained. The blood I have chosen for trial has been that of the common fowl, obtained by dividing the great vessels in the neck. I have selected it because the blood of birds exhibits the phenomenon in question in the most striking and rapid manner. I shall describe a few of the experiments made.

Experiment 1.—A half-ounce vial in a few seconds was filled with blood to within a quarter of an inch of its neck, and was immediately closed with a glass stopper. In two minutes coagulation had taken place throughout; now, on withdrawing the stopper, a glass rod dipped in muriatic acid was brought near, as a test of ammonia; no fume, not the slightest, was perceptible. The blood, still warm, was next mixed with hydrate of lime; no ammoniacal odour could be detected.

Experiment 2.—Two ounces of blood were received in a glass

* The Cause of the Coagulation of the Blood, being the Astley Cooper Prize Essay for 1856, with Additional Observations and Experiments, etc. By B. W. Richardson, M.D. 8vo., London, 1858.

vessel, into which two drops of aqua ammoniæ had just before been poured; as soon as caught, it was gently moved by inclining the vessel backwards and forwards to favor the action of the volatile alkali; in less than two minutes coagulation had taken place. A distinct odour of the volatile alkali was emitted, and when a plate of glass, moistened with dilute muriatic acid, was for a short time kept over the vessel, crystals of muriate of ammonia formed on it, that is, after evaporation, and as viewed under the microscope. The clot was of about equal consistence throughout, and tolerably firm; cut into pieces, each piece, tested by the approach of the rod dipped in muriatic acid, shewed by the fumes produced the presence of ammonia. The serum, which in a few hours had separated from the crassamentum, had an ammoniacal odour.

Experiment 3.—To a mixture of water and aqua ammoniæ, formed of 12 grs. of the former and 1 gr. of the latter, in a vial of one-half ounce capacity, 277 grs. of blood were added as it flowed from the divided vessels; the glass stopper was instantly introduced, and, to secure admixture, the bottle was inverted two or three times. In about two minutes and a-half, coagulation had taken place.

Experiment 4.—277 grs. of blood from the same fowl were caught in a larger and thicker bottle, exceeding that used in the preceding experiment by 856 grs. (more than double its weight), and exercising therefore a greater cooling influence. This blood coagulated in about three minutes.

Experiment 5.—A small portion of blood from the same fowl fell on a flag-stone in the open air, the temperature of which, ascertained by the thermometer, was 40°. Ten minutes after the last had coagulated this retained its fluidity, and after other ten minutes it was only feebly coagulated.*

Experiment 6.—To a mixture of 13 grs. of water and 14 grs. of aqua ammoniæ of specific gravity ·88, in the half-ounce vial, the blood of a fowl as it issued from the divided vessels was added to overflowing; the stopper was immediately introduced. The blood which overflowed, it is remarkable, became tenacious

* In another experiment since made, the blood of an old hen received into a porcelain tea-cup coagulated in half-a-minute; a drop of blood that fell on a flag-stone of the temperature of about 42°, was liquid after eighteen minutes, and was only partially coagulated after thirty.

and viscid in less than a minute, even before another portion caught in a separate vessel had coagulated; the latter undergoing the change in about two minutes. The stopper taken out after an hour, the contained blood was found coagulated; the coagulum was soft and easily penetrated, very tenacious and viscid, of a dark color, and pungently ammoniacal. In twenty-four hours it had become somewhat firmer, and in three days a little more so, shewing a slight degree of contraction; but no serum had separated from the clot. A minute portion of it detached, which was not easily effected, owing to the tenacity of the crassamentum, exhibited, under the microscope, a confused appearance; diluted with serum and stirred, no blood corpuscles were detached, and using a compressor to spread out the little mass, the whole had the aspect of a fine granular tissue. This blood after twenty days experienced little change; and the remark applies equally to the two portions—that contained in the stoppered vial, and that which overflowed, received into a wine glass and merely loosely covered with tin-foil; both emitted an odour of the volatile alkali, slightly tainted as if from incipient putrefaction; the color of each had become of a rusty brownish red, but with little change of consistence; no serum had separated. After twenty-eight days the blood in the vial had become darker and softer and less viscid, whilst that in the wine glass retained its tenacity and consistence and color.

These results appear to me clearly to shew—1st, that there are no indications afforded of the escape of volatile ammonia during the coagulation of the blood of the fowl, or of its presence in the blood; 2nd, that the addition of ammonia in a notable quantity does not prevent coagulation; 3rd, that a sudden reduction of the temperature of the blood, even when fully exposed to the air, has a greater influence in retarding the coagulation than the assigned cause of its fluidity, the volatile alkali, when added.

The few trials I have made with the sesquicarbonate and neutral carbonate of ammonia have given results equally unfavorable, as they appear to me, to the hypothesis. I shall mention briefly two.

Experiment 7.—272 grs. of blood were received into the half-ounce vial, containing 12 grs. of water and 4 grs. of the sesquicarbonate of ammonia. An hour and a half after, the blood continued fluid; no separation of lymph had taken place, and no change of color. Three quarters of an hour later it had partially coagulated; this was in its inferior portion, the upper remaining liquid. The bottle was perfectly tight: no air had escaped; none had entered. On the day following, on withdrawing the stopper, the whole was found coagulated; the clot formed was soft and viscid, of a dark hue, and without any separation of serum. Under the microscope the red corpuscles were distinct, some appearing nearly of their natural form, some elongated, but none in piles. The blood had a strong ammoniacal odour.

Experiment 8.—To 20·5 grs. of water, containing 2·9 grs. of the neutral carbonate of ammonia, 234 grs. of blood were added, in the manner described, and in a vial of the same capacity. Partial coagulation occurred in about twelve minutes, and in fifteen it was complete throughout; the crassamentum was soft and florid. After twenty-four hours it was a little firmer, but no serum had separated, nor had any exuded after forty-eight hours, when it was of moderate firmness.

Thus it appears that these salts of ammonia, used in small quantities, have the effect of retarding coagulation, analogous in this their influence to most of the salts of the alkalies; and, like them, we know that in a large proportion they prevent it, and yet that on dilution with water coagulation takes place.*

I shall now briefly notice some other experiments which I have made with the same intent as the preceding, viz., the testing of the hypothesis advanced, that the presence of ammonia in the blood is the cause of its fluidity. My respect for its author, and the reputation that his work has acquired, induce me to go rather more into details than otherwise I should have felt inclined to do. The experiments I have to make mention of are of two kinds: one set had for their object to ascertain what is the amount of loss of ammonia from evaporation or volatilization, supposing it to exist in the blood, and it has an opportunity to escape in the act of coagulation; the other set were designed to ascertain the solvent power of ammonia acting on coagulable lymph—that substance, the passing of which from the liquid to the solid state, gives rise to the phenomenon under consideration.

Experiment 9.—In the same half-ounce vial as was used in the experiments already described, the aperture of which was one-thirtieth of an inch in diameter, 13·7 grs. of aqua ammoniæ

* See my Researches Physiological and Anatomical, vol. ii., p. 105, for instances.

were introduced, carefully weighed, confined by a glass stopper. The stopper was withdrawn ; it was replaced after eight minutes, and again weighed ; there was no appreciable loss ; the thermometer in room 52°. Added 13·95 grs. more of the solution of ammonia ; after half an hour the loss sustained was ·05 gr. There was next added a portion of white of egg, equal in weight to 171·26 grs. ; after forty-six minutes the loss was ·03 gr. The same immersed in water of 98° for five minutes suffered a loss of ·01 gr.*

In these experiments the specific gravity of the aqua ammoniæ had not been ascertained ; it was certainly higher than ·88, having been kept some months, and the bottle holding it repeatedly opened. In the following trials, aqua ammoniæ of the specific gravity named was used.

Experiment 10.—The same vial containing the blood mentioned in experiment number 6, holding 14 grs. of aqua ammoniæ, was carefully weighed ; the stopper was withdrawn, replaced after ten minutes and again weighed, the loss experienced was ·01 gr. The case of the balance, in which the vial was left open during the ten minutes, had a strong smell of ammonia.

Experiment 11.—To 214·7 grs. of water in the same vial, 16·9 grs. of aqua ammoniæ were added ; in eight minutes after withdrawing the stopper, the loss sustained was ·01 gr. The body of the vial was now immersed in water at 95°, and kept there for ten minutes ; the loss suffered was equal to ·04 gr.

Do not the results of these experiments go far to prove that, even on the supposition of ammonia existing in the blood in a volatile form, no appreciable quantity can escape in the short period of two or three minutes, which is about the time required for the coagulation of the blood of the fowl?

* Plunged into water of 210°, the white of egg coagulated, though a large proportion of the ammonia still remained in the solution. A mixture of white of egg and bicarbonate of potash similarly treated, an effervescence occurred, and coagulation took place. This I notice to shew that the presence of an alkali does not prevent the coagulation of albumen by heat, leading to the inference that its liquid state is not owing to the little alkali which it contains. I may mention another fact having the same bearing. A portion of milk, to which a little ammonia had been added, was put by to see how long it would keep sweet, air being excluded, the bottle being filled and well secured with a glass stopper. It was long forgotten; after two or three years it attracted attention. Now, it was found that a film of black matter, which proved to be sulphuret of iron, was formed, covering the whole of the inside of the vial; that the milk had acquired a brown hue, the cream on its surface remaining almost colorless, and that, though it was alkaline, yet a small portion of curd had formed.

For trial of the solvent power of the volatile alkali, fibrin was obtained from the blood of the fowl in the ordinary way by washing, but not carried so far as to remove the whole of the coloring matter, an effect in the instance of this blood not easily accomplished. By thorough drying, this lymph or fibrin was found to lose 85 per cent. of water.

Experiment 12.—To a mixture of 134·4 grs. of water and of 44·5 grs. of aqua ammoniæ, of specific gravity ·88, 1·2 gr. of moist fibrin was added. In twelve days, at a temperature ranging from 50° to 60°, the greater part of it was dissolved. When shaken it became slightly turbid: it had no color, as if the ammonia had destroyed the very little coloring matter adhering.

Experiment 13.—To a mixture of 127·6 grs. of water and of 5·2 grs. of sesquicarbonate of ammonia, 1·1 gr. of the moist fibrin was added. In the same time it was only partially dissolved; the undissolved portion was not inconsiderable; it was colorless: the solution was slightly colored red.

Experiment 14.—To a mixture of 146·2 grs. of water and of 3·1 grs. of neutral carbonate of ammonia, 2·8 grs. of the moist fibrin were added. The result, in the same time, differed but little from the last, both as regards color and proportional residue. Moreover, it may be mentioned that after a month no further alteration in either of the three was appreciable.

Experiment 15.—To a mixture of 1,400 grs. of water and 19 grs. of aqua ammoniæ, of specific gravity ·88, 18 grs. of moist fibrin were added. The mixture filled the bottle, which was closed with a glass stopper. It was kept at the same temperature as the preceding, and was occasionally shaken. After fourteen days the fibrin seemed little diminished in bulk; it had become viscid as well as transparent. The former quality was evident from the manner in which it was drawn out when inverted. As the fluid part, it was found, could not be separated by filtration, it not passing through the filtering paper, to ascerthe the proportion dissolved decantation was employed. About a half of it was poured off; but even this was not quite free from viscid lymph. Evaporated to dryness, it yielded ·7 gr.; the other moiety evaporated yielded 2·8 grs.

Experiment 16.—To a mixture of 1,400 grs. of water and of 26 grs. of aqua ammoniæ, 19 grs. of moist lymph were added, put into a bottle of larger capacity, so as to half fill it only.

The object was to see if any marked difference of effect would be produced by the presence of atmospheric air. The result in this respect was negative; as in the former instance, the fibrin was rendered transparent and viscid, and the decanted portion, on evaporation, yielded about the same proportional residue—viz., ·8 gr.

I have made other trials on the solvent power exercised by ammonia on moist fibrin, and both for a longer time and with more ammonia, but with results so much alike that I do not consider it necessary to detail them. They have all led me to the conclusion that the volatile alkali, in its action on fibrin, is chiefly remarkable for rendering it viscid, and that its solvent power is inconsiderable.* My results do not perfectly accord with those obtained by Dr. Richardson. In all the experiments he describes, quantities of lymph (10 grs.) were dissolved, he states, completely in from fifteen to twenty-one days in mixtures of water and ammonia, composed of 1,000 grs. of the former, and from 5 to 2 grs. of the latter, designated by "$N\ H_4\ O$," a proportion this, even the largest, less than that I used. The difference I cannot account for. I thought it possible that as fibrin freshly separated from the blood, and in its moist state, has a powerful attraction for oxygen, and in part liquifies with evolution of heat when exposed to its action,† that his experiments having been made, as I presume they were, without the exclusion of atmospheric air, the want of concord might thus be explained, but the last trial I have noticed does not support the conjecture. It is possible that the fibrin of birds may be less soluble than that of the mammalia, which he employed. But should it be so, the difference as to degree should not affect the argument. Even Dr. Richardson's own results, considering the proportions used of fibrin and ammonia, and the length of time required for the solution, seem to me in no wise to bear out the idea that the volatile alkali can be the solvent of fibrin in the blood in its healthy state.

Dr. Richardson, in support of his views, refers to some observations of the late Dr. Blair in proof of the existence of ammonia

* The fibrin, after solution, appears to be altered in its properties: obtained by evaporation (as the ammonia is expelled it forms as a pellicle), though still soluble in a slight degree in the volatile alkali, it is not rendered viscid, and, in consequence, there is no obstacle in the way of its filtration.

† See Researches Anatomical and Physiological, vol. ii., p. 343.

in the blood in disease. On consulting his writings, I find one instance recorded in which this alkali was detected in the blood of a person who had died of yellow fever, but in so small a proportion that none was "yielded by heat alone, and not much on the addition of liquor potassæ."* In the majority of instances that he examined the blood from patients laboring under this disease, he found it, contrary to his expectations, normal, its corpuscles unaltered in form, and collected in rouleaux—a circumstance this, I cannot but think, incompatible with the presence of ammonia—if its tendency is, as I have found, to render the blood viscid. My own experience, I may mention, as regards the state of the blood in yellow fever, accords with the ampler one of my valued friend. During life, I never found it otherwise than normal; after death, in some cases, I found that "it gave off an ammoniacal odour when mixed with quicklime; whilst in others this odour was not perceived." This I have stated in a note to Dr. Blair's "Treatise on Yellow Fever" (the epidemic which preceded the one last mentioned), of which I was the editor.†

Besides these opposing facts, which seem to me quite irreconcilable with Dr. Richardson's hypothesis, I might mention many more.‡

I have tested for ammonia the blood of other animals, and have in most instances been unable to detect its presence, provided the animals at the time were in a healthy state. The test employed was either hydrate of lime or liquor potassæ and muriatic acid, which, as is well known, when so diluted as not to fume perceptibly on exposure to the air, occasions a distinct fume when brought near any source from whence ammonia proceeds. I shall give one instance, and that from notes of experiments made more than twenty years ago, when engaged in an inquiry concerning the air in the blood. Venous and arterial blood were obtained from a sheep—the one from the jugular

* Report on the First Eighteen Months of the Fourth Yellow Fever Epidemic of British Guiana, p. 37.

† That ammonia should be found in the blood in most cases after death from disease is no more than might be expected, considering the tendency of this fluid to undergo rapid change, especially if there be any considerable lapse of time between the death and the examination. In my work "On Some of the more Important Diseases of the Army," many observations are given illustrative of this tendency.

‡ What follows was part of a paper which was read in the Physiological Section of the British Association for the Advancement of Science, at Cambridge, in 1862.

vein, the other from the carotid artery. They were kept excluded from the air by immersion under water in bottles completely filled and closed by glass stoppers. Tested after more than twenty-four hours, in the manner described (it was in winter), one, the venous blood, afforded a feeble trace of ammonia; the other, the arterial, not the slightest trace.

I shall mention another experiment of which I find a note, made eleven years earlier, viz., in 1827:—50 grs. of the sesquicarbonate of ammonia were added to 3½ oz. of venous blood. The color changed immediately to dark red. After twenty-four hours, the upper, the supernatant portion, appeared of an orange yellow; the under, abounding in red corpuscles, of a dark red. The former had not its viscidity increased—it was as liquid as before the addition; the latter, the under portion, was rendered more viscid. Both coagulated on the addition of water.

I shall venture to give the particulars of another experiment made about the same time; this was on the freezing of blood. About an ounce of venous blood, the instant it was drawn, was put into a freezing mixture. After about ten minutes its temperature was found to be 26° Fahr.; it remained at this temperature about an hour, all the time liquid; the red corpuscles had subsided, leaving the supernatant fluid colorless. It was then transferred to another freezing mixture of greater power, and placed under the receiver of an air-pump. The air-pump was worked till the blood was frozen. After having been some minutes frozen it was taken out; the temperature of the freezing mixture was now 10°. On pressing the frozen mass, a little fluid exuded from its centre. The experiment of freezing was repeated until the whole was frozen. The lymph was kept frozen about twenty minutes; the cruor in the central part three or four minutes. Brought into a room the temperature of which was 55°, the frozen mass gradually thawed; when thawed, it had recovered its liquidity, except that it was perhaps a little thicker than when first drawn. In about eight minutes from the time it was completely melted, it began to coagulate, and in two minutes more a soft crassamentum had formed.

I am tempted to add another fact in corroboration of those already given: this is the influence of carbonic acid on the blood. From the experiments I have made, it would appear that the saturating of the blood with carbonic acid rather retards than accelerates its coagulation, and at the same time tends to prevent its putrefaction. Now, were its liquidity owing to the presence of a minute quantity of the volatile alkali, surely the acid gas, by combining with and neutralising the ammonia, ought to have directly the opposite effect!

Were the question of the coagulation of the blood an abstract one, and not liable to involve practice, I should not have thought it necessary thus to dwell on it. Physiology and pathology are so intimately connected that they are almost constantly influencing one another. It is right they should do so. A physiological truth may lead to an improvement in therapeutics; and *vice versâ*, a physiological error may conduce to mistaken practice. The instance under consideration is, I believe, not an exception. Since the enunciation of Dr. Richardson's views, the volatile alkali has been recommended and prescribed in cases of thrombosis, with the legitimate hope, were the view correct, of its having a curative influence, a resolvent power. But, if the coagulation of the blood be not owing to a separation of ammonia, there is an end of the *rationale* of the treatment and a beginning of apprehension of resulting mischief from the charging the system with a compound of so much power as ammonia.

I wish I could conclude by offering even any hints likely to aid in the solution of the problem of the blood's coagulation. At present it seems to be an inexplicable property—the liquidity of the fibrin not necessarily depending on vitality, or the coagulation of the fibrin on a loss of vitality. Fibrin, coagulable lymph, the coagulable principle of the blood, seems indeed, as it were, a link between dead and living matter, partaking of the properties of both, much in the same manner, we presume, as chyme and chyle, especially chyme, has this double character.* The same may be said of the serum of the blood, a fluid equally essential to vitality as any other of its constituent parts, but in consequence of having properties less strongly marked than fibrin, not coagulating as it were spontaneously, but only at a temperature of about 160°, or when brought in contact with cer-

* In relation to vitality, what are we to think of the water which forms so large a proportion of the animal system both in its liquid and solid parts? If the lymph, when in its fluid state, possesses vitality, is the water to which it owes its fluidity destitute of vitality?

tain agents, it has drawn to itself less attention.* Yet, strictly speaking, are not its properties as difficult of theoretical explanation as those of fibrin? All the allied compounds, more or less of unstable composition, are marked by peculiarities, which, whilst they are characteristic of the individual compounds, seem to owe these their peculiarities to a something in their nature which has as yet eluded research. If we cannot say why fibrin, which circulates in the flowing blood as a liquid, coagulates, as it commonly does, when abstracted from the blood vessels, neither can we say why serum and the white of egg, which resist in a remarkable manner change at a temperature below 160°, rapidly pass from the liquid to the solid state when they attain this temperature. No more can we say why the liquid contents of the ova of fishes retain their transparency and liquidity, so long as water does not penetrate their enclosing membrane, the admission of water in this instance having a solidifying effect similar to that of heat and acids on the albumen ovi of birds and the serum of the blood of the mammalia. And are we not equally in the dark as to the rationale of the coagulating effect of rennet on the casein of milk, or the like effect of acetic acid on that substance? This, however, is clear, that the property peculiar to each of these unstable compounds is admirably adapted to the economy of the organism which it subserves.

* In the serum of the blood of a healthy animal I have sought in vain for ammonia: if ammonia escaped in the act of coagulation, ought it not to be retained by the serum? If the healthiest blood be kept twenty-four hours, even at a temperature of 55°, on admixture with hydrate of lime, and the approach of a rod dipped in muriatic acid of proper strength, fumes will be perceived indicating the production of ammonia; but not so with the serum after even double that time. The effect of the lime on the blood corpuscles—for on them I apprehend it acts—is the more remarkable, inasmuch as it exercises a preservative influence on muscle, and on animal and vegetable substances generally. Perhaps it acts conjointly with the oxygen contained in the blood corpuscles. That the ammonia is formed in this instance, may be inferred from its not being detected in the fresh blood.

XXXVI. SOME OBSERVATIONS ON THE ALBINO.

Amongst the natives of Ceylon, the occurrence of the albino, the offspring of dark-skinned parents, is not very uncommon. In looking over my note books, kept whilst I was in that island, between the years 1816 and 1820, I find mention made of five several examples. Now that the subject of species and varieties is attracting so much attention, perhaps the particulars I then collected of these abnormal instances of the human race may not be altogether without interest, even if given a little in detail.

In a work "On the Interior of Ceylon," published in 1821, in describing the native races, I have stated that "the color of their skin varies from light brown to black;" that "the color too of their hair varies, but not so much as that of the skin;" that "black hair and eyes are most common; that hazel eyes are less uncommon than brown hair; that grey eyes and red hair are still more uncommon; and that light blue or the red eye of the albino is the most common of all."

The albinos whom I had an opportunity of examining were all children of natives of ordinary color. As already mentioned, they were five in number, and besides these I heard of no others then alive, with the exception of one, whom I did not see.

Two of these albinos were brother and sister. The latter was twenty-three years old, of average height, well made, and in the enjoyment of uninterrupted good health. Her skin was very white and soft, especially where it had not been exposed to the sun. Her eyes were of a very light color, not very weak; she could bear moderate light well, but disliked strong light. The pupil shewed the absence of the pigmentum nigrum; it was of a light flesh color. The iris was nearly of the same color, but lighter; its converging fibres were of a light grey hue, and very distinct, having interstices between them of a flesh color. Her hair was nearly white, or rather of a light cream color, shining and fine, long, with a tendency to curl. Her eyebrows and eye-lashes were of the same hue, but rather lighter. Her brother, who was three years older, had the same color of skin, hair, and eyes ; and was well made, and in good health. He had a thin beard. His voice was feeble and somewhat effeminate. The disposition of both seemed to be mild and cheerful ; the expression of their countenance not disagreeable. In conversation they expressed themselves readily, and seemed, if not acute, not deficient in intelligence. Neither of them was married. When questioned on the subject, the brother said he should like to have a wife, did not his poverty prevent him. They were orphans and beggars. I was informed that their parents had other two children, who were also albinos: they died young.

The albino of whom I have next to make mention was a young woman, well grown, and rather tall, the only child of black parents. Her hair and eyes differed in color from those of the preceding. Her hair, long and fine, was much darker, of a yellowish brown. Her eyes were stronger, very like the eyes of a European, of the same light complexion—a pure blonde—the pupil being black, the iris blue. In England this young woman would be considered very fair, but not an anomaly. She too was unmarried.

The next I have to describe were sisters, two of a family of seven children, three of whom were albinos, four of the color of their parents. The eldest of the albinos I did not see ; she was married and living at a distance. She was described as being very like her sister next in age, a girl twelve years old. This girl was fully formed, her breasts well developed. Her skin was fair, but not remarkably so ; where it had been exposed to the air it was a little sunburnt. Her hair was of a yellowish brown color, long and fine. The eyes were large, well formed, and not weak ; the pupils were black, the iris bluish grey, its outer margin hazel colored. Her countenance was agreeable and intelligent ; and she was described as lively and well disposed.* Her

* Whether there is any mental peculiarity belonging to the albino is doubtful : I believe not. In a letter from a friend mention is made of three adult albinos, natives of the United Kingdom, a brother and two sisters, unmarried, the children of parents (these first cousins) of ordinary complexion. All three are described as unusually fair, with red, ferrety, tremulous eyes and feeble vision. The brother is said to have a first-rate talent for music, and would appear to be above par in intellect, as is also one of the sisters. They have two brothers and two sisters, who are no wise peculiar as to color. All of them are married, but, with the exception of one sister, are childless. Her children take after their father, who has dark eyes and hair.

I have been reminded by another correspondent of another example of the kind—*i.e.*, of albinism associated with more than ordinary intellectual power—in the instance of a Right Honorable holding at the present time (1861) a high official appointment.

sister, five years old, was fairer; the pupils of her eyes flesh colored, and much dilated; the iris bluish grey. In features she resembled her sister.

All these albinos were natives of the south-west coast of Ceylon, where the average heat is about 80° of Fahr., the yearly range remarkably small, little exceeding 10°, with a climate distinguished for salubrity, and the absence of malaria. The temperature of those I tried—the albinos—was not peculiar; like that of the other natives and of European residents, it was about 1° higher than that of a man in a cool climate.

I have heard it said that the albino is held in contempt by the Singalese; this, on inquiry, I was assured was a mistake; on the contrary, as regards color, that they are rather respected. Nor is such a feeling towards them surprising, considering that in the East a light hue is held to be distinctive of high caste, and *vice versâ*, the lowest castes, those in least easy circumstances and most exposed to the sun, being dark. Moreover, the ethnologists of Ceylon, the Singalese savants, are of opinion that the white races of mankind are sprung from the albino, and *ab origine* were merely an accidental variety.

It would be interesting to know what would be the offspring of albinos. The inquiry I made on this point was unsatisfactory; I could not hear of any descendant from albino parents, either pure or mixed. Judging from analogy, whether we regard the blondes of the human race or the white varieties of any domesticated animals, is it not probable that the complexion would be hereditary?*

The distinctive quality of the albino, at least in the highest degree, appears to depend on the absence of the pigmentum nigrum, and of its analogne to the skin, the rete mucosum (using the term conventionally), and of a like secreting structure, it may be inferred, in the bulbs of the hair. Now these we know exist greatly varied in different peoples, and even in different individuals of the same family. In those in whom the rete mucosum is least developed, the less we find their skin to be darkened by exposure to the sun's rays, and the fairer they

* Amongst the rarer instances of the hereditary transmission of peculiarities, I may mention one, well authenticated, and thus described in a letter from a friend:—"I once met with a man with brown hair, except one white lock in front of his head. He told me that this *white topping*, as he called it, was hereditary and characteristic of the family, Dowbiggin by name."

remain, even within the tropics, and from generation to generation, as is witnessed in the whites of Barbados and of the other West Indian Islands longest settled. On the contrary, where there is a well developed rete mucosum, the action of the sun's rays is found to have a well marked darkening effect. A gradation, feeble indeed, was noticeable in the skin of the albinos I have described, and in one of them, the least colorless, a tendency to sun-burn was mentioned. Taking into account this gradation, and this effect of the sun's rays, the speculation of the Singalese respecting the origin of the white races of men is not without the semblance of probability; and the more so, if we admit what seems to be proved by all experience, that the colored races are best adapted for warm climates, and that in the most unwholesome of these climates, they have a better chance of escaping disease and a premature death, and thereby extinction of race, than the whites.

XXXVII. ON THE EARLY GENERATIVE POWER OF THE GOAT.

In the young salmon, the parr, we have a remarkable example, now well authenticated, of the precocious development of the testes, with functional activity. What I have witnessed in the young male goat in Barbados, as regards its generative power, is hardly, it appears to me, less remarkable. I shall briefly notice the few circumstances which have come to my knowledge illustrating it, such as I can state with certainty as facts.

On the 2nd May, 1846, a goat which belonged to me gave birth to two kids, a male and a female. When less than a month old, the former exhibited strongly the sexual propensity. When about five weeks old the penis was protruded in his attempts to copulate. When four months old, the mother was in heat, and was covered and impregnated by her offspring. Five months after, viz., on the 2nd February, 1847, she gave birth to four kids, three females and one male, all of the usual size and vigorous. On the 10th of February I had the male kid castrated. Each testis was about the size of a French bean. A little transparent fluid was obtained from the vas deferens, which under the microscope, viewed with a high power, exhibited some granules, a few fine fibres, and one that had the appearance of a pretty well formed spermatozoon. The fluid procured from the incised substance of the testis contained many blood corpuscles, some dark granules, and a few small spermatozoa: these were best seen after having been dried on the glass support.

The young female received the male shortly after the mother, but was not then impregnated.

It is said that the goat breeds at six months old. It is also said that both male and female are two years in attaining their full size.

The goat of Barbados appears to resemble in every respect the common goat of Europe, from whence it is supposed to have been originally brought.

The precocity of the young male, as I have described, and of the effect of which in its generative power there can be no doubt, as the female had access to no other male, was there not considered extraordinary. Whether the same function at so early an age is exercised in a cooler climate, I am ignorant.* Should it be found to be so exercised, it may perhaps be considered a provision of nature to secure the preservation of the species, endangered by the localities the animal in its wild state inhabits amongst precipitous rocks, subject to the attacks of birds and beasts of prey. In accordance with this idea, I may mention that the young pair of kids, when five weeks old, when they began to eat grass freely kept constantly together, and were more frequently absent from than with the mother. The colostrum and milk of the goat, I may add, containing an unusual proportion of nutritive matter, as indicated by their specific gravity, may also be considered in accordance with the idea. The colostrum, the first drawn, I have found to be of the high specific gravity 1,088; it coagulated about 170°. The milk drawn the following day was of the specific gravity 1,041; it formed a soft coagulum at about 182°, and a firm one at about 190°. The milk drawn two days later was of specific gravity 10,343. After this it underwent very little change. Some drawn a week after was of specific gravity 1,033, and some drawn three weeks later was of the same specific gravity.†

* It probably is, judging from the analogy of the sheep—an animal so nearly allied to the goat, and which breed together. In Westmoreland the period of gestation of the sheep is held to be twenty-one weeks. An intelligent farmer informs me that a well-fed lamb will attempt the procreative act when threc months old; and that it is usual to put young males born in April amongst the ewes in October, and this with the design of crossing and improving the breed—the males chosen being white-faced. The offspring are considered superior in quality to those begotten by older and full-grown rams.

† The goat, I believe, like the ewe, eats the placenta and membranes, deriving, no doubt, from them nourishing support when so much needing it; and also the lubricating matter adhering to the newly-born, analogous to the vernix caseosa of the infant. One of the first acts of the ewe after parturition is to lick the lamb, every now and then stopping to nibble a little grass, as it were for the purpose of mixing with the animal matter before swallowing it.

XXXVIII. ON THE INFLUENCE OF A TROPICAL CLIMATE ON THE WOOL OF THE SHEEP.

The sheep of Barbados, originally from an English stock, affords a striking example of the change that may be effected by climate in a few generations in the character of the hair of an animal. In that island instances are frequently to be seen of sheep in which hair has so taken the place of wool (using the terms in their usual acceptation), that were it not for the form of the animals—that not altogether free from change—it would be impossible to suppose that they belonged to the same species as our English sheep.

Considering the subject of such a change not undeserving of attention, I have examined two specimens of hair procured for the purpose, one from a sheep two years old, the other from one about a year old, which were obligingly sent at my request by a friend, a resident.

Both were nearly of the same color, a light reddish brown, and were nearly of the same length, that is, the individual hairs varying from about an inch to an inch and a half. The hair of the three year old was coarser than that of the one year old. It consisted chiefly of harsh fibres, slightly tortuous, each about $\frac{1}{180}$ of an inch in diameter. Some were cylindrical, others were more or less flattened, all were tapering towards a point at their distal extremity. The hair of the one year old consisted of coarse and fine fibres in about equal portions; the one about $\frac{1}{363}$ of an inch in diameter, the other about $\frac{1}{1333}$ of an inch. The former resembled the hair of the older sheep; the latter had the appearance of wool, both in its fineness and general aspect, whether seen by the naked eye or under the microscope. The presence of a portion of wool mixed with the hair of the younger sheep accords, I may remark, with the belief expressed by my friend, by whom the samples had been sent, "that all the very young lambs of the island have wool, which, as they grow older, gradually passes into hair." This, he writes, he thinks is the fact, though he cannot say positively that it is, not having attended sufficiently to the subject.

Interesting in itself as exemplifying how nature fits an animal the native of a cool climate by a change in its clothing, to endure without discomfort the heats of a tropical region, it is not, as it appears to me, without value in its analogical applications. Though so much changed in appearance as is the wool in passing into hair, the one differs as little from the other in intimate structure as the hair of the woolly headed African does from the straight lank hair of the North American Indian, or as this does from the hair of the European. Examined under a high magnifying power, and with care, the difference in the qualities of all these kinds of hair, whether of the sheep or of man, color apart, appears to be merely in degree. The wool of the sheep and its hair are both solid, both exhibit the same transverse markings, the one strongly, the other feebly, and so of their other properties. The same may be said of the hair of the several varieties of the hnman race.* And keeping to the analogy with which all experience is in accordance, we may, I think, confidently conclude that provident nature has not been less careful of man than of the brute, and that what is peculiar in the hair of each variety of the human race, as in the color of the skin of each, is to be viewed rather as an excellence connected with climate, and the effect of the adapting power

* The above statement is founded on the result of the examination of the hair of the following individuals—English men and women, a white creole of British Guiana, a native of Madeira, a laborer, a Hindoo, a Madras coolie, a half-blood Indian of British Guiana, an Arrawack Indian of the same country, a Kaffer boy, a negro of the west coast of Africa, a creole negro of Demarara. These several specimens of hair, as seen under the microscope, were, as stated in the text, very similar in structure. The differences were chiefly the following. The hair of the Europeans, including that of the white creole, was comparatively most regular, smooth, and transparent; that of the Africans was least smooth, less transparent, more strongly marked transversely, and most fringed with offsets; that of the Hindoo, and also of the American Indian, differed least from that of the European. As to the coarseness and fineness, that differed (measuring the diameter of the hair) as much in persons of the same race as in those of different races. I have spoken of the hair as solid: its medullary part is described by some authors as containing air. May not this be owing to a shrinking in drying? In the strong hair of the tail of the horse, I have observed a central cavity after drying, but not when examined the instant it was pulled out. A section of the still coarser hair of the elephant, varying in diameter from $\frac{1}{50}$ of an inch to $\frac{1}{25}$ of an inch, is decidedly solid, like the substance of nail or horn; coloring matter is diffused through it. In the human hair coloring matter is also more or less diffused; the outer layer seems to be the portion most colored, and next to that the medulla.

of climate, than as a deformity in any instance, or as an unseemly degrading defect.*

* That climate may have in successive generations a considerable effect in modifying the quality of the hair it is easy, even *à priori*, to conceive; and that a hot *dry* atmosphere, not such as that of the West Indies, hot and commonly moist, may conduce to a crisp, curly state of it. Captain Sturt, in his very interesting narrative of an Expedition into Central Australia, gives the following account of the effects of a hot wind to which his party were subjected at Cooper's Creek. After stating its destructive agency on plants and animals, and how it was nearly fatal to him, he adds, " After this exposure my muscles became rigid and limbs contracted. Gradually also my skin blackened; I was reduced to a state of perfect prostration. Every screw in our boxes had been drawn, and the horn handles of our instruments, as well as our combs, were split in fine laminæ. The lead dropped out of our pencils; our signal rockets were entirely spoiled; our hair, as well as the wool on the sheep, had ceased to grow, and our nails had become as brittle as glass." A thermometer graduated to 127° burst from excessive heat, though placed in the fork of a large tree.

XXXIX. ON THE QUESTION, IS THE HAIR SUBJECT TO SUDDEN CHANGE OF COLOR?*

THIS is a question respecting which there is a great difference of opinion. The popular notion is decidedly in favor of the affirmative; and even many naturalists and physiologists have come to the same conclusion. In advocating it they adduce instances of persons who, according to report, when under strong emotions of grief or terror, have had the hair of their head suddenly changed, rendered white or grey, in a few days, or even in a few hours.

Haller, in his Elementa Physiologiæ,† refers to no less than eight different authorities for examples of such change. Yet he himself was far from satisfied of their accuracy. His brief comment is "Vehementer tamen improbabilis est historia." All that he can admit seems to be that, under the influence of impaired health, such a change of color may take place, but even then only slowly. His words are—"A morbis utique crederim, sed lente canitiem obrepere."

Marie Antoinette, Queen of France, is cited by those who give credit to the popular notion as a striking and well authenticated instance in its favor. In the account of this illustrious and unfortunate woman, in Rose's Biographical Dictionary, it is stated, in describing her appearance at her execution—"She was then in the thirty-eighth year of her age, but she appeared much older. Her misfortunes had changed the color of her hair to a silvery white, and her countenance had assumed an aspect of dejection and settled melancholy." It is added—"Her body was thrown into a pit in the churchyard of the Madeleine, and was immediately consumed by quicklime." Now, when we consider the age of the queen, and more, when we reflect on the trials to which she was exposed from the commencement of the revolution in 1789, and her close imprisonment for many months

* Read at the Meeting of the British Association in 1861.

† Vol. v., p. 37.

under circumstances which could hardly fail to impair her health, there is nothing very surprising in the effect on her hair; it comes under the condition admitted by Haller. That the change was sudden I am not aware there is any evidence, though De Lamartine asserts it, no more than of the asserted effect of quick-lime in immediately destroying her body—this, too, a popular notion, and altogether unfounded on fact, quicklime, instead of consuming, having the property of preserving animal matter. Had it been possible for mental emotion, whether of terror or of grief, to render hair suddenly grey, surely in the queen's case it should have been witnessed at an earlier period than that of the arrest of the royal family in their attempt to leave France. The night of horrors at Versailles—the murder of the gardes-du-corps—the sight of their bloody heads in the procession to Paris—were scenes more likely to make a deep impression than the detention referred to. Even were we assured by an attendant on the queen, as stated by De Lamartine, that her hair in one night had changed its color,* are we bound to believe it? I think not, inasmuch as there might be a motive, for the sake of exciting compassion, to make the statement. Who does not know how difficult it is to obtain a true account of any event, especially when the feelings are concerned, the individual of high rank, and an object of universal interest!

If emotional feelings could have the effect presumed, might we not expect to witness it amongst troops engaged in an active campaign amidst all the dangers and horrors of war? I have examined thousands of soldiers, men prematurely worn out in the service in various climates, and concerned in many a hard fought battle—many of them grievously wounded—and never met with an instance of the kind. There is indeed a story of a soldier in whom the phenomenon is said to have occurred, and that soldier the hero-king of Navarre, Henry IV. of France. It is gravely related of him, that when under the influence

* De Lamartine thus describes the circumstances: he had previously stated that the queen burst into tears earlier in the night of the detention. "The king, the queen, Madame Elizabeth, and the children, lay down for a short time dressed as they were in the rooms of M. Sausses, amidst threatening murmurs of the people and the noise of footsteps, that at each instant increased beneath their window. Such was the state of affairs at Varennes at seven o'clock in the morning. The queen had not slept; all her feelings as a wife, mother, queen—rage, terror, despair—waged so terrible a conflict in her mind that her hair, which had been auburn on the previous evening, was in the morning as white as snow." Hist. of the Girondists, i., 69.

of surprise and sorrow one of his whiskers suddenly became grey.*

Another anecdote, and hardly less marvellous, has recently been published, of a similar change of color of hair of a soldier, a rebel Sepoy, in whom it is stated by Dr. Laycock in the April number (1861) of the British and Foreign Medico-Chirurgical Review, on the authority of Surgeon Parry, that in a short time, in half an hour, his hair, previously black, became gradually grey. The man was brought in a prisoner, and subjected to an examination, it is said, naked, and that under the influence of terror in apprehension of being condemned to death the effect was produced.† Not questioning the accuracy of the recorded observation, may not the phenomenon be accounted for without having recourse to other than well known causes? Having been hurried in a prisoner, it is easy to imagine that he might have been profusely perspiring, and that his hair might have been wet from sweat; and that, being naked and cooling and drying rapidly, the hair, if before grey, darkened by moisture, may have recovered its natural color. The effect of water in intensifying color is well known; it is strikingly exhibited on a flagged pavement during a shower of rain, when every drop that falls produces a black spot on the stone, that being grey when

* When the news came to Henry of Navarre that the king (Henry III.) had really promulgated this fatal edict [that of Nemours in 1585 revoking all previous edicts in favor of the Huguenots], he remained for a time with amazement and sorrow, leaning heavily upon a table, with his face in his right hand. When be raised his head again—so he afterwards asserted—one side of his mustachios had turned white." Motley's Hist. of the Netherlands, vol. i., p. 132, quoting Mathieu as his authority. He quotes L'Estoile for a counter-anecdote, one more credible and in character. "When the news of the day of the barricades [threatening destruction to the Huguenots in Paris] was brought at night to that cheerful monarch (Henry of Navarre), he started from his couch. 'Ha!' he exclaimed with a laugh, 'but they havn't yet caught the Bernese.'" Idem ii. 432.

† The following is Mr. Parry's account of the case:—"On February 19, 1858, the column under General Franks in the south of Oude was engaged with a rebel force at the village of Chamda, and several prisoners were taken. One of them, a sepoy of the Bengal army, was brought before the authorities for examination, and I being present had an opportunity of watching from the commencement the fact I am about to record. Divested of his uniform and stripped completely naked, he was surrounded by the soldiers, and then first apparently became alive to the dangers of his position. He trembled violently, intense horror and despair were depicted in his countenance, and although he answered the questions addressed to him, he seemed almost stupefied with fear. While actually under observation, within the space of half an hour, his hair became grey in every portion of his head, it having been, when first seen by us, of the glossy jet black of the Bengalee; aged about 24. The attention of the bye-standers was first attracted by the serjeant whose prisoner he was, exclaiming 'He is turning grey,' and I with several other persons watched its progress. Gradually, but decidedly, the change went on, and a uniform greyish color was completed within the period above named."

dry. And what adds to the probability of this explanation, is the circumstance that the natives of Bengal are in the habit of staining their hair: this I have learnt through a friend, from a medical officer of long experience in India, and who, when consulted as to the cause of the asserted change of color, said it might be in some way connected with this habit.

I have referred to the Transactions of the Royal Society, thinking it not impossible that in that great record of phenomena and marvels—the latter such as are to be found in its earlier volumes—an instance might be given of the change in question; but my search was vain—a circumstance this alone, I cannot but think, opposed to the occurrence ever having taken place; for had it been at any time undoubtedly witnessed during the last two hundred years (the period to which the record extends), is it likely that so remarkable a fact would pass undescribed? In the index to the abridgment of that work, in the references respecting hair, there is indeed one of a marvellous kind, viz., "of a corpse long buried being almost wholly converted into hair." This seems to have been believed at the time, 1681; even a specimen of the hairy growth was produced. It was placed, we are informed, so much was it valued, in the Repository of the Royal Society in Gresham College. It is described as "stiff, red, somewhat curled, but rotten." And, not only was belief attached to it, but even an attempt was made to explain the mutation and growth, on the hypothesis of hair being of a vegetable nature, "an animal plant," in common with "wool, feathers, nails, horns, teeth, etc." The author of this hypothesis was Honoratus Fabri; but the individual who used it to account for the marvel was no less a person than Dr. Edward Tyson, a man to whom sound and exact anatomy is not a little indebted.

Viewing the subject irrespective of historical or reported evidence, can anything be adduced in support of the popular notion on physiological grounds? I am not aware that anything can. The hair, like the nail, like the cuticle, is without sensation—is without a circulating fluid—is without life; it has been well called "anorganic except at the root" from which it springs. It cannot be injected. In trials which I have made, using coloring fluids, such as a solution of nitrate of silver, and a solution of iodine, I have not observed any change of color beyond the portion of hair actually immersed, that is, when employing single hairs. Whatever it owes its color to, whether to a fixed oil or to a peculiar arrangement of its constituent molecules, or to both, it resists decay in a remarkable manner; it resists the action of acids and alkalies, all but the strongest, those which dissolve or destroy it.* It resists maceration, and even boiling water, except continued for a long time and under pressure, when it suffers disintegration and decomposition.

Such being some of its qualities, can we credit its being subject to any sudden change of color? I think not. That in progress of growth from its living bulb, the gland which secretes it, after the same manner that the cuticle is secreted and the nails, it may at different times vary in color according to the state of the secreting organ, is an admitted fact. In accordance, it is not unusual to see a single hair of different colors, one portion grey, its superior, or *vice versâ*, its inferior grey, its superior portion still colored; this last the more common occurrence connected with advancing age—the former shewing an effort to the restoring of color, associated, it is probable, with improved health and vigor.†

Another fact is admitted respecting hair, viz., that it may lose color from exposure to the sun's rays, which act on it and on the living cutis in a totally different manner—bleaching in one instance, as when acting on dead substances, cotton and wool; darkening or blackening in the other—exciting, it would seem, an organic change. But this effect of the sun's rays in bleaching the hair will hardly account for any very sudden change of color. It takes place slowly, and is only to be witnessed in persons who are very much in the open air with their heads uncovered.

* I have observed no change of color of hair (light brown, and also black) from immersion in strong muriatic and sulphuric acid for twenty-four hours, or from exposure to muriatic or sulphurous acid gas for the same time. Strong nitric acid decomposes it with effervescence, nitrous gas being disengaged. Chlorine bleaches it, and at the same time renders it tender and glutinous. On exposure to the air, light brown hair recovers its color, but not its strength and elasticity.

† If it were possible for hair to become suddenly white, might not the contrary be expected, viz., a sudden recovery of color? Of this latter I am not aware that a single instance has been recorded, or even hinted at as possible. Examples, however, of a slow recovery of color have been witnessed, and have been well authenticated. In the discussion which took place after the reading of this paper at the meeting of the British Association for the Advancement of Science held at Manchester in 1861, a physician of eminence stated that a young lady had come under his observation whose hair had become grey gradually during an illness, and during convalescence slowly recovered its pristine hue.

Those who adopt the popular belief, endeavour to support it and render it credible by referring to the changes which take place in the plumage of certain birds, such as the ptarmigan, and in the hair of certain quadrupeds, such as the mountain hare and ermine, at different seasons of the year—each becoming white towards winter, and of a darker hue when the winter is passed—resting their argument on the belief that the change of color is not the effect of moulting or of change of coat, but that it takes places in the existing feather or hair. Belief, however, is not proof; and I am not aware that there is any satisfactory evidence of either the feathers or the hair in the living animal undergoing such a change; and, considering the qualities of both, it seems most improbable.

In the instance of the ptarmigan there seems good proof that the change of color is decidedly connected with moulting and a fresh growth of feathers. This, at least, was the impression I received from inspecting the fine series of specimens of this bird, shot at different seasons, belonging to Mr. Gould, of which he indulged me with a sight. According to this eminent ornithologist, the ptarmigan "is always moulting," the changes of color being from white to brown and speckled: the brown the summer plumage, the speckled the autumn, the white the winter. The speckled are few and large, overlapping the white, then in process of growth; as soon as the few speckled ones are shed, the bird displays its under white dress. The brown are decidedly a new growth after the moulting of the white, probably immediately after the breeding season; and they, the brown, are succeeded after a fresh moult by the speckled. That the moulting of the warm winter plumage takes place after the breeding season, I am led to infer from the analogy of the goose and of other birds in the Arctic regions. From Sir John Richardson, I learn that the bird named moults at the time mentioned, and is then so defenceless, from want of power of flight, that it is easily run down and killed. It is thus endangered only for a short time. So rapid is the growth of the new feathers, their bulbs at the period being very vascular, that in a week or ten days they attain their mature size.*

* The same rapidity of growth is witnessed in the wild swan. The Rev. T. Metcalfe (the Oxonian in Iceland) says, "these birds with broods do not begin to moult till the middle of August; and in two weeks they are again able to fly."

At the discussion which took place after the reading of this paper, two or three of

And that the change of color of those quadrupeds which are most remarkable for a whitening of their coat on the approach of winter is owing to the same cause, seems most probable.

the gentlemen present expressed an opinion in favor of a change of color in the feathers of birds, irrespective of growth; and the late Mr. Yarrell was referred to as an authority in support of it. Mr. Yarrell, in 1833, contributed a paper to the Linnæan Society, entitled "Observations on the laws which appear to influence the assumption and changes of plumage of birds." After a careful reading of it, I must confess that my scepticism on the subject has not been removed. According to him, there are three modes by which changes in the appearance of the plumage of birds are produced: "by the feathers themselves becoming altered in color; by birds obtaining a certain number of new feathers without shedding the old ones; and by an entire or partial moulting, at which old feathers are thrown off and new ones produced in their place." Relative to the first, the questionable change, he admits that there is a difficulty in understanding it, inasmuch as no vascularity can be shewn in the part, that is, beyond the bulb and its pulp, when the feather is growing, and he prefaces the remark by stating that Colonel Montagu, an eminent ornithologist, and who had made the feathers of birds a subject of his special inquiry, "was unwilling to believe that the feathers themselves changed color." Mr. Yarrell meets the difficulty by the assertion that the change in question, however inexplicable, is a fact. His main proof, as well as I can gather from what he has written, rests on one experiment—the marking of feathers and observing them during their growth. He says, "This experiment I have performed with the exact result which had been anticipated. A herring gull at the Society's Gardens was examined at Christmas. Several tertial feathers were found to have their basal halves blue-grey, the other parts mottled with brown. Two notches were made with a scissors on the webs of these feathers, intended to refer to the colors then present. Some other feathers were wholly mottled with brown, and were marked with one notch. This bird was re-examined in April. The tertial feathers, which, when marked, were of two colors, were now entirely blue-grey; one was tipped with white. The other feathers, which, when marked, were wholly mottled, were now for two-thirds pure white, the terminal third alone retaining the mottled brown." This appears to have been a solitary experiment, and made with a confident anticipation of the result, and at the Zoological Gardens, and it may be inferred with the help of the keeper who had charge of the bird. It is not for me to say that any deception was practised to make the result tally with the anticipation; but it is well known that frauds of the kind have been put on men of science, who trusted to mercenary assistants. Had the experiment been made under Mr. Yarrell's sole care, I should have had more confidence in its accuracy. Mr. Yarrell adduces in support of his belief very many observations of one of the keepers, James Hunt, which seem to me to want the precision requisite to give them value. What adds to the difficulty of imagining such a change of color in feathers as Mr. Yarrell describes, is the fact that when taken from the bird they are retentive of their colors in a remarkable manner, much more so than dyed feathers, as is well known to the angler: water does not wash out their hues; and exposure to the sun's rays has a very slight bleaching effect on them. That the feather of fresh growth may be brighter than an old feather it is easy to conceive, being cleaner, probably less dry, and not worn like the old, nor its delicate surface impaired; but that it should lose one color, and acquire another, cannot be so accounted for; and as such a change is not witnessed in the plucked feather, is there any proof of molecular action? Mr. Yarrell seems to think that the qualities of the feathers are not identical in the living and dead bird. He says that in certain birds, especially ducks, they have not the power of resisting the constant action of water as in the living birds. He seems to forget that during life there is constant attention given by these birds to their feathers, which the term preening expresses, and that they are provided with a special gland furnishing an oil fitted for the purpose. He says also that it is well known to ichthyologists that the scales of fishes (these the analogues of feathers), during the breeding time "become brighter as the season for spawning approaches." Again, he seems to forget that the coloring matter is not situated in the scales, which are more or less transparent, but in the underlying integument.

Such facts as I have been able to collect lead decidedly to this conclusion. Besides the striking examples of wild animals, such as the mountain hare, the ermine, the Arctic fox, the lemming, there are others in whom a like change takes place, though in a less marked manner—examples always under our eyes, such as the horse and the cow, in whom the alteration of color may be seen if carefully observed. Their winter coat is formed of hair longer and thicker than their summer coat. As the spring advances, they lose the former; it comes off, and is commonly succeeded by one not only of shorter but also of darker hair than the winter coat. In the month of April, when the shedding is in progress, the contrast is well marked, and the change very clearly is not in the individual hairs, but from the growth of new hairs. Professor Rolleston of Oxford has brought to my notice an example of a pony which has been observed to experience a change of its coat from tawny in summer to nearly white in winter, and has kindly sent me a portion of its hair when in the act of exchanging its winter for its summer coat. The new tawny hairs are of uniform color throughout, as are also the white winter hairs.* And in the skins which I have examined of the ermine and mountain hare, killed in winter and in summer, I have observed the like, indicating clearly a distinct growth.†

* I have since been favored with portions of the hair of this pony, taken from it in February, March, April, May, on the 1st of each month. In the portion of the February hair there are extremely few colored ones, also very few in that of March; in that of April there is an increase of them; and in that of May the colored hairs are the majority. Their color, called tawny in the text, is reddish-brown. Irrespective of color, there seems but little difference in them. Those that are white, are so throughout; those that are colored, are colored also throughout. As seen with the microscope, their diameter is little different, so too their structure; the colored appear, if anything, rather more cellular than the white. Many of them have been pulled out with their bulbs. The bulbs of the white are smaller than those of the colored. The mane of this pony was subject to no change of color with change of seasons; it was always nearly white. So in the mammals and birds, which are most remarkable for change of color, there is some portion of the hair and feathers exempt from change; another circumstance in accordance with the conclusion that the individual hair or feather is not subject to change.

† In the specimens which I have examined of the ermine I have found the summer-fur to consist of hair colored most towards the upper end, towards the tips, and colorless towards the roots and the inferior portion, the colorless part very much finer than the upper, so as to be easily broken.

Through the kindness of a friend I have now before me four specimens of the skin of the mountain hare, from Sutherlandshire, ticketed January 1, 1862, March 28 of the same year, and July 3, and October 30 of the year preceding. They are now in the Museum of the University of Oxford. The first is uniformly white, with the exception of a just perceptible greyish tinge, confined to a small space over the back. The second is more colored over the back, mottled—grey, black, and very light brown.

An author, Mr. Erasmus Wilson, who advocates the popular doctrine, adduces in support of it an experiment made by Sir John Ross on a lemming, which, exposed to a cold of 30° below zero, in one night, it is stated, became partially white, and at the end of the week was entirely white. It is added, " the white hairs were found to be much longer than those of the unchanged patch, the blanching being confined to that portion which exceeded in length the natural hairs," . . . . . . " so that when the white ends were cut off, the animal appeared to have regained with very little alteration its summer coat, without any reduction of the length of its fur."* This account is nowise satisfactory; if correct, would it not imply that the longer white hairs were a new growth ?† An author, a naturalist, Mr. Blyth, whom we are more disposed to rely on for accuracy than the Arctic navigator, states that he has examined this animal, the lemming, one which was killed during its autumnal change, and satisfied himself " that the white hairs were all new, and not the brown changed in color."

That the summer coat and plumage should be of a darker hue than the winter coat and plumage is no more than might be expected, that is, if the darker color is indicative of greater vigor, such vigor commonly belonging to animals in spring-time, arising, it may be, from more causes than one—partly from an increase of temperature, partly probably from a fuller and more nourishing diet, and partly from an increase of *vis vitæ*

The third, exhibiting the summer coat, is colored generally over the back and sides; the prevailing color dark brown: when narrowly observed it has a speckled appearance, the tips of the hairs being a lighter brown. The fourth is more widely colored than the second, but of a fainter hue, brown predominating, that lighter than the summer brown, and less mottled with grey and black than the second. The under surface of each skin, that corresponding to the abdomen, is white; but in that of the third, only a third portion is without color. Whether colored or colorless, the hair is very fine towards the roots, only about $\frac{1}{2000}$ of an inch in diameter; and also fine, but in a less degree, towards the apex. The structure of the white hair, as seen under the microscope, is more distinctly cellular than that of the colored. The latter are often colored almost throughout, their finest portion only being destitute of color. The summer hair is shortest, and, as a whole, less thick than the winter coat; the latter is rather more than an inch in length, the former hardly three quarters of an inch, and its individual hairs are coarser than those of the winter coat. The under surface hair, as well as whitest, is longest, some of it exceeding two inches in length. It need hardly be remarked how admirably the varying quality of the coat of the mountain hare is adapted to its wants, and especially its preservation—the color according with the prevailing color of its haunts at the several seasons, and the thickness of its fur, and consequently its bad conducting power, increasing in winter when most needed.

* See Mr. Wilson's Popular Treatise on the Skin, Hair, etc., p. 104.

† Foot note to Bell's British Quadrupeds, p. 153.

following the depressing influences of the chilling winter season, and in the instance of birds the process of incubation; and circumstances, many of them of an opposite kind, may be in operation during the decline of the year, and as much conducing to the growth of a finer and warmer and whiter coat and plumage in those animals endowed with an idiosyncracy marked by the change in question.

In conclusion, I cannot help thinking that, whether we consider one side of the argument or the other—the human evidence so questionable, the physiological so much more reliable—the idea of fallacy is unavoidable as to the hair being subject to sudden change of color from mental impression. The attempts that have been made to explain such a change by those physiologists who have given credence to the popular belief, are allowed to be complete failures. Other and more amusing attempts have been made to account for the phenomenon—these on the ground of a fallacy. I shall mention only one; and I can vouch for the accuracy of what I am about to relate. Many years ago, when on foreign service in Malta, the assistant-surgeon of a regiment stationed there became insane, and in consequence was placed under restraint in confinement. I paid him a visit about a fortnight or three weeks after. To my surprise, I saw that his hair, before brown, had become grey. I called the attention of his medical attendant, the surgeon of the regiment, to the fact, who, better acquainted with his patient than I was, immediately explained it, saying, "Your surprise will cease when you know that * * * has, since he has been afflicted with his malady, discontinued dyeing his hair." Now, when we consider how prone the hair of some individuals is to turn grey at an early age, and this even without any accompanying or preceding bodily ailment, and how there are persons who would wish to conceal this blemish, and for the purpose have recourse to chemical means, it is easy to imagine that this source of error, as illustrated by the preceding example, may be not unfrequent. Another tendency, that of a mental kind, ought not to be overlooked, viz., the disposition in some individuals to make statements merely for the sake of exciting momentary surprise or of acquiring ephemeral notoriety. If we consult the records of imposition and delusion, we shall find many a thing attested and for a time believed, of as marvellous a kind as the sudden whitening of the hair. Has not witchcraft had its defenders! Has not table-turning, clairvoyance, spirit-rapping, had believers! Have there not been even physiologists who have given their credence to the spontaneous combustion of the human body, and to equivocal generation!

XL. ON THE ACTION OF QUICKLIME ON ANIMAL MATTER.*

It is commonly believed that quicklime acts powerfully on animal matter, and is capable, to use a popular expression, of consuming it in a very short time. Passages to this effect are to be met with not unfrequently, not only in newspapers, but also even in works of literary repute. In a preceding paper I have given an instance of the kind from Rose's Biographical Dictionary. Moreover, from the same popular notion, quicklime not only of old was, but even recently—as recently as the Crimean campaign—has been recommended for use in graves, and we are informed was employed largely in the interment of the dead in the camp before Sebastopol.

In a work published now twenty-two years ago † I endeavored to prove experimentally that the effect of lime is the opposite of destructive, that it exerts a preservative rather than a destructive influence on the majority of animal and vegetable substances. The trials then described were made in the moist way, as with cream of lime or lime-water, with a great excess of the alkaline earth. I shall now describe some other trials in which dry lime, unquenched lime was employed.

On the 3rd of February, 1859, some insects, one of which was a beetle (*Geotrupes stercorarius*), the ova of the salmon, sugar-cane, and portions of some other vegetables were buried in quick-lime in powder, and the including vessel was merely covered with paper. Not till the last week of June of this year, 1861, was an examination made as to the effects, if any, that were produced.

First, of the lime itself: this, for the most part, was found to be converted into carbonate. Next, of the objects included: these were in a shrunken state, such of them at least as were capable of shrinking, and were all intensely dry and brittle. Their color was little altered, nor were the forms of those altered

* Read at the meeting of the British Association in 1861.
† Researches Physiol. and Anat., vol. ii.

which were capable of resisting shrinking from drying, such as certain leaves, the sugar-cane, the beetle. And that little other change than that resulting from desiccation was produced, was proved by subjecting the parts most prone to decomposition to the microscope. The mention of one trial may suffice. A leg of the beetle, after having been steeped a short time in water, was laid open, and what appeared and proved to be the muscular portion of the limb was detached from the horny integument. It exhibited, under a high power, the muscular fasciculi in an unmistakeable manner, and the striated fibres even as distinctly as in the fresh voluntary muscles.

A second trial was made in May of the present year. The subject of this trial was a mouse. It was buried in quicklime in a garden-pot, covered with mould, and over all a paper. When taken out on the 27th of June, after forty-four days, most of the lime was still either quicklime or hydrate; the body emitted a slight disagreeable smell; but, with the exception of being shrunk, as if from drying, its form was little altered, its fur was unchanged. A portion of muscle taken from the thigh, after being moistened, subjected to the microscope, was found to display the striated fibres with perfect distinctness.

Are not these results conclusive that lime *per se* has no consuming power over animal matter? And as regards desiccation and its preservative power witnessed in the instance of the muscle—the latter effect depending probably on the former, are not these such as might be expected *à priori*, keeping in mind the powerful attraction lime has for water, as shewn in its conversion into a hydrate, when as much as 24 per cent. of water is solidified?

The same desiccating effect of lime, carelessly observed, may have given rise to the popular belief and error already alluded to, and may help to reconcile practice and theory, explaining how quicklime may be usefully employed in interments, when the main object is to check putrefaction and prevent the disengagement of the noxious gases evolved in the putrefactive process.

In the work already referred to, after describing the results of the trials made in the moist way on animal and vegetable substances, proving that lime exercises on them a preservative influence, an opinion was offered, founded on the same results, that lime-water possesses in a low degree a solvent power. In confirmation, I shall mention an additional trial—one since made —in which the following substances, viz., salmon ova, two or three different kinds of flies, a cricket, sugar-cane, and lettuce seed were kept in a stoppered bottle from the 3rd January, 1859, to the 1st July, 1861. When examined at the expiration of the time specified, all the different specimens were found, with the exception of the ova of the salmon, little altered in form, rather distended than contracted. A leg of the cricket was laid open, and what appeared to be muscle was subjected to the microscope. It had all the character of muscle, with the exception of deficiency of striæ, accompanied by an appearance of attenuation. The salmon ova had disappeared, leading to the inference that they had been dissolved. The clear fluid, the lime having subsided, was of a yellowish color. A portion of it heated became turbid; it frothed when in ebullition; and evaporated it yielded a residue proportionally small in quantity. This residue burnt with flame, and left a little coal, which was readily incinerated; the ash, in minute quantity, was chiefly lime.

These results need little comment. They seem to prove that, whilst lime tends to prevent putrefaction and fermentation, and thus to preserve from rapid decay animal and vegetable substances, it allows, when moisture is present—that being essential to change—of partial solution; and further, in process of time, when the lime has become saturated with carbonic acid, of disintegration and decomposition of the same substances; and these thus slowly set free may become active and exercise that fertilizing influence in agriculture for which they are distinguished.

XLI. ON THE ELECTRICAL CONDITION OF THE EGG OF THE COMMON FOWL.

Reflecting on the parts of which the egg is composed, so much resembling a galvanic arrangement, and on some well-known facts connected with the changes which occur within it essential to its life and development, it appeared to me as probable that marks of electrical action might be obtained by experimenting on it.

The first trial that I made was with a feeble galvanometer, one which I had used in researches on the torpedo, and with which I had obtained positive results: but with the egg it was otherwise; no appearance of electrical action was perceptible; the needle was not in the slightest degree affected. Other trials made with a much more delicate instrument have been not without success. These I shall briefly describe. The galvanometer employed was one belonging to Mr. C. Becker, of London, of the firm of Messrs. Elliot and Brothers; and to this gentleman I have been indebted also for assistance in preparing the apparatus and in making the experiments. The galvanometer contained 200 yards of wire, 38th gauge. In the first trial, the terminal wires were of platinum, insulated, excepting near their points for contact, with a coating of sealing-wax. The egg, believed to be newly laid, had two small holes drilled in its shell at its smaller extremity for the admission of the wires, and it was placed on a stand of glass.

First, contact was made by applying one wire to the outer shell, the other to the inner membrane, that next the shell, but without effect on the needle. Secondly, to the shell internally, moistened with water, and to the white, also without effect. Thirdly, to the white and yolk, now with some effect, especially when the wire in contact with the latter was plunged deeply into it. By changing the wires, the deflection of the needle was reversed. The trials were repeated on two other eggs, and even with results somewhat more strongly marked. In these, copper wires were substituted for those of platinum, and each wire had attached to it a platinum foil of about one quarter of an inch square, and both were insulated as before, the foil merely taking the place of the points. Now, on making the contact, plunging one wire fairly into the white, the other into the yolk, and stirring the latter gently about to secure the removal of the adhering white, such as might be attached to the foil in its passage, the deflection of the needle was to the extent of five degrees, and it was increased as to quickness of motion, and slightly also as to space, about one degree, by repeated contacts; and, as before, when the wires were changed, the course of the needle also was changed. With both eggs, the result differed but little. The results obtained, too, were very much the same, when the white and yolk, removed from the shell and received into a porcelain cup, were experimented upon without being mixed. On the contrary, when the two were mixed, broken up together, no effect was perceptible on contact, except a restlessness of the needle. And the same restlessness was perceived when the contact was made with the white alone, which perhaps is no more than might be expected, keeping in mind how difficult it is to render either the white alone, and more especially the white and yolk together, perfectly homogeneous.

After witnessing these results with the galvanometer, it appeard not improbable that indications might be obtained of a current of electricity in the egg capable of producing chemical effects; and on trial it has turned out so. The apparatus used consisted of platinum foil, rather more than a quarter of an inch square, attached to platinum wires, to which were fastened fine silver wires as terminal points. The same attention was paid to insulation as in the preceding experiment. The chemical mixture used consisted of water containing a little gelatinous starch and a small quantity of iodide of potassium; a mixture which I had previously found yielded marks of the liberation of iodine when acted on by a wire of zinc and another of platinum, the other extremities of which were immersed in a solution of common salt of the specific gravity 10,425. With these means, in a few minutes after the connection had been made with the white and yolk of an egg, signs of chemical action appeared; a distinct purple tint was perceptible surrounding one of the silver terminal wires, whilst the fluid round the other wire remained colorless. Further, when the wires were taken out of the mixture, the former shewed a yellowish discoloration, whilst the other remained bright. Also, when platinum wires alone were used, the mixture having been rendered more sensitive of change by the addition of a few drops of muriatic acid, distinct results were obtained; one of the terminal wires was strongly discolored by iodine liberated, whilst the other remained free from discoloration. In the instance of the newly laid egg, the wire which displayed the effect corresponded to that attached to the copper when a single voltaic combination was used for testing the delicacy of the mixture. The contrary was seen in the instance of eggs that had been kept some time; those operated on had been laid about three weeks, reckoning from the 23rd of April, and had been kept in a cool place. As to the reversal of effect just described, owing to the age of the egg, I could have wished to have seen whether it would have been indicated also by the galvanometer. I can hardly doubt about the result; but at home, being without the instrument requisite—a galvanometer sufficiently delicate—I have not been able to determine it experimentally.

On the results described I shall not at present speculate, merely remarking that the agency which they indicate can hardly be inoperative in the economy of the egg in the changes so varied and wonderful to which it is subject during incubation and the growth of the chick. And the same electro-chemical action, it may be inferred, cannot but perform an important part in the ovum generally, at least in all instances in which, like the egg of the common fowl, it is composed of a white and yolk, or of substances in juxta-position of heterogeneous natures. Even in the seeds of plants, it may be conjectured, where there is any analogy of composition, it may exercise an influence.

XLII. SOME MISCELLANEOUS OBSERVATIONS ON THE EGG OF THE COMMON FOWL.

The very great importance of the egg in its physiological relations induces me to offer some results which I have obtained in experimenting upon it.

1. *Of the Specific Gravity of the Albumen and the Yolk.*

Harvey, in his admirable work on Generation, says, "In the hen's egg I have observed that there are not only differences in the albumen, but two albumens, each surrounded with its proper membrane. One of these is thinner, more liquid; the other is thicker, more viscid, and rather whiter in its color."*

Such a difference is now generally admitted. In confirmation of it I have found the specific gravity of the one somewhat less than that of the other. The thinner portion from a newly laid egg evaporated thoroughly to dryness yielded 11·2 per cent. solid matter; the thicker and more tenacious 13·5 per cent. I have not given the difference of their specific gravities, which was tolerably in accordance, having, owing to the difficulty of weighing the albumen perfectly free from air, and in small quantities, less reliance on its accuracy; and this, though there is nearly as much difficulty in thoroughly desiccating the yolk as is experienced in burning it; the oil in the one instance acting as the impediment, phosphorus in the other, when converted into phosphoric acid. This it is necessary to keep in mind to avoid conflicting results when experimenting upon the yolk and albumen.

As is well known, the specific gravity of the yolk is less than that of the albumen; but I am not acquainted with the results of any trials made to determine the precise difference. Of a newly laid egg the specific gravity of the yolk was found to be 10,346, that of the mixed albumen 10,376. Trials made with

* Harvey's Works (Dr. Willis's excellent translation for the Sydenham Society), p. 212.

other eggs have shewn slight differences, but uniformly in favor of the white exceeding the yolk. This comparatively low specific gravity of the yolk is remarkable and strongly indicative of the large quantity of oil it contains, the ponderable matter which it yields when thoroughly dried being proportionally so much greater than that yielded by the white; thus from the former thoroughly dried over the steam bath, the residue obtained was 55 per cent.; but from the latter no more than 10·19 per cent.

2. *On the Fibrinous Portion of the Albumen.*

When the white of a fresh egg from which its chalazæ have been detached is placed on a filter, none of its albumen passes through, even though the filter is saturated with water. A few drops indeed may be obtained, which, if heated, become milky in a slight degree, leading to the inference that the drops are principally water from the moistened filter, holding in solution a little albumen. When, however, the white is poured into water, about an equal quantity, then at first the two remain apart, the water above the white. Now, if stirred so as to effect a mixture, the fluid is rendered turbid by the separation of a whitish matter in flakes and fibres, which rapidly subside, leaving the fluid nearly transparent. If not stirred, a like phenomenon is witnessed in the course of a few hours, from diosmetic action. Afterwards, the water holding the albumen in solution readily passes through the filter. The white matter which thus separates—it is considerable in quantity as to bulk—has many of the properties of coagulable lymph. Like that substance, it is viscid, being readily drawn out into fine threads; like it, it contracts slowly, becoming from semitransparent of an opaque white; and, like it, when seen under the microscope with a one-eighth inch power, it appears to be composed of fibres of various degrees of fineness. This fibrinous matter is for the most part soluble in liquor potassæ and in a solution of nitre, but in a slight degree only in acetic acid, with an increase of its transparency. In the liquor potassæ it acquires color, and when gently heated, gradually from brown it becomes almost black, from its action on the iron contained in the solution. I am disposed to think that it differs chiefly from the coagulable lymph of the blood in being richer in sulphur.

3. *On the Chalazæ.*

The substance of the chalazæ of the egg in appearance and most of its properties resembles very much the filamentous portion of the white. Its character under the microscope is very similar: it contracts in the same manner, but it is more soluble in acetic acid and in a solution of nitre, and equally, if not more soluble in liquor potassæ; and, what is noteworthy, with little discoloring effect, as if it contained less sulphur, and even less than albumen, for when the albuminous filtered solution is acted on by liquor potassæ, it becomes strongly discolored—slowly, heat not being applied; rapidly, on its application.

4. *On the Air in the Egg.*

It is stated by Harvey that air is always to be found in the egg, even before exclusion. He says, "I have discovered it even in those that are still contained in the uterus, as soon as they had become invested with the shell."* Such observations as I have made have not confirmed this, but the contrary. In a former work I have mentioned that in the newly laid egg I could find no air.† Recently I had an egg in less than a minute after being laid, whilst still warm immersed in water; opened under water, not a particle of air could be obtained from it. It is easy to account for the conclusion arrived at by Harvey on the supposition that he allowed the egg before he examined it to cool from exposure to the atmosphere. In cooling, undoubtedly owing to the unequal contraction of the shell and of its contents, and the porous nature of the former, a little air must enter. An instance or two may be mentioned, though hardly necessary, in proof.

An egg brought to me the instant it was laid was left exposed to the air at 58° for an hour and a half to cool. Then carefully opened under water a bubble of air was collected, about $\frac{1}{100}$ cubic inch. Another newly laid egg was left to cool in the open air at 42° for about an hour; opened under water, it yielded a little more air than the preceding, about $\frac{1\cdot5}{100}$ cubic inch.

Besides the contraction owing to the cooling of the egg, there are other circumstances doubtless which have an influence on

* Opus cit., p. 214.

† Research. Physiol. and Anat., ii., 222.

the quantity of the air and its quality independent of organic development such as the loss of weight of the egg from day to day from the evaporation of its aqueous part, and the action of the contents of the egg on the air within its shell in its air vesicle. From the trials made by Dr. Prout it would appear that an egg of the common fowl, of the weight of 907,5 grs., loses ·75 gr. during the twenty-four hours, and this pretty equally, and with slight variation only in summer and winter, in the former a little more, in the latter a little less.* The results which I have obtained, these of trials more limited as to time, differ a little from his, the average daily loss having been less; thus one egg, which on the 26th of December weighed 883·7 grs., kept in a room, the temperature of which varied from about 55° by day to 50° at night, on the 22nd of February was reduced to 848·8 grs., a loss of 34·9 grs., or ·6 gr. per day. Another egg, which on the 10th of January weighed 917·3 grs., was on the 22nd of February reduced to 897·7 grs., a loss of 19·6 grs., or ·45 gr. per day; the first mentioned egg during the same time losing 23·5 grs., or ·54 gr. per day. As I believe Dr. Prout's experiments on the egg were all made in London, he residing in a crowded part of it, Sackville Street, it may be inferred that his result, shewing a greater daily loss, was owing to exposure to a drier and warmer air. Moreover, if the shell of one egg be thicker than that of another, a difference of loss can hardly fail of occurring: this may account for the larger of the two eggs which I weighed losing less per diem than the smaller, its shell being thicker.

A daily loss, such as the preceding, supposing it to be chiefly from water evaporated, can diminish the volume of the included albumen and yolk very little, so as to need the entrance of air to supply its place.† This is confirmed by comparing the propor-

* Phil. Trans. for 1822, p. 379.

† The above remark applies *à fortiori* to the egg during incubation, its temperature then being raised, and evaporation from it being greatly checked owing to the sitting fowl. The comparatively large quantity of air that is found in the egg at its full time, may be attributed to that vital action during development to which oxygen is essential—analogous to a feeble respiration. In further illustration, I will give an instance. Of thirteen eggs put under a hen, seven were productive; six aborted. These were examined when still warm from the nest. The quantity of air they contained was small, so that they all sank in water; in no instance was it appreciably different from common air. Yet the yolk had become dilute, the albumen contracted; and, the chalazæ having apparently become ruptured, there was a change of place, the yolk being outside the albumen. The yolk of some of them mixed with hydrate

tion of air yielded by the egg at different ages. The following are some of the results which I have obtained. The first column gives the age of the eggs examined in days, the second the quantity of air, in parts of the cubic inch, obtained:—

1	$\frac{1}{100}$
6	$\frac{2\cdot5}{100}$
19	$\frac{7}{100}$
43	$\frac{8\cdot5}{100}$
58	$\frac{13}{100}$

Provided putrefaction have not begun, I have found the air in the egg, whatever the age, to differ but little from atmospheric air; thus the air obtained from three eggs which had been kept ten days in January consisted of 80 azote and 20 oxygen, without any carbonic acid; and nearly the same was the composition of the air from one which had been kept fifty-eight days. That the air should be free from carbonic acid is no more than might be expected, reflecting on the porous quality of the shell, and the power which the albumen has to absorb this gas, as will be shewn in the following section.

These results accord with those of other inquirers, who have found the air of the air vesicle to differ but little from common air,* and opposed to the statement that in the contained air there is an excess of oxygen. Other results which I have obtained seem to be equally opposed to this inference. I shall make mention of two. A newly laid egg was placed in a receiver confined over mercury, with a limited quantity of atmospheric air, 7 cubic inches. The experiment was commenced on the 22nd of February, and finished on the 7th of March. There was a little moisture deposited on the inside of the receiver, and a very slight diminution of the air it contained. This air, examined, was found to consist of 91·8 azote, 2·7 carbonic acid, 5·5 oxygen, thus shewing a loss of oxygen. The egg, before fresh exposure, was opened under water; a small bubble of air was all that was obtained from it, it did not exceed ·05 of a cubic inch; neither

of lime gave a slight smell of ammonia; from that of others only a trace of the volatile alkali could be obtained by the test of fume with muriatic acid. None of them had the slightest putrid odour. In none of them were there any indications of incipient development.

* See Wagner's Elements of Physiology, translated by Dr. Willis, p. 131.

oxygen nor carbonic acid, using the tests of lime-water and phosphorus, could be detected in it.

The other instance was that of an egg taken from under the hen on the twentieth day of incubation, when nearly ready for hatching, and was opened under water: the air obtained from the air vesicle, not including a small bubble lost, was equal to $\frac{19}{100}$ of a cubic inch. It was found to be composed of 81·5 azote, 2·6 carbonic acid, and 15·9 oxygen. The egg was brought to me warm, wrapped in flannel, and the chick, even after opening of the egg in water, was found still alive, and barely alive, its heart acting feebly: another, and the only other indication of life it gave, was a gasping movement once or twice of its mandible. Now, were there normally an excess of oxygen in the air vesicle, might it not have been expected to have been found present in one or other of these instances?

5. *On the Reaction of the Albumen and Yolk, and their Power of Absorbing Carbonic Acid Gas.*

As is well known, the white of the egg has distinct alkaline reaction. The yolk, on the contrary, I find has an acid reaction,* rendering litmus paper red, a quality which it probably owes to the phosphoric acid which it contains.†

As might be expected, therefore, the power which each of these has of absorbing carbonic acid gas is different. The white of the newly laid egg I find, when kept over mercury with a measured quantity of this gas, absorbs about 1·83 its volume, becoming at the same time from transparent slightly milky, the change beginning at the surface and extending downwards, as the gas penetrates in that direction. The yolk of a newly laid egg similarly treated absorbed only about ·17 its volume.

It is worthy of remark that the acid and alkali in each are so nearly balanced, that when the white and yolk are mixed, the

* In making the trial, the white should be removed from the yolk; for if the test-paper is passed through the white into the yolk, it being coated with the white, the action will be alkaline.

† During fœtal development it probably loses its acid quality, owing, it may be conjectured, either to admixture with the white or to its dissolving, or the blood dissolving a portion of the calcareous shell—an occurrence the more likely, as in the advanced stage of fœtal growth the allantois is in contact with the lining membrane of the shell. In a chick ready to quit the egg, the abdominal aperture closed, I have found the included yolk alkaline.

mixture has hardly an appreciable effect on test paper ; the very slight effect is indicative of a little excess of alkali.

In an egg that had been kept seven weeks in confined air, the alkalinity of the albumen was diminished, which, it may be inferred, was owing to the carbonic acid generated and absorbed. Now the white absorbed no more than ·25 its volume, instead of nearly twice its own volume as before. The particulars of the experiment were the following. On the 30th of December a bantam's egg was placed over mercury in about 7 cubic inches of common air. On the 21st of February the air was examined ; it was found to consist of 88·5 azote, 11·5 carbonic acid. The albumen appeared to be less viscid, more liquid than common. Mixed with hydrate of lime, it afforded a trace of ammonia, by the muriatic acid test. The yolk, too, seemed a little less viscid ; but of this I was less certain : with lime it indicated stronger traces of ammonia.

6. *On the Action of Atmospheric Air on the Albumen and Yolk apart.*

We have seen what very little change the egg undergoes from exposure to the atmosphere. For the sake of comparison, I thought it worth while to make a like trial on the albumen and yolk apart, confined in a limited portion of common air. The following are a few of the results obtained :—

1. On the 24th of March, 1·5 cubic inch of mixed albumen was introduced into common air over mercury. After a few days the albumen became slightly turbid. On the 3rd of April, when there was no well marked change of the volume of air, it was found on examination to consist of 77·6 azote, 22·4 carbonic acid gas. The albumen had now a slight unpleasant smell, and, mixed with hydrate of lime, afforded a distinct indication of ammonia.

2. On the 3rd of April, 4·5 parts of liquid albumen, the thinner portion, taken from a newly-laid egg, was put over mercury in 8·5 of atmospheric air ; the albumen was not quite transparent. On the 11th of the same month there was no appreciable change of volume of the air. Examined now, it was found to consist of 18·2 oxygen and of 81·8 azote.

3. On the 11th of April, 2·75 parts of clear albumen, the merely liquid portion, from an egg that had been kept in lime-

water since the preceding January,* was placed over mercury carefully cleaned in 8·5 of common air. It remained up to the 19th of the same month perfectly transparent and without change of volume of the air. On examination, the air was now found to consist, using lime-water and phosphorus, of 20 oxygen, 80 azote.

4. On the same day, viz. the 11th of April, a portion of yolk, about one cubic inch, from a newly-laid egg, as free as I could obtain it from membrane, was put over mercury with common air, also about one cubic inch. At first there was a slight absorption of air, a small diminution of its volume; the yolk at the same time becoming at its surface of a paler hue, followed shortly by a slight increase of volume, owing evidently to the disengagement of a little gas. Examined on the 19th, the air was found to consist of 21·8 carbonic acid gas and of 78·2 azote; it had the offensive smell of sulphuretted hydrogen.

These results are given as proximate, the mode of analysis not admitting of perfect accuracy, yet sufficiently so for the purpose for which the experiments were made.

Comparing them one with the other, and those on the albumen with that on the yolk, they seem to shew in a striking manner the little tendency the pure albumen (its purity denoted by its perfect transparency) has to absorb oxygen and to undergo change; how that tendency is altered with impairment of purity, and how very prone the yolk is when taken from its inclosing membrane to absorb oxygen and to putrefy. I shall have to revert to this further on, when I shall have to state some additional facts in proof. The change that took place in this instance in the yolk was not what I had expected, it was so rapid, and was so strongly marked. It reminded me of an analogous example already adverted to, that of the fermentation of the juice of the grape, when expressed, or of the rapid decay of most kinds of fruit when bruised, their cellular texture broken up and their parts intermingled—one acting on another as a ferment.

7. *On the Changes which take place in the Egg from Keeping.*

There are two changes which the egg is liable to undergo from keeping, under ordinary circumstances of exposure to the

* The albumen was without smell; with hydrate of lime a slight trace of ammonia was indicated by the muriatic acid test. The egg, dressed, was not distinguishable by its flavour from a newly laid one.

atmosphere—one, the well-known one of putrefaction; the other, of desiccation.

In the latter, the change seems to depend chiefly on the loss of the greater part of the aqueous portion from evaporation, accompanied with but little alteration of the albumen and yolk: that alteration, it may be inferred from the preceding results, connected with the absorption of some oxygen and the formation and retention of some carbonic acid. In addition to the examples already given, I shall mention another.

A newly-laid egg, weighing 915 grs., in April, 1861, was put by where light was almost excluded, air pretty freely circulating, the temperature varying from about 50° to 60° throughout the year. In November, 1862, it was found reduced to 399 grs. Now opened, it had no unpleasant smell; the white was a thick semifluid and very viscid, as was also the yolk. A month later, the shell having been broken, the desiccation had made more rapid progress, and both the white and yolk had become firm. The albumen detached had very much the appearance of amber, being transparent and lustrous, of a bright wine-yellow, and brittle, breaking with a conchoidal fracture. The yolk more resembled unbleached bee's-wax, was nearly of the same color and lustre and degree of translucency and hardness. Moistened and mixed with hydrate of lime, it gave off a slight ammoniacal odour; triturated with water, it formed an emulsion which putrefied more rapidly than an emulsion of fresh yolk. Under the microscope it appeared to differ from fresh yolk in exhibiting, besides oil globules and fine granules, cells, as if formed from a coalescence of these granules and a few minute prisms, not unlike those of phosphate of lime. The albumen appeared to differ very little from fresh desiccated white, obtained at a temperature little exceeding that of the atmosphere.* Like it, it pretty rapidly absorbed water on immersion, became viscid, and was gradually dissolved; and like it also, its solution required a higher temperature for its coagulation; not like fresh albumen, undergoing the change at 160° to 170°, but like the solution of the desiccated, needing about 180° to 190° to produce the full effect. Mixed with hydrate of lime, it did not afford a trace of

* If, after slow evaporation at a moderate temperature, it be exposed to a temperature near that of boiling water, it is rendered insoluble, and does not become viscid on the addition of water.

ammonia. With liquor potassæ gently heated, it gelatinized, forming a transparent stiff jelly, which presently became brown, and soon black.

The other change, that of putrefaction, is clearly connected with the breaking up of the structure of the egg, and a new arrangement of its elements, as dead matter, presenting a most striking contrast to that arrangement during incubation under the control of vital force, which is productive of the chick. The circumstances most conducive to the putrefactive change seem to be the mixing of the white and yolk with access of atmospheric air; or the agitation and breaking up of the yolk itself, if I may so speak, when probably more or less of such admixture takes place; or simply, as we have seen, the intermixture of the yolk particles after the rupture of the yolk membrane.*

The breeders of poultry are well aware of the necessity of keeping eggs for incubation still; † and I am told that those who have choice breeds, and are desirous of not making them common, puncture their eggs, thereby probably admitting a little of the lighter yolk into the white, before sending them to market. Harvey made the noteworthy observation that the spot where the putrescence of the egg begins in unproductive eggs during incubation is "the very spot where the reproductive germ appears." Accompanying the change we know that there is an absorption of oxygen, and eventually a disengagement of carbonic acid gas and sulphuretted hydrogen, and the production of ammonia with a decomposition of the albumen and yolk. From the experiments I have made, I select the two following in the way of illustration and for contrast.

On the 25th of April a portion of the white and yolk of an egg were mixed and poured into a bottle with atmospheric air. After closure with a cork, the bottle was inverted in water. After forty-eight days, viz., on the 11th of June, the cork was withdrawn; much gas escaped of the most offensive kind, such as is

* Occasionally a speck of blood is seen in a newly-laid egg boiled for the table: I have noticed it only in the yolk, and near its surface, suggestive of its origin from the rupture of a small vessel on its leaving the ovary. In one instance that I examined it, it had the character of a clot, and under the microscope appeared to consist of fibrin including minute corpuscles (blood corpuscles altered?); they were about the size of the nuclei, and had a dark circular outline. The presence of a clot of this kind, is it not likely, may conduce to putridity?

† They commonly place them with their big end, that containing the air vesicle, uppermost.

yielded by "rotten eggs," consisting of azote, carbonic acid, and some sulphuretted hydrogen. The mixture was in a state of putrid decomposition.

On the 19th of December the yolk, in its membrane entire, and albumen of a newly laid egg carefully separated, were put each into a wine glass apart, covered with silk paper, fastened by a ligature, and placed on the chimney piece, where the temperature varied from about 60° to 55°. On the 21st of March following, the albumen had become quite hard, retaining its transparency, with irregular markings, most of them concentrically circular, owing, it may be inferred, to fine fractures from contraction in the drying.* There was no mildew about it, but many white achari, which at first were mistaken for crystals. They were all at the surface, some at the top, some between the dried mass and the side of the glass; all were encased in a film of albumen, and were dead. On the addition of water to the dried mass, it first became viscid, and when largely diluted, the greater part of it became dissolved. In its viscid state it was arrested by a filter. In this state, when heated, it coagulated, shewing a great tendency to form membrane.† When dissolved so as to admit of filtration, it retained its viscidity only in a very slight degree. Tested before dilution it had an alkaline reaction, and yet afforded no trace of ammonia when acted on by hydrate of lime. Mixed with liquor potassæ and heated, it rapidly became discolored, and shortly almost black. The insoluble portion, that which resisted solution, on dilution was first very viscid; gradually it became of an opaque whiteness, and lost its viscidity.

The yolk also had acquired considerable firmness; it was of the consistence of good cheese, which it resembled also in smell; its color was little altered. In one or two spots a mould had formed, the same as that of cheese (*Mucor mucedo*). With water as an emulsion, its appearance under the microscope was also

* If a portion of albumen be evaporated in a watch glass at a temperature of about 100°, it exhibits when dry, from the manner in which it contracts, an appearance very like that of the spider's web, or of the *Arachnidiscus ehrenbergii*; on exposure to the air, of ordinary temperature and humidity, the cracks close, owing to expansion from absorption of moisture; dried again, a new arrangement of fissure-lines takes place; these, too, symmetrical.

† The white of the fresh egg is like it in this respect: if left to evaporate exposed to the air at a temperature much below the point of coagulation, a film forms on its surface, which, like fibrin, is insoluble in water.

very like that of good cheese similarly treated, seeming to consist chiefly of fine granular matter and of oil globules, the granules contiguous and membrane-like. When moistened, triturated with hydrate of lime, it gave off a distinct smell of ammonia; thus, too, resembling cheese which had been kept some time, and yet it had no distinct alkaline reaction. This, its resemblance to cheese, accords with Professor Lehmann's view of the composition of the yolk, viz., that one of its chief ingredients is casein.*

8. *On the Shell of the Egg.*

The shell of the egg chemically considered is composed chiefly of carbonate of lime and animal matter. This, its well known composition, I barely mention; I propose to confine my remarks to the nature of its structure and its investing membranes.

If it be examined with a magnifying glass, there is seen over almost every part of its outer surface minute depressions, some of them, and those mostly at the big end, like foramina, suggestive of spiracula. This suggestion receives support, if the egg, one a few days old, be placed in water deprived of air under a receiver, on the plate of the air pump. On working the pump air will be found to gush out from them, and most remarkably from the larger, corresponding to the air vesicle, and to continue to rise for some minutes in an unbroken stream. When no more air can thus be extracted, if the shell be opened under water the air vesicle will be found collapsed and destitute of air.

By careful manipulation under water, taking for the purpose half the shell of a newly laid egg, that half containing the vesicle, the membrane including the vesicle may be removed entire, retaining the form of the egg.

The inner membrane of the vesicle, that which is next to the albumen, is exceedingly delicate, very much thinner than the outer. Seen under the microscope with a one-eighth inch power, it appears to be formed of very fine fibres, in fact, to be a tissue of fibres, on a hyaloid ground. This layer, if I may so speak of the inner membrane, is easily separable from the outer for a short distance from the vesicle, and only for a short distance, beyond, the two being firmly connected.

* Physiolog. Chem., vol. ii., p. 357.

The outer membrane similarly examined seems to be composed of two layers, the inner very like the preceding, excepting that its fibres are larger; it is best seen at the cut margin, as if the coarser layer, the outermost, had contracted most, leaving the inner exposed to view.

The outer membrane, too, seems to be similarly formed, consisting chiefly of fibres with the addition of minute cells; but, owing to its thickness, its structure is not well displayed. It is commonly of a white color; in the instance of one egg, it was of a delicate pink hue, best seen when moist: after drying this hue was hardly visible.

Now, if the shell be immersed in dilute muriatic acid, and left until a portion of it be dissolved, another membrane will be found detached, and this from the part of the shell over the air vesicle as well as from the shell generally. The membrane thus separated, as seen under the microscope, is found also to be a tissue of fibres with cells apparently interposed—the fibres less delicate than those of the inner membrane, but more so than those of the outer.

When the whole of the calcareous matter is dissolved, what remains is a fine cellular tissue, covered externally with an extremely delicate hyaloid membrane. So delicate is this tissue that it rises as a froth with the air disengaged; and so tender is the membrane when detached, that it is difficult to touch it without breaking it. The tissue appears to be of the same hyaloid character as the membrane; no fibres or cells could be seen in it.

If the shell when reduced very thin by the action of the acid be examined, then what I have conjectured to be spiracula are distinctly to be seen, permitting the light to pass through them —that is, through a transparent membrane, which may be held to be the medium of communication with the atmosphere.

The membranes of the shell of the egg of the common fowl are commonly described as two—those which by their separation constitute the air vesicle. May they not be more correctly stated to be three, if not four—viz., the very delicate external one; the delicate internal one separated by the action of the acid; the still more delicate inner one of the vesicle, and the coarser one formed of two layers situated underneath the first-mentioned inner membrane.

It is noteworthy that the outer coat of the air vesicle, though I have described it as coarse, is finer than the same membrane over other parts of the shell.

I need hardly remark that the strength of the shell of the egg depends on the combination of calcareous matter and of animal matter—the one imparting adequate hardness, the other sufficient toughness. It is remarkable how fragile the shell becomes even on the removal of the two inner membranes. The quantity of animal matter in the shell, including its membranes, is small in comparison with its calcareous portion. Of a newly-laid egg, weighing 764·5 grs., I found the several parts to be as follows:

	GRS.
Yolk	192·8
Albumen	493·2
Membrane mechanically detached	10·0
Shell	68·5

Of another egg, weighing 891·5 grs., the shell with its membranes, after the adhering albumen had been removed by washing, was found equal to 85 grs. The entire membrane, that removed by the hand, in its damp state weighed 6·6 grs.; dried in the open air, it was reduced to 3·1 grs.; and further dried over a steam-bath to 2·15 grs. The shell, dried in the open air, weighed 74·6 grs.; thoroughly dried, it was reduced to 73·9 grs. The membranes and cellular tissue remaining after the removal of the calcareous matter by an acid,* weighed, when thoroughly dried, no more than 1·3 gr.

I give these results, though indeed they are of little importance, inasmuch as Dr. Prout has shewn that no constant data are attainable from the shells, no two being the same; and the remark I believe applies equally to the eggs themselves considered as wholes, comparing one with another, and likewise to their several products, although their hatching takes place in nearly the same time—shewing, however different the eggs may be, how equalizing are the vital energies brought into action by incubation.†

* The solution, evaporated to dryness, and heated to dull redness, became of a dark greyish hue, presenting only a slight appearance of charring; thus tending to prove how very little soluble are the membranes and cellular tissue of the shell.

† An interval of twenty-four hours between the first and last egg hatched of a batch of thirteen is seldom exceeded: the greatest I have heard of has been thirty-six hours; and yet of the thirteen there may be a difference as to the time of their being

9. *On the Relations of the Ingredients of the Egg to the Bony Skeleton of the Chick.*

Whence the bones of the chick in ovo obtain their phosphate of lime was considered an unsolved problem by Dr. Prout, inasmuch as he found less lime in the albumen and yolk of the egg before fœtal development began than in the bones at its completion. He was of opinion that the proportion required to make up the deficiency was not derived from the shell, and that it might be formed *de novo* "by transmutation from other matter," he believing that such a power "is to be ranked amongst the capabilities of the vital energies."*

In his reasoning on the subject there are two circumstances which he lays most stress on as tending in his opinion to shew that it cannot be derived from the shell. The first, to use his own words, is, that "the membrane putaminis never becomes vascular, and seems analogous to the epidermis; hence the lime of the shell which is exterior to this membrane is generally considered by physiologists as extra-vascular; it is, therefore," he adds, "extremely difficult to conceive how the earth in question can be introduced into the economy of the chick from this source, particularly during the last week of incubation, when a very large portion of the membranes are actually separated from the shell. Secondly," adverting to the other circumstance, "both the albumen and yolk contain at the end of incubation a considerable proportion of earthy matter (the yolk apparently more than it did originally); why," he asks, "is this not apropriated in preference to that existing in the shell?"

This reasoning seems very specious, and were the statements on which it is founded altogether correct, it would be difficult to avoid the conclusion arrived at.

First, as to the membranes. Such observations as I have made do not accord with his account of their condition. In a chick, as before described in page 342, taken from its egg at its full time, I found the vascular membrane, the allantois, in such close connection with the membrane of the shell, that it was

laid, or of their age, of nineteen days, as the hen when in full vigor commonly lays only every second day, an interval of one day almost invariably occurring. And eggs kept with care in a cool place, with their big end upwards, have been hatched after having been kept a month.

* Opus cit., p. 400.

difficult to distinguish the one from the other—so much so, indeed, that at first inspection it might have been pronounced to be the lining membrane of the shell. On careful examination, however, it appeared that the proper membrane of the shell underlaid it; and, what is noteworthy, the former when examined with the microscope seemed to have lost some of its substance: it appeared thinner and worn: moreover, and this too is noteworthy, it was found very slightly adhering to the shell, as if it also had sustained a loss of its substance; and such was its appearance whilst still moist.

As to the second part of the argument, is not the excess of earthy matter which Dr. Prout found in the residual albumen and yolk at the end of incubation rather in favor of the opposite conclusion—viz., that the shell so abounding in lime is the true source of it? In support of this I may refer to the fact already mentioned, of having found in the membrane next to the allantois, viz. the amnion (?), that which as well as the allantois is left by the chick on quitting the egg, a certain quantity of excrementitious matter consisting chiefly of lithate of ammonia, phosphate of lime, and common salt,* which, it may be inferred, was separated during the growth of the chick from the blood in the fœtal circulation by the kidneys or Wolffian bodies; and this, like the fact noticed by Dr. Prout, is indicative at least of no want but rather of excess of lime.

Now finding thus rather an excess of lime brought into use, and keeping in mind that the quantity of phosphoric acid, or phosphorus convertible into phosphoric acid, which is contained in the yolk and albumen, chiefly in the first, is, according to Dr.

* This membrane, with its contents dried, weighed 5·5 grs.; the proportion of lithate of ammonia was considerable, of fæcal matter minute; incinerated there remained 3·2 grs., of which all but ·1 gr. was dissolved by water: this ·1 gr. of matter insoluble in water was found to consist of phosphate of lime with a very little carbonate of lime and magnesia. The saline portion dissolved by water had an alkaline reaction; I could detect in it no potash; it consisted chiefly of common salt.

The examination of the egg of the goose, at the full period of incubation, when the young bird had commenced the operation of breaking the shell, has afforded similar results, with indeed this difference, that there appeared to be at a few points a slight connection by cellular tissue between the very vascular membrane which I have called allantois and that within it, which I infer to be the amnion, or its analogue; and also another difference, viz., that the latter was not entirely destitute of blood-vessels, for two were distinctly seen ramifying in it. The quantity of lithate of ammonia contained in this sac was large, the quantity of greenish fæcal matter small.

I may here mention that in the examination of a chick which had died when nearly ready for hatching, I have found lithate of ammonia in its semifluid opaque white state in the ureter of one of the kidneys.

Prout's analysis, thrice as much as is required for union with the accompanying alkaline and earthy bases, am I saying too much in stating that the problem seems solved, if it can be shewn, as I have done, that there is a vascular system, the allantois, sufficiently near the shell to receive from it lime? Further, may not the carbonic acid which is formed during incubation, and with which the fœtal blood must be impregnated, aid also in the solution and transfer of the lime? And, moreover, is it not probable that the electrical condition of the egg, such as that described in a preceding section, may likewise be concerned in the operation?

I need not comment on Dr. Prout's opinion concerning the commutation of matter—that a doctrine incompatible with the severe logic of modern chemistry, requiring that all substances be considered simple until they are actually decompounded; indeed, as Dr. Prout propounded his conjectural inference, it was evidently with hesitation, shewing that he placed little confidence in it.

I venture to offer a few remarks in conclusion. Reflecting on the results described in this partial examination of the egg, one cannot but be struck by the beautiful harmony and adaptation of its several parts to each other, viewed in relation to the function each performs in the economy of the whole.

First, we have the outer protecting shell, formed chiefly of carbonate of lime, sufficiently firm to afford adequate resisting support during incubation, and yet so constructed as not to exclude the ingress of atmospheric air.

Secondly, we have the air vesicle formed by the separation of membranes, and this where the shell by its spiracula, if I may use the expression, renders the entrance of air most easy, and also its exit, the two constituting, as it were, an incipient respiration.

Thirdly, follows the albumen, well adapted, owing to the manner in which it resists putrefaction and change, and the little affinity it has for oxygen, to protect the yolk, performing for it the same moderating influence, which I have suggested that the serum of the blood performs for the red corpuscles; and, moreover, by its alkaline quality guarding the yolk from excess of carbonic acid.

Even in its dilution, in the comparatively small quantity of solid matter it yields, do we not see its fitness for becoming the first material that comes into use, as pointed out by Harvey, in the growth of the fœtus? Further, as it is consumed, and as the fœtus, it may be inferred, stands in greater need of oxygen, it secures by its diminution a nearer approach of the blood circulating in the membranes to the action of the included air.

Next, as to the chalazæ, how manifest is their design, so well fitted to counteract the difference of the specific gravity of the albumen and yolk, and to preserve the latter, so long as is necessary, enclosed in the former.

Lastly, of the yolk, this the most important part of all, as containing the vital speck, as richest in nourishing material and affording nutriment, not only to the fœtus in its advanced stage, but even after the chick has left the egg, thus serving to the young bird like milk to the young mammal.

The late Dr. Prout, in his excellent paper on the egg, already referred to, makes the interesting remark that, towards the end of incubation the young chick, by its greater weight, assumes such a position that its beak is uppermost, and, as he says, "consequently fully exposed to the air when it first makes its way through the shell," a position, be it remembered, the more admirable, as it is by its beak it breaks the shell, effecting it, as we have seen, by a special means, the provisional point or armature, which, when no longer needed, is speedily parted with.

Amongst the happy adaptations belonging to the egg of the fowl, there is another which may be mentioned, considering it as an important article of food, and not only of man, but including other eggs of all kinds, of a large number of carnivorous animals; this is its little tendency to putrefaction. By simple precautions, as is well known, eggs may be kept for a very long time fit for use;* and were the albumen and yolk thoroughly dried, they might be kept, I believe, for an indefinite period. Even in its

* Of all known methods for preserving eggs, immersion in lime water with excess of lime seems to be the best. Left undisturbed, a pellicle of carbonate of lime forms at the surface, in a great measure excluding air. For perfect success the mouth of the vessel had best be closed with a cover tightly fitting. An egg just before immersion, on the 22nd of February, weighed 778 grs.; when taken out on the 24th of March, it weighed 778·8 grs., and on the following morning, after exposure to the air, 778·2 grs. The very small gain thus indicated was probably owing to the entrance of a little water to supply the place of the minute quantity of oxygen that was in the air vesicle and was absorbed after conversion into carbonic acid gas. This egg boiled was like a newly laid one, equally delicate.

putrefying state, a certain adaptation may be witnessed. If during incubation an egg becomes putrid, then, owing to the including shell, little or no emanation takes place from it injurious to the other eggs; but taken from the nest and broken, so offensive are the effluvia arising from it, that it is universally shunned, and is allowed quietly to sink into the soil, or to be washed away by rain, to become, whether concentrated or diffused, a fertilizing manure.

XLIII. ON CENTENINE EGGS.

Harvey, when treating of the differences of eggs, remarks, "Some are larger, others smaller, a few extremely small; those in Italy are called *centenina*. 'The vulgar,' says Fabricius, 'think that the small egg is the last that will be laid, and that it comes as the hundredth in number, whence the name; that it has no yolk, though all the other parts are present—the chalazæ, the albumen, the membranes, and the shell.' Fabricius proceeds: 'The ova centenina are met with of two kinds, one being without a yolk; and this is the true centenine egg, because it is the last which the hen will lay at that particular season—she will now cease laying for a time. The other is also a small egg, but it has a yolk, and will not prove the last which the hen will then lay, but is intermediate those of the usual size that have preceded and others that will follow.'"*

These smaller abnormal eggs, the true centenine of Fabricius, are of rare occurrence in this country. According to an intelligent man who has charge of poultry, fifteen good hens in the course of one year lay (supposing no interruption from sitting) about 3,750 eggs, and of these about two may be of the kind in question. And his experience accords with that of the celebrated doctor of Padua, that they are the last of the season.

As such eggs seem to have escaped observation of late years, I shall give a brief account of the few I have been able to procure.

The first that I examined was laid in April, 1860. It then weighed 199 grs., and measured 1·4 inch, by 1·1. With the exception of its diminutive size, there was nothing peculiar in its form or appearance, or in the quality of its shell. Put by where there was little light, but a moderate circulation of air, and there kept until the following April, it was found reduced in weight to 48·5 grs. Returned to the same place, and left until July, 1862, there was no further reduction of weight.

* Op. cit., p. 223.

When opened, its contents were found contracted, shrunk into a small space, on one side, that side on which the egg had rested. The contained substance was hard and brittle; where thinnest, of a light yellow, and transparent; of a darker yellow, but as bright, and nearly as transparent, where thickest. Detached from the shell, it weighed 25·5 grs.; the shell 23 grs.; so, allowing for the shell, the dry contents were just 14·5 per cent., which, making further allowance for the small loss it might have sustained, if thoroughly dried over a vapor bath, is but little different from the proportion of the solid matter that the ordinary egg yields when deprived of its water. It was examined for a yolk, but none could be detected; the whole seemed to be merely albumen. The darkest portion, reduced to powder, was treated with both alcohol and ether, without affording even a trace of oil or fatty matter, indicative of yolk.

The albumen seemed to differ chiefly from fresh albumen in being insoluble in water. Immersed in water, it slowly became expanded from imbibition, tough, translucent, and elastic, but not gelatinous nor viscid, nor was it rendered opaque by boiling. The little that water extracted was not coagulated by heat; it had an alkaline reaction.

Under the microscope the albumen had a hyaloid appearance, and was traversed by delicate fibres not unlike those of fibrin. It dissolved for most part in liquor potassæ; the solution, unaided by heat, became discolored, and eventually almost black. In acetic acid, it lost its color, but did not appear to be dissolved, nor did it appear to be acted on immediately by a strong solution of nitre.

The second egg which I examined had been laid, it was believed, about three months before. It measured 1·2 inch, by 1·1 inch. Its blunt end perforated under water, ·61 cubic inch of air were obtained, which were found to consist of 20 per cent. of oxygen and 80 azote; tested by lime water, not a trace of carbonic acid gas could be detected. The proportion of oxygen was ascertained by phosphorus.

The shell of this egg was thinner than that of the preceding, and rougher—its roughness owing to granules of carbonate of lime scattered over its surface. It weighed, when detached, after the further desiccation of the egg at a moderate temperature, 14·4 grs.; the weight of its contents was 28·9 grs. Besides membranes, these proved to be chiefly albumen, in which was included a minute globular mass, weighing ·7 gr.; it had the character of yolk, was of the same color and of similar composition; its appearance under the microscope differing chiefly in presenting cells which contained and were intermixed with oil globules, these somewhat larger than the oil globules of the fresh yolk.

In its dry state the albumen had very much the appearance of gum-arabic, both as to lustre and color. It was very friable, readily broke into pieces under the pressure of the fingers. Like ordinary dried albumen, it became viscid when immersed in water, and for most part dissolved, and its solution was coagulated by heat. At 180° it became milky; at 200° it was pretty firmly coagulated and rendered opaque.

The third egg which I have to notice had been laid about six weeks. It was of an elegant form, measured 1·4 inch by 1 inch, and weighed 115 grs. Opened under water, it afforded a good deal of air, some of which was lost in the collecting, so that the exact quantity was not ascertained. 7·5 measures of it agitated with lime water experienced no absorption; exposed to the action of phosphorus, there was a diminution of 1·25 measure: thus denoting about 16·6 per cent. oxygen, 83·4 azote.

Dried over steam, the entire egg was reduced in weight to 57·4 grs., of which 15·4 grs. were due to the shell, 42 grs. to its contents. The appearance of the latter was very like that of the preceding as to color and lustre, but its substance was less brittle. Carefully inspected, a minute spot was seen in the albumen, of a darker hue, little larger than a mustard-seed, which from after examination appeared to be of the nature of yolk, as oil globules were obtained from it when triturated with water, an emulsion then being formed in which these globules were distinct under the microscope.

The albumen immersed in water slowly absorbed it, but resisted solution, becoming of an opaque white, like ordinary coagulated albumen. This, its insolubility, I apprehend to have been owing to its having been exposed in its moist state to a temperature nearly of 212° in drying over a steam-bath.

The last egg I have to make mention of was examined eight days after it had been laid, viz., the 30th December. It was believed to have been either from an old hen or from a pullet,* if from the one, the last of her laying; if from the other, the first. It measured ·9 inch by ·7 inch, and weighed 298 grs. It was so symmetrical that it was difficult to say which end was largest. Opened under water, no air could be procured from either end. Its shell weighed 42 grs., and was, with its membranes, as thick as those of eggs of ordinary size. Its contents weighed 256 grs. These consisted of transparent albumen, similar to ordinary albumen, coagulating at about the same temperature, and of small short chalazæ attached to an included spot of diminished transparency, little larger than a barley-corn. This detached with the chalazæ was found on close examination to contain what I believe was a rudimentary ovum; it was circular, about one-fiftieth of an inch in diameter, and, as seen with a one-eighth-inch power, was of cellular structure, in which were scattered a few oil globules.†

The albumen acted on by liquor potassæ, by a strong solution of nitre, and by acetic acid, gave results similar to those from ordinary albumen, being blackened and partially dissolved by the first, only slightly dissolved by the acid, and slowly after many days by the salt.

These observations on the albumen of centenine eggs are confirmatory of those preceding on the albumen of the ordinary egg, as shewing how little disposed it is *per se* to putrefaction. In the instance of the first examined, which had been kept nearly two years, another change appears to have taken place, as indicated by the albumen (like fibrin) being no longer soluble in water, nor when saturated with water coagulable by heat. Whether the albumen of ordinary eggs would be similarly affected by keeping I am unable to say. Should it not, would not the circumstance either point to some original difference of quality, or to the yolk exercising a protective influence?

As in three of the four eggs a minute yolk or rudimentary

* Pullets' eggs are always small, weighing about one-third less than those of full grown hens; occasionally, but very rarely, hens of full growth lay perfect eggs as small as those of the pullet.

† Since the above was written, I have received from the same keeper of poultry two small eggs, one weighing 316·8 grs., laid on the 22nd of March; the other weighing 370 grs., laid on the 25th of that month. With the exception of yolk, they differed little from the ordinary egg. Their yolk, it may be remarked, was of an unusually orange yellow. They were from the same pullet, and neither her first nor last. The others were of the common size.

ovum was found, is it not probable that in the first examined an abortive ovum existed, so minute as to escape detection; and that, as in the other three instances, its presence in the uterus served as a stimulus to the production of the albumen and shell?

The origin of these abnormal eggs gave rise amongst the older writers to much speculation. Harvey says, "Our country folks still believe that such eggs are laid by the cock, and that were they set they would produce basilisks." Fabricius compared them to "a failure of the vegetative function, as happens to the peach and other fruit, of which we see many of adequate size, but a few that are very diminutive." Harvey conjectured that they may be the consequence of the inclemency of the weather, or the want of sun, or from defective nutriment in point either of quantity or quality." He adds, and justly, "I should not readily allow, however, that the eggs last laid are always small."

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THE END.

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