Peak, in charge of Mr. Marvin, accompanied by Mr. Gardner, and another under Mr. Gannett, accompanied by Dr. Peale, as geologist, and Mr. Batty as naturalist. According to the *Denver News*, the cattle, finding these constructions extremely convenient places for scratching, and thinking them apparently erected for their accommodation, have at once commenced appropriating them to that purpose, and evidently with great satisfaction, as it is said that they concentrate in their vicinity for miles around.

"ANNALEN des Physikalischen Centralobservatoriums" is the German title of the record for 1871 of the work done at the great Physical Observatory of St. Petersburg. It is a very thick quarto in Russian and German, and contains full and well-arranged meteorological statistics for fifty-five Russian towns for the year 1871.

THE following are the principal additions to the Brighton Aquarium during the past week:—10 Thornback Rays (*Raia clavata*), 1 Large Tope (*Galeus canis*), 1 Large Smooth Hound (*Mustelus vulgaris*) 3 Three-bearded Rockling (*Motella tricirrata*), 1,000 Sticklebacks (*Gasterosteus spinosus*), 1 fine group of *Actinoloba dianthus* (orange variety); a Smooth Hound (*Mustelus vulgaris*) gave birth to seven young ones, which died immediately, or were born dead.

THE additions to the Zoological Society's Gardens during the past week include two Maugé's Dasyures (*Dasyurus maugæi*) from Australia, presented by Mr. George Heath; a Tytlers Paradoxure (*Paradoxurus tytleri*) from the Andaman Islands, presented by Mr. J. S. Campbell; a Bactrian Camel (*Camelus bactrianus*) from Asia; a Gibbon (*Hylobates* sp. ?); a Crowned Eagle (*Spizaëtus coronatus*) from Senegal; three Blue crowned hanging Parrakeets (*Loriculus galgulus*) from Malacca; an Egyptian Fox (*Canis niloticus*); an Egyptian Vulture (*Neophron percnopteros*), purchased; an Ocelot (*Felis pardalis*) from America; a Hobby (*Hypotriorchis subbuteo*) from this country, and four red-billed Tree Ducks (*Dendrocygna autumnalis*) from America, deposited.

ON THE TEMPERATURE AT WHICH BACTERIA, VIBRIONES, AND THEIR SUPPOSED GERMS ARE KILLED*

WHILST a heat of 140° F. (60° C.) appears to be destructive to *Bacteria*, *Vibriones*, and their supposed germs in a neutral saline solution, a heat of 149° or of 158° F. is often necessary to prevent the occurrence of putrefaction in the inoculated fluids when specimens of organic infusions are employed. What is the reason of this difference? Is it owing to the fact that living organisms are enabled to withstand the destructive influence of heat better in such fluids than when immersed in neutral saline solutions? At first sight it might seem that this was the conclusion to be drawn. We must not, however, rest satisfied with mere superficial considerations.

The problem is an interesting one; yet it should be clearly understood that its solution, whatever it may be, cannot in the least affect the validity of the conclusion arrived at in my last paper, viz., that living matter is certainly capable of arising *de novo*. We were enabled to arrive at the conclusion above mentioned regarding Archebiosis by starting with the undoubted fact that a heat of 158° F. reduces to a state of potential death all the *Bacteria*, *Vibriones*, and their supposed germs which an organic infusion may contain. The inquiry upon which I now propose to enter, therefore, touching the degree of heat *below this point* which may suffice to kill such organisms and their supposed germs in an organic infusion, and touching the cause of the delayed putrefaction apt to take place in inoculated organic infusions which have been heated to temperatures above 140° and below 158° F., is one lying altogether outside the chain of fact and inference by which the occurrence of Archebiosis is proved.

* Extracts from a paper by Dr. H. Charlton Bastian, F.R.S., read before the Royal Society May 1, 1873.

It seems to me that the solution of the problems which form the subject of the present communication can only be safely attempted by keeping constantly before our minds two main considerations:—

Thus, in the experiments whose results it is now our object to endeavour to explain, the fluids have been inoculated with a compound consisting partly (*a*) of living units, and partly (*b*) of a drop of a solution of organic matter in a state of molecular change; so that in many cases where putrefaction has been initiated after the inoculating compound has been heated to certain temperatures, there is the possibility that this process of putrefaction may have been induced (in spite of the death of the organisms and their germs) owing to the influence of *b*, the dissolved organic matter of the inoculating compound; that is to say, the heat to which the mixture has been exposed may have been adequate to kill all the living units entering into the inoculating compound, although it may not have been sufficient to prevent its not-living organic matter acting as a ferment upon the infusion.

And there are, I think, the very best reasons for concluding that in all the cases in which turbidity has occurred after the organic mixtures have been subjected to a heat of 140° F. (60° C.) and upwards, this turbidity has been due, not to the survival of the living units, but rather to the fact that the mere dead organic matter of the inoculating compound has acted upon the more unstable organic infusions in a way which it was not able to do upon the boiled saline fluids.

The reasons upon which these conclusions are based are the following:—

1. Because the turbidity which has occurred in inoculated organic infusions that have been subjected to a temperature of 140° F. has always manifested itself appreciably later, and advanced much more slowly than in similar mixtures which had not been heated above 131° F.; whilst it has commenced even later, and progressed still more slowly, when occurring in mixtures previously heated to 149° F. Such facts might be accounted for by the supposition that exposure in these organic fluids to the slightly higher temperature suffices to retard the rate of growth and multiplication of the living units of the inoculating compound, although the facts are equally explicable upon the supposition that the later and less energetic putrefactions are due to the sole influence of the mere organic matter of the inoculating compound.

2. So far as the evidence embodied in the Tables goes, it tends to show that the more unstable different specimens of similar infusions are (that is, the stronger they are), the more rapidly and frequently does late turbidity ensue, and the more this late turbidity approaches, both in time of onset and in rate of increase, to that which occurs when inoculated infusions are not heated to more than 131° F.—when both living and non-living elements of the inoculating compound act conjointly as ferments. Such facts show quite clearly that where the intrinsic or predisposing causes of change are strong, there less potent exciting agencies are more readily capable of coming into play; but they still do not enable us to decide whether the exciting cause of this delayed turbidity is in part the living element whose vitality and rate of reproduction has been lowered by the heat, or whether the effects are wholly attributable to the mere organic matter of the inoculating compound.

So far, therefore, we have concomitant variations which are equally compatible with either hypothesis. But it will be found that each of the three succeeding arguments speaks more and more plainly against the possible influence of the living element, and in favour of the action of the organic matter of the inoculating compound, as an efficient exciting cause of the delayed putrefactions occurring in the cases in question.

3. As stated in my last communication,* when single drops of slightly turbid infusions of hay or turnip previously heated to 140° F. are mounted and securely cemented as microscopical specimens, no increase of turbidity takes place, although drops of similar infusions heated only to 122° F. do notably increase in turbidity (owing to the multiplication of *Bacteria*) when mounted in a similar manner. Under such restrictive conditions as these, in fact, a drop of an inoculated and previously heated organic infusion behaves in precisely the same manner as a drop of a similarly treated ammonic-tartrate solution. In each case, when heated to 140° F., turbidity does not occur, apparently because there are no living units to multiply, and because in

* See NATURE, vol vii. p. 435.

these mere thin films of fluid dead ferments are as incapable of operating upon the organic fluids as they are upon the ammonic-tartrate solutions.

4. Because, in the case of the inoculation of fluids which are not easily amenable to the influence of dead ferments, such as a solution containing ammonic tartrate and sodic phosphate, this delayed turbidity does not occur at all. Such inoculated fluids become rapidly turbid when heated to 131° F., though they remain clear after a brief exposure to a temperature of 140° F. When the living units in the inoculating compound are boiled, there is nothing left to induce turbidity in such solutions. The mere fact that these fluids do not undergo change when exposed to the air proves conclusively that they are very slightly amenable to the influence of the ordinary dead organic particles and fragments with which the atmosphere abounds. The absence of delayed turbidity in these fluids serves, therefore, to throw much light upon the cause of its occurrence in the organic infusions.

5. And, lastly, I can adduce crucial evidence supplied by the "Method of Difference," speaking with its accustomed clearness. Two portions of the same hay- or turnip-infusion can be inoculated in such a manner as to supply us with the information we require. In the one case we may employ a drop of a turbid ammonic-tartrate solution previously heated to 140° F., in which, therefore, the living units would certainly be killed: whilst in the other we may add an unheated drop of the same turbid saline solution to the organic fluid, and then heat this mixture also to the temperature of 140° F. The comparative behaviour of these two inoculated fluids (placed, in the ordinary manner, in previously boiled corked phials) should be capable of showing us whether the living elements of the inoculating compound were able to survive when heated in the organic infusion. If they did survive, the fluids inoculated in this manner ought to undergo putrefaction earlier and more rapidly than those inoculated with the drop of turbid fluid, in which we know that the *Bacteria*, *Vibriones*, and their supposed germs would have been reduced to a state of potential death. With the view of settling this question, therefore, the following experiments were made:—

Description of Experiments.	Results.	Inferences.
A. Boiled ammonic-tartrate solution, inoculated with an unheated drop of a similar solution turbid with *Bacteria*, &c.	Turbid in 40 hrs.	That boiled ammonic-tartrate solution is a fluid inoculable by living *Bacteria*, &c., and favourable for their growth and rapid multiplication.
B. Boiled ammonic-tartrate solution, inoculated with a drop of a turbid saline solution previously heated to 140° F.	Clear at expiration of 8th day.	That *Bacteria*, *Vibriones*, and their supposed germs are either killed or deprived of all power of multiplication when heated to 140° F. in this fluid.
C. Boiled turnip- and hay-infusions, inoculated with a drop of a turbid saline solution previously heated to 140° F.	Turnip-infusions turbid in 2½ days. Hay-infusions clear at expiration of 8th day.	The precisely similar behaviour of the turnip- and hay-infusions of series C and series D respectively shows that *Bacteria*, *Vibriones*, and their supposed germs are as inoperative in series D as they are known to be in series C; whilst the behaviour of the hay-infusions shows that they are little amenable to the influence of the drop of the saline fluid when its living units are killed.
D. Boiled turnip- and hay-infusions, inoculated with a drop of an unheated turbid saline solution, the inoculated fluid being subsequently heated to 140° F.	Turnip-infusions turbid in 2½ days. Hay-infusions clear at expiration of 8th day.	
E. Boiled turnip- and hay-infusions, inoculated with a drop of an unheated saline solution, the inoculated fluid being subsequently heated to 131° F.	Turnip-infusions turbid in 23 hrs. Hay-infusions turbid in 38 hrs.	Shows that a heat of 131° F. is not sufficient to kill *Bacteria*, *Vibriones*, and their supposed germs in organic infusions, and, again, that turnip-infusions are more rapidly influenced by such an inoculating agent than some hay-infusions.*

No experiments could speak more decisively. Those of series B show that *Bacteria*, *Vibriones*, and their supposed germs are either actually or potentially killed when heated to 140° F. in the neutral saline fluid, which the experiments of series A show to be eminently favourable for their growth and reproduction. Being certain, therefore, that the living units are killed in the drops with which the fluids of series C were inoculated (because they were drops of the same fluid as was employed in series B), we may be equally certain that the turbidity and putrefaction which did ensue in the turnip-solutions of series C were due to the influence of the mere dead constituents of these drops of the turbid saline fluid; whilst, seeing that the behaviour of the fluids of series D was precisely similar to those of series C, we have a perfect right to infer that this series of fluids (D) was as devoid of living units as those of C are known to be—that is, that *Bacteria*, *Vibriones*, and their supposed germs are killed by the temperature of 140° F. in organic fluids, just as they are in saline fluids, although, as shown by the experiments of series E, they do not succumb to a heat of 131° F.

* These experiments of series C, D, and E were many times repeated with specimens of the same turnip- and hay-infusions, the specific gravity of the former being about 1008 and that of the latter 1005. Different specimens of hay especially vary so much that it becomes absolutely essential to use portions of the same infusion for the comparative experiments of these different series.

The evidence now in our possession shows, therefore, that whilst the temperature at which living ferments cease to be operative varies within very narrow limits (131°–140° F.), that which destroys the virtues of non-living ferments varies within much wider limits, and depends not only upon the amount of heat employed, but also upon the nature of the putrescible or fermentable liquid to which such ferment is added, in conjunction with the degree of heat and other conditions to which the mixture is subsequently exposed.* Here, therefore, we have evidence as to the existence of a most important difference between living and not-living ferments, which has always been either unrecognised or more or less deliberately ignored by M. Pasteur and his followers.† This difference is, moreover, thoroughly in accordance with the broad physico-chemical theory of fermentation which has been so ably expounded by Baron Liebig and others, and the truth of which may now be regarded as definitely established. According to this theory "living" matter, as a ferment, would take rank merely as a chemical compound having a tolerably definite constitution; and this, we might reasonably infer, would, like other chemical compounds, be endowed with definite properties, and amongst others that of being decomposed or radically altered by exposure to a certain amount of heat. Looked at also from this essentially chemical point of view, it would be only reasonable to expect that the molecular movements of living ferments with a lowered vitality might not be more marked or energetic than those which many not-living organic substances are apt to undergo; and this being the case, we might expect that there would often be a great practical difficulty in ascertaining whether a ferment belonging to the arbitrary and artificial (though, in a sense, justifiable and natural) category of "living" things had or had not been in operation.

Dr. Bastian then refers to certain statements made by M. Pasteur, and afterwards classifies the various fermentable fluids under three main divisions:—I. Self-fermentable fluids; II. Fluids which will not ferment without the aid of unheated organic matter, either not-living or living; III. Fluids which will only ferment under the initiating influence of living matter.

Dr. Bastian's conclusions from these investigations are thus expressed:—

Thus it can now be proved, by evidence of a most unmistakable nature, that the process of putrefaction which invariably occurs in previously boiled putrescible infusions contained in flasks with narrow but open necks is not commonly (is, perhaps, only very rarely) initiated by living germs or organisms derived from the atmosphere; it can also be proved that putrefaction and the appearance of swarms of living organisms may occur in some boiled fluids when they are simply exposed to air which has been filtered through a firm plug of cotton-wool or though the narrow and bent neck of a flask, to air whose particles have been destroyed by heat, or even in fluids hermetically sealed in

* See "The Beginnings of Life," vol. i. p. 437.

† See, for instance, all M. Pasteur's celebrated experiments in which he had recourse to an "ensemencement des poussières qui existent en suspension dans l'air," as recorded in chaps. iv. and v. of his memoir in "Ann. de Chimie et de Physique," 1862. M Pasteur was engaged in an investigation, one of the avowed objects of which was to determine whether fermentation could or could not take place without the intervention of living organisms, which M. Pasteur held (in opposition to many other chemists) to be the only true ferments. In his inoculating compound (dust filtered from the atmosphere), there was, as M. Pasteur was fully aware, a large amount of what his scientific opponents considered non-living ferment, whilst *possibly* there existed a certain number of living ferments. In explaining the results of his experiments, however, M. Pasteur and others thought he was pursuing a logical and scientific method when he attributed these results to the action of the possibly existing element of the inoculating compound, whilst he ignored altogether the other element which was certainly present in comparatively large quantity, and the testing of whose efficacy was the ostensible object of his research.

flasks from which all air has been expelled. The evidence in our possession is therefore most complete on this part of the subject: it shows beyond all doubt, not only that putrefaction may and does very frequently occur under conditions in which the advent of atmospheric particles, whether living or dead, is no longer possible, but also that living particles derived from the atmosphere can only be very rare and altogether exceptional initiators of the putrefaction which invariably occurs in previously boiled infusions exposed to the air.

Again, the evidence which we now possess with reference to the influence of heat upon *Bacteria*, *Vibriones*, and their supposed germs is no less decisive. It has been unmistakably proved that such organisms and their imaginary germs are either actually or potentially killed by a brief exposure to the temperature of 140° F. when in the moist state; and it had also been previously established that they are invariably killed by desiccation even at much lower temperatures.*

But if living germs do not come from the air to contaminate the previously boiled fluids, and if it is not possible for any of them to have escaped the destructive influence of heat in the boiling fluid or on the walls of the vessel in which the fluid is contained, what can be the mode of origin of the swarms of living things which so rapidly and invariably appear in such infusions when contained in open flasks, and which so frequently appear when the infusions are contained in flasks whose necks are closed against atmospheric particles of all kinds? They can only have arisen by the process which I have termed Archebiosis.

Conclusions

If a previously boiled ammonic-tartrate solution remains free from *Bacteria* and *Vibriones* when exposed to the air, it is because the air does not contain living organisms of this kind or their supposed germs, and because mere dead organic particles are not capable of initiating putrefaction in such a fluid.

And if ordinary organic infusions previously boiled and exposed to the air do rapidly putrefy, though *some* of the same infusions when exposed only to filtered air remain pure, it is because such fluids are, in the absence of living units, quite amenable to the influence of the dead organic particles which the air so abundantly contains, although they are not self-fermentable.

Whilst if other more changeable fluids, after previous boiling, when exposed to filtered air or cut off altogether from contact with air, do nevertheless undergo putrefaction or fermentation, it is because these fluids are self-fermentable, and need neither living units nor dead organic particles to initiate those putrefactive or fermentative changes which lead to the evolution of living organisms.

* See the experiments and conclusions of Dr. Burdon Sanderson in Thirteenth Report of Med. Officer of Privy Council, p. 61. This fact of the inability of these organisms and their germs to resist desiccation shows the futility of some objections which have been from time to time raised by those who thought that *Bacteria*, *Vibriones*, and their germs might resist the destructive influence of heat by adhesion to the glass above the level of the fluid, or even in the fluid itself, just as dried and very thick-coated seeds have been known to do. Dry heat would seem to be even more fatal to such organisms and their germs than a moist heat of the same degree, owing to their extreme inability to resist desiccation; if they become dry they are killed at a temperature of 104° F., whilst if they remain moist they succumb, as we have seen, to a temperature of 140° F.

SCIENTIFIC SERIALS

The June number of the *Journal of Anatomy and Physiology* contains several papers of special interest, as well as the excellent summaries by Profs. Turner and Rutherford, of the progress of Anatomy and Physiology during the last six months. Prof. Turner describes, for the first time, the Visceral Anatomy of the Greenland Shark (*Læmargus borealis*) from two specimens caught near the Bell Rock. The larger was 11 feet 8 inches long, and the other 8½ feet: they were both females. The most important peculiarities of this fish, wherein it differs from other sharks are, that the *bursa entiana* is not developed; that there are two large duodenal cæca, one of which is closely adherent to the pyloric tube, as well as a true pancreas, corresponding with the similar condition found by Alessandrini in the Sturgeon; and that there are no oviducts, so that the ova must be discharged into the peritoneal cavity. From these peculiarities the author places *Læmargus* in a family by itself, named by him *Læmargidæ*.—Prof. Turner also, in a short paper on the so-called claw at the end of the tail of the lion, shows that no true claw exists, but that the tip of the tail is hairless, and becomes hard on drying.—Prof. Rutherford tabulates experiments proving that the retardation of the pulse in the rabbit, which follows closure of the nostrils, depends on the obstruction of the respiration, and not as Drs. Brown-Séquard and Sanderson supposed, on direct reflex action. Mr. Dewar and Dr. McKendrick describe experiments on the Physiological Action of Light, an account of which has already appeared in this journal.—Mr. Blake, of San Francisco, has a paper on the action of the salts of the metals sodium, lithium, cæsium, &c., when introduced directly into the blood. Mr. A. H. Smee, in a paper on the physical nature of the coagulation of the blood, endeavours to prove that it coagulates in obedience to a purely physical law, namely, the power of soluble colloid matter to pectinise, or spontaneously to coagulate. Mr. Garrod, on the law which regulates the frequency of the pulse, proposes as a substitue for that given by Marey, the following:—the heart re-commences to beat when the arterial tension has fallen an invariable proportion, this being the only possible explanation of the facts that pulse rate varies with arterial resistance and not with blood pressure. He also gives a new theory of the source of nerve force.—Dr. Charles, Prof. Curzon, and Prof. Drachmann, record peculiarities in anthropotomy, the first in the arterial system, the second in the muscular and nervous system, and the third in the muscular.—There is an excellent and very careful review, by Mr. Trotter, of the Rev. Samuel Haughton's "Principles of Animal Mechanics," which will be very valuable to many physiologists, who here have the opportunity of seeing the opinion of a mathematician, who is also a biologist, of a work which might by itself lead them to think that the physiogical basis for work was in a better position than it really is.

Bulletin Mensuel de la Société d'Acclimatation de Paris for June. A great portion is devoted to the description of the best modes of rearing silkworms and the more suitable kinds of food for feeding them. A paper is devoted to the Japanese Mulberry (*Morus japonica*), which is being introduced into France as producing a superior food for the silkworm.—The cultivation of various kinds of beans and melons is advocated by M. Bossin, and his paper might be read with advantage in this country, where these vegetables are not sufficiently valued as an article of diet. Not only the acclimatisation of useful, but the destruction of hurtful animals, plants, and insects, forms part of the programme of the society, and we have therefore some remarks on insecticides and on the preservation of insectivorous birds.—The American notes on pisciculture, on the grey wolf, and the commerce of Chicago are interesting. A black monkey from Sumatra has just arrived at the Jardin d'Acclimatation, but it is not expected to live.

SOCIETIES AND ACADEMIES

London

Quekett Microscopical Club, July 25.—Dr. Braithwaite, F.L.S., president, in the chair.—This being the annual meeting, the report of the committee for the past year was read, and testified to the continued prosperity of the club, which now numbers 570 members.—The president delivered the annual address, in the course of which he noticed the progress of microscopical investigation in Botany and Zoology during the past year.—The ballot then took place for the election of officers. Dr. Braithwaite was re-elected president; Dr. Matthews, Messrs. B. T. Lowne, T. W. Burr, and C. F. White, vice-presidents; and Messrs. Bywater, Crisp, Hailes, Hind, Waller, and T. C. White, were elected to fill the six vacancies on the committee. Mr. J. E. Ingpen succeeded Mr. T. C. White, who retires from the office of hon. sec. (owing to increase of his professional duties), after four years of unremitting and valuable service. The proceedings terminated with the usual *conversazione*.

Belgium

Royal Academy of Sciences, May 13.—Reports were given in on the following papers:—On the Superficial Tension of Liquids considered in reference to certain movements observed on their surface, by M. G. Van der Mensbrugghe, which it was resolved to print in the *Memoires*.—On the Osculatory Sphere, a note by M. L. Saltel, which is printed in the *Bulletin*.—On the chloric acetonitriles, by M. L. Bisschopinck, also printed in the *Bulletin*.—Essay on the state of vegetation at the