but carefully protected from currents of air by screens appropriately arranged. The horizontal gas-pipe with its perforations, ¼ inch between each, having been so adjusted that the distance between the central perforation and the thermometer was 14 in., the 17 jets were ignited, and the supply of gas regulated at precisely 5 cubic feet per hour. The temperatures imparted to the thermometer during the experiment are recorded in Table A. It

TABLE A.

Time.	Indication of Thermometer.	Temperature of surrounding Atmosphere.	Differential Temperature.
m. s.	Deg. Fah.	Deg. Fah.	Deg. Fah.
0 0	62·8	62·8	0·0
6 2	64·4	62·8	1·6
12 5	65·6	63·0	2·6
18 6	66·5	63·2	3·3
24 8	67·0	63·4	3·6
30 11	67·3	63·6	3·7
36 9	67·4	63·7	3·7

will be seen on examining this table that the temperature imparted to the thermometer at the expiration of 36 minutes, was only 3·7° F., in place of 61·2° observed by Mr. Williams; the rate of discrepancy being thus 3·7 : 61·2 = 1 : 16·56.

This extraordinary discrepancy between the temperatures published by Mr. Williams in his original communication, and the distances assumed in his reply to Lord Rosse, having been fully established by the experiment, the arrangement was changed, the thermometer being brought within 3 in. of the flames. But even at this short distance, the thermometer exposed to the radiant heat during an interval of 29 minutes, indicated a differential temperature of only 22·6° F., in place of 61·2° F. as stated by Mr. Williams—a fact clearly showing that the high temperature observed by him was owing to the intervention of the box in which he inserted the thermometer. Such a box composed of polished tin plate, open at the end presented towards the flame, was accordingly applied; its position being such that the space between the thermometer and the flame measured 3 in., as before. The 17 jets being ignited and the supply of gas regulated at 5 cubic feet per hour, the column of the thermometer rose rapidly, attaining a height of 136° in 20 minutes. Deducting the temperature of the surrounding air, 63·5°, the increment of heat proved to be 72·5°, thus showing that by the intervention of the box, the differential temperature was increased threefold. It scarcely requires explanation that owing to the close proximity of the flame the air in the box becomes heated, imparting its heat to the thermometer, by convection; while the reflection of the heat rays against the sides and bottom of the polished box, imparts radiant heat to those parts of the bulb which are not exposed to the direct radiation of the flame.

In view of the foregoing explanation it will be evident that, in a properly conducted experiment, the temperatures recorded in Mr. Williams' table cannot be produced unless the thermometer be placed even nearer than 3 in. from the flame. But admitting that the recorded temperatures could be developed at a distance of 3 in., it will be found that the mistake to which Lord Rosse has called attention is fatal to Mr. Williams' deductions. Referring to Table B, constructed in accordance with

TABLE B.

No. of jet from Thermometer.	Energy Transmitted.	No. of jet from Thermometer.	Energy Transmitted.
1	2·77	10	0·90
2	2·37	11	0·82
3	2·04	12	0·75
4	1·77	13	0·69
5	1·56	14	0·64
6	1·38	15	0·59
7	1·23	16	0·55
8	1·11	17	0·51
9	1·00		

the theory pointed out by Lord Rosse in his letter to NATURE, it will be seen that each pair of jets so far from developing an equal amount of radiant energy, as indicated by Mr. Williams' table, they differ to a very great extent. For instance, while the two jets on each side of the centre develope respectively 1·11 and 0·90 (the energy transmitted by the central jet being represented by unity), the two outside jets develope respectively 2·77 and 0·51. Accordingly, the energy developed by the central pair will be 1·11 + 0·90 = 2·01, while the outside pair develope 2·77 + 0·51 = 3·28. Leaving out of sight the imperfections of the method adopted in making the observations, this great difference of the radiant energy transmitted to the thermometer by each pair of jets, is conclusive against the deduction concerning diathermacy of flame, which Mr. Williams has based on his published table of temperatures. J. ERICSSON

SCIENTIFIC SERIALS

THE *Archives des Sciences Physiques et Naturelles* contains a long and admirable article by Prof. Plantamour, on the meteorology of Geneva and the Great St. Bernard for 1871, a year of very exceptional weather at these places. In a series of carefully compiled tables, the various meteorological phenomena observed are compared in every possible way, and deserve the study of meteorologists. The second paper is an exceedingly interesting one from the work published by M. de Candolle, "Histoire des Sciences et des savants depuis deux Siècles," &c., containing the result of much acute and original research, on Transformations of Movement among Organised Beings. The other two principal papers are one by M. Ernest Favre, on the Geology of the Ralligstöcke on the banks of Lake Thun, and one by MM. de la Rive and Sarasin on the rotation under magnetic influence of the electric discharge in rarefied gases, and on the mechanical action which this discharge may exercise in its movement of rotation.

Transactions of the Wisconsin Academy of Science, Arts, and Letters, 1870-72. This academy was organised in 1870, "by a convention called by the Governor and more than one hundred other prominent citizens of the State," its general objects being "the material, intellectual, and social advancement of the State," as well as, or rather by means of the advancement of, science, literature, and the arts. This first volume of Transactions contains some specimens of the work already done by the Academy in its various departments, to which is prefixed an extremely interesting *résumé* of what has already been done by Wisconsin for science. This is followed by a long list of papers on various subjects read before the Academy since its formation. Of the scientific papers contained in the volume before us, Dr. Lapham contributes one "On the Classification of Plants;" Mr. J. G. Knapp "On the Coniferæ of the Rocky Mountains;" Prof. Irving on "The Age of the Quartzites, Schists, and Conglomerate of Sank Co. Wisconsin;" Prof. Chamberlain a few suggestions, some of them original, as to a Basis for the Gradation of the Vertebrata; and Prof. Davies "On Potentials and their Application to Physical Science;" in which he attempts to give a *physical* interpretation to the potential function, and to illustrate it and its use by some simple examples. We hope the Academy will continue to produce as satisfactory work in the future as it has done since it commenced.

WE have received numbers 8, 9, 10, and 11 of the *Australian Mechanic and Journal of Science and Art* for August, September, October, and November, and highly creditable is the quality of the contents to its able editor, Mr. Ellery, Superintendent of Melbourne Observatory, and a hopeful sign of the intelligence and progress of the Australian people it is, that such a high-class scientific journal should have a paying circulation in so young a colony. Mr. Ellery himself is contributing a series of valuable and well-illustrated articles on "How to make and how to use a Spectroscope," while another contributor, "Delta," concludes in the August number a series of seven papers on "Spectrum Analysis." The subjects treated of are very various, and mostly practical, but whatever the subject of an article may be, science and the application of scientific principles are never lost sight of. There is a series of articles on agriculture, in which the application of science to this department of industry is well illustrated; and in an article on "Science and Government," principally with reference to the supply of coal, the writer concludes thus:—"There is scarcely any subject within the range of material science, however trifling it may at first appear, that has not a direct and important interest for the whole community, and especially for those who hold the responsibility of conducting the affairs and guarding the interests of the State." Would that all ministers would realise and act upon the great truth, so clearly and pithily expressed. Mr. Ellery contributes monthly a very valuable and interesting set of "Astronomical Notes," in which he gives all the details in a tabular form necessary to find out the positions, on the first of each month, of the planets, nebulæ, clusters, and double and other peculiar stars. We hope the journal will have all the success it well deserves.

SOCIETIES AND ACADEMIES

London

Royal Society, Jan. 30.—Prof. George Busk, vice-president, in the chair. The following communications were read:—"Note on the Origin of *Bacteria*, and on their Relation to the Process of Putrefaction." By Dr. H. Charlton Bastian, F.R.S.

In his now celebrated memoir of 1862, M. Pasteur asserted and claimed to have proved (1) that the putrefaction occurring in certain previously boiled fluids after exposure to the air was due to the contamination of the fluids by *Bacteria*, or their germs, which had before existed in the atmosphere; and (2) that all the organisms found in such fluids have been derived more or less immediately from the reproduction of germs which formerly existed in the atmosphere.

The results of a long series of experiments have convinced me that both these views are untenable.

In the first place, it can be easily shown that living *Bacteria*, or their germs, exist very sparingly in the atmosphere, and that solutions capable of putrefying are not commonly infected from this source.

It has now been very definitely ascertained that certain fluids exist which, after they have been boiled, are incapable of giving birth to *Bacteria*, although they continue to be quite suitable for the support and active multiplication of any such organisms as may have been purposely added to them. Amongst such fluids I may name that now commonly known as "Pasteur's solution," and also one which I have myself more commonly used, consisting of a simple aqueous solution of neutral ammonic tartrate and neutral sodic sulphate.* When portions of either of these fluids are boiled and poured into superheated flasks, they will continue quite clear for many days, or even for weeks—that is to say, although the short and rather narrow neck of the flask remains open the fluids will not become turbid, and no *Bacteria* are to be discovered when they are submitted to microscopical examination.

But in order to show that such fluids are still thoroughly favourable media for the multiplication of *Bacteria*, all that is necessary is to bring either of them into contact with a glass rod previously dipped into a fluid containing such organisms. In about thirty-six hours after this has been done (the temperature being about 80° F.), the fluid, which had hitherto remained clear, becomes quite turbid, and is found, on examination with the microscope, to be swarming with *Bacteria*.†

Facts of the same kind have also been shown by Dr. Burdon Sanderson‡ to hold good for portions of boiled "Pasteur's solution." Air was even drawn through such a fluid daily for a time, and yet it continued free from *Bacteria*.

Evidence of this kind has already been widely accepted as justifying the conclusion that living *Bacteria* or their germs are either wholly absent from, or, at most, only very sparingly distributed through the atmosphere. The danger of infection from the atmosphere having thus been got rid of and shown to be delusive, I am now able to bring forward other evidence tending to show that the first *Bacteria* which appear in many boiled infusions (when they subsequently undergo putrefactive changes) are evolved *de novo* in the fluids themselves. These experiments are moreover so simple, and may be so easily repeated, that the evidence which they are capable of supplying lies within the reach of all.

That boiling the experimental fluid destroys the life of any *Bacteria* or *Bacteria* germs pre-existing therein is now almost universally admitted. It may moreover be easily demonstrated. If a portion of "Pasteur's solution" be purposely infected with boiled *Bacteria* and subsequently boiled for two or three minutes, it will continue (if left in the same flask) clear for an indefinite period; whilst a similarly infected portion of the same fluid, not subsequently boiled, will rapidly become turbid. Precisely similar phenomena occur when we operate with the neutral fluid which I have previously mentioned; and yet M. Pasteur has ventured to assert that the germs of *Bacteria* are not destroyed in neutral or slightly alkaline fluids which have been merely raised to the boiling-point.*

Even M. Pasteur, however, admits that the germs of *Bacteria* and other allied organisms are killed in slightly acid fluids which have been boiled for a few minutes; so that there is a perfect unanimity of opinion (amongst those best qualified to judge) as to the destructive effects of a heat of 212° F. upon any *Bacteria* or *Bacteria* germs which such fluids may contain.

Taking such a fluid, therefore, in the form of a strong filtered infusion of turnip, we may place it after ebullition in a superheated flask with the assurance that it contains no living organisms. Having ascertained also by our previous experiments with the boiled saline fluids that there is no danger of infection by *Bacteria* from the atmosphere, we may leave the rather narrow mouth of the flask open, as we did in these experiments. But when this is done, the previously clear turnip infusion invariably becomes turbid in one or two days (the temperature being about 70° F.), owing to the presence of myriads of *Bacteria*.

Thus if we take two similar flasks, one of which contains a boiled "Pasteur's solution," and the other a boiled turnip infusion, and if we place them beneath the same bell-jar, it will be found that the first fluid remains clear and free from *Bacteria* for an indefinite period, whilst the second invariably becomes turbid in one or two days.

What is the explanation of these discordant results? We have a right to infer that all pre-existing life has been destroyed in each of the fluids; we have proved also that such fluids are not usually infected by *Bacteria* derived from the air—in this very case, in fact, the putrescible saline fluid remains pure, although the organic infusion standing by its side rapidly putrifies. We can only infer, therefore, that whilst the boiled saline solution is quite incapable of engendering *Bacteria*†, such organisms are able to arise *de novo* in the boiled organic infusion.

Although this inference may be legitimately drawn from such experiments as I have referred to, fortunately it is confirmed and strengthened by the labours of many investigators who have worked under the influence of much more stringent conditions, and in which closed vessels of various kinds have been employed.‡

Whilst we may therefore infer (1) that the putrefaction which occurs in many previously boiled fluids when exposed to the air is not due to a contamination by germs derived from the atmosphere, we have also the same right to conclude (2) that in many cases the first organisms which appear in such fluids have arisen *de novo*, rather than by any process of reproduction from pre-existing forms of life.

Admitting, therefore, that *Bacteria* are ferments capable of initiating putrefactive changes, I am a firm believer also in the existence of not-living ferments under the influence of which putrefactive changes may be initiated in certain fluids—changes which are almost invariably accompanied by a new birth of living particles capable of rapidly developing into *Bacteria*.

"On Just Intonation in Music; with a description of a new Instrument for the easy control of all Systems of Tuning other than the ordinary equal Temperament." By R. H. M. Bosanquet.

The object of this communication is to place the improved systems of tuning within the reach of ordinary musicians; for this purpose the theory and practice are reduced to their simplest forms. A notation is described, adapted to use with ordinary written music, by which the notes to be performed are clearly distinguished. The design of a key-board is described, by which any system of tuning, except the ordinary equal temperament, can be controlled, if only the fifths of the system be all equal. The design is on a symmetrical principle, so that all passages and combinations of notes are performed with the same handling, in whatever key they occur. The theory of the construction of scales is then developed, and a diagram is given, from which the charac-

* In the proportion of 10 grains of the former and 3 of the latter to 1 ounce of distilled water.

† The Modes of Origin of Lowest Organisms, 1871, pp. 36, 51.

‡ Thirteenth Report of the Medical Officer of the Privy Council (1871), p. 59.

* How unwarrantable such a conclusion appears to be, I have elsewhere endeavoured to show. See "Beginnings of Life," 1872, vol. i. pp. 326-333, and pp. 372-399.

† See "Beginnings of Life," vol. ii. p. 35, and vol. i. p. 463.

‡ See a recent communication by Prof. Burdon Sanderson, in NATURE January 9.