THE SPONTANEOUS GENERATION CONTROVERSY.

In a brief note recently made by us of Professor Tyndall's late investigations into the question of spontaneous generation we mentioned his statement that his results effectually upset the views of the adherents of that theory, but expressed the opinion that the latter, and especially Dr. Bastian as their leader, would not rest quietly under any such sweeping assertion. Professor Tyndall has chosen the pages of the *Nineteenth Century* as his battle ground, and in the January number of that review he presents a historical retrospect of the subject, and a *résumé* of the experiments and arguments deduced therefrom upon which he relies. In the succeeding number of the same periodical Dr. Bastian replies, and biologists therefore now have before them succinct discussions of the rival theories from the two eminent scientists who lead the opposing camps.

Professor Tyndall, in reviewing past investigations, notes first those of Redi, in 1668, whereby it was proved that the maggots of putrefying flesh are derived from eggs of flies, thus destroying the belief that they were due to spontaneous generation in the meat. He then glances briefly at the labors of Needham, and their overthrowing by those of Spallanzani, and notes the proof obtained by Schwann that putrefaction itself is a concomitant of far lower forms of life than those dealt with by Redi. Pasteur's important discoveries, and notably that of the non-generative power of air on the Alpine glaciers or in subterranean caves in Paris, are reviewed, and with this much introduction the writer brings forward his own researches. Fifty flasks filled with strong organic infusions are heated first to 250°, and the necks are hermetically sealed. Of these twenty-seven are opened at an elevation on the Alps 7,000 feet high; the remainder are unsealed in a hay loft. All are then placed over a stove in a temperature varying from 50° to 90° Fah., and in three days out of all the flasks opened in the hay loft but two remained free from organisms, while of those opened on the mountain, although kept warm for three weeks, not one became infected. Professor Tyndall regards the inference from this as imperative that something in the air produced the effects observed, and that something might be the dust. He then proceeds to detail experiments in which organic infusions of all kinds were submitted to purified air for more than a year without putrescence setting in, whereas when exposed to dust-laden air the reverse took place in a few days. This, he argues, must prove that the dust particles are the cause of putrefactive life. The submission of the flasks to higher temperatures in a Turkish bath caused no change in the results. Other experiments are quoted to show that the resistance to heat of germs widely varies, the limit of time being eight hours' exposure to boiling. Probably more extended researches, it is urged, would reveal germs more obstinate still, so that there is no foundation for speaking of a death point of bacteria and their germs. Still further experimenting is adduced to show that either in a clear mineral solution containing in proper proportions all the substances which enter into the composition of bacteria, or in a turnip solution, the addition of an infected piece causes life in twenty-four hours. If, however, instead of the infected piece, a pinch of laboratory dust be added to each clear solution, the mineral solution remains unaffected. The inference is that while both liquids are able to feed the bacteria and to enable them to increase and multiply after they have been once fully developed, only one of the liquids is able to develop into bacteria—the germinal dust of the air. Professor Tyndall concludes his paper with a number of instances going to disprove the argument that bacteria and their germs being destroyed at 140° must if they appear after exposure to 212° be spontaneously generated.

Dr. Bastian, in his reply, says that all this discussion about the nature of atmospheric dust, with the elaborate experiments to prove its infective nature, so far as fermentations are concerned, has not advanced the question one iota; that Professor Tyndall has never been able to get beyond Schwann's simple conclusion that the air contains a "something" that is infective. The issue, Dr. Bastian says, rests upon the extent to which it can be proved that living things resist the action of water at a high temperature, and not at all upon the points brought out in Tyndall's experiments. He refers to his researches of 1873, which conclusively prove that the bacteria and all the reproductive particles which they may possess were killed at a temperature of 140° Fah., and the confirmation of this result by Cohn and Horrath. He denies any confounding of germ and its offspring, or that he attempts to make special kinds of living matter do duty for all kinds, as was imputed by Tyndall, and presents a table showing the fatal temperatures to various organisms, from those of a simple aquatic nature to eggs. Regarding Professor Tyndall's statement that further researches might reveal germs capable of withstanding more than eight hours' boiling, Dr. Bastian says: "He argues from a one-sided analogy that bacteria *must* spring from seeds, and then uses this *must* as the ready interpretation of all his experiments, shutting his eyes apparently to all other considerations, even though this interpretation 'violates all antecedent knowledge,' as it certainly does. What present warrant is there for supposing that a naked or almost naked speck of protoplasm can withstand four, six, or eight hours' boiling? To which I only answer, None."

Dr. Bastian quotes Professor Lester, who considers it extremely improbable that bacteria have germs, and states that he has never found any organisms in the moist state which resisted the temperature of 212° continued for half an hour. Dr. Burdon Sanderson agrees with Professor Lester, that no proof has been given of any such seed with reference to common bacteria. Finally, Dr. Bastian states that those who would show that the balance of evidence is against spontaneous generation being a common process, at the present day can only do so by bringing forward proofs that ferment organisms are really able to withstand a brief exposure to 212° Fah. in fluids—proofs that are stronger than the evidence which up to 1870 had engendered the almost universal belief that nothing of the kind was possible.

The Evaporative Power of Locomotive Boilers.

In a recent communication to the Institution of Civil Engineers, by Mr. J. A. Longridge, M. Inst. C. E., the author endeavored to set at rest certain widely diverging opinions which existed among practical men, with reference to the evaporative efficiency of the various elements of a locomotive boiler—such as the area of the fire grate compared with the total heating surface, the ratio between the tube surface and the fire box surface, and the rate of combustion per square foot of fire grate. The cause of such divergence of opinion was due to the multitude of variable conditions, and it was only by embodying these in a symbolic formula that the relative effects could be estimated.

After adverting to Mr. D. K. Clark's formula, $w = ar^2 + bc$, and pointing out that, from its empirical nature, it was only applicable within certain limits, the author investigated a new formula, based upon well known physical laws and mathematical principles. Assuming any given consumption of fuel per hour, the amount of heat generated was first determined; then, from the laws of the transmission of heat through plates, the quantity which passed through the fire box surface into the water was deduced, and from what remained the temperature of the gases entering the tubes was found. From this the loss of temperature in passing through the tubes was calculated, based upon the same law of transmission, and thus there was obtained the temperature of the gases in the smoke box. From the loss of temperature in passing through the tubes the evaporative effect of the tube surface was ascertained, and this, added to that of the fire box, gave the total evaporative effect of the boiler.

From the author's formula the evaporative powers of twenty engines were calculated, and the results compared with actual experiment, and with those given by Mr. D. K. Clark's formula. It was shown that the tube surface was a very important element, and that on an average the tubes effected nearly 80 per cent of the whole evaporation. Also that the generally received idea, that 1 foot of fire box surface was equal to 3 feet of tube surface, was fallacious; indeed the proportion was very variable, for while in the Ixion 1 foot of box surface was only equal to 1·7 foot of tube surface, yet in No. 33, Caledonian engine, 1 foot of box surface was equal to 5 feet of tube surface. Consequently no fixed ratio could afford a safe rule for practice. It was then demonstrated that the length of the tubes had nothing to do with economy of evaporation, but that this depended simply upon the ratio between the consumption of fuel per hour and the total absorbing surface. The question of the diameter of the tubes was next discussed, the late Mr. Zerah Colburn's views being dissented from; and it was shown that the diameter was a matter of no consequence so long as the proper amount of surface was obtained. The same remark might be made regarding the ratios between the fire grate and the heating surface. It was not the area of the fire grate, but the weight of fuel consumed per hour which had to be considered; and as regarded economy of evaporation it mattered little whether 50 lbs. of coke per square foot per hour were burned in a grate of 20 square feet area, or 100 lbs. per square foot per hour in a grate of 10 square feet area. In each case, if the absorbing surface were the same the economy of evaporation would be the same.

The question how far the combustion of fuel was perfect was then examined, and it was pointed out that in many cases it was very far from being so, some French experiments exhibiting losses of from 22 to 39 per cent.

The general conclusions arrived at might be thus summed up: That no fixed rule could be established as the best for the relative proportions of the fire grate, fire box, and tube surfaces; that length of tube had nothing to do with economic effect; that the diameter of the tube was also a matter of indifference; that economy of fuel did not depend upon the rate of firing; that when the quantity of fuel burnt was moderate, say 50 lbs. or 60 lbs. per square foot of grate per hour, the combustion was nearly perfect, while with hard firing there was considerable loss from carbonic oxide passing away unconsumed; and that a large increase of heating surface in proportion to coal burnt only slightly increased the economic effect, which within the limits of practice in locomotive engines was nearly in proportion to the fourth root of the heating surface.

In an addendum the action of the blast pipe was discussed. It was contended that, though a powerful agent in effecting rapid combustion, it was, *per se*, a very extravagant one; yet in general in the case of locomotive engines this extravagance was not chargeable to it, since there was a large quantity of steam which was available, and would otherwise be wasted. A formula was given for calculating the power of a jet of steam as an agent for creating a draught, based upon experiments made by the author in 1851 and 1852. When applied to the blast pipe of the locomotive this showed that on an average the power required to force the air and gases through the fire grate and tubes was only about 8½ per cent of the potential power of the steam escaping through the blast pipe. In conclusion it was pointed out that a large increase of effect would be obtained by subdividing the exhaust steam into a number of small jets instead of relying upon one large one, and that under certain circumstances this increase of power would be of great utility.

Improved Helioscope.

*To the Editor of the Scientific American:*

Your number of March 16 contains, on page 163, under the name of Helioscope, the illustrated description of an instrument in which a polarizing contrivance is used to absorb part of the sunlight, so as to make it endurable to the eye during solar observations. Allow me to state that I used this method more than 30 years ago. As my apparatus is much simpler and more convenient it may be of public interest to give its details. At that time I did not suppose that there was much merit or novelty in it, and hence that I did not publish it before; but as it appears to be expected that the arrangement of Herr Metz, inconvenient as it looks to me, "will soon be one of the implements at every observatory and scientific academy," I enter my simple arrangement in competition. All are welcome to use it.

I take a piece of plate glass of the same width as the diameter of the objective, and about three times as long, ground at the back so as to destroy the exterior reflection of that surface, and coat it with black varnish. I attach this before the objective under an angle of 35° 25' with the axis of the telescope, so that the solar rays entering the instrument are polarized by the reflection of the polished surface. To the eyepiece I simply attach as analyzer a Nicol prism, and by turning the latter round its axis I reduce the intensity of the solar light to any degree desired.

I will add that I have adapted this arrangement to one of my large microscopes for the observation of sun spots, etc. For this purpose the polarizing reflector described is attached below the secondary stage, and in the latter a small long focus objective is placed, while the ordinary short focus microscopic objective is removed. The tube is elongated so as to make it correspond to the focal length of the objective, and a telescopic eyepiece with Nicol prism used as explained above.

Another arrangement, perhaps not so good, is to place the Nicol prism in the place of the ordinary microscopic objective, so as to avoid its use near or in the eyepiece. The course of the light is in any case this: First, reflection by polarizer under an angle of 35° 25', passage through the objective, passage through the Nicol prism (the analyzer) either before or after the image has been formed, and lastly, inspection of the image by the eyepiece.

I use also a tube bent at an angle of $2 \times (90° - 35° 25') = 109° 10'$; in the bend a piece of plate glass is placed under the proper angle, serving as an analyzer. This bent tube is placed under the eyepiece, and does away with the somewhat expensive Nicol prism, but it is not as convenient, as it does not allow the observer, when he turns it round, to keep his eye in the same position in the axis of the instrument.

P. H. VANDER WEYDE, M.D.

The Liquefaction of Gases.

M. Dumas has thought that the marvelous experiment of the liquefaction of hydrogen by M. Pictet, of Geneva, facilitated in exact manner the determination of the density of oxygen. It suffices in fact, to weigh the quantity of liquid obtained by M. Pictet in order to see what is the volume of this same quantity. Now M. Pictet, having obtained the considerable quantity, relatively, of forty-five grammes of liquid oxygen, and this liquid occupying in the tube a space of forty-five cubic centimeters, it is seen at once that the density of the liquefied oxygen is, like that of water, equal to unity. Theory had already established this quantity, but it is now confirmed by experience.

M. Dumas has also given a *résumé* of a second communication from M. Pictet, showing, without the possibility of doubt, that not only has oxygen been liquefied in his apparatus, but also solidified, which is the complete realization of the prophecy of Lavoisier, the renowned creator of modern chemistry. In fact, the jet of liquefied oxygen issuing from the tube, illuminated by the electric light, has been examined with the polariscope, and it has given indisputable signs of polarization. Now it is known that for this phenomenon to be produced it is necessary that the light should be reflected from solid isolated particles. In the liquid itself there are in suspension small crystals of oxygen "snow," as crystals of watery "snow" are seen in the middle of those white clouds known to meteorologists under the name of "cirrus."

Doubt is no longer possible that liquid or solid oxygen is really obtainable, it is, therefore, clearly evident that chemists may succeed quite easily in solidifying the atmospheric air, now that it has been liquefied; and thus will be realized the curious result of the transformation of a volume of air into a solid block.

The solidified hydrogen was preserved in this state for several minutes by M. Pictet, and produced in falling on the ground the sound of metallic grains. The liquid jet or stream had a steel blue color.—*W. Harrison, in British Journal of Photography.*

DR. HUGGINS has received a letter, dated January 15, from Mr. E. J. Stone, in which the Royal Astronomer at the Cape says, that, from an examination of the observations of the transit of Venus, he finds the solar parallax to be 8·88", or a distance as nearly as possible of 92,000,000 miles. This value agrees within 0·03" with that deduced by Mr. Stone from the observations of the transit in 1769.