them the centre of a large and admiring circle, and their residence was one of the most favourite gathering-places of the literary and scientific celebrities of Vienna.

BACTERIA[1]

IN a short paper communicated to the Royal Society at the close of last session, Prof. Tyndall did me the honour to criticise certain words reported to have been used by me at a meeting of the Association of Medical Officers of Health in January last. Although I am much indebted to him for the opportunity he has thus afforded me of discussing an important subject before this Society, I cannot refrain from expressing my regret that he should have thought it desirable to quote at length, and thus to place on permanent record in the Society's *Proceedings*, the expressions used on the occasion above mentioned. I regret this because these expressions occur in an abbreviated and incomplete abstract of a hastily prepared discourse not intended for publication.

As, however, I am well aware that Prof. Tyndall's purpose in his communication was not to criticise the language, but the erroneous views which the language appeared to him to contain, I shall make no further reference to the quotation; but shall regard it as the purpose of the present paper, first to reply to the reasoning embodied in his last communication, and secondly to corroborate certain statements previously made by me, to which he has taken exception in the more extended memoir published in the 166th volume of the *Philosophical Transactions*.

It will be my first object to enable the Fellows of the Royal Society to judge how far the views I entertain differ from those which have been enunciated here and elsewhere by Prof. Tyndall. Biologists are much indebted to him for the new and accurately observed facts with which he has enlarged the basis of our knowledge, as well as for the admirable methods of research with which he has made us acquainted. As regards the general bearing of these facts on the doctrine of Abiogenesis, I imagine that we are entirely agreed. So far as I can make out, the difference between us relates chiefly to two subjects, namely, the sense in which I have employed the words "germ" and "structure," and the extent of the knowledge at present possessed by physiologists as to the structure and attributes of the germinal particles of *Bacteria*.

Although Dr. Tyndall, in the title of his paper, refers to my "views of ferment," yet as he makes no further allusion to them, I will content myself with stating that in the passage quoted, the first sentence (from the words "In defining" to the word "living") has nothing to do with the following sentences, having been placed in the position which it occupies in the quotation by the abstractor. The paragraph ought to begin with the words "Ten years ago."

Of the meaning which attached itself to the word "germ" in the days of Panspermism a correct idea may be formed from the following passage from M. Pasteur's well-known memoir "Sur les Corps organisés qui existent dans l'Atmosphère." "There exist," says he, "in the air a variable number of corpuscles, of which the form and structure indicate that they are organised. Their dimensions increase from extremely small diameters to one-hundredth of a millim., 1·5 hundredth of a millim., or even more. Some are spherical, others ovoid. They have more or less marked contours. Many are translucent, but others are opaque, with granulations in their interior. . . . . I do not think it possible to affirm of one of these corpuscles that it is a spore, still less that it is the spore of a particular species of microphyte, or of another, that it is an egg or the egg of a particular microzoon. I confine myself to the declaration that the corpuscles are evidently organised; that they resemble in every respect the germs of the lower organisms, and differ from each other so much in volume and structure that they unquestionably belong to very numerous species." Such are the "germs" of M. Pasteur, and such is the conception of a germ which was entertained by informed persons up to 1870, and is very generally entertained up to the present moment.[1] It is obvious that these "corpuscules organisés" were, if they had any relation to *Bacteria*, not bacterium germs in Dr. Tyndall's sense, but "finished organisms," and yet it was of these that M. Pasteur said that it was "mathematically proved" that they were the originators of the organisms which are developed in albuminous liquids containing sugar, when exposed to the atmosphere.

With reference to the word "structure" I would point out that in the passage quoted from my lecture it is distinctly stated that the bacterial germ is endowed with structure in the molecular sense, but not in the anatomical sense. The meaning of the expression "anatomical structure" was, naturally, not defined, considering that the persons whom I was addressing might be supposed to be familiar with it. As, however, my failing to do so has apparently led to some uncertainty as to my meaning, I must, to avoid future misunderstandings, define more completely the difference between the two senses in which the word was used by me.

The anatomical sense of the word structure may be illustrated by referring to its synonyms, to the English words texture and tissue, to the Greek word ἱστίον, and to the German word *Gewebe*, from which two last the words in common use to designate the science of structure, viz., histology and *Gewebelehre* are made up. What I have asserted of the germinal particles of *Bacteria* is, that no evidence exists of their being endowed with that particular texture which forms the subject of the science of histology. In biological language there is a close relation between the words structure and organization, the one being an anatomical, the other a physiological term; either of these words signifies that an object to which it is applied consists of parts or structural elements, each of which is, or may be, an object of observation. As the observation is unaided or aided, the structure is said to be macroscopical or microscopical. The biologist cannot recognise ultra-microscopical structure or organisation except as matter of inference from observation, *i.e.*, from observing either that other organisms, which there is reason to regard as similar to the object in respect of which structure is inferred, actually possess visible structure, or that the object can be seen to possess structure at a later period of its existence. As instances in which the existence of structure is inferred the following may be mentioned:—The protoplasm of a Rhizopod is admitted to have structure because, although none can be seen in the protoplasm itself, the complicated form of the calcareous shell which the protoplasm makes or models can be seen. By analogy therefore other organisms which are allied to the Rhizopod are inferred to have structure, and from these, or from similar cases, the inference is extended to all kinds of cells, with respect to which it is taught by physiologists that although, in certain cases, no parts can be distinguished, the living material of which they consist is nevertheless endowed with structure or organisation. Similarly, we assume, that a *Bacterium* possesses a more complicated structure than we can actually observe, because in other organisms which are allied with it by form and life history, such complications can be seen. Again, in all embryonal organs we admit the existence of structure before it can be seen, because in the course of

[1] "Remarks on the Attributes of the Germinal Particles of *Bacteria*, in reply to Prof. Tyndall," by J. Burdon-Sanderson, M.D., LL.D., F.R.S. Paper read at the Royal Society, November 22.

[1] Before I became aware that the contaminating particles of water are ultra-microscopical I myself was engaged earnestly in hunting for germs both in water and air. The search has been continued by others up to a much later period. Those who desire information on the organised particles of the atmosphere will find the subject exhaustively treated by Dr. Douglas Cunningham in a report entitled "Microscopical Examinations of Air," lately issued by H.M. Indian Government.

development we observe its gradual emergence. So far, inference of the existence of structure from historical evidence is justifiable; but if we were to carry this inference back to the ovum itself, and say that the characteristic structures of nerve, of muscle, or of gland, exist in the ovum at the moment after impregnation, every physiologist would feel the assertion to be absurd.

In the familiar comparison of the origin of the elephant with that of the mouse, in which the perfect anatomical similarity of the ova in the two species is contrasted with the enormous difference of the result, we should be justified in saying that the difference of development is the expression of structural difference between the primordium of the one and the primordium of the other; but inasmuch as it is not possible to indicate any anatomical distinction, it is understood that structural difference of another kind is meant, namely, difference of molecular constitution. In other words, we assume that the potential difference between the one and the other is dependent on an actual difference of molecular structure. Whether this is accompanied with an anatomical difference, such as we might expect to be able to see if we had more perfect instruments, we do not know.

From the moment that it is understood that the word structure means anatomical structure, the argument used by Dr. Tyndall loses its relevance. After referring to the "germ limit," he says, "some of those particles" (by which, I presume, is meant atmospheric particles) "develop into globular *Bacteria*, some into rod-shaped *Bacteria*, some into long flexile filaments, some into impetuously moving organisms, and some into organisms without motion. One particle will emerge as a *Bacillus anthracis*, which produces deadly splenic fever; another will develop into a *Bacterium*, the spores of which are not to be microscopically distinguished from those of the former organism; and yet these undistinguishable spores are absolutely powerless to produce the disorder which *Bacillus anthracis* never fails to produce. It is not to be imagined that particles which, on development, emerge in organisms so different from each other, possess no structural differences. But if they possess structural differences they must possess the thing differentiated, viz., structure itself." Throughout this passage it is evident that it is not anatomical but molecular structure that is referred to.

In the other passages relating to the subject, I venture to think that Dr. Tyndall has overlooked the distinction made by me between anatomical organisation and molecular structure. When, for example, he speaks of "germ structure" in the passage quoted from his Liverpool Address, he evidently refers to molecular structure exclusively, for he gives ice as his first example, and argues that as ice possesses structure so do atmospheric germs—a proposition which I should not have thought of questioning.

The experimental evidence which we have before us goes to prove that in all the known cases in which *Bacteria* appear to originate *de novo*—that is to say in liquids which are at the moment of their origin absolutely free from living Bacteria—they really originate from "particles great or small," which particles are therefore germs in the sense in which that word is used by Prof. Tyndall. To illustrate the views I myself entertain, and always have entertained on this question, I need only refer to my paper on the origin of *Bacteria*, published in 1871. The experiments made by me at that time brought to light the then new fact, now become old by familiarity, that all exposed aqueous liquids, even when absolutely free from visible particles, and all moist surfaces, are contaminated and exhibit a power of communicating their contamination to other liquids. As regards water and aqueous liquids in general, I insisted on the "particulate" nature of the contaminating agent, and coined for the purpose the adjective I have just employed (which has been since adopted by other writers), at the same time pointing out that the particles in question were ultra-microscopical, and consequently that their existence was matter of inference as distinguished from direct observation. Dr. Tyndall has demonstrated by the experiments to which I have already alluded, that the ordinary air also contains germinal particles of ultra-microscopical minuteness. Of the completeness and conclusiveness of those experiments I have only to express the admiration which I, in common with all others whose studies have brought them into relation with the subject, entertain. That such particles exist there can be no question; but of their size, structural attributes, or mode of development, we know nothing.

Prof. Tyndall, I am sure by inadvertence, has accused me of assuming that there is some relation between the limit of microscopical visibility and what he calls the molecular limit, by which I presume to be meant the size of the largest molecule. Nothing that I have said or written could justify such a supposition. My contention is not that the particles in question are of any size which can be specified, but, on the contrary, that we are not in a position to form any conclusion as to their size, excepting that they are so small as to be beyond the reach of observation. Dr. Tyndall has taught us, first, that the optical effects observed when a beam of light passes through a particulate atmosphere are such as could only be produced by light-scattering particles of extreme minuteness; and, secondly, that by subsidence these particles disappear, and that the contaminating property of the atmosphere disappears with them. He has thus approximately determined for us the upper limit of magnitude, but leaves us uncertain as to the lower; for we have no evidence that the particles which render the atmosphere opalescent to the beam of the electric lamp may not be many times larger than those which render it germinative. Consequently, the fact that the air may be rendered sterile by subsidence, while affording the most conclusive proof that germinal matter is not gaseous, leaves us without information as to the size of the particles of which it consists.

Of each germinal particle, whether inhabiting an aqueous liquid or suspended in the atmosphere, it can be asserted that under conditions which occur so frequently that they may be spoken of as general (viz., moisture, a suitable temperature, and the presence of dead proteid matter, otherwise called organic impurity), it produces an organism. If, for the sake of clearness, we call the particle *a* and the organism to which it gives rise A, then what is known about the matter amounts to no more than this, that the existence of A was preceded by the existence of *a*. With respect to A we know, by direct observation, that it is an organic structure; but inasmuch as we know absolutely nothing as to the size and form of *a*, we cannot even state that it is transformed into A, much less can we say anything as to the process of transformation.

Considering that it is admitted on all hands that there exist in ordinary air particles which are potentially germs, it might at first sight appear needless to inquire whether or not this fact is to be regarded as carrying with it the admission that they must necessarily possess the other attributes of organised structure. Very little consideration, however, is requisite in order to become convinced that this question stands in relation with another of fundamental importance in biology—that, namely, of the molecular structure of living material.[1] It is not necessary for my present purpose to do more than to indicate the nature of this relation. As regards every form of living matter, it may be stated that, quite irrespectively of its morphological characteristics, which, as we have seen,

[1] The reader who is interested in this subject will find it discussed with great ingenuity by Prof. Pflüger, in his paper "Ueber die physiologische Verbrennung in den lebendigen Organismen," *Pflüger's Archiv*, vol. x. p. 300.

must be learnt by the application of the various methods of visual observation at our disposal, it possesses molecular structure peculiar to itself. We are certain of this, because the chemical processes of which life is made up are peculiar, that is, such as occur only in connection with living material. Even the simplest instance that we can mention, that of the elevation of dead albumin into living (a process which in the case now before us must represent the very earliest step in the climax of development) is at the present moment beyond the reach of investigation; for as yet we are only beginning to know something about the constitution of non-living proteids. But this want of knowledge of the nature of the difference between living and non-living material in no wise impairs the conviction which exists in our minds that the difference is one of molecular structure.

The sum of the preceding paragraphs may be stated in few words. Wherever those chemical processes go on, which we collectively designate as life, we are in the habit of assuming the existence of anatomical structure. The two things, however, although concomitant, are not the same; for while anatomical structure cannot come into existence without the simultaneous or antecedent existence of the kind of molecular structure which is peculiar to living material, the proof is at present wanting that the vital molecular structure may not precede the anatomical. At the same time it must be carefully borne in mind that there is no evidence of the contrary. It is sufficient for my purpose to have shown that the existence of organised particles endowed with anatomical structure in the "atmospheric dust" has not been proved. I do not dispute its probability.

Before leaving this subject I may be permitted to add a word as to the bearing of this discussion on a question which, to myself, is of special interest—that of *contagium vivum*. According to the view which these words are understood to express, the morbific material by which a contagious disease is communicated from a diseased to a healthy person consists of minute organisms, called "disease-germs." In order that any particle may be rightly termed a disease-germ two things must be proved concerning it, viz., first, that it is a living organism; secondly, that if it finds its way into the body of a healthy human being, or of an animal it will produce the disease of which it is the germ. Now there is only one disease affecting the higher animals in respect of which anything of this kind has been proved, and that is splenic fever of cattle. In other words, there is but one case in which the existence of a disease-germ has been established.

Comparing such a germ with the germinal particles we have been discussing, we see that there is but little analogy between them, for, first, the latter are not known to be organised; secondly, they have no power of producing disease; for it has been found by experiment that ordinary *Bacteria* may be introduced into the circulating blood of healthy animals in considerable quantities without producing any disturbance of health. So long as we ourselves are healthy, we have no reason to apprehend any danger from the morbific action of atmospheric dust, except in so far as it can be shown to have derived infectiveness from some particular source of miasma or contagium.

I now proceed to the second part of my communication, which relates to Prof. Tyndall's serious, but most courteously-expressed, criticisms of my experiments on spontaneous generation.[1]

The fact that Dr. Tyndall blames me for incautiously vouching for is, "that in boiled and hermetically-sealed flasks *Bacteria* sometimes appear in swarms." From multiplied experiments he concludes that this is not true, and infers that I who vouched for it was incautious. The paper referred to was one in which I, as a bystander, gave an account of certain experiments which Dr. Bastian performed in my presence. So far as relates to the fact above quoted, these experiments were, to my mind, absolutely conclusive; but inasmuch as I was unable to admit with Dr. Bastian that they afforded any proof of spontaneous generation, I followed them as soon as practicable by a series of experiments (NATURE, vol. viii. p. 141) (the only ones which I myself ever made on this subject), in which I tested the influence of two new conditions, viz., of prolonged exposure to the temperature of ebullition, and of exposure for short periods to temperatures above that of ebullition at ordinary pressure. The experiments accordingly consisted of two series, in the first of which a number of retorts or flasks charged with the turnip-cheese liquid, *i.e.* with neutralised infusion of turnip of the specific gravity 1017, to which a pinch of pounded cheese had been added, and sealed hermetically while boiling, were, after they had been so prepared, subjected to the temperature of ebullition for longer or shorter periods. In the second series the period of ebullition was the same in all cases, but the temperature was varied by varying the pressure at which ebullition took place.

The conclusion arrived at, as expressed in the final paragraph of the paper, was, that in the case of the turnip-cheese liquid, the proneness of the liquid to produce *Bacteria* can be diminished either by increasing the temperature employed to sterilise it, or if the ordinary temperature of ebullition be used, by prolonging its duration.

I did not think it necessary after 1873 to occupy myself further with the subject for two reasons, first, that I had accomplished my object, which was to show that as a ground for believing in spontaneous generation the turnip-cheese experiment was a failure; but secondly, and principally, because in the meantime the subject had been taken up by the most competent living observers, who had in every particular confirmed the accuracy of my results. I conclude this paper by referring shortly to some of these researches.

The first was made by P. Samuelson under the direction of Prof. Pflüger[1] in 1873. Its purpose was to ascertain whether it is true that certain liquids can be boiled for ten minutes without being sterilized, and secondly, to determine the influence of prolonged periods of exposure. The flasks employed were charged with the neutral turnip-cheese liquid, and sealed while boiling in the way already described. Some were subjected to the temperature of ebullition for ten minutes, the rest for an hour, the result being that whereas those heated for the longer periods remained without exception barren, an exposure of only ten minutes was followed, in the majority of cases, by an abundant development of *Bacteria*.[2] At about the same period a similar series of experiments was made under the direction of Prof. Hoppe-Seyler at Strasburg. The results were essentially the same.[3]

[1] The expressions referred to are the following:—"I have worked with infusions of precisely the same specific gravity as those employed by Dr. Bastian. This I was especially careful to do in relation to the experiments described and vouched for, I fear incautiously, by Dr. Burdon-Sanderson, in vol. vii. p. 180 of NATURE. It will there be seen that though failure attended some of his efforts, Dr. Bastian did satisfy Dr. Sanderson that in boiled and hermetically-sealed flasks *Bacteria* sometimes appear in swarms. With purely liquid infusions I have vainly sought to reproduce the evidence which convinced Dr. Sanderson. . . . . I am therefore compelled to conclude that Dr. Sanderson has lent the authority of his name to results whose antecedents he had not sufficiently examined." *Phil. Trans.*, vol. clxvi. p. 57. In the abstract of a lecture delivered at the Royal Institution, January 21, 1876, similar words occur, as also in a letter to NATURE, dated February 27, 1876, in which Dr. Tyndall, after remarking that the experiments of Dr. Bastian, witnessed by me, were too scanty and too little in harmony with each other to bear an inference, suggests that I should repeat them.

[1] "Ueber Abiogenesis," von Paul Samuelson aus Königsberg, *Pflüger's Archiv*, vol. viii. p. 277. The paper is designated as a report of experiments made "im Auftrag und unter der Leitung des Geh.-Rath Prof. Pflüger." I refer in the text only to those experiments which were virtually repetitions of my own. The research actually extended over a wider field.

[2] "Als Resultat dieser Versuchsreihe, ergab sich eine massenhafte Entwickelung von Bacterien in den meisten nur 10 Minuten lang gekochten Flüssigkeitsmengen nach 3–4 Tagen" (*loc. cit.* p. 283).

[3] "Ueber die Abiogenesis Huizinga's," von Felix Putzeys aus Lüttich (aus dem chemisch-physiologischen Laboratorium des Herrn Prof. Hoppe-Seyler), *Pflüger's Archiv*, vol. ix. p. 391. In a note appended by Prof. Hoppe-Seyler to this paper he states that he has recommended its publica-

During the next year the second question which I had attempted to solve, viz., the influence of temperatures above 100° C., was taken up with much greater completeness by Prof. Gscheidlen, of Breslau.[1] After a *résumé* of the proofs already given by his predecessors, that certain fluids are not sterilised by boiling; and, secondly, that as means of sterilising such liquids the action of prolonged exposure and that of increased temperature may be regarded as complementary to each other, he proceeds to relate his own researches, the purpose of which was rather to fill up defects in the evidence than to establish new conclusions.

The flasks employed were capable of containing 100 cub. centims. (three and a half oz.); they were charged in the usual way with the turnip-cheese liquid, and exposed for short periods in chloride of calcium baths, of which the strengths were carefully adjusted so as to obtain the requisite temperatures. It was thereby definitely proved that whereas the germinal matter of *Bacteria* can stand a temperature of 100° for five or ten minutes it is destroyed by temperatures varying from 105° to 110°.[2]

In an appendix to my first paper, published in NATURE in the autumn of 1873, I showed that the solution of diffusible proteids and carbo-hydrates employed by Prof. Huizinga, of Groningen, in the first of the valuable series of experiments[3] published by him, relating to the subject of spontaneous generation, require a temperature above that of ebullition under ordinary pressure to sterilise them. This observation has since been established by Prof. Huizinga himself on the basis of very carefully made experiments,[4] by which he has proved at the same time that the liquids in question are rendered completely incapable of producing *Bacteria* without extrinsic contamination by exposing them to higher temperature. The only points of difference between us, either as regards method or result, are, first, that the sterilisation limit (Grenze zur Bacterienerzeugung) fixed by me was too low—the true limit being 110° C.—and secondly, that the experiments from which I had inferred that the liquids in question had been sterilised at lower temperatures than this were, in Prof. Huizinga's opinion, rendered inconclusive by the fact that my flasks were sealed hermetically, whereas in his exchange of air was allowed to take place during the period of incubation, through a septum of porous porcelain. To this last objection I might perhaps have thought it my duty to answer, had it not been shown by the subsequent researches of Gscheidlen to have no bearing on the question at issue. As regards the limit of sterilisation I can entertain no doubt as to the accuracy of Huizinga's measurements, and am quite willing to accept 108° C. as the lowest temperature which could be safely employed under the conditions laid down by him.

It will be understood that in bringing these facts before the Society my only purpose is to show, as I trust I have done conclusively, that the statements which Dr. Tyndall in 1876 characterised as incautious, and which he virtually invited me to retract, had been two years before confirmed in every particular by experimenters of acknowledged competence.

tion notwithstanding that the results obtained were mere confirmations of those of former observers; adding "für den wissenschaftlichen Fortschritt hat nicht die Priorität des einen oder des anderen Beobachters, wohl aber die Zahl, Mannigfaltigkeit, und Zuverlässigkeit der Beobachtungen eine hohe Wichtigkeit."

1 "Ueber die Abiogenesis Huizinga's," von Richard Gscheidlen, *Pflüger's Archiv*, vol ix p. 163

2 "Es folgt aus den eben angegebenen Versuchen, nach meiner Meinung, dass in Huizinga's Gemengen die Bacterien einer Temperatur von 110° 5 10 Minuten lang zu widerstehen vermögen, nicht aber einer von 105°-110° in eingeschmolzenem Glasrohre während der nämlichen Zeit" (*loc. cit.* p. 167). Here the author clearly fails to make the necessary distinction between *Bacteria* (which, as is well known, lose their vitality at a much lower temperature) and the material out of which they spring. The mixtures referred to were either the cheese and turnip liquid or solutions containing peptones and grape sugar, to be immediately referred to. As affording an elegant demonstration that in the turnip-cheese liquid it is the cheese and not any other constituent which contains the resistant element, the following form of experiment is worthy of notice:—A tube A drawn out and closed at both ends is fused into the open mouth of a second tube B, of which the opposite end is drawn out and closed in a similar manner. In this way a compound tube is formed which is divided by a conical septum into two chambers A and B. A small knob of glass having been previously introduced into the chamber B, the septum can be easily broken by shaking the tube. With tubes so prepared two experiments are made. In Experiment 1, compartment A is charged with infusion of cheese, sealed, and then exposed to a temperature of 110° before it is united to the compartment B. In like manner B is charged with neutral decoction of turnip, so that when the compound tube is complete it contains cheese in one compartment, turnip in the other. If, after boiling for ten minutes, it is placed in the warm chamber its contents remain barren. In Experiment 2 the experiment is varied by simply omitting the preliminary heating of A. The compound tube is boiled as before, but now its contents promptly give evidence that the conditions are present for an abundant development of *Bacteria*.

3 Prof. Huizinga's papers on the Question of Abiogenesis are four in number. The references are as follows:—*Pflüger's Archiv*, vol. vii. p. 225, vol. viii. pp 180, 551; vol. x. p. 62.

4 The solution employed in these experiments was neutral, and contained, in addition to the requisite inorganic salts, 2 per cent. of grape sugar, 0·3 per cent. of soluble starch, 0·3 per cent. of peptones, and 1 per cent. of ammonic tartrate. As in my experiments, the flasks were heated in a Papin's pot, of which the temperature was 102° C. Even after half an hour's exposure to this temperature all the flasks became in two or three days "stark trübe und voll Bacterien," third paper, p. 555, January, 1874.

DIFFUSION FIGURES IN LIQUIDS[1]

IN making some experiments on the mixture of liquids entering into another liquid at the extremity of a tube of small diameter, a phenomenon presented itself which attracted my attention as both new and singular. A certain quantity of coloured alcohol, remaining in suspension in the centre of a body of water, assumed, by spreading gradually out, a form resembling that of a shrub having its trunk and its branches terminated by leaf-like expansions. I sought to reproduce the phenomenon, believing at first that this mode of diffusion was purely accidental; but the phenomenon always recurring very nearly in the same manner, I devised a mode of experimenting which enabled me to study it more advantageously.

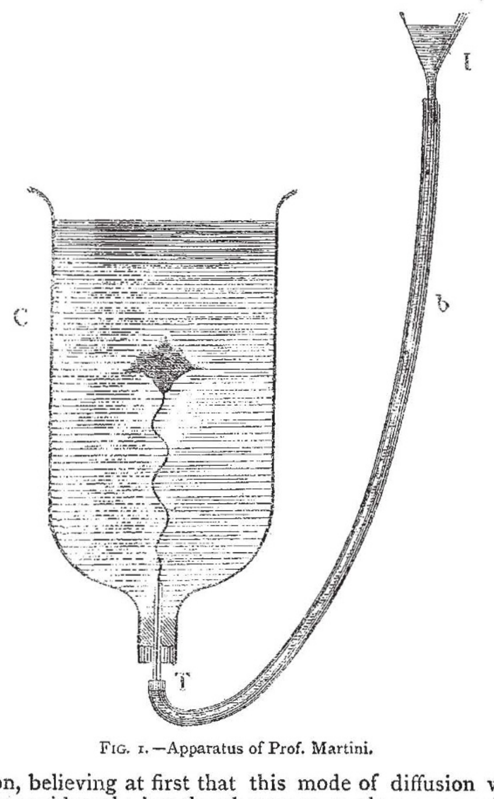

FIG. 1.—Apparatus of Prof. Martini.

C (Fig. 1) is a sort of cylindrical funnel of glass, to the neck of which is fitted a small capillary thermometrical tube T, about eight centimetres long. The capillary tube communicates by means of a caoutchouc tube *a b*, with a

1 From an article in *La Nature* by Prof. Tito Martini, of Venice.