I have entered upon these explanations concerning Schwann's experiments in deference to the readers of the JOURNAL, though the point is one of no great importance, and does not in the least affect the general question. Even if Schwann's results afforded no material evidence in my favour (and, of course, I never attached much importance to them), there are about twenty other experimenters of whom not a word is said in Professor Tyndall's letter. Why did he waste so much space upon this one point, and say nothing concerning the work of other investigators, and especially of the confirmation of my experiments by Dr. Burdon Sanderson? But I am wrong in saying he is silent on the subject. After giving his own very imperfect version of the support afforded to my statements by the experiments of Schwann, Professor Tyndall adds, "and it is on similar grounds that he quotes others". He thus disposes of them all by one broad assertion. This is an assertion more audacious than I should have expected even from Professor Tyndall.

With regard to this subject, I will subjoin a few quotations—some from those whom I cited in the JOURNAL of February 5th, as well as one from an investigator whom I did not mention.

In the list which I previously gave, the honoured name of the late Professor Jeffries Wyman was unintentionally omitted. I am glad, therefore, now to rectify this oversight. At the conclusion of an important memoir (*American Journal of Science*, vol. xlv, 1867), Professor Wyman said:—"Solutions of organic matter boiled for twenty-five minutes and exposed only to air which had passed through iron tubes heated to redness, became the seat of infusorial life. . . . Similar solutions contained in flasks hermetically sealed, and then immersed in boiling water for periods varying from a few minutes to four hours, also became the seat of infusorial life".

Professor Cantani, the present professor of natural philosophy in the University of Pavia, has made many experiments on the subject, both by himself, and, at various times, in concert with Professor Mantegazza, Professor Oehl, or with Professors Balsamo and Maggi. Amongst the numerous passages which I might quote from Professor Cantani, one will suffice to show the nature of the results obtained. In reference to experiments with solution of yolk of egg, made in concert with Professors Balsamo and Maggi, he says (*Gaz. Med. Ital. Lombard.*, Ser. VI, t. i, 1868):—"We began by observing that this solution enclosed with plenty of air in a flask, hermetically sealed, and heated from 105 deg. to 110 deg. C., produced a large number of vibrios in two days. We heated it, in different experiments, to 112 deg., 114 deg., 116 deg., and 117 deg., and always obtained the same result, if the temperature of the air was from 25 deg. to 70 deg." In heating the flasks above the boiling-point a Papin's digester was employed.

Dr. W. Roberts, of Manchester (*Phil. Trans.*, vol. clxiv, part 2, p. 463), says:—"March 27th, 1873. Seven plugged bulbs, charged with alkali-hay-infusion, were boiled in a can of water for fifty minutes. On March 31st, four days after, all were turbid and covered with a thick film of leptothrix filaments. . . . March 31st, 1873. Three plugged bulbs, charged with alkali-hay-infusion, were boiled in a can of water for two hours. On April 3rd, all three were turbid, and covered with an abundant film."

In my previous communication, I cited a passage from Dr. Burdon Sanderson's writings, in which he states that, by following my directions, he was assured that bacteria would appear abundantly in previously boiled and guarded fluids; and also another, in which he refers to the confirmation of these results in the laboratories of Pflüger and Hoppe-Seyler by S. Samuelson and Gscheidlen. In reference to the same experiments, Professor Huizinga, of Groningen, also says (*Nature*, March 20th, 1873):—"I fully agree with Professor Burdon Sanderson in the confirmation of Dr. Bastian's statements regarding the well-known turnip-and-cheese experiments." Professor Huizinga also found, as I have done, that other fluids will behave in a similar manner.

It does not specially concern me to discover why Professor Tyndall has failed to obtain like results. And, though I have offered some suggestions on this subject, it is not possible, in the light of the very imperfect details which he has published as yet, to discover, with any certainty, the precise causes of his failure. He will, doubtless, soon ascertain these for himself, and will, perhaps, simultaneously awake to the fact that many others, besides the experimenter whom he considers in error, have obtained living organisms in previously boiled fluids protected from atmospheric particles. Let us hope that, when this time arrives, the extent and gravity of Professor Tyndall's present misrepresentations will be made clear to his mind.

I have only to add, in conclusion, that Professor Tyndall has, in his last communication, brought forward nothing whatsoever to modify the view which I have already expressed in the JOURNAL concerning his recent work. Looking to his own statements and to the quotations I have cited, it must, in fact, be manifest to your readers

that he has completely misapprehended the present state of knowledge in reference to "spontaneous generation" and the germ-theory; that he has uniformly failed to obtain results, the possibility of which is now firmly established; and that, as regards the only question which is at present in dispute (as to whether Bacteria and their germs are rapidly killed in boiling organic solution) he unhesitatingly coincides with me.

A GERM-THEORY: REMARKS ON SOME OF DR. TYNDALL'S RECENT OBSERVATIONS.

By LIONEL S. BEALE, M.B., F.R.S.,
Physician to King's College Hospital.

IN a paper published in the BRITISH MEDICAL JOURNAL of February 12th, Dr. Tyndall, quoting from his letter to the *Times* (February 3rd, 1876), says that many of his infusions, prepared with "most scrupulous adherence" to Dr. Bastian's own prescription, have been exposed for many weeks to temperatures varying from 80 to 90 deg. Fahrenheit, and "have remained perfectly sterile throughout".

Dr. Tyndall, as on some former occasions, compliments the medical press, "with few exceptions"; and I venture, therefore, to suggest whether, without giving offence, the medical press might not request Dr. Tyndall to be so good as to state upon what evidence the above assertion that the infusions remained "perfectly sterile" is based. To my certain knowledge, a fluid that appears perfectly "clear" and "pellucid" may contain hundreds or thousands of living bacteria. I can find no conclusive evidence in Dr. Tyndall's paper to prove that bacteria did not exist in every one of his "one hundred and thirty-nine flasks". It is scarcely necessary to state that multitudes of bacteria may exist in a fluid without its falling into "rotteness" or being "muddy", or having "mould above and slime below".

Next, as Mr. Wanklyn has observed, it is very doubtful whether *all* the particles suspended in the air would fall to the bottom of the boxes in three days or in three years. The boxes are, in fact, "simply wooden filters".

I now appeal to the "press" and to the public, and beg for fair play for those who differ from Dr. Tyndall, whose mode of expressing himself in this and some other papers towards one or more who do not agree with him is so very peculiar that comparatively few dissentients are likely to express their views in print. Dr. Tyndall may, therefore, wrongly conclude that the press and all the world are with him in everything he says, while in point of fact he may be alone and almost without support as regards not a few of the opinions he has expressed.

In his last paper, Dr. Tyndall states that it is not his intention "to carry on a paper war with a writer of this vein"! No one desires that he should do so; but surely he might explain, if he be asked to explain, without speaking offensively, and indulging in such grand utterances as "comment is needless, and here for the present I must end". The discussion may surely go on even if Dr. Tyndall does end; but I do not think Dr. Tyndall will end, even for the present. He so often speaks as if his utterances were to be final, and yet vouchsafes to say a good deal more, and perhaps unsays something he has said, that we may hope he has more to tell us concerning the exact meaning he attaches to "sterile throughout", and the manner in which the sterility of the one hundred and thirty-nine flasks constituting his "cloud of witnesses" was determined.

It is, however, to be hoped that what Dr. Tyndall may think fit to say for the benefit of "medical men" and others may not be embellished with such euphemisms as adorn a paper by him in the *Fortnightly Review* for November 1st, 1875, as expressions of the kind are not usual in the "medical press", and are not calculated to carry conviction to the minds of medical men. From amongst many, I select the following examples: "drivellers", "roaring lions of Belfast", "noisy and unreasonable assailants", "anger rising in weaker heads", "ignorant and passionate", "bands of Jesuits", "angry pulpits", "mostly fools", "the nobler Bereans of the press and pulpit who have honoured me with their attention". If, in the discussion of questions of great interest, this mode of argument should become at all general, there will soon be an end of scientific discussion. This end, no doubt, is desired by some of the brethren who call themselves "strong"; but terrorism is likely to be detrimental to science, though it may ensure the survival of the strongest. Let workers and thinkers on different sides be heard upon these matters; and it is to be hoped that the day is not very distant when those interested in the Royal Institution will admit that if it be desirable that such subjects as spontaneous generation, disease-germs, and some others, should be brought under the notice of a general audience, it is but mere justice that those whose investigations have

led them to conclusions different from those taught by their lecturer should be also heard, instead of being ignored, or perhaps contemptuously spoken of, and practically debarred from telling the very different story the same "nature" has taught them.

Dr. Tyndall concludes his paper with the information that, if it were thought desirable, he would willingly make arrangements to enable "medical men" to see his "infusions protected and unprotected", though the apparatus is now in disarray. The "medical men" who have studied Dr. Tyndall's lecture probably do not want "to contrast the hyaline purity of the protected infusions" with the "muddy turbidity of the unprotected"; but not a few having a knowledge of the results of previous investigations would like to be informed concerning the exact method or methods pursued by Dr. Tyndall, which enabled him to so confidently conclude that not one of the hundred and thirty-nine flasks contained bacteria, and that "there was neither mistake nor ambiguity about the result". There are, unfortunately, some scientific persons, and probably not a few "medical men", who are not quite convinced that a few hundred or thousand bacteria suspended in a *clear* fluid or atmosphere would be certainly made manifest by the "searching beam".

A COMMENTARY ON DR. PATRICK BLACK'S THEORY OF ASPHYXIA.

By GEORGE JOHNSON, M.D., F.R.S.,

Professor of Medicine in King's College; Physician to King's College Hospital.

DR. BLACK has done me the favour to send me a copy of his recently published lecture on the Physiology of Respiration;* and, having carefully read the pamphlet, I venture to take up my pen in defence of the commonly received theory of asphyxia, which is assailed by Dr. Black. He tells us in his preface that "the doctrine of asphyxia, as laid down by the highest authorities, cannot, he believes, be maintained; and, as this subject is very closely connected with the heart's physiology—so closely, indeed, that his reasoning on each of these subjects must stand or fall together—it is necessary to give such an account of the latter as shall be in perfect harmony with all the phenomena of the former". He goes on to say that "the heart's physiology has been in some important points entirely misapprehended", and he again brings before the public the views to which he gave utterance more than twenty years ago in the Croonian Lectures delivered by him at the Royal College of Physicians. He now "challenges the verdict of philosophic minds on these, and respectfully asks that they may be considered".

Dr. Black's doctrine is, that "the primary end" of respiration is not the replacement of the carbonic acid of the blood by oxygen, but that "the movement of respiration is absolutely necessary to the maintenance of life, for, without this movement, the blood is obstructed in its progress through the lungs. Arrest the movement, and you have immediate asphyxia; allow the movement, even if the air admitted contains no free oxygen, and is, therefore, wholly unfitted to vivify or arterialise the blood, and the circulation will go on for a considerable time without let or hindrance". Professor Huxley, amongst other physiologists, he says, "has fallen into the error of referring asphyxia to the double cause of privation of oxygen and excessive accumulation of carbonic acid in the blood". In a subsequent part of his lecture, Dr. Black repeats the statement that, while "the movements of the chest are absolutely necessary to enable the blood to pass through the lungs, this passage of the blood is entirely independent of the chemical changes which are in a manner incidental to the movement". (P. 32.) He goes on to explain the final cause of the relative weakness of the right side of the heart. "Nature (*i.e.*, the Author of Nature), in placing this barrier to the pulmonary stream, has by this very act ensured the blood's aëration. For, had she endowed the right heart with sufficient power to sweep the hindrance before it, instead of requiring that it should be lifted away by the act of inspiration, the necessity for this act would not have been felt; medulla's function would not be invoked; the blood would pass through the lungs without her sanction; it would not put on its red garment, for it would pass unchanged. But Nature has so weakened the heart, that the barrier is effectual until another power is called in; and, as every inspiration lifts off this barrier, so the circulation through the lungs is continued, and the blood's changes are duly accomplished." (P. 33.)

I have given Dr. Black's theory as much as possible in his own words, and he will doubtless admit that I have fairly stated his doctrine.

* *Respiration; or, why do we Breathe?* A Lecture delivered before a popular audience. By Patrick Black, M.D., Physician to St. Bartholomew's and Christ's Hospitals. London: 1876. 8vo. Pp. 36.

In reply to Dr. Black, I commend to his notice Dr. John Reid's well known essay *On the Order of Succession in which the Vital Functions are arrested in Asphyxia*. He will there find that the old Hallerian doctrine, which attributed the arrest of the circulation through the lungs to the cessation of the mechanical movements of the chest, is completely disproved by very simple and conclusive experiments. I give the following in Dr. Reid's own words.

"A tube with a stop-cock was fixed into an opening in the trachea (of a dog), and one of Poiseuille's hæmadynamometers was introduced into the femoral artery, for the purpose of obtaining definite information upon the force with which the blood was transmitted along the arterial system. The stop-cock of the tube in the trachea was then shut; and, when the respiratory process had been suspended sufficiently long to cause a decided fall in the column of mercury supported by the blood sent along the femoral artery, a large bladder full of pure nitrogen, with a brass nozzle provided with a stop-cock, was fixed in the tube in the trachea, which it fitted accurately; and both stop-cocks were then opened. After the effects of the respiration of nitrogen gas had been ascertained, a bladder of the same size as the other, similarly provided with a nozzle, and full of atmospheric air, was then substituted for the bladder containing the nitrogen, and the results compared. The difference between the effects of the respiration of the nitrogen gas and the atmospheric air was most marked, and of such a nature as could not be mistaken; for, while the mercury continued to fall in the instrument during the respiration of the nitrogen gas, it rose very rapidly immediately after the atmospheric air had entered the lungs and acted upon the blood. In this experiment, the same mechanical movements of the chest, which failed to renew the free circulation of the blood through the lungs when nitrogen gas was inspired, rapidly effected that object when atmospheric air was permitted to enter the lungs, even when tried on the same animal subsequently to the failure of the nitrogen, and consequently at a more advanced stage of the process of asphyxia. This experiment was repeated several times, and, when the requisite care was taken to procure and employ pure nitrogen (unmixed with atmospheric air), invariably with the same result."

Surely Dr. Black must see that the results of this experiment are utterly irreconcilable with his statement that the circulation through the lungs is entirely dependent on the respiratory movements, and in no degree dependent on the chemical changes effected between the blood and the respired air. If the arrested pulmonary circulation were, as Dr. Black believes, a mechanical result of the suspension of the respiratory movements, the arrest of the pulmonary circulation should follow immediately upon the suspension of those movements. But this is not so. Dr. Reid, in the essay before referred to, clearly demonstrates that the first impediment to the circulation consequent on the exclusion of air from the lungs occurs, *not in the pulmonary, but in the systemic vessels*. For a period of about two minutes after shutting off the air, black blood continues to pass freely through the lungs into the left side of the heart; and a dynamometer in the femoral artery shows a continuous increase of pressure, in consequence, as Dr. Reid says, of "an impediment to the passage of the venous blood through the capillaries of the systemic circulation". After about two minutes, the pressure in the artery rapidly falls, in consequence of the impeded circulation through the lungs, and the resulting comparative emptiness of the left side of the heart.

Dr. Reid's observations on the blood-pressure were somewhat interfered with by the struggling of the animal; but, when the experiment is done upon a dog under the paralyzing influence of curare, as I have seen it done by the present distinguished Professor of the Institutes of Medicine in the University of Edinburgh, the results are in strict accordance with Dr. Reid's statement that the first effect of suspended respiration is to cause an impeded systemic circulation, while the impediment and the final arrest of the pulmonary circulation occur later. The most probable explanation of the impeded circulation through both systems of vessels is that which attributes it to the contraction of the muscular arterioles, excited by the influence of the abnormal blood upon the vaso-motor nerves and centre. (See my paper on Certain Physical Phenomena connected with the Circulation, etc., *BRIT. MED. JOURNAL*, Jan. 8th, 1876, p. 44.)

Dr. Black makes the surprising statement that the two ventricles of the heart discharge "their contents through orifices of exactly the same diameter". I can imagine Dr. Herbert Davies' dismay at seeing such an assertion, after his ingenious demonstration of the mathematical relations of the areas of the four orifices of the heart. The relatively greater thickness of the walls of the left ventricle is required, not merely, as Dr. Black states, to overcome the greater impediments which exist in the terminal systemic vessels, but, the orifice of the aorta being narrower than that of the pulmonary artery, a more vigorous left