1. If a phthisical patient in any stage can take a fair amount of exercise without contracting fresh lung-irritation from exposure to meteorological changes, and if he have a good appetite, with diminishing cough and increasing weight, I should unhesitatingly advise him to remain at home, and trust to treatment and to the food of the best dietary in the world—that of Old England. If, however, he cannot take exercise without constantly catching cold; and if, when confined to the house, he lose appetite, become low-spirited, pine for fresh air, sunshine and change of scene; and, above all, if the confinement act unfavourably on the digestive organs, causing bilious derangements and disinclination to the cod-liver oil, equally unhesitatingly I advise a trial of another climate, if the strength permit. There are a number of consumptives who profit more by food than climate, and never seem to thrive except on British fare. These had better stop at home, and try the sunshine of the south coast.

All cases deemed *advanced*, whether because of extensive tubercular consolidation of both lungs, or because of considerable excavation of one or of both lungs, and cases of active tubercular disease; or, again, those manifesting great irritation of the pulmonary or of the gastric mucous membrane, had better remain in England, as change of climate will not yield them a sufficient return for the alteration of food, life, and habits, as well as the risk of the journey.

Nevertheless, these are often the patients who are most anxious to winter abroad; and even when at death's door, they will not give up their hope that in another and a sunnier clime new life may be vouchsafed to them.

On the other hand, first-stagers, and even where the consolidation is extensive, if the consumptive disease arise from inflammatory and catarrhal attacks, and if it be confined to one lung, do well to leave England; and we may also advise the same to third-stage cases, where the vomica is small and quiescent.

2. As regards the second point, be it remembered that a sea-voyage is a long and trying experiment, to which the invalid has to submit when once he embarks, whether it suit him or not, for several months. In the trip to Australia, there are few halting places, and a patient may die before reaching them. Then, again, the element of sea-sickness must be borne in mind, though, fortunately, consumptives appear to suffer less than other people; and there is also the chance of the invalid being confined to his berth in case of severe attacks. All these considered, and remembering the character of our patients who profited by the voyages, we must be very cautious in recommending their use, and as much consultation and deliberation should precede a decision as is given by surgeons to a life-or-death operation.

The cases which I have seen do best are, first, cases of hæmorrhagic phthisis; second, cases of limited consolidation with no pyrexia, occurring in young men overworked at in-door occupations, and who have suffered from the septic influences of life in great cities, such as clerks, shopmen, secretaries, and the like. This form of treatment is far better suited for men than for women.

3. As to the third query, as to what class of patients profit most by dry climates, it has been shown that, taking collectively all forms and degrees of phthisis, the dry climates are the most likely to arrest the disease; and also that a dry and moderately warm climate, like that of Southern Europe, is most successful in the treatment of consumption of inflammatory origin. The question whether a cold dry, or a warm dry atmosphere is the best for ordinary chronic phthisis, depends to a great extent on the individual's power of maintaining circulation and temperature. When these suffice, the cold climates are preferable; but in the majority, and especially for women, whose circulation is weaker, the warm and dry are the best, for they are thus enabled to live more in the open air. Elevation is of some importance; and I should always choose a mild climate with elevation than one without it. Mountain-air is not beneficial solely on account of its purity, for on this point sea and desert air may vie with it; there is another factor in the low barometric pressure and atmospheric rarefaction, and the expansion of the lungs thereby caused may be of great value in chronic first-stage cases. At present, the trial of mountain climates must depend on the supply of suitable accommodation and food for invalids. If in the Andes sanitaria these articles were of a nature fit to offer to our comfort-loving British consumptives, I would not hesitate, after the evidence of Archibald Smith, Walshe, and others, to recommend them, as some can also boast of a warm winter temperature; but, alas! those who repair thither at present, must be content with Spanish habits, Spanish food, and an unsettled government. The Alpine elevated sanitaria do not, according to my experience, supply in winter sufficiently good food for British consumptives; and, although they attract crowds of Germans and Swiss, they must not expect our countrymen in equal numbers until they feed them properly.

4. As to the desirability of moist climates for consumptive pa-

tients, the evidence is decidedly against their use in the treatment of ordinary chronic phthisis. The addition of warmth only makes the damp tell more unfavourably, though a strong saline element and invigorating breezes do something to counteract the humid influence; still even these do not place a moist climate on the same level as a dry one. There is one exception, however. Phthisis, of catarrhal origin, has been shown to profit most by a warm and equable climate, even though accompanied by a certain amount of moisture, as the evidence of Madeira witnesses.

Finally, in all climate questions, full note must be taken of the patient's inclinations, means, and, above all, of his disposition and temperament; and exile must not be decreed to those who are incapable of making themselves happy under the changed conditions of life, or all our scientific grounds for a climate decision may collapse like a house of cards.

Climate is only one portion of the system of attack which we organise against the dread foe which decimates our population, and would be worth little if not combined with medicine and hygiene, and a determined will to wrestle bravely against the home-thrusting enemy.

A REJOINDER TO DR. TYNDALL'S REPLY ON THE DEVELOPMENT OF GERMS IN INFUSIONS.

By H. CHARLTON BASTIAN, M.A., M.D., F.R.S.,
Professor of Pathological Anatomy in University College; Physician to University
College Hospital; and Physician to the Hospital
for the Paralysed and Epileptic.

PROFESSOR TYNDALL'S article in the JOURNAL for February 12th contains certain misrepresentations of too serious a nature to be allowed to pass without correction.

I cited a number of investigators who had obtained evidences of fermentation or putrefaction in previously boiled organic fluids exposed to purified air. It is the occurrence of this fact which Professor Tyndall, from the basis of his own experiments, denies. Now, amongst the authorities whom I cited, there were some who regarded the appearance of bacteria under the conditions specified as evidence in favour of so-called "spontaneous generation", whilst others of them explained such results on grounds which are as various as they are mutually contradictory. To enter into the reasons or interpretations would have required too much space—it was moreover unnecessary, as this had been fully done elsewhere. It was the fact itself, of the occurrence of living organisms under these conditions, which had been denied by Professor Tyndall.

In citing Schwann, I did not for one moment suppose it was not known that the interpretation he had given of his experiments was adverse to the occurrence of a "generatio æquivoca". The readers of the JOURNAL were, therefore, perhaps not so deeply moved as Professor Tyndall represents himself to have been by the discovery that Schwann did not attach very much importance to the experiments in which he obtained positive results. In his excitement on this subject, moreover, Professor Tyndall omitted to mention one important point; viz., that the positive results alluded to were obtained with a different kind of fluid from that which Schwann had previously employed in the experiments, the details of which are given by Professor Tyndall. In these latter experiments, Schwann worked with an infusion of meat, and his results were, truly enough, uniformly negative in character. But in operating with a different fluid—viz., cane-sugar mixed with beer-yeast—the results became contradictory, being sometimes negative and sometimes positive. Although Schwann fancied that the positive results referred to might be easily explained (and, be it observed, this is a matter of interpretation), he nevertheless, like a good experimenter, did not regard the matter as wholly settled. The very note from which Professor Tyndall quoted (*loc. cit.*, p. 188) ends with the following reference to the question of the interpretation of the above mentioned experiments. Schwann says: "The matter can, however, be decided by another more certain method." In further support of my position that these positive results were not accounted for by Schwann in a manner which is free from doubt, I need only quote from one who is opposed to me on the question of the interpretation of such experiments generally. In the historical summary with which he prefaced his celebrated memoir of 1862, M. Pasteur says (*loc. cit.*, p. 15), in reference to the experiments of Schwann, that, though his experience in relation to the putrefaction of meat-infusion was "très-nette", as regards the alcoholic fermentation, "les expériences du savant physiologiste étaient contradictoires".

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