one of these may be mentioned the climate of the tract of country extending from Toulon to Spezzia, and sheltered more or less completely from the north by the various ranges of the maritime Alps. The parts lying between Nice and Spezzia are called the Eastern and Western Riviera, though the term is sometimes applied to the whole district. Within it are the well known sanitarium, Hyères, Cannes, Nice, Mentone, Bordighera, San Remo, Nervi.

Its southern exposure and the protection from northerly winds give it a warmer mean temperature in winter than many parts of the adjacent Italian peninsula. The winter mean varies from 47 deg. Fahr. to 49 deg. Fahr.; the rainfall is about 25 inches, and the number of rainy days from 45 to 80. The climate of the whole region is dry, as hygrometrical observations show, and there is abundant sunshine throughout the winter, enabling patients to live much in the open air. But, in spite of the northern screen, this region is subject to occasional visitations of cold weather, of which we often read complaints in the newspapers; and every winter brings round its series of grumbling letters from invalids who have left their homes in search of summer, and who find, after all, but a modified winter, bright and cheerful, it is true, but still necessitating winter clothing and warm wraps. And herein lies their mistake; for if our invalids could indeed find a lotus-aeter's land,

"In which it seemed always afternoon,
All round the coast the languid air did swoon;"

I would predict that the results on their health would be rather pernicious than otherwise, and loss of appetite and diarrhoea would probably be induced.

The real advantage of the Riviera climate is the combination of the bracing element with dryness, sunshine, and moderate warmth; and this first quality can hardly be supplied without a certain number of days of mistral, or a certain percentage of cold weather. This is as necessary to the maintenance of the appetite as the warm air is beneficial to the cough and other local symptoms.

Many opinions have been expressed as to the relative advantages of the Mediterranean health-stations, and it is amusing to hear the patrons of each vaunt it and decry the others; but the fact is, that there is not such a very great difference between any of them. Mentone and San Remo are probably the most sheltered from north winds; but they are consequently the least bracing, while Cannes and parts of Nice are the most so. The real distinction which should be taken into consideration is, first, whether a place be situated on the sea, or lie at a distance from it; and, again, whether the protecting ranges are close to or at a distance from the town; for, while the sea-shore ensures greater equability of temperature, it also ensures a large amount of stimulating influence, sufficient at times to preclude sleep, and to excite unduly the nervous system; on the other hand, a few miles inland means greater extremes of temperature, although generally sounder sleep and more repose of the nervous system may be reckoned on. Hyères, Cannel, and Cimiez belong to the less stimulating class, and Mentone, Cannes and Nice to the more stimulating. Our 82 patients who tried the Riviera climate give as results 58½ per cent. of improved, 20¼ per cent. stationary, and 20¼ per cent. worse: a good result, but not so good as that of the whole 152 wintering in the South of Europe. Still, it must be remembered that the difference is made up by the favourable results given by those patients who were able to travel from place to place, and, therefore, were probably not so advanced in disease as those limited by their doctors' orders or their own wishes to one health-station.

Besides the Riviera patients, 8 spent 10 winters at Malaga, and, though a small number, returned excellent results, the whole of them becoming better or remaining *in statu quo*, and testifying to the benefits of that beautiful climate, which, if it were not for the unsanitary state of the town, would probably eclipse the Riviera in the estimation of invalids, from its greater dryness. The winter mean temperature is 56 deg. Fahr.; the rainfall is only 16½; the number of rainy days is 40.

A few of the patients spent winters in some of the Mediterranean islands—Sicily, Cyprus, Malta, Corsica, and Corfu—which all appear to have a much warmer climate than the Riviera, but are open to winds and storms. Algiers is placed on the Mediterranean, but local reasons give it a somewhat different climate from the other sanitarium. It is moister and warmer, and this comes about from the Atlas Mountains attracting a large annual rainfall. Looking at the dryness of the south-eastern corner of Spain, it always appears as if Algeria, in the battle of the elements, had secured to itself the rainfall lost by Spain, as the contrast between the almost arid eastern coast of Spain and the well watered provinces of Constantine and Algiers is very striking. The mean winter temperature, according to Dr. de Pietra Santa, is 56 deg. Fahr., the amount of rainfall 32 inches, and the number of

rainy days 87. The prevailing winds are the west, north-west, and east (from the Mediterranean quarter); of these the westerly bring moisture from the Atlantic. The chief drawback to the climate is the fact that the rainfall takes place principally in the winter, the summer and autumn being the dry seasons.

Since making out these statistics, I have come across my notes of a young lady of strong consumptive predisposition, who went to Algiers three years ago for the winter, with a considerable sized cavity and great emaciation. She has passed three winters there, taking cod-liver oil, and living in a villa in Mustapha Supérieur, with the greatest possible benefit. I have examined her each year on her return home, and have watched a gradual improvement in general and local signs; and last spring, to her and my great satisfaction, after examination of the lungs, I failed to detect any trace of the cavity. She has become very stout, lost her cough, and is wintering this year in England fairly well. Before we quit this part of Africa, I may allude to Tangiers, where three of the patients spent eleven winters with great benefit. There some of the Atlantic influence is felt in the form of cool westerly breezes; but, at the same time, this does not appear to increase the rainfall or the number of rainy days, neither of which is large. Recently, a well known Fellow of this Society has called attention to Mogador, on the Atlantic coast of Morocco, as a fitting resort for consumptives, and has published some good tables illustrative of the climate; but, as this lecture does not aspire to give accounts of all good climates for consumption, but merely analyses those of which I have practical results, I cannot now dwell more fully on this and other climates, which I hope may each in turn have a trial, and be found in certain cases to be of especial benefit.

[To be continued.]

REPLY TO DR. CHARLTON BASTIAN'S REMARKS ON THE DEVELOPMENT OF GERMS IN INFUSIONS.

BY JOHN TYNDALL, F.R.S.,
Professor of Natural Philosophy in the Royal Institution.

I HAVE been favoured with a copy of the BRITISH MEDICAL JOURNAL for February 5th, and have read therein, with interest, the article of Dr. Bastian, in which, to his own obvious satisfaction, he convicts me of insufficiency of knowledge, insobriety of reasoning, and inattention to facts. He urges, in the first place, against me, a striking departure from the conditions laid down by him as necessary to the success of his experiments. I have sought, he says, to obtain, with a temperature of 60 or 70 deg., results which could only reveal themselves at a temperature of 90, 100, or 115 deg. If, then, failure attended my efforts, my own carelessness is to blame. Having erred so seriously where my instructions were so plain, what confidence, asks Dr. Bastian, is to be placed in my mode of preparing those infusions that refuse to verify the statements which he has so ardently enunciated, and which others have so trustingly believed?

In briefer terms, Dr. Bastian produced the same criticism in the *Times* of January 29th; and I cannot, perhaps, do better than publish here the reply which I thought it expedient for the time being to make. It runs thus:

"To the Editor of the *Times*."

"Sir,—Unwilling to cumber your space with a discussion which, for the present, at least, can be carried on more fully and decisively elsewhere, I refrained from answering Dr. Bastian's letter published in the *Times* of the 28th of January.

"A considerable portion of your readers, however, feel an interest in the question of warmth there raised, and I therefore hope you will grant me room to say that Dr. Bastian's temperature hypothesis will not answer. Many of my infusions, prepared with the most scrupulous adherence to his own prescription, have been exposed for weeks to temperatures varying from 80 deg. to 90 deg. Fahrenheit, and have remained perfectly sterile throughout. A temperature 20 deg. lower than these rapidly develops putrefaction in the common air.

"This subject was taken up by me with a full consciousness of its gravity. Upon its right solution depends the practice of the physician, and, therefore, the lives of men. Knowing this, I have spared neither care nor toil in examining the facts and doctrines which Dr. Bastian has of late years so successfully spread abroad. The question of temperature is a case in point. To obtain sufficient mastery over it, I had a special supply of gas brought in from the main, and entirely cut off from the interference of servants. I was thus enabled to keep four

gas stoves, with their heated iron flues passing through the air, burning night and day. These, in addition to a hot-water pipe which surrounded the laboratory, placed the temperatures mentioned above at my disposition. Of still higher temperatures than these more anon.

"With regard to the authorities cited by Dr. Bastian, and his right to cite them under the circumstances, the proper word shall be spoken at the proper time. Suffice it here to affirm the absence of all warrant for the assertion that in reference to the questions treated and the errors demonstrated in my paper, there is the slightest discord between the most eminent of those authorities and myself.—I am, sir, your obedient servant,

"JOHN TYNDALL.

"Royal Institution, February 3rd."

In the letter to which this is a reply, Dr. Bastian exhorts me, as aforesaid, to employ, as he has done, temperatures of "90 and 100 deg., or even 115 deg. Fahrenheit". These are his words; but he omitted to tell the *Times* that the temperature which actually sufficed for his "generations", and which was that employed by me, "mostly ranged between 75 and 86 deg. Fahrenheit, though it occasionally has been even higher than this". This is his prescription, taken *verbatim* from page 354 of the first volume of his *Beginnings of Life*. I submit, therefore, that the suppression of the temperatures laid down in his book, and the substitution for them of "recent" and unpublished ones in the *Times*, is a needless exacerbation of the difference between Dr. Bastian and me.

In the communication recently made to the Royal Society, the following statement is quoted from Dr. Bastian's latest book. "We know that boiled turnip- or hay-infusions, exposed to ordinary air, exposed to filtered air, to calcined air, or shut off altogether from contact with air, are more or less prone to swarm with bacteria and vibrations in the course of from two to six days." I went to considerable trouble in proving to Dr. Bastian that each of these assertions is the exact reverse of the truth. His reply is characteristically frank. He had already disposed of M. Pasteur, whose labours in connection with this subject may be fitly described as immortal, by labelling him "a mere experimenter"; and now he tells me that my failure to verify him is due "to my own lack of learning and want of attention to prescribed conditions".

Again, before the Pathological Society on the 6th of April last, Dr. Bastian circumstantially stated to his professional hearers how they were to proceed in order to satisfy themselves of the truth of the doctrine of "spontaneous generation". "Carefully prepare," he says, "an infusion from some animal tissue—be it muscle, kidney, or liver. Place it in a flask whose neck is drawn out and narrowed in a blow-pipe flame. Boil the fluid, seal the vessel during ebullition, and, keeping it in a warm place, await the result. After a variable time, the previously heated fluid, within the hermetically sealed flask, swarms more or less plentifully with bacteria and allied organisms."

With a discipline of six-and-twenty years in experimental inquiries of no easy kind, I thought it not beyond me to follow the directions thus given to the members of the Pathological Society, young and old. I accordingly took the trouble of preparing infusions of fish, flesh, and fowl, of the most varied kinds; also of kidney, liver, and other viscera. On January 13th, I had the honour of submitting one hundred and thirty-nine of these infusions to a large assembly of the Fellows of the Royal Society. Every infusion of the group negated the foregoing statement. The Fellows of the Royal Society, embracing, I may say, some of its most eminent names, regarded the result in a serious light. Not so Dr. Bastian. He describes it as "truly laughable"; and adds that "it seems far more like burlesque than the sober pursuit of science".

It is not my intention to carry on a paper-war with a writer of this vein; but I may be allowed to adduce a single specimen of the morality with which he treats one of the gravest practical questions that ever came before the high court of science. In his last communication, he professes to supplement my lack of learning by introducing to the readers of the two foremost medical journals of England a list of authorities which corroborate his views and refute mine. That list is headed by the celebrated memoir of Schwann, published in Poggendorff's *Annalen* for 1837.

His dealings with this memoir I will take as my specimen. A pretty distinct recollection of what Schwann had written caused me to see with surprise and suspicion his name thus cited. But recollection sometimes fails to recall the truth; and I therefore checked my memory by carefully reading Schwann's paper. So incredible did it appear to me that Dr. Bastian could have meant to cite it in support of his results, that I turned to the index of the volume to ascertain whether some other of its articles would not give a show of meaning to the reference. There was, however, no other paper from Schwann to be found either in this volume or in the two other volumes published in the year referred to. Hence it is certain that the paper cited by Dr. Bastian in his own favour is that which begins at page 184 of Poggendorff's

Annalen, vol. 41, 1837. It also is the paper which, despite my lack of learning, I had pretty distinctly borne in mind.

Schwann heads this brief but pregnant memoir by the title "Preliminary Communication regarding Experiments on Vinous Fermentation and Putrefaction". The subject is thus introduced. "At the last meeting of naturalists in Jena, I communicated experiments on spontaneous generation, by which it was proved that, when a closed glass globe containing a small quantity of an infusion of muscle, and filled with air, is exposed to the temperature of boiling water, so that both the liquid and the air are heated to 80 deg. Réaumur, then, even after a period of several months, no infusoria are generated, and no putrefaction occurs." The sole purport of the paper in Poggendorff's *Annalen* for 1837 is the further following out and confirmation of the result thus distinctly enunciated.

With the scrupulous accuracy of a true experimenter, Schwann describes his methods and precautions. He placed flesh in a flask filled to one-third of its capacity with water, stopped the flask securely with a cork, through which passed two thin glass tubes. Outside the flask, these two tubes were carried through fusible metal, raised nearly to the temperature of boiling mercury. The free end of one of the heated tubes was connected with a gasometer, and then the liquid in the flask was boiled until the air was driven away through the other tube with the escaping steam. For several weeks without intermission, a current of air was sent from the gasometer through the first heated tube into the flask, and thence through the second tube into the atmosphere. The result expressed in Schwann's own words is this. "In these experiments, also, of which several were made, there appeared no mould, no infusoria, no putrefaction. The flesh remained unaltered, and the liquid as clear as immediately after boiling."

Schwann then varied his method, and described, with the minutest detail, the nature of the variation. For the straight glass tube, he substituted a spiral one, and, for the bath of fusible metal, the flame of a spirit-lamp. The precautions which he thought it necessary to observe are described in the note to pp. 185 and 186 of his memoir. No man trained in the severities of experimental research can fail to see that Schwann is a master of the art of experiment. The result, again expressed in his own language, is this. "In this way, with a temperature of 14 deg. to 20 deg. Réaumur, I have kept such flasks for six weeks without putrefaction, infusoria, or mould, while, on opening the flask, the liquid putrefied in a few days." The writer adds a word of caution to which more confident, but less trained, experimenters would have done well to take heed.

These are the definite conclusions of Schwann; and it will now be understood why I at first deemed the citation of Schwann, in support of Dr. Bastian, simply unbelievable. It will, perhaps, be urged that it must be all a mistake; that Dr. Bastian must have meant some other paper or some other author. It would be a great relief to me to think so, but, unhappily, it is impossible. At the bottom of page 188 is a note, in which Schwann informs us that in an experiment conducted in a different fashion, and where the precautions above referred to could not be observed, the liquids fed with heated air sometimes fermented. He distinctly points out the source of this anomaly. He distinctly states that it is "leicht erklärlich". Stoppers had to be inserted in his flasks and removed from them while their necks were inverted in mercury. "In removing and restoring the stopper," he says, "a little unboiled organic substance could readily enter the flask".

It is on this result that Dr. Bastian founds his claim to quote Schwann, and it is on similar grounds he quotes others. The justification he offers for this proceeding is contained in the following words: "What explanations these investigators severally give of the facts they record need not now detain us, since it is the facts themselves which Professor Tyndall ignores, and, in fact, denies." It is a cause of real grief to me to find English science the matrice of a birth like this.

The note here referred to occurs on page 188 of Schwann's memoir. On page 191, he sums up his results without reference to the note, and the two first paragraphs of his summary run thus.

1. "A boiled organic substance, or a boiled liquid previously capable of fermentation, does not putrefy, and does not ferment when placed sufficiently long in contact with calcined atmospheric air.

2. "For putrefaction, as for fermentation, or more generally for processes in which new animals or plants appear, the introduction of either unboiled organic matter or uncalcined atmospheric air is necessary."

Comment is needless, and here, for the present, I must end. The public have already had enough of this desultory paper war. It is not by spurts of dialectic skill, but by patient, steadfast thought, and strict experiment, that any real insight of this subject is to be gained. In the quiet of the laboratory, and in view of those momentous interests which I believe to be imperilled by Dr. Bastian's errors, the investigation shall be carried on.

On one point I have reason to congratulate myself, and that is the liberal tone which the medical press, with few exceptions, has observed towards me. Our science, theoretic and practical, is an organism every part of which shares the life of the whole. Isolate any portion of it from the general circulation, and that portion is doomed to atrophy and death. That the physician and the physicist are mutually helpful units in this organism, will become more and more evident as time moves on.

Though the apparatus is now in disarray, I should willingly, if it were thought desirable, make arrangements to enable medical men to see my infusions, protected and unprotected. Words can scarcely convey a clear notion of the contrast between the hyaline purity of the one and the muddy turbidity of the other.

SEQUEL TO A NOTE ON MALTA FEVER.

By W. C. MACLEAN, M.D., C.B.,

Professor of Military Medicine in the Army Medical School, Netley.

SOME months ago, I addressed some observations to the JOURNAL on this remarkable form of fever, and referred to the case of a young medical officer at that time under treatment at Netley, suffering severely from some of the protean sequels of the disease. My young friend left Netley in the first days of October, and was removed to Edinburgh in an invalid carriage; and, although the neuralgic and rheumatic symptoms had much abated, he was terribly prostrated, incapable of locomotion, and quite anæmic. A few days ago, I received a letter from him, giving the history of his very sad suffering from the date of his leaving Netley to the present time. From this I give a few extracts, which will not only illustrate what I said before, but show how strongly the malarial element developed itself after he left Netley. While with us, he had a superabundance of febrile attacks; but they were not at all paroxysmal.

The patient has been advised to go to Karlsbad in May, advice strongly seconded by me, and afterwards to take a chalybeate course, which will, I have no doubt, as in many other instances, restore him to health. "Thinking you may be interested to hear of the progress of my somewhat unusual fever, I take the opportunity afforded by an intermission to tell you about it. I daresay you heard that, on reaching Edinburgh in October, I was attacked by a severe pleurisy, which well-nigh finished my history. I completely recovered from that, and began to pick up with wonderful rapidity, the fever having left me entirely. I soon regained the use of my legs, which you may remember was completely in abeyance at the time I was at Netley, and was able to go out walking a little. About November 30th, I was suddenly seized with a very severe attack of lumbago, the acute symptoms of which gave way in a few days to an attack of distinctly paroxysmal fever. This was utterly unlike anything I had ever suffered from in Malta or at Netley. Every day, about 4 P.M., I was seized with a cold stage, sometimes with marked rigors, sometimes only sensations of chilliness, lasting from twenty minutes to an hour, or an hour and a half. This was followed by great heat of skin, utter prostration, and frontal headache, with mental confusion, amounting sometimes to actual delirium. Sweating generally began at 9 P.M. At its first approach, I fell into a sudden quiet sleep, and awoke feeling calm and comfortable about the head, and almost free from fever. There were no actual intermissions, but very distinct remissions. The attack lasted about a week, and left me much pulled down. I soon regained strength, and was nearly quite myself again, when, on January 25th, I had a slight return of sciatica, quickly followed by fever; that is to say, I was feverish that night and on the following morning. At 1 P.M. on the 26th, I was seized with a very severe rigor, or rather, a regular ague, which lasted with considerable violence for about twenty minutes, followed by a short but severe hot stage. The paroxysm returned at 9 o'clock of the same night, and was also short but severe. On the following day, the usual paroxysm, slightly modified by quinine, came on at 4 o'clock, but was not repeated that day. Next day, the 4 P.M. paroxysm was slight. Yesterday, there was none, and to-day (January 29th) I enjoy a complete intermission. My spleen, which, if I remember aright, was little or not at all enlarged on landing from Malta, is now distinctly so, and has been ever since my last attack in November. My appetite is good, and I satisfy it well; but I am shockingly anæmic."

The history of this case, which is only a particularly severe example of many such sent from Malta, well bears out the remark of my colleague Mr. Webb, quoted in my last notice of "Malta Fever", that invalids from Malta are admitted into the Royal Victoria Hospital, as a rule, in a more shattered condition than those sent from India.

CASE OF ACCIDENTAL POISONING BY TINCTURE OF CANTHARIDES.

By W. R. SPENCE JEFFERISS, M.D., C.M. Edin.;

Parochial Medical Officer, Lochmaben.

CASES of poisoning, especially of an accidental nature, from cantharides, are very rare. Such a case has just occurred in my neighbourhood, and proved fatal to the unfortunate person. I beg to furnish the following particulars, in the hope that they may be of interest to members of the profession.

I was asked lately to visit Margaret Green, a pauper lunatic, who, I was informed, had gone into the druggist's shop here with the intention of stealing some laudanum. She had effectually done so on a former occasion; but this time she, from some unexplained cause, seized upon the wrong bottle; viz., one containing tincture of cantharides, of which she partook freely. On investigation, it was elicited that she must have taken three fluid ounces from the bottle.

When I saw the poor woman about two hours and a half after the accident, she was quite sensible of what had happened; and, when asked by me where she got the poison, she said, in the presence of witnesses, that she got it at the apothecary's; and, when further asked why she took it, she said that the lads (apprentices) in charge told her to take it.

I may here mention that the deceased was a lunatic, living in a private dwelling, under the care of her mother (sanctioned by the Lunacy Board); she had been in a weak state of mind for about twenty years, and subject to epileptic fits: all attributed to a fright. At each menstrual period, she became more excitable than usual, and had a series of fits; after the menstrual period was over, she became quiet, and almost quite rational, performing all the duties of her mother's house. She had never shown any suicidal or homicidal tendencies; and, although she was fond of drinking laudanum, she was never really known to take a poisonous dose, although she had frequent opportunities of doing so had she desired.

On the day she took the poison, I met her about noon, and spoke to her, and observed nothing excitable about her manner. Her mother informed me that she had menstruated about ten days previously, and that she was not unwell at the time of the accident. When I saw her after she had taken the poison, she was suffering much from a burning pain and constriction in the throat; her eyeballs were prominent; her lips and mouth were parched and bleached (blistered). She was salivating, retching, and vomiting a glairy tenacious mucus; her tongue was also bleached along the sides and tip, but was blackish-brown in the centre, and much swollen. The breathing was very irregular and imperfect, sometimes quick, sometimes slow, and at great intervals. The pulse at the wrist was full and bounding; the vessels of the head and neck were much congested. The poor creature appeared to be suffering great agony, as was indicated by her expression of countenance and the throwing about of her arms and body in her endeavours to obtain relief.

Vomiting being free, and the patient complaining much of pain in the gullet and great difficulty in swallowing, and considering the length of time since the poison had been swallowed, and the very soluble form of the drug, I was of opinion that emetics were unnecessary, and the use of the stomach-pump would have been fraught with danger (through mechanical injury to the weakened, and probably blistered, condition of the gullet and stomach). I, therefore, administered a draught of white of eggs, olive-oil, and warm milk, and encouraged the continued use of warm diluent drinks, with warm poultices to the throat, neck, breast, and stomach.

I again visited the patient in a few hours, and found her somewhat easier, so far as the burning pain in the throat went; and her mental condition was more composed. She was vomiting a coffee-ground-like substance, and complained of pain over the bladder, uterus, and kidneys, especially severe over the hypogastrium; she also appeared to have greater difficulty in swallowing: when fluids were given to her to drink, she kept them in her mouth for a while, and then allowed them to escape over her lips again. When persuaded to make an effort to swallow, she choked and spurted the whole out again. This was seven hours after swallowing the poison.

I prescribed a continuance of the remedies already in use, and the further use of fomentations over the bladder and kidneys. On the following morning, I found her no better, having passed a restless night. On inquiring whether she had passed urine or not, her attendants informed me that she had frequently tried to do so, but ineffectually, only passing a little blood instead of urine. The bowels had slightly acted,